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## Abstract

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## HOW CITY CHIEFS USE LOCAL INFORMATION TO INCREASE TAX COMPLIANCE IN THE D.R. CONGO

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July 31, 2020

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# 1 Introduction

There is increasing agreement about the importance of state capacity — including tax capacity — for economic development (Besley and Persson, 2009; Acemoglu and Robinson, 2017). But how fragile states build capacity remains a puzzle. Almost by definition, low-capacity states operate alongside a range of local and traditional elites (Michalopoulos and Papaioannou, 2013, 2015; Sanchez de la Sierra, 2019; Marchais et al., 2019). Whether these elites are an impediment or an asset to state modernization and to development is a subject of debate. Although local elites at times capture local politics (Anderson et al., 2015) and civil society (Acemoglu et al., 2014), there is growing interest in whether low-capacity states can collaborate with local elites to improve governance outcomes from law and order to public service delivery (Acemoglu et al., 2019; Basurto et al., 2019; Alatas et al., 2019). This paper explores whether fragile states can increase their fiscal capacity by delegating tax collection responsibilities to local elites.

Indeed, a fundamental decision facing rulers today and throughout history is whether to deploy their agents to collect taxes or to delegate collection to local elites.[1] In weak states, local elites are thought to have greater enforcement capacity because they have access to rich local information about taxpayers that state agents lack.[2] Collection by local elites is also thought to lower administrative costs, as there is no need to staff a tax office in every province.[3] The obvious tradeoff is that local elites are harder to monitor, and rulers may fear leakage eating into revenues as well as the risk of overzealous taxation causing real economic damage or even tax revolts (Stella, 1993). Since Weber (1922), scholars have therefore posited that a revenue-maximizing sovereign will tend to delegate tax collection to local elites when the state is weak,

[1] Importantly, the choice to engage state or local tax collectors is distinct from the choice of tax contract, and in this paper we focus on the former while holding contracts constant. Historically, there is a correlation between local collection and tax farmer contracts, in which private actors paid a fixed rent for the right to be the residual claimant on tax revenues. But, rulers also engaged local elites with wage and share contracts (Azabou and Nugent, 1988), particularly for direct tax collection. While high-powered tax farming contracts may have been efficient for indirect taxes, for which monitoring was more difficult due to the unpredictability of economic activity, rulers seldom used them for land and poll taxes, which led to a more predictable stream of revenue and thus made leakage easier for rulers to detect (Kiser, 1994). Thus, until the early 18th Century, tax farming was the norm for customs and excise taxes, while wage contracts prevailed for land and other direct taxes (Kiser, 1994).

[2] The informational advantages of local tax collectors noted in historical literature (Azabou and Nugent, 1988; Brewer, 1990; Kiser, 1994; Scott, 1998) resonates with the emphasis on third-party information as a precondition to high tax compliance in recent public finance literature (Kleven et al., 2011; Pomeranz, 2015; Naritomi, 2019).

[3] Levi (1989) emphasizes transaction costs of collection such as "negotiating agreements, measuring revenue sources, monitoring compliance, using agents and other middlemen, punishing the noncompliant" (p. 23). In 17th-Century England, Kiser (1994) estimates that tax administration costs amounted to roughly 20% of total revenue for state-administered customs taxes, while contracting with local elites reduced this cost to 8% (p. 303).

while relying on their own agents when the state is strong.[4] The key difference is that state collectors are thought to surpass local elites in enforcement capacity as the state's legal and informational apparatus expands and eventually outweighs the local informational advantage once enjoyed by local elites.[5] Consistent with this prediction, local elites continue to play an important role in tax collection today primarily in countries with weak or fragile states, many of them in sub-Saharan Africa.[6]

This paper investigates the tradeoff between local elites and state agents as tax collectors in the D.R. Congo, a low-capacity state seeking to raise revenue through property taxation. We study a policy experiment embedded in the Provincial Government of Kasaï Central's 2018 property tax campaign, which randomly assigned the 356 neighborhoods of the capital city of Kananga, spanning roughly 45,162 properties, to "Central" or "Local" collection. In Central neighborhoods, state agents hired by the provincial tax ministry were responsible for door-to-door collection, while in Local neighborhoods, local city chiefs were responsible. City chiefs are locally embedded elites who help resolve disputes over property, act as intermediaries between neighborhoods and the government, and oversee an informal labor tax, *salongo*, through which citizens maintain roads and other neighborhood infrastructure. Although they must be approved by the city government (with indefinite and often lifelong, heritable tenure), city chiefs receive no regular compensation and are typically high-status individuals who have lived for a long time in the neighborhood. City chiefs are thus analogous to the types of local elites whom states have engaged in tax enforcement historically and in many African countries today.[7]

Aside from the type of collector, all other aspects of tax collection — property registration and assessment, tax liabilities, the technologies used to issue receipts, collector compensation, etc. — were identical across treatments. Collectors first went door

[4] For example, in her seminal book, Levi (1989) discusses how pre-Augustan Rome had limited capacity in the peripheries and so delegated tax collection to provincial elites. After Augustus rationalized imperial administration, however, a more centralized state collection strategy became optimal because the greater enforcement capacity of state agents outweighed the higher transaction costs of collection (Levi, 1989, p. 79). Local elites frequently collected taxes in the medieval and early modern periods (Ertman, 1997), exemplified by the Ottoman Timar system (Azabou and Nugent, 1988) and land tax collection by English Justices of the Peace (Harriss, 1993; Kiser and Karceski, 2017). Modern state tax administration then emerged in England in the 18th Century (Brewer, 1990).

[5] Higher enforcement capacity of state collectors could result from deliberate past investments in fiscal and legal capacity (Besley and Persson, 2009), or from structural changes in the economy that create more third-party information available to tax authorities (Jensen, 2018).

[6] On the role of local and customary elites in taxation in Africa, see Mamdani (1996); Boone (2003); Baldwin (2015); Michalopoulos and Papaioannou (2015); Sanchez de la Sierra (2019).

[7] City chiefs are not customary chiefs, however, even though they share many characteristics. They are a common institution across Francophone Africa (de Russel, 1998; Boone, 2003; de Sardan et al., 2009; Honig, 2017; De Herdt and Titeca, 2019), and often play a role in property taxation (Nguema, 2005; Knebelmann et al., 2019).

to door registering properties and assessing if taxpayers were subject to a roughly $2 or $9 annual liability based on the quality of building materials.[8] Collectors then solicited the property tax, issuing receipts using handheld printers to payers. For households that could not pay, they scheduled follow-up tax visits. Consistent with standard tax ministry policy, both types of collector received compensation in proportion to the amount they deposited in the state account. By holding constant collector incentives and tax procedures, the experiment enables us to estimate the causal effect of tax collection by local elites rather than state agents.

According to administrative data, chiefs achieved 3.3 percentage-point higher tax compliance than state collectors.[9] This increase in compliance due to chief tax collection amounts to a 43% increase in property tax revenues. We rule out several alternative explanations for this result, including (i) that chiefs collected from properties that should have been exempted, and (ii) that awareness of (or competition with) other treatment arms motivated chiefs. To benchmark the magnitude of this effect, we examine cross-randomized enforcement messages on tax notices distributed by collectors.[10] This message treatment caused a percent increase in tax compliance roughly one fifth the size of the increase from delegating collection responsibilities to chiefs.

We then assess whether, despite increasing revenues, chief collection led to local mismanagement that could cause backlash against the government, consistent with principal-agent concerns (Kiser, 1994). According to multiple measures, city chiefs were about 2 percentage points more likely to collect bribes than state collectors. However, we find no evidence of adverse outcomes on other margins. According to third-party verification, chief collectors were in fact more accurate in assessing the liability of properties, and they were more likely to exempt the elderly and the disabled, as Congolese law requires. There is also no evidence that chief tax collection undermined citizens' views of the government. If anything, it increased self-reported trust in the formal state and crowded in payments of other taxes, such as market vendor fees — indicative of positive fiscal externalities. Finally, we find no evidence that delegating collection responsibilities to chiefs crowded out their other responsibilities, such as the administration of *salongo*.

Why did chief collectors achieve higher tax compliance than state collectors? We

[8] These liabilities represent an average tax rate of 0.32% of property value, according to machine-learning estimates (Bergeron et al., 2020a), comparable to the rate in certain US states. Fixed-rate property tax schemes are common in poor countries, which often lack up-to-date property valuation rolls, and have been used in rich ones, too.

[9] This estimate reflects our preferred specification, which includes house type and time fixed effects (cf. Section 5).

[10] A large literature finds compliance effects of similar enforcement messages embedded in tax letters. See for example: Blumenthal et al. (2001); Fellner et al. (2013); Pomeranz (2015); and (Scartascini and Castro, 2007).

explore three families of mechanisms. First, as residents of the neighborhoods in which they worked, chiefs might have conducted *more tax visits* after property registration, which could have increased compliance if households faced time-varying cash-on-hand constraints, or if more tax visits increased the perceived risk of enforcement. Examining treatment effects on reported visits from collectors, however, we find no differences on the extensive or intensive margin.[11]

A second possible mechanism is that chiefs were able to *target* potential tax payers more efficiently because they had better information about citizens' underlying payment propensities.[12] To investigate this mechanism, we examine a third, hybrid treatment arm, "Central + Local Information" (CLI), in which state agents collected taxes after a half-day consultation with the local chief. During these meetings, chiefs went line by line through the property register, indicating the ability and willingness to pay for *each* household in the neighborhood.[13] The meetings endeavored to transfer local knowledge about households' payment propensities from chiefs to state collectors. Comparing CLI to Central thus provides a direct test of whether more-informed targeting explains chief collectors' performance.

Central + Local Information achieved 2.2 percentage-point higher compliance than Central, but did not fully recover the gap with Local. State collectors in this arm appear to have generated higher compliance by changing which households they targeted in response to the chief's information, visiting and taxing those recommended by the chief with higher probability, conditional on households' observable characteristics. Indeed, comparing the characteristics of households visited by collectors after registration in each treatment arm, CLI resembles Local more than Central. Moreover, consultations with more informed chiefs — as measured by a short quiz-type survey module about a random selection of households in their neighborhood — led to larger compliance gains for state collectors in CLI. By contrast, as a placebo check, we show that there is no correlation between chiefs' knowledge and tax compliance in Central neighborhoods, where state collectors did not consult with chiefs.

A third possible family of mechanisms is that chiefs may have been better able to *persuade* households to pay, conditional on having visited them. Chiefs might have been better able to activate citizens' tax morale (Luttmer and Singhal, 2014) — e.g.,

[11] Citizens in Local also did not report more informal consultations with collectors (outside of official tax visits).

[12] This mechanism resonates with theoretical work on local elite tax collection (Levi, 1989) and recent empirical work about the importance of information in tax enforcement (Kleven et al., 2011; Pomeranz, 2015).

[13] We validate that chiefs were sincere and provided informative recommendations (rather than settling scores with local rivals, for instance) by showing that characteristics of "high type" properties identified during consultations also predict collector tax visits and compliance in the Local arm, where chiefs themselves were collecting taxes.

if they were more trusted, or had a closer link to public services — or more credibly threaten sanctions for non-compliance.[14] To test this possibility, we examine if chiefs still collected more tax when their targeting ability was neutralized. During property registration, collectors were required to take a linear, house-by-house route through neighborhoods, which eliminated any informational advantage about which households to target.[15] Tellingly, chiefs did not collect more tax than central agents during registration, as a persuasion channel would have predicted. Additional tests — examining heterogeneity by the baseline legitimacy and power of chiefs, and interactions with cross-randomized messages on tax notices — also provide little evidence in support of a persuasion mechanism.

Ultimately, then, the evidence suggests that chiefs outperformed state collectors because of informational advantages that enabled them to better target tax visits based on households' underlying payment propensities. In addition to identifying the likely mechanism behind our reduced-form results, this analysis sheds light on the benefits of — and limits to — leveraging local information to build state capacity. The fact that (i) Local realized higher tax compliance than CLI, and (ii) chiefs did not exhibit greater persuasive power suggests that some local information possessed by local elites is codifiable — and hence transferrable to state collectors — while other forms of local information relevant for tax collection are uncodifiable.[16] The idea that chiefs may have local contextual or tacit knowledge about households' payment propensities that they cannot easily codify and communicate to the state is consistent with scholarship across the social sciences since at least Polanyi (1958).

Having demonstrated the value of local information in tax collection, we examine its substantive content and the implications for the distribution of the tax burden. After property registration, chiefs were less likely than state collectors to visit high-quality properties — an observable characteristic — yet more likely to visit properties with attributes that predict compliance but which are difficult to observe, such as having owners with liquidity and positive views of the government. These differences in collectors' tax visit strategies shifted the distribution of the tax burden toward lower-quality properties in Local. Indeed, the higher compliance achieved by chiefs was en-

[14] Although it is unlikely that the threat of formal state sanctions would have been more credible from chiefs, they might have threatened informal sanctions, such as increased labor taxes.

[15] We validate that collectors complied with this protocol using GPS points and time stamps during registration.

[16] For instance, chiefs may also have information about the *timing* of households' cash-on-hand constraints, which could enhance chief tax collection but would be difficult to convey in a one-off consultation with state collectors. It is also possible that chiefs chose not to reveal certain information during consultations, though the available evidence suggests otherwise (cf. Section 9.2).

tirely concentrated among low-value band properties facing the $2 rate. Compliance in the high-value band was identical. However, according to survey estimates, taxpayers in Local did not have less income or liquidity than those in Central.[17] Moreover, we find no evidence that chief collection reduced household well-being more than state collection. Thus, the additional households whom chiefs brought into the tax net possessed lower-value properties — the asset thought to be the ultimate base of property taxation — and yet had willingness and ability to pay similar to other taxpayers.

All told, should low-capacity governments delegate tax collection responsibilities to local elites in urban and peri-urban areas? Chief collection raised more revenue — and proved 53% more cost-effective[18] — than state collection, yet it also increased bribes and was more regressive in terms of house quality (though not in terms of income or liquidity). In a simple theoretical framework, we estimate that the government would need to weight the social cost of $1 paid in bribes 15 times higher than the value of $1 in net revenues to prefer state to chief collection. We therefore conclude that, in the short run, fragile states seeking to establish rudimentary fiscal capacity could benefit from greater engagement with local elites.[19]

To our knowledge, this paper is the first to examine the tradeoff between employing state agents or local elites in tax collection in a randomized policy experiment. While governments have always confronted this tradeoff when setting tax policy (Levi, 1989; Kiser, 1994; Ertman, 1997), the Provincial Government of Kasaï Central's decision to randomize neighborhoods of Kananga into Central and Local collection allows us to estimate the causal effects of these models on state revenues, tax incidence, corruption, and views of the government. Closest in this regard is Khan et al. (2015), which studies the effects of tax farming contracts tying collectors' compensation to the tax they raise. This experiment, by contrast, holds contracts constant and studies variation in whether state agents or local elites were charged with collection responsibilities. Also closely related is Sanchez de la Sierra (2019), which explores conditions under which non-state actors establish tax capacity in fragile states.

More generally, the paper contributes to a growing literature on the role of local and

[17] The weak correlation between liquidity and wealth — of which property value is one measure — is common in African cities where urbanization has run apace yet real estate markets are illiquid (Fjeldstad et al., 2017).

[18] We estimate cost-effectiveness using tax administration data on collector transport and compensation.

[19] In the longer run, the informational advantages of local elites will likely be offset by more prevalent third-party information available to the tax ministry, due to growth of the formal sector (Jensen, 2018) and financial development (Gordon and Li, 2009), as well as cumulative investments in state capacity (Besley and Persson, 2009). Indeed, the historical record suggests that as economies modernize and as states acquire greater fiscal capacity, local elites have less of a role to play in tax collection (Levi, 1989; Brewer, 1990).

traditional elites in governance and development in low-capacity states. In particular, scholars have recently explored the importance of such elites in governance and politics,[20] law and conflict resolution (Acemoglu et al., 2019), land governance (Banerjee and Iyer, 2005; Goldstein and Udry, 2008; Boone, 2003), and the administration of development programs (Basurto et al., 2019; Alatas et al., 2019). Although a large observational literature notes the importance of local elites in tax collection in low-capacity states,[21] this topic has received less attention from empirical economists.[22] While most past work focuses on how local elites shape governance outcomes by allocating public resources to their clients or by leveraging a legitimacy that formal authorities lack, we identify their local information as a source of state capacity.[23]

The paper also contributes to the literature on taxation in developing countries.[24] Recent work highlights that tax policy choices, such as the use of different tax instruments, thought ex ante to be optimal can prove second-best in developing countries due to low enforcement capacity (Best et al., 2015). We extend this insight into the domain of tax administration by showing that the optimal choice of tax collector likely varies in developing countries as a function of state capacity. Past work has also underscored the importance of third-party information in enabling high levels of tax compliance,[25] which is a particular challenge in developing countries given high rates of informality and weak financial sectors (Jensen, 2018). This emphasis on information aligns with our evidence that chiefs' local knowledge is the key channel explaining their effectiveness as collectors. We advance this literature by illustrating (i) the value of information possessed by local elites in tax collection, and (ii) the returns — and limits — to the state's attempts to codify and transmit local information to its tax collectors.[26]

Our evidence on the informational advantages of local tax collectors also resonates with literature on informal institutions in developing countries. For instance, studies

[20] See, for example: Michalopoulos and Papaioannou (2013, 2015); Acemoglu et al. (2014); Anderson et al. (2015); Baldwin (2015); Sanchez de la Sierra (2019), and Marchais et al. (2019).

[21] See, for example: Levi (1989); Mamdani (1996); Boone (2003); Kiser and Karceski (2017). Acemoglu et al. (2014) and Glennerster et al. (2013) also note that paramount chiefs in Sierra Leone collect a range of formal and informal payments from citizens, but they do not focus on this topic.

[22] The exception, noted above, is Sanchez de la Sierra (2019), which examines non-state actors collecting taxes in lieu of the state, not in collaboration with the state.

[23] By revealing the value of local information in tax collection, our findings thus provide a useful foil for recent work on the use of technology to increase the informational capacity of the state (Muralidharan et al., 2016).

[24] Besley and Persson (2013) lays out a research agenda on taxation and development.

[25] Some of the key references include Kleven et al. (2011); Kleven and Waseem (2013); Pomeranz (2015); Brockmeyer and Hernandez (2016); Carrillo et al. (2017); and Naritomi (2019).

[26] With several important exceptions (Del Carpio, 2013; Khan et al., 2015; Okunogbe, 2019; Brockmeyer et al., 2019), this literature has also focused less on property taxation, a key revenue source for local governments.

of informal finance and insurance also focus on differences in information (Stiglitz, 1990; Varian, 1990) and enforcement (Besley and Coate, 1995) available to formal and informal actors. In contrast to evidence that formalization can crowd out valuable functions of informal institutions (Fafchamps and Lund, 2003; Townsend, 2011), we find no evidence that chiefs' role as tax collectors impacts their other responsibilities, such as the administration of *salongo*.

Finally, the paper extends recent work in the decentralization literature on the informational advantages of local bureaucrats (Dal Bó et al., 2018). Although delegating tax collection to city chiefs is not strictly speaking a form of decentralization, the theoretical issues it entails are analogous to the key tradeoff in decentralization between greater responsiveness of local government, thanks in part to local information, versus the risk of mismanagement (Bardhan and Mookherjee, 2006). We contribute to this literature by credibly estimating the returns to policies seeking to leverage local information to increase tax revenues mobilized by a higher level of government.

# 2 Setting

The D.R. Congo (DRC) is one of the most populous countries in Africa and also one of the poorest. Kananga, the capital of the Kasaï Central Province and the setting for this study, is a city with nearly 1 million inhabitants and an average monthly household income of $106 (PPP$168). The DRC is a low-capacity "fragile state," with tax-GDP ratio ranking 188 of 200 countries. In the years before this study, the Provincial Government of Kasaï Central had tax revenues equal to roughly $0.30 per person per year. Most provincial revenues come from national transfers. What provincial tax revenues there are come from levies on mining, gatekeeper-style fees on trade and transport, and property taxes enforced among a small set of firms in downtown Kananga. Heeding international advice about promising sources of revenue for local governments (Franzsen and McCluskey, 2017), the provincial government began to extend the property tax net by launching its first citywide collection campaign in 2016 (Weigel, 2020).

Public goods and services in Kananga are scarce and of low quality. Public schools charge fees that limit access among the poor (Paler et al., 2016). Almost no households have running water, and only 18% have any source of electricity (Table 3). Other public goods typically funded through local tax collection, such as road repair, are similarly underprovided. In sum, we study an equilibrium with low tax compliance, weak state capacity, and minimal service provision. This paper explores the government's attempts to escape this low-capacity trap by raising citizen tax compliance.

## 2.1 The 2018 Property Tax Campaign

The experiment we study was embedded in the 2018 property tax campaign in Kananga conducted by the Provincial Government of Kasaï Central. This section describes the rules and procedures of the tax campaign, which were identical across treatment arms. What varied across treatments, as we discuss in detail in Section 3.1, is whether state or chief collectors worked on the campaign.

**Training.** Before the campaign, collectors received training by the provincial tax ministry, conducted separately for state and chief collectors. The primary sessions, taught by the ministry's chief inspector, concerned the rules and protocols of property taxation in Kananga, including rates, exemptions, and fines for late payments. Collectors also learned to use handheld receipt printers. The inspector informed collectors that, in the foreseeable future, property tax collection would occur according to this aforementioned set of procedures and would be carried out by the same actors.

**Campaign Stages.** The campaign had two stages — property registration and tax visits — as summarized in Table 1. First, collectors in teams of two went door to door to construct an up-to-date *property register*. As in many developing settings, the government lacked a complete property valuation roll, and a recent conflict in early 2017 caused considerable in- and out-migration.[27] When registering households, collectors recorded information about the property owner, and assigned a unique tax ID. They also delivered tax notices to property owners showing the liability due and other information about the property tax (Figure A1). Collectors determined to which of two tax liabilities a property was subject based on the quality of the principal house's construction, as described in detail in the next subsection, or whether it was exempt.[28] Households' locations, tax IDs, and other details gathered by collectors were recorded by independent surveyors trained with GPS devices. Finally, during the registration visit, collectors solicited and, if the household could pay, collected the property tax. If households did not have the cash on hand, collectors made appointments for follow-up tax visits.

Second, after completing the neighborhood property register, the two assigned collectors returned to households for follow-up *tax visits* for the remainder of the month. They used printed copies of the new neighborhood property register, containing for

[27] Although the Kasaï region has historically been peaceful, fighting broke out in 2017 between the national government and *Kamuina Nsapu* militias, leaving thousands dead and hundreds of thousands displaced.

[28] Property tax exemptions, which make up 29% of properties in Kananga, include: (1) state-owned properties, (2) schools, churches, and scientific/philanthropic institutions, (3) properties owned by the elderly (55 years or above), widows or disabled people, and (4) properties with houses in construction.

each compound: the owner's name, its tax ID, rate, and exemption status. They were instructed during training to revisit households until they paid the tax during the assigned month.[29] Collectors used handheld receipt printers to issue receipts to taxpayers, with the transaction recorded in the device's memory and downloaded to the government database on a weekly basis. Collectors deposited tax revenues at the ministry, and were required to account for discrepancies with the receipt data.

**Timing.** The 2018 tax campaign ran from May to December. Collectors had one month to work in each assigned neighborhood. They completed registration in the first few days of the month and conducted follow-up tax visits for the remainder. Collectors typically worked in two neighborhoods simultaneously, alternating between them during the assigned month.

**Collector Compensation.** Consistent with standard practice at the tax ministry, all collectors received piece-rate compensation with two components. First, they received 30 Congolese Francs (CF) per house registered. Second, they received an average bonus of 30% of the total amount they submitted to the state account.[30] Collectors were also reimbursed for transportation expenses incurred while traveling between assigned neighborhoods and the tax ministry. The same compensation scheme applied across treatments.

**Tax Rates.** Rather than facing a property tax schedule that applies marginal tax rates to property value or the quality of property characteristics — common in high and middle-income countries (Khan et al., 2015; Brockmeyer et al., 2019) — properties in Kananga face flat, fixed fees according to two property value bands. Of the 45,162 registered properties in Kananga, 40,183 (89%) were classified in the *low-value band*, and 4,979 (11%) in the *high-value band*.[31] Low-value properties are those in which the principal building is made of non-durable materials, such as mudbricks. In 2018, such properties faced an annual official tax liability of 3,000 CF (roughly $2). By contrast, high-value properties, with structures made of cement or other durable materials, faced an official tax liability of 13,200 CF (roughly $9).[32] These liabilities represent an average tax rate of roughly 0.32% of property value, according to machine learning

[29] Actual rates of tax visits were at collectors' discretion and vary considerably, as discussed in Section 7.1.

[30] The magnitude of this bonus is analogous to that studied in Khan et al. (2015). Households were randomly assigned to a collector bonus of 30% the rate or a flat 750 CF, as discussed in Bergeron et al. (2020b). We show robustness to controlling for and interacting treatments with household-level collector bonuses. In 2018, $1 was worth roughly 1,500 CF.

[31] Additionally, 285 very high-value properties, classified as *villas*, are taxed according to a different schedule and procedure. They are thus outside the 2018 campaign and our evaluation.

[32] Cross-randomized within these categories, the government assigned certain households to partial rate reductions, the focus of a separate paper (Bergeron et al., 2020b).

estimates (Bergeron et al., 2020a). This is comparable to the property tax rate in certain U.S. states, which range from 0.27% to 2.35%. Simplified property taxation — here, a fixed annual fee — is common in settings of low state capacity, including India, Tanzania, Sierra Leone, Liberia, Malawi, and elsewhere (Franzsen and McCluskey, 2017).[33]

Delinquent properties are subject to fines equal to 1.5 times the original liability (plus arrears) and the possibility of a court summons. Although such sanctions are rare among residential property owners, a majority of citizens at baseline believed the government would likely enforce sanctions among those who refused payment.

# 3 Design

After its first citywide property tax campaign in 2016, in which agents of the tax ministry went door to door soliciting the tax, the Provincial Government of Kasaï Central reasoned that engaging local city chiefs in collection could increase revenues further.[34] To test this idea, we partnered with the government in the design and evaluation of a policy experiment varying the type of tax collector by neighborhood in the context of the 2018 property tax campaign.

## 3.1 Collector Treatments

**1. State Collectors (Central).** In Central neighborhoods, agents of the provincial tax ministry were charged with all campaign responsibilities.[35] State collectors in this arm were unsalaried contractors who frequently undertook work for the tax ministry and other parts of the provincial government. Some of these agents had worked on the 2016 property tax campaign; others had prior experience collecting firm taxes. The most productive collectors could expect to be competitive for full-time (salaried) positions at the tax ministry.[36] There were 50 such state collectors, who were almost entirely male, with an average age of 31 years and a high school education (Table A1). Collectors worked in teams of two, with each team randomly assigned to two neighborhoods per month. Every month collectors were re-randomized into pairs.

**2. Chief Collectors (Local).** In Local neighborhoods, city chiefs were charged with campaign responsibilities. These chiefs are locally embedded elite leaders whose

[33] The UK and Ireland have also experimented with similar property tax schemes in recent decades.

[34] According to the provincial finance minister, the idea came from emulating collection strategies in other parts of the DRC as well as heeding the 2016 collectors who had also recommended involving chiefs in future campaigns.

[35] This collector treatment is analogous to the 2016 property tax campaign studied in Weigel (2020).

[36] Indeed, several of the top collectors in the 2016 campaign subsequently took up full-time posts.

main responsibilities include: (i) mediating local disputes, especially over property; (ii) acting as an intermediary between citizens in the neighborhood and state authorities; and (iii) organizing a weekly informal labor tax in which citizens undertake local public goods provision (*salongo*). The position is technically approved by city government authorities, but chiefs have indefinite and often lifelong tenure, which at times passes through families.[37] Indeed, according to our surveys, the primary qualification for becoming chief is being a well-known, respected, long-time resident in the neighborhood. Chiefs do not receive regular salaries and often hold other remunerative positions, e.g., as teachers or pastors. The main benefit of being chief, then, is the status it confers. Although they share many characteristics with customary chiefs — including land dispute mediation, informal labor tax administration, and long-lasting, sometimes heritable tenure — city chiefs are a distinct institution that is common across Francophone Africa. Known as *chefs d'avenue, chefs de localité,* or *chefs de quartier*, such chiefs frequently play a role in property tax collection.[38]

The 111 chiefs who worked on the tax campaign were 95% male.[39] The average chief was 59 and had completed 13 years of education. Chiefs thus differ in several ways from state collectors (Table A1): they are older, less educated, and less wealthy. They also tend to have less trust in the provincial government, and they are less certain that taxation is important for Kananga's development. These demographic and attitudinal differences should work against chiefs as tax collectors. Indeed, education and wealth are positively correlated with effectiveness as a collector (i.e. the tax compliance rate in assigned neighborhoods), as are tax morale and views of the government (Figure A4). Each chief had a local assistant who completed the training and worked on each step of the campaign.[40] Collectors thus always work in teams of two across all treatment arms.

**3. Central + Local Information (CLI).** This arm is identical to Central with one addition. After completing property registration, but before follow-up tax visits,

[37] The average city chief in Kananga had worked in the position for 10 years, and 19% of chiefs inherited the position from a family member.

[38] For instance, other scholars have studied city chiefs in Senegal, Cote d'Ivoire, Niger, Cameroon, other parts of DRC, and elsewhere (de Russel, 1998; Nguema, 2005; de Sardan et al., 2009; De Herdt and Titeca, 2019; Knebelmann et al., 2019). City chiefs are perhaps best described as "neo-customary," fusing elements of modern and traditional political institutions (Boone, 2003). The institution officially came into being with a 1972 law that was part of President Mobutu's state modernization initiative seeking to integrate customary authority into the prefectorial apparatus (Nzongola-Ntalaja, 1975).

[39] In neighborhoods with multiple chiefs — e.g., with multiple principal avenues — the chief with the larger jurisdiction worked on the campaign. Section A2.3 provides more details about such cases.

[40] Assistants hailed from the neighborhood and were accustomed to helping with the chief's responsibilities.

state collectors met with the neighborhood chief for a consultation about potential taxpayers. During this meeting, the chief and state collectors went through the register line by line, guided by owners' names as well as photos of each compound. For each property, the chief indicated the owner's (i) ability and (ii) willingness to pay, each on a three-point scale.[41] The collectors recorded the chief's recommendations next to the relevant line on their paper property register. After the meeting, armed with the chief's information, collectors resumed follow-up tax visits. This treatment arm helps isolate the value of local information for tax collection.

**4. Central X Local (CXL).** In this arm, one state and one chief collector worked together on the campaign. The other rules and procedures of tax collection remained as above. State collectors were re-assigned randomly to new neighborhoods (with different chiefs) each month. This arm represents a policy-relevant hybrid collection strategy, given potential complementarities between chief and state collectors.

**5. Pure Control.** A handful of neighborhoods were assigned to keep the old 'declarative' system (the status quo until 2016), in which individuals were supposed to pay themselves at the tax ministry. In this arm, two agents from the tax ministry conducted the property register, assigned tax IDs, and distributed tax letters as in other neighborhoods. The exception was that property owners were informed that they should pay themselves at the tax ministry. Although we focus on the comparison between Central and Local, this arm provides a benchmark of whether providing information alone is sufficient to stimulate tax compliance.

Table 2 shows the allocation of neighborhoods (and properties) by treatment. The same number of neighborhoods were assigned to Central and Local, our main comparison. Fewer neighborhoods were assigned to CLI and CXL given that these arms were secondary comparisons designed as potentially policy-relevant hybrid collection strategies that would also shed light on potential mechanisms behind any observed differences between Central and Local. Only five neighborhoods were allocated to Control because evidence from the 2016 campaign suggested compliance would be near zero (Weigel, 2020), and thus it would not take a large sample to reject a difference with other arms. Due to an implementation error, one neighborhood randomly assigned to CXL received the Local treatment.[42]

[41] This second quality — willingness to pay — was added as an explicit category to be addressed by the chief and recorded by state collectors midway through the campaign, after chiefs themselves pointed out that ability to pay is not the only relevant dimension.

[42] We use the de facto assignment throughout and show robustness to dropping this neighborhood in Table A6.

## 3.2 Randomization

The unit of randomization is the neighborhood (Figure A2), defined using a satellite map to approximate the finest administrative unit, the *localité*. Boundaries are roads, ravines, and other features easily identifiable from the ground. Of the 364 neighborhoods in Kananga, we excluded 8 that were the site of a logistics pilot several weeks before the campaign launch (cf. Section A2.4), leaving 356 neighborhoods for the randomization.[43] We use a block-randomized design and stratify on (1) geographic location, (2) treatment status in the previous property tax campaign, (3) past experience of the city chief with tax collection.[44] To avoid chance imbalances, we followed Banerjee et al. (2017) and ran the full randomization 100 times, selecting the run with minimum t-statistics from a series of balance checks on eight variables.[45]

## 3.3 Balance

Table 3 summarizes a series of balance checks. In Panel A, we consider a range of property owner characteristics collected at baseline and midline.[46] In Panel B, we consider property characteristics, as measured in the property register and in the midline survey. In Panel C, we consider key neighborhood characteristics. Overall, only one variable (years of education) is systematically different compared to the Central arm based on simple $t$-tests, as one would expect under random assignment.[47] In Table A2, we report tests of the omnibus null hypothesis that the treatment effects for the variables in Table 3 are all zero using parametric $F$ tests for bilateral treatment comparisons. Comparing Local to Central, we fail to reject the null for baseline characteristics ($F = 1.08, p = 0.37$), registration and midline characteristics ($F = 0.98, p = 0.47$), and neighborhood characteristics ($F = 0.39, p = 0.68$).[48] In this bilateral Local v. Central comparison, one covariate (distance to schools) is imbalanced at the 10% level.[49]

[43] These neighborhood counts exclude the commune of Nganza, where the *Kamuina Nsapu* violence in 2017 was most severe, and the government judged it impossible to collect taxes.

[44] Section A2.1 contains detailed descriptions of these variables used to construct randomization strata.

[45] These include neighborhood-level baseline averages in terms of (1) education, (2) proximity to a ravine, (3) quality of house walls, (4) knowledge of the chief, (5) perceived responsiveness of the chief, (6) tax compliance in 2016, (7) conflict-affectedness, and (8) the number of chiefs active in the neighborhood.

[46] We provide more details on the baseline and midline survey in Section 4.

[47] Table A3 alternatively reports balance tests relative to the Pure Control arm.

[48] We run these tests separately by the sources of variables to allow the maximum number of observations to be included in the joint tests. For midline variables we include variables from registration. We fail to reject the null for all other bilateral treatment comparisons of the CLI and CXL treatments to the Central treatment, except for midline characteristics in the CLI v. Central comparison. However, tests for baseline and neighborhood characteristics, which provide a richer set of data on households, are insignificant for this comparison, and we include robustness checks of CLI v. Central comparisons controlling for imbalanced covariates in Table A13.

[49] In Table A6, we re-estimate the main results controlling for this covariate.

# 4 Data

We use administrative data generated during registration and tax collection as well as three household surveys. Table 1 summarizes these data sources.

## Administrative data

Property registration generates data on the set of potential taxpayers in each neighborhood. Registration data, covering 45,162 properties, include tax ID numbers, geographic coordinates, property owner names, property classifications (cf. Section 2.1), exemption status, tax rates, and other notes on registration visits, such as whether the owner paid. The handheld receipt printers used by tax collectors stored details of each transaction in their memory.[50] These data were integrated directly into the government's tax database. The printers recorded the collector's name, a time stamp, neighborhood number, tax ID, property value band, tax rate, and amount paid. By matching payment records to registration data using tax IDs, we observe property tax compliance and revenues — our main outcomes — in the universe of registered properties.

## Household surveys

Enumerators working for the research team administered baseline surveys to 4,343 randomly selected households from July to December in 2017. Enumerators randomly sampled households by visiting every $X^{th}$ house, where $X$ was determined by the estimated number of houses in the neighborhood to yield 12 surveys per neighborhood. The baseline survey covered demographics, taxation, politics and governance, views of and engagement with chiefs, and social networks.

Enumerators then administered a midline survey at every compound in Kananga two to four weeks after tax collection had finished in a neighborhood. This survey asked households about their experiences in the tax campaign, including the number of visits from collectors, any reported payments (formal or informal), and whether any receipts were issued. We have 35,650 complete midline surveys.[51]

Finally, from March to September, in 2019, enumerators successfully tracked 3,950 baseline respondents to complete the endline survey. Attrition from baseline to endline

[50] If citizens chose to visit the tax ministry themselves to pay — required in Pure Control, but possible everywhere — an official there similarly issued a receipt, such that these transactions appear in the administrative data.

[51] More registration surveys were completed than midline surveys because the former includes all compounds, including (exempted) government buildings, churches, and empty lots, while enumerators only conducted midline surveys at privately owned plots liable for the property tax. In addition, enumerators at times were unable to find respondents to take the midline survey.

was 8.3% and is balanced across the Central, Local, and CLI treatments (Table 3).[52] In cases in which the baseline respondent was traveling or unavailable to complete the endline survey for more than 3 weeks, enumerators surveyed another member of the household (12% of respondents). The topics were analogous to the baseline survey.

# 5 Estimation

We primarily use OLS to compare Local to Central:

$$y_{ijkt} = \beta_0 + \beta Local_{jkt} + \mathbf{X}_{ijk}\mathbf{\Gamma} + \alpha_k + \theta_t + \varepsilon_{ijkt} \quad (1)$$

where $i$ indexes individuals, $j$ neighborhoods, $k$ randomization strata, and $t$ campaign time periods. Standard errors are clustered at the neighborhood level (356 in total). $y_{ijkt}$ is the outcome of interest (e.g., tax compliance), $\alpha_k$ are stratum fixed effects, $\theta_t$ are time fixed effects (discussed below), and $\mathbf{X}_{ijk}$ is a covariate vector. The main analyses contain only dummies for house type (low- or high-value band), and robustness checks in the Appendix (e.g., Table A6) include different vectors of covariates, as specified in the pre-analysis plan.

Although our main results table contains a specification without $\theta_t$, our preferred specification when examining tax outcomes includes time fixed effects corresponding to waves of the tax campaign to net out time trends in tax compliance that occurred over the course of the 2018 tax campaign for reasons unrelated to collector characteristics.[53] These fixed effects are important because (i) there were significant trends in tax compliance in 2018, and (ii) treatment arms were not all implemented simultaneously but in a staggered fashion over time.[54] Although the staggered rollout ensures considerable overlap in time across treatments, some time imbalance remains and affects our estimates. Figures A5 and A6 show this issue visually. Including fixed effects for time periods corresponding to waves of the campaign helps restrict our analysis to periods with sufficient overlap among the treatments under comparison.[55] For robustness, we

[52] The most common reasons for attrition include moving from Kananga (37%), traveling (35%), being ill or deceased (15%), and refusing to participate without a reason (13%). Attrition is lower in the CXL treatment; yet, it is not significantly different from the Pure Control group (Table A3). Moreover, we do not examine impacts of CXL on endline measures in this paper, so do not undertake adjustments for this attrition.

[53] We do not include time fixed effects when examining outcomes from the endline survey, which were collected in all neighborhoods *after* the tax campaign. We also exclude house type fixed effects when examining endline outcomes to avoid matching survey and registration data on tax IDs, which reduces our endline sample size.

[54] The trends in compliance do not reflect collectors working in "easy" neighborhoods first because the timing of collection by neighborhood was randomly assigned. Treatments were staggered primarily for logistical reasons, given the fixed number of state collectors and campaign supervisors at the tax ministry.

[55] Because it maximizes time balance on both ends, our preferred fixed effects are two months in length, beginning

also consider five alternative approaches to dealing with time imbalance, which we discuss in Section A2.5.[56]

# 6 Main Results

## 6.1 Effects on Tax Compliance and Revenues

We first compare tax compliance and revenue in Central and Local by estimating Equation 1 with OLS. Our household-level measures of tax compliance and revenue come from administrative data on the universe of registered properties, as noted in Section 4. Table 4 summarizes the results, with Column 1 unadjusted for time imbalance and Column 2 containing our preferred specification with time fixed effects. According to this specification, chief tax collectors achieved tax compliance of 9.5% compared to 6.3% in Central, a 3.2 percentage-point increase.[57] This translates into an additional 79.9 Congolese Francs per property, a 43% increase relative to Central.[58]

Although average compliance may appear low, it is analogous to property tax compliance in the capital cities — where compliance is generally higher — of many low-income countries.[59] Moreover, 2018 was only the second time the government had solicited the great majority of citizens to pay the property tax, or any formal tax. Top tax officials view their goal as the creation of a "fiscal culture" in Kananga, whereby citizens who enter the tax net today will feel more obligated to pay taxes again tomorrow.[60] These compliance numbers must then be considered in the context of a fragile

on the midpoint between the first days of the two treatments being compared, and end on the midpoint between their last days. However, strictly speaking, when a two-month period starts (and ends) is arbitrary for the purposes of including time fixed effects, so for the main outcomes, we also run and report our estimations using fixed effects defined at every possible start date (Figure A7).

[56] These robustness checks include: (1) adding two-month fixed effects defined by selecting the median estimate among all permutations of the start date, (2) using an interaction weighted estimator (Gibbons et al., 2018), (3) including one-month fixed effects, (4) trimming observations on either end if comparison treatments were not also active, and (5) using coarsened exact matching to identify clusters of comparable observations across treatments (Iacus et al., 2012).

[57] If we exclude exempted properties, these numbers increase to 11.3% and 7.3%, respectively. As discussed below, we do not condition on exemptions in the main estimations because exemptions were at collectors' discretion and thus potentially an outcome of treatment.

[58] As a comparison, in the Pure Control arm, where households were asked to pay at the ministry themselves, tax compliance was 0.1%, far lower than all treatment arms.

[59] For example, property tax compliance is approximately 7% in Haiti (Krause, 2020), 7.7% in Liberia (Okunogbe, 2019), 12% in Senegal (Cogneau et al., 2020), and 25% in Ghana (Dzansi et al., 2020). Each of these estimates reflects property tax compliance in national capitals, which tend to have higher compliance due to greater economic activity and enforcement capacity (Franzsen and McCluskey, 2017). By contrast, Kananga is the 4$^{th}$ largest city in the DRC.

[60] A study of tax holidays in Uruguay indeed finds that paying taxes can be habit forming (Dunning et al., 2015).

state attempting to initiate formal taxation as a source of revenue.[61]

For robustness, we re-estimate the results after collapsing the data to the neighborhood level (Column 3).[62] We also re-estimate the results with and without fixed effects for low or high-value band properties (Column 4).[63] Further, in Table A4, we re-estimate Equation 1 using each of the adjustments for time imbalance described in Section A2.5, which yield similar estimates to our preferred specification. In Table A5 we estimate a fully saturated model with dummies for cross-randomized treatment arms and their interactions with the Local treatment.[64] Finally, we explore a range of robustness checks in Table A6, including (i) controlling for basic covariates (age, age squared, and gender), (ii) controlling for basic covariates plus proximity to schools (the imbalanced covariate in the Local v. Central comparison), (iii) controlling for further socioeconomic covariates, (iv) re-estimating results including pilot neighborhoods, (v) excluding the neighborhood misassigned to Local, and (vi) re-estimating results at the neighborhood level after winsorizing the top 10% of outcomes.

To benchmark the magnitude of the difference between Local and Central, we compare it to the effect of a standard enforcement tax letter treatment.[65] As discussed in Section A2.2, tax letters distributed by collectors during registration contained randomized messages, one of which reminded households that they could face fines and be summoned to the tax ministry if they did not comply. Consistent with past work on tax letter interventions, we find that the enforcement message raised compliance (Table A21). Importantly, however, delegating responsibilities to chiefs caused a percent increase in compliance five times as large as this typical enforcement intervention.[66] The percent increase in revenues is also analogous to that caused by high-powered incentives (performance pay) for property tax collectors in Pakistan (Khan et al., 2015).

We rule out two potential alternative explanations for the reduced-form effect of Local on compliance and revenues. First, chiefs may have violated official tax proce-

[61] Moreover, given the government's meager budget and the resulting low level of public goods provision, the marginal value of additional tax revenue is likely very high in this context.

[62] This test is motivated by the concern that unequal numbers of units within clusters can introduce bias in cluster-randomized designs (Imai et al., 2009).

[63] We include house type (property band) fixed effects in most estimations to restrict treatment comparisons within property categories, while also reporting robustness to excluding these fixed effects.

[64] These cross-randomized treatments include property tax reductions and collector bonus amounts randomized at the property owner level (cf. Bergeron et al. (2020b)). The fully saturated model assuages the concern that treatment effects could be biased by interactions with cross-cutting treatments (Muralidharan et al., 2020).

[65] A large literature studies the effects of embedding enforcement messages in tax letters sent or delivered to taxpayers. See for instance: Blumenthal et al. (2001); Pomeranz (2015); and Scartascini and Castro (2007).

[66] Specifically, assignment to the state enforcement message increased compliance by 58% (Table A21, Column 3). By contrast, in the subsample of respondents who received a randomized tax message, which were introduced in the last phase of the campaign, chief collection increased compliance by 300% (Table A21, Column 1).

dures and collected taxes from property owners who were exempt.[67] In other words, the higher revenues achieved by chiefs could stem from unlawful collections. To investigate this possibility, we re-estimate the treatment effect among non-exempt properties, and find that its magnitude increases to 4.1 percentage points (Table 4, Column 5).[68] Chiefs were thus *more*, not less, likely to grant exemptions. As we examine in Section 6.2, chiefs were more likely to exempt disabled and elderly property owners, in accordance with the law. In contrast to the view of local chiefs as exploitative "local despots," the higher tax compliance achieved by chiefs appears to have arisen despite, not because of, their exemption choices.

A second concern is that awareness of other treatment arms, in which different types of collectors were working, could have generated competition (or demoralization) and thus artificially increased the treatment effect. For instance, chiefs might have sought to secure future tax responsibilities by demonstrating competence relative to state collectors. The mechanics of the campaign assuage such concerns, to some extent. Collectors in each treatment arm were trained independently to minimize cross-arm comparisons. During trainings, tax ministry leadership announced that the 2018 procedures, including the collector type by neighborhood, would remain in place for the foreseeable future. More formal evidence comes from estimating externalities by exploiting the cluster-randomized design, which generates random variation in the number of adjacent neighborhoods with different treatments. Following Miguel and Kremer (2004), we re-estimate the treatment effect while controlling for the number of previously or simultaneously active adjacent neighborhoods with contrasting collector types and the total number of adjacent neighborhoods (Table A8).[69] Having more adjacent neighborhoods in other treatments, in which the perceived "competition" between collectors would have been more salient, is not associated with higher tax compliance. We also estimate heterogeneous treatment effects by the number of randomly assigned adjacent neighborhoods with different collector types. This analysis provides little evidence to support the view that awareness of and competition with other treatments motivated chiefs' higher performance (or discouraged state collectors).

We then consider whether delegating property tax collection to city chiefs crowded in (or out) contributions to other formal or informal taxes. The most obvious potential

[67] This alternative is motivated by scholarship that views chiefs in sub-Saharan Africa as "local despots," corrupted by indirect colonial rule (Mamdani, 1996; Boone, 2003; Acemoglu et al., 2014).

[68] Our main specification does not condition on exemptions because exemptions were at the discretion of tax collectors, so this involves conditioning on an outcome and should be interpreted cautiously.

[69] Alternatively, in Column 5, we control for the length of borders shared with neighborhoods in different treatments as well as the total length of borders.

fiscal externality concerns informal labor taxes, which chiefs themselves administer. Indeed, past work finds that formalization can crowd out important functions of informal institutions.[70] The administration of *salongo* — an informal tax in which citizens contribute labor to help improve roads and other local public infrastructure — is among chiefs' most important responsibilities. In pure control neighborhoods at midline, 38% of individuals reported participating in *salongo*, for an average duration of 4.2 hours, in the past two weeks. We examine if chief tax collection impacted informal labor taxation by re-estimating Equation 1 with self-reported contributions to *salongo* at midline and endline as the outcome. We find no statistically significant treatment effects on the extensive or intensive margin in the short run — 2 weeks after tax collection — or longer run, 8 months after collection (Table A9).[71] According to our survey measures, then, delegating property tax collection to city chiefs did not meaningfully interact with informal labor taxation.

Although chiefs are not involved with the collection or enforcement of other formal taxes, their role in property tax collection could have formal fiscal externalities if it caused changes in tax morale, beliefs about enforcement, or if households have a fixed budget for all taxes. We thus re-estimate Equation 1 with self-reported compliance with the most commonly paid taxes in Kananga as the outcome. Assignment to chief tax collection caused more citizens to report having paid market vendor fees and the income tax at endline (Table A9). To test if these increases reflect experimenter demand effects, or other forms of cheap talk, we also included an obsolete tax in the list of taxes enumerators asked about in the endline survey.[72] We find no treatment effects on this obsolete tax. There is thus suggestive evidence of positive fiscal externalities, which may be attributable to improvements in views of the government caused by chief tax collection, discussed in the next section.

[70] For instance, crowd out of informal insurance and borrowing is a key theme in the literature on informal finance (Besley and Coate, 1995; Fafchamps and Lund, 2003; Townsend, 2011).

[71] To examine further if tax payment and *salongo* participation are substitutes, complements, or neither, we include an indicator for tax payment on the righthand side of the equation and interact it with a Local treatment indicator in the familiar Local and Central comparison (Table A10). Payment is an outcome and thus a 'bad control,' but it can nonetheless provide suggestive evidence of whether tax compliers are also contributing more or less labor. Paying the property tax and participating in *salongo* are positively correlated in Central but uncorrelated in Local. This result is consistent with certain compliant types both paying taxes and doing *salongo* when chiefs do not know who paid taxes (in Central), but chiefs permitting some, but not all, payers to avoid such double contributions when they are in charge of tax collection and thus aware of household-level compliance.

[72] The obsolete tax was the "poll tax," which existed in the past and so translates into Tshiluba (the main local language). The poll tax thus provides a credible, yet fictitious, tax with which we assess survey response bias.

## 6.2 Effects on Mismanagement and Views of the Government

A key concern in the historical literature (Kiser, 1994; Ertman, 1997) is that delegating collection responsibilities to local elites could fuel corruption and other forms of local mismanagement, thereby undermining the perceived legitimacy of the government. To investigate this possibility, we examine whether chief tax collectors were more likely to incorrectly assess or exempt properties or to collect bribes. We also test if they caused citizens to update negatively about the government.

First, we examine the degree to which collectors respected the official tax rules and protocols. They had discretion over two key assessment margins: exemptions and tax brackets (i.e. whether a property was classified in the low- or high-value band). For each, we compare the collectors' assessment with that of an independent enumerator informed of the official rules to identify deviations.[73] According to this measure, the exemption status of 4.9% of properties was determined incorrectly, and 2.4% of houses were incorrectly assessed. Comparing Central and Local, we find that chiefs were more likely to (correctly) exempt households (Table 5, Rows 1 and 2), and this is driven by higher exemptions of the elderly and disabled property owners (Table A7).[74] Chiefs were also more accurate with their assessments of house type (Table 5, Rows 3 and 4). If anything, then, chiefs appear to have respected these rules and procedures of the tax campaign more than state collectors.

We next examine bribe payments using three measures. First, in the midline and endline surveys, we ask property owners if they paid "transport" to the tax collector, a colloquial expression for bribes that is not taboo to discuss in Kananga (Reid and Weigel, 2017).[75] According to this measure, just shy of 2% of households reported paying bribes to collectors, and essentially all of these payments were made in lieu of, not in addition to, the tax.[76] In other words, these resemble collusive bribes, not extortion. Comparing treatment groups in Table 5 (Panel B), we find that chiefs were more likely to collect bribes (by 1.6 percentage points) according to the endline mea-

[73] Importantly, the official rules are simple and easy to verify. As noted above, low- and high-value properties are distinguished by the type of building materials, which is easily observable to enumerators. Similarly, exemptions are straightforward and verifiable by enumerators speaking with household members. Even if there were measurement error in our detection of such mismanagement, it would likely be constant across treatments.

[74] Additionally, chiefs were not more likely to exempt members of the same tribe (Table A7). Chiefs were slightly more likely to exempt property owners who know them, but this effect is difficult to interpret because of large baseline differences in knowing collectors by treatment (43% in Local, 3% in Central).

[75] Indeed, Reid and Weigel (2017) report nearly half of mototaxi drivers openly admitting to bribing Kananga's toll officers. Similarly, 8.2% of baseline survey respondents reported paying bribes to officials in the last 12 months.

[76] Only 41 of the 491 property owners who reported paying a bribe at midline also paid the property tax according to the administrative data. The modal bribe was 1,000 CF, one third the official liability for low-value properties.

sure ($p$ = 0.051), but not the midline measure. While the midline sample is larger, enumerators may have been more trusted by endline respondents, whom they knew since baseline. To help resolve this disagreement, we examine another measure of bribery: the gap between administrative tax data and citizen self-reports of payments in the midline survey.[77] According to this measure, chiefs were 1.8 percentage points more likely to collect bribes ($p$ = 0.067), similar to endline estimate. Finally, as a last measure of bribes, the endline survey also asked if households had paid any other informal payments or fees in general (not limited to payments made during the property tax campaign).[78] Citizens were 3.1 percentage points more likely to report such payments in Local than Central. All told, it appears that chief tax collection increased bribe payments by between 1.6 and 3.1 percentage points, consistent with reports of leakage from taxation by local elites historically (Kiser, 1994; Ertman, 1997).[79]

Finally, we examine how chief tax collection impacted views of the government, of chiefs themselves, and of taxation overall, drawing on data from the endline survey.[80] We again estimate Equation 1, this time controlling for each respondent's baseline belief, where we have repeat measures.[81] We find no evidence that empowering city chiefs to collect taxes undermined the perceived legitimacy of the government (Table 5, Panel C). An aggregate index of views of the government is in fact positive but not statistically different from zero. If anything, self-reported trust in the government increased by 0.127 standard deviations — but given the null results for the overall index, this increase is only suggestive. Similarly, chief tax collection did not change citizens' overall views of chiefs (Panel D). With all of these analyses, we can only rule out effects larger than about 0.1 standard deviations. It is thus of course possible that there were smaller treatment effects that we are not sufficiently powered to detect. Regarding views of taxation (Panel E), we find that citizens in Local perceived that more people on their avenue paid the property tax, which mirrors our main results. We find no statistically significant changes in trust in the tax ministry, the perceived fairness of property taxation, tax morale, or in perceptions about the probability of

[77] This is also an imperfect measure because it conflates corruption and social desirability bias — households claiming to have paid the tax when in fact they did not — so the level should be interpreted as an upper bound. However, assuming cheap talk is constant across treatments, estimating causal effects with this measure will generate unbiased estimates.

[78] Again, while the level of this variable will capture more than bribes paid to property tax collectors, the difference across treatments should isolate additional bribes caused by empowering chiefs to collect taxes.

[79] We consider theoretical explanations for more frequent bribe payments to chiefs, including differences in bargaining power and in the costs of detection, in Section A3.1.

[80] Detailed variable explanations, standardized to facilitate interpretation of magnitudes, are in Section A2.6.

[81] We have baseline values for all variables except *Perceived tax compliance* and *Fairness of property taxation*.

sanctions for the non-compliant.

Ultimately, accompanying the increase in revenues, chief tax collection appears to have increased bribes. But we find little evidence that chiefs abused their responsibilities in other ways or damaged citizens' views of the government.

# 7 Mechanisms

Why did chiefs outperform state collectors in raising revenue? This section considers three potential channels: (1) chiefs made *more tax visits* to households than state collectors; (2) chiefs could more efficiently *target* their visits to households with higher payment propensity using local information; or (3) chiefs could better *persuade* citizens to pay, conditional on having visited them, because they could activate their tax morale or more credibly threaten sanctions for non-compliance.

## 7.1 More Tax Visits

The first possible mechanism is that chief collectors simply made more follow-up tax visits after property registration — which henceforth we refer to as "tax visits" — than state collectors.[82] Chiefs hailed from the neighborhoods in which they worked, whereas state collectors were dispatched from the city center to assigned neighborhoods by motorbike and rarely collected in their own neighborhoods. Even though state agents' transport costs were covered (up to one trip per day), it may have been easier for chiefs to make additional tax visits. More visits on the extensive margin could have raised compliance as more potential payers were solicited. More visits on the intensive margin could have increased compliance by (i) improving the chances that households have had on hand at the time of visit, or (ii) causing citizens to update their beliefs about enforcement and view tax payment as unavoidable.

To investigate this channel, we examine differences in tax visits by collectors, as reported by citizens during the midline survey. Comparing Local to Central, we find no difference in visits on the extensive margin — whether collectors ever returned after registration — or intensive margin, the number of times collectors returned after registration (Table 6, Columns 1–2).[83] Could chiefs have encountered citizens by chance in the neighborhood and asked them about taxes then, in such a way that would not

[82] To be clear, tax visits exclude collectors' initial visits to households for property registration. According to campaign protocols, registration visits occurred at essentially all properties — which we verify using GPS points in the property register — and thus could not explain differences across treatments.

[83] The fact that chiefs did not do more tax visits likely reflects the fact that tax collection is difficult work. Kananga is hilly, hot, and the roads are bad. Chiefs are also on average 28 years older than state collectors.

register when asking households about official collector visits? To check, we examine whether citizens reported talking to collectors about the property tax outside of their home visits. We find no evidence of more informal contact with collectors on the extensive or intensive margin (Table 6, Columns 3–4).[84] Chief tax collectors do not appear to have achieved higher tax compliance by making more tax appeals.

## 7.2 Targeting

Conditional on making similar numbers of tax visits, chiefs may possess local information about property owners that enabled them to target those with higher propensity to pay. For instance, imagine that chiefs observe a more accurate signal about each household's payment propensity compared to state collectors. Then, if both types of collector simply ranked households by payment propensity and visited them in this order, chiefs would achieve higher compliance — assuming (a) they visited the same number of households after registration, as noted above, and (b) collectors did not visit every household in a neighborhood, which we confirm in the data.[85] We discuss this logic more formally in Section A3.1 and express it visually in Figure A18.[86]

To motivate this mechanism, we first compare knowledge levels about taxpayers among chief and state collectors using a quiz-type survey module after the tax campaign concluded. Both types of collectors were shown photos of a set of randomly selected property owners in the chief's neighborhood and asked to provide their (1) names, (2) jobs, and (3) education levels. We know the correct answers to these questions from household surveys, and can therefore estimate a knowledge index for each collector-neighborhood dyad.[87] Chiefs were indeed much better informed about the residents of their neighborhoods than state collectors, scoring 73% more accurately on this quiz (Figure A8). We thus examine whether this wedge in local knowledge may explain the higher compliance realized by chief tax collectors.

As a first test, we consider evidence from the hybrid Central + Local Information (CLI) treatment arm, in which state collectors consulted with chiefs about the ability

[84] We report robustness to excluding property type fixed effects in Table A11.

[85] On average, 43% of households reported any tax visits after registration.

[86] We also outline conditions under which chief and state collectors would choose the same number of tax visits, conditional on the former having informational advantages over the latter. The key assumption is that chiefs have higher marginal costs of making tax visits than state collectors, which we find reasonable because (i) chiefs were nearly 30 years older on average, and (ii) chiefs likely have higher opportunity costs given given their other responsibilities.

[87] Chiefs took the "quiz" for their neighborhood, while state collectors took it for neighborhoods where they had not worked to estimate the knowledge they would have had at the outset of the campaign. On average, 2.5 state collectors took the knowledge test for each neighborhood, for whom we compute the average accuracy and compare this to the local chief's score.

and willingness to pay of each property owner in the neighborhood. State collectors could then use chiefs' information about these less observable margins of payment propensity to target their tax visits (conducted without the chief),[88] offering a direct test of this mechanism.

We compare tax compliance and revenues in CLI and Central, using an analogous specification to Equation 1, except that instead of the $Local_{jkt}$ indicator we substitute a $CLI_{jkt}$ indicator.[89] On average, CLI outperformed Central in compliance and revenues (Table 7). When armed with chiefs' information, collectors achieved 2.4 percentage-point higher compliance and 31.7% higher revenues. Importantly, CLI collectors did not conduct more tax visits on the extensive or intensive margin (Columns 3-4). Rather, they were more successful in collecting taxes at the houses they chose to visit (Column 5), consistent with a shift in the targeting of their tax visits.

If targeting were the only mechanism, and if chief consultations perfectly transmitted all relevant information to state collectors, then CLI would have completely recovered the gap between Central and Local. This was not the case: chiefs also outperformed "informed" state collectors in CLI (Table 7, Column 7).[90] There may thus have been other dimensions of chiefs' information useful for targeting tax visits that were not transmitted during consultations, or other mechanisms also at work.

To investigate further if the higher compliance in CLI relative to Central reflects collectors using chiefs' information to target households more efficiently, we consider several pieces of evidence. First, state collectors were indeed more likely to visit and to collect taxes from households recommended by chiefs as having high ability or willingness to pay (Table 8, Columns 1–2).[91] This positive association is robust to controlling for observable house characteristics (Columns 3–4), such as the quality of roof and walls and the proximity to a ravine ("erosion threat"), which (uninformed) state collectors could also use when targeting tax visits. State collectors in CLI thus appear to have followed chiefs' advice when choosing whom to visit after registration. Indeed, consistent with a compositional shift in the types of properties targeted by state collectors in CLI, the characteristics of households reporting tax visits in CLI resemble

[88] We confirm in household surveys that chiefs did not work with state collectors after the consultation.

[89] Table 7 shows estimates from our preferred specification with time fixed effects. The difference in coefficients for CLI in Columns 1 and 6 derives from a change in the definition of the time period fixed effects described in Section 5, which are defined based on the start and end date of the treatments being compared. Thus, when Local is included the time period definition changes to account for the trends in compliance over the full period under examination. Table A12 shows the aforementioned alternative specifications for robustness. Table A13 also shows results while controlling for imbalanced midline covariates.

[90] The gap between CLI and Local is also evident in Figure A5.

[91] These estimates reflect our preferred specification; we show results excluding house fixed effects in Table A14.

those in Local more closely than those in Central.[92]

Second, we verify that the information provided by chiefs about households' payment propensities predicts high-compliance households. Chiefs could have used the opportunity to settle scores with local rivals, for instance, rather than internalizing the state collectors' objective. The fact that recommended households were more likely to comply conditional on observable house characteristics (Table 8, Column 4) is suggestive that chiefs' information captures less observable components of payment propensity. But higher compliance among recommended households is endogenous to collectors changing their tax visit strategy to follow chiefs' advice. We therefore examine if recommended households were still more likely to pay when restricting the sample to all households that received tax visits.[93] Tellingly, among this set of households that state collectors chose to visit after registration, a one-point increase in the chief's ability-to-pay ranking is associated with an 8.3 percentage-point increase in the probability of payment (Column 5).[94]

Moreover, the properties recommended by chiefs in CLI resemble those whom chiefs themselves visited after registration when working as collectors in Local neighborhoods. For this analysis, we predict the properties that chiefs would have recommended in Local and Central using a propensity score approach on a set of household characteristics measured in surveys.[95] These predicted chief recommendations align closely with the households that chiefs did in fact visit and collect from in Local neighborhoods, even conditional on observable house characteristics (Table 8, Columns 6–7).[96] By contrast, predicted chief recommendations are uncorrelated with visits in Central, highlighting again the different set of households targeted by informed (CLI) and uninformed (Central) state collectors (Columns 8–9). Yet the predicted chief recommendations do correlate with tax compliance in Central. This empirical pattern has an intuitive interpretation: if state collectors in Central happened upon one of these high-propensity households, the owner would still be more likely to pay; but, absent

[92] Section 8.1 characterizes the distributional implications of chief collection in detail, and Figure 1 shows the similarity in characteristics of households visited by CLI and Local relative to Central.

[93] This analysis should be taken with a grain of salt because it involves conditioning on an outcome (tax visits).

[94] The corresponding estimate for the chief's willingness to pay ranking is 5.8 percentage points.

[95] Specifically, following Alatas et al. (2012), we regress chiefs' payment propensity scores on a range of household characteristics. We store the coefficients for all statistically significant characteristics and use these to predict how the chief would have scored each property in other treatment arms where no consultations in fact took place. These characteristics include the age and gender of the property owner, whether the property owner is employed, salaried, or works for the government, and whether the property owner is from the majority tribe. We then bin this predicted measure into a 1-3 rank to be analogous to the CLI measure. It is this predicted measure that we correlate with tax visits and tax compliance in Columns 6–9 of Table 8.

[96] Importantly, house quality and erosion threat were not included in the prediction procedure.

chiefs' information about whom to target, state collectors were not more likely to visit high-propensity households compared to other households in the neighborhood.

Third, if the transfer of local information to state collectors explains the gap between CLI and Central, then one would expect that consulting with more informed chiefs would lead to larger treatment effects. To investigate this possibility, we use our estimates of chiefs' local information levels from the knowledge quiz described above. We correlate chiefs' estimated knowledge of a neighborhood with the level of tax compliance achieved by state collectors in CLI. State collectors who consulted chiefs with above-median knowledge achieved 2.8 percentage-point higher tax compliance (significant at the 10% level) than those who consulted with less informed chiefs (Table A15, Column 4).[97] By contrast, if we compare chiefs' knowledge and tax compliance in the Central arm — a placebo check since collectors in these neighborhoods did not consult with chiefs — there is no correlation (Column 2).[98] More informed chiefs appear to have indeed made better consultants.

Finally, another implication of the idea that local information enables better targeting of taxpayers is that state collectors should also be better at collecting taxes when assigned to work near their own homes. Random assignment of collectors in Central to neighborhoods generates variation in how far from their own houses state collectors worked. In Table A16, we regress visits and compliance on the distance between each state collector's house and the centroid of the assigned neighborhood. An additional kilometer is associated with a 0.3 percentage-point decrease in payment. This pattern is consistent with local information conferring a targeting advantage during collection.

Did state collectors rival chiefs when working in neighborhoods at a similar distance from their houses? To investigate this possibility, we calculate the maximum distance between city chiefs' houses and the limits of their neighborhood. We then identify the set of Central neighborhoods with at least one collector living within that distance, and compare it to Local. We find that chiefs still achieved 2.7 percentage-point higher compliance compared to Central neighborhoods with at least one "nearby" collector (Table A17). By contrast, the treatment effect rises to 3.4 percentage points when comparing Local to Central neighborhoods with no nearby state collectors. This finding could be explained by the fact that chiefs' information is superior due to their

[97] Figure A9 expresses these correlations visually.

[98] We also observe a positive correlation between chiefs' knowledge and tax compliance in Local (Column 6), which is further evidence of the importance of information in tax collection. That said, this positive correlation should be interpreted cautiously because chiefs' knowledge was measured after the tax campaign, and chiefs could have learned about taxpayers while collecting taxes. This same endogeneity issue is not relevant for the comparisons in Central and CLI, where chiefs did not collect taxes.

long history of living in the neighborhood and their leadership position, or it could be consistent with other possible mechanisms, to which we now turn.

## 7.3 Persuasion

In a third possible family of mechanisms, chiefs were better able to persuade households to pay, conditional on having targeted them for a tax visit. Greater persuasive power could reflect two factors. First, chiefs may have been better able to stimulate citizens' tax morale and collected more revenue as a result (Luttmer and Singhal, 2014). For instance, chiefs may have been more trusted, giving citizens confidence that money collected would reach the government. Chiefs also play a leadership role in local public goods provision (*salongo*), meaning citizens may have perceived a clearer reciprocal taxes-for-services link when solicited by the chief.[99] Second, chiefs may have been more credible in threatening sanctions for non-compliers. Although it is unlikely that chiefs would have more credibly threatened official sanctions — fines from the tax ministry and possible legal consequences — they could have threatened local sanctions, such as increasing demands for informal taxes, withholding favors and services (e.g., dispute resolution), or possibly social exclusion.[100]

A first test of this mechanism is to examine if chiefs outperform state collectors when their ability to selectively target households is held constant. During property registration, collectors were instructed to solicit the tax from each household they visited. Yet, collector teams in all arms followed a linear, house-by-house pattern during registration in order to map all the properties in a neighborhood.[101] Because chiefs could not selectively visit properties during registration, any gap in tax payment during registration across treatments would be attributable to differential persuasive power. However, we find no differences between Central and Local in tax compliance during registration (Table A18). Although the total level of payments during registration is low, these results cast doubt on persuasion-based mechanisms.

As a further test, we estimate heterogeneous treatment effects by baseline proxies for chiefs' power and role in public goods provision. Specifically, we explore if chiefs collected relatively more tax in neighborhoods in which (a) they were more highly regarded at baseline, (b) they were more active in the provision of local services, (c)

[99] Besley (2019) notes the importance of reciprocity motives in the evolution of tax compliance.

[100] We discuss potential differences in willingness to pay — as a function of intrinsic motivations like tax morale and the costs of punishment — by collector type more formally in Section A3.1.

[101] We validate that collectors complied with these instructions using the time stamps and GPS coordinates taken during registration (Figure A10).

they were more embedded in the population (and may have more reciprocal, patron-client ties with citizens), (d) they were more distant from the city center (a proxy for chiefs' legitimacy, the validity of which we confirm in the data), and (e) they work in neo-customary zones called *chefferies*, in which chiefs have more responsibilities and power.[102] If chiefs achieved higher compliance through greater powers of persuasion, then the treatment effect should be more pronounced where chiefs were more trusted, active, and powerful. Yet, we find little evidence of heterogeneity in the treatment effect along these dimensions (Table A19). The exception is a somewhat larger effect in neighborhoods with more active chiefs ($p = 0.078$). While this relationship is consistent with reciprocity driving compliance through a taxes-for-services link (Besley, 2019), it is also consistent with a targeting mechanism: more active chiefs were likely also better informed about citizens given that they interacted with them to a greater degree.

In a third test, we examine the interaction between chief collection and cross-randomized messages on tax notices that were designed to interact with the main collection treatments to help isolate mechanisms.[103] As noted when benchmarking the magnitude of the main effect, and discussed in depth in Section A2.2, the following messages were randomly embedded in the tax letters that all collectors distributed during property registration:

(1) *Central Deterrence*: non-compliers could be sanctioned by the tax ministry.

(2) *Local Deterrence*: idem, substituting *chef de quartier* for "tax ministry."[104]

(3) *Central Public Goods*: taxes are needed to improve infrastructure in Kananga.

[102] The measure for (a) is the same index of citizens' views of the chief examined in Table 5. For (b), we use an index of chief activity increasing in the recent activity of chiefs in organizing *salongo* and advocating for the neighborhood to higher authorities, as reported by citizens. For (c), we measure embeddedness as the share of neighborhood residents who knew the chief's name at baseline. For (d), we calculate the distance from households to the city center. According to focus group discussions, and as we confirm in our data (Table A20), chiefs have more legitimacy and power in more remote, peri-urban parts of the city, some of which resemble villages more than the city. For (e), we use data collected during property registration in 2016 to define the jurisdictions of *chefferies*, in which chiefs have more authority and responsibilities similar to village chiefs (cf. Balán et al. (2020)). For each measure, we calculate baseline averages, then define an indicator for above-median neighborhoods and interact this with treatment.

[103] Randomized messages were introduced in the last phase of the campaign. Previously, collectors distributed tax letters identical to those in Figure A1 but without randomized messages. This analysis thus restricts the sample to the 5,434 properties subject to randomized messages on tax letters. Although this smaller sample reduces our power to some extent, a back-of-the-envelope ex-post calculation suggests that we are still powered to reject a flier message main effect of 1.4 percentage points and an interaction effect of about 3 percentage points. Based on the magnitudes reported in past work, we think these are plausible minimum effect sizes. For instance, perhaps the closest such letter experiment is Scartascini and Castro (2007), which finds that enforcement messages increased extensive-margin property tax compliance in Argentina by 5 percentage points.

[104] The *chef de quartier* is the most powerful type of city chief in Kananga and the authority to whom lower city chiefs often seek counsel or assistance in resolving neighborhood problems.

(4) *Local Public Goods*: idem, substituting the neighborhood name for "Kananga."

(5) *Trust*: payment shows trust in the state and its agents.

(6) *Control*: it is important to pay the property tax.

As discussed in our pre-analysis plan, the "Central" ("Local") versions of these messages should have been more credible coming from, and thus complemented the efficacy of, state (chief) collectors. If chiefs collected more taxes because of greater local sanctioning capacity, there should be a more pronounced treatment effect of the Local Deterrence message in Local compared to Central. Similarly, if chiefs activated tax morale due to their link with local services, we should observe an interaction between Local Public Goods and the Local collection treatment. Finally, if greater trust in chiefs explains their effectiveness as collectors, then the Trust message should be more potent in Local than Central.

Despite these predictions, we find no significant interactions of these flier messages with the Local treatment arm (Table A22). These null heterogeneous effects could reflect low literacy, collectors not reading the messages, or simply ineffective message treatments. However, we do observe positive overall treatment effects of the Central Deterrence and Trust messages on compliance (Table A21). Some messages were thus capable of shifting compliance at the margin; they just did not interact with the collection treatments in ways predicted by persuasion mechanisms. Ultimately, then, we find little evidence that chiefs realized higher tax compliance because they were more able to persuade households to pay, conditional on having visited them.

# 8 Distributional Impacts

The previous section finds that chiefs possess information about taxpayers that enabled them to better target tax visits to households with higher payment propensities — and to achieve higher tax compliance as a result. We now open the black box of chiefs' information, presenting descriptive evidence about the types of households visited after registration by different collector types and the implications for the distribution of the tax burden. This investigation is motivated by the concern that chiefs may be more regressive than state collectors, as discussed in historical accounts (Kiser, 1994) and in recent work on informal taxation (Olken and Singhal, 2011).

## 8.1 The Distribution of Tax Visits by Collectors

We first examine the characteristics of households revisited by collectors after registration. Motivated by the revealed value of chiefs' local information, we explore

differences in collectors' tax visit strategies based on *observable* household characteristics — such as house quality, a signal accessible to both chiefs and state collectors — and *unobservable* characteristics — such as liquidity and tax morale, signals to which chiefs may have exclusive access. To do this, we compare these characteristics among the set of households that received tax visits after registration across treatment arms.

Compared to state collectors, chief collectors were more likely to visit lower-quality properties, measured using survey data about property and house characteristics (Figure 1, Panel A).[105] Importantly, this difference does not mean that chiefs systematically sought out low-quality properties in their neighborhoods. On the contrary, Figure A11 confirms that chief collectors were also much more likely to visit and tax properties with above-median house quality in the neighborhood. Rather, the difference in house quality among visited properties in Central and Local reflects the more pronounced reliance of state collectors on the house quality signal when choosing whom to solicit for tax payment after registration.

This interpretation is reinforced by the fact that chief collectors appear more likely than state collectors to have visited households with unobservable characteristics that predict payment. We examine four such characteristics, drawn from baseline survey data: (1) the predicted ease of payment measure derived from chiefs' consultations in CLI and described in Section 7.2; (2) an index of liquidity, which includes cash on hand, income, consumption, employment, and possessions; (3) an index of revealed tax morale, proxied by self-reported payments of taxes in the past; and (4) an index of households' views of the government.[106] Finally, we construct a payment propensity index from these four unobservable characteristics. According to this index, chiefs were more likely than state collectors to have visited households with unobservable characteristics associated with high payment propensity (Figure 1). Each of the subcomponent variables is more positively associated with visits in Local than in Central, though not all of the differences are statistically significant.[107]

[105] All correlations in this figure control for the "leave-one-out" neighborhood mean of the characteristic — excluding each individual property when calculating the mean — to ensure that we capture differences in relative targeting *within*, not across, neighborhoods. However, excluding this control returns similar results (Figure A13), as does excluding property type fixed effects (Figure A12). Figure A15 plots these distributions by treatment.

[106] Each of these indices, and their underlying variables, is explained in detail in Section A2.6. The cash-on-hand measure for the liquidity index is measured at endline and thus post-treatment. We think it is unlikely to be affected by treatment given that on average 8 months passed between tax collection and endline enumeration. We also find no significant differences in cash on hand between Local and Central at endline (Table A26.)

[107] As further evidence, Figure A11 depicts within-neighborhood correlations — rather than comparing across treatment arms — which again reveals that chiefs were more likely to visit households with high predicted ease of payment and high liquidity, whereas this is not true for state collectors. The comparisons in Figure 1 do not map directly to the correlations by treatment in Figure A11 because the regressions include the usual fixed

Although suggestive, these correlations are difficult to interpret because the unobservable variables studied here may be correlated with observable factors like house quality that collectors also used for targeting tax visits. To mitigate this interpretative challenge, we bin households based on the median values of (1) observable house quality, and (2) unobservable ease of payment and examine correlations in the four cells of this 2x2 matrix. This partitioning of household types reveals that, relative to state collectors, chiefs were (i) less likely to visit high-quality houses with low predicted payment propensity, and (ii) more likely to visit low-quality houses with high predicted payment propensity (Figure 1, Panel B). In other words, chiefs appear to have targeted their tax visits based on households' underlying payment propensities rather than their external property characteristics, as state collectors did.[108]

## 8.2 The Distribution of Property Tax Compliance

Given the observed differences in tax visit strategies between chiefs and state agents, coupled with higher compliance in the Local arm, does chief collection carry implications for the distribution of the tax burden? We first examine whether tax compliance varies by treatment arm across the different value bands of the property tax schedule. As noted in Section 2.1, low-value properties (facing a $2 rate) are those constructed with non-durable materials, such as mudbricks, while high-value properties (facing a $9 rate) are constructed with concrete or other durables — and these characteristics indeed predict property value (Bergeron et al., 2020a). Compliance by band thus provides a coarse measure of incidence. Re-estimating Equation 1 for each band reveals that the average treatment effect of chief collection derives entirely from higher compliance among low-value properties (Table 9). Properties in the high-value band were no more likely to pay in Local compared to Central.

What does this mean for the wealth and income of the average tax complier? We find that taxpayers in Local were systematically less wealthy — as measured by the familiar house-quality index — relative to Central (Table 9, Column 3).[109] Figure

effects for time period and randomization strata.

[108] One might also expect that chiefs factor in households' loyalties and patron-client links when choosing whom to solicit for taxes. We thus consider three variables capturing different dimensions of households' prior links with chiefs (Figure A14). Perhaps surprisingly, we find little evidence that chiefs were more (or less) likely to visit households (i) to whom they had provided services in the past, (ii) who reported knowing the chief at baseline, or (iii) who belong to the chief's family. We also find no evidence of differential visits to property owners of the same ethnicity as the chief (Table A23). If anything, state collectors were more likely to visit members of the same subtribe, though we detect no corresponding differences in compliance.

[109] This pattern in consistent with evidence that informal taxation in developing countries, typically administered by local elites, is often regressive (Olken and Singhal, 2011).

A15 (Panel B) shows the distribution of house quality among tax compliers in Local, lying to the left of that in Central.[110] However, using survey data on respondents' monthly income and estimated liquidity, we find no differences between taxpayers in Central and Local (Table 9).[111] Although the sample size in this analysis is small — restricted to tax compliers in the endline sample[112] — this null is reinforced by evidence that chief collectors were more likely to make tax visits at households with higher liquidity (Figure 1). Relative to Central, chief tax collection thus appears more de facto regressive in terms of house quality, but not in terms of income and liquidity.

How should the government evaluate these distributional impacts? The underlying base of the property tax is thought to be the value of the property (Netzer, 1966). Yet in a setting with very little taxation of income, it is not obvious that this de facto distribution of the property tax burden represents an adverse outcome. We take stock of these and other tradeoffs of chief tax collection in the next section.

# 9 Policy Implications

All told, should low-capacity governments delegate tax collection responsibilities to local elites in urban and peri-urban areas? On the one hand, chief collection raised more revenue and did not undermine citizens' views of the government. Further, in Section A3.2, we estimate that chief collection was also more cost-effective: the return on $1 in tax administration was 53% higher in Local compared to Central, due to higher revenues and lower administrative costs.[113] On the other hand, chief collection led to higher bribes and de facto regressivity by house quality (but not income or liquidity). In Section A3.1, we think through these tradeoffs in detail. Here, we provide suggestive evidence that Local did not have greater welfare costs than Central, and we discuss what social cost of bribery would be necessary for a government to prefer state collectors to chiefs.

To explore the implications of chief collection on welfare, we estimate average treatment effects on several proxies of endline household well-being, including income, cash on hand, consumption, and hunger. We find no differences between Local

[110] As robustness, we re-estimate results excluding property type fixed effects in Table A24. Table A25 shows results without controlling for the leave-one-out neighborhood mean, which are similar but less pronounced.

[111] The fact that income and wealth are only weakly correlated in Kananga is consistent with evidence from elsewhere in urban sub-Saharan Africa (Fjeldstad et al., 2017). This phenomenon likely reflects rapid urbanization in the absence of liquid real estate markets. Often families find themselves living in properties with valuations (and tax liabilities) that exceed their incomes.

[112] We lack data on households' cash on hand or income in the larger midline sample.

[113] For this analysis, we use campaign data on the marginal costs of tax administration, including transport costs and collector compensation.

and Central according to reduced-form estimates (Table A26, Panel A). To capture local average treatment effects on tax or bribe payers, we also report IV estimates instrumenting payment status with assignment to Local (Table A26, Panels B–C). There are again no clear differences between treatments. We also find little evidence to suggest that tax or bribe payers — in either treatment arm, or differentially in Local — held more negative endline views of the government or chief, as one might expect if such payments had large welfare costs.[114] Though this analysis is only suggestive, and does not address whether welfare losses of taxation in general are compensated by the value of public funds, it indicates that chief collection did not reduce citizen welfare more than state collection.

One way to express the government's problem when weighing the tradeoffs between chief and state tax collection is to ask what social cost of bribery would justify the choice of Central over Local in this setting. By social cost of bribery, we do not mean the mechanical effect of lowering revenues but rather the combination of (i) potential welfare costs of bribes to citizens, and (ii) potential costs to the perceived legitimacy of the government, which affect its ability to raise revenue non-coercively in the future. If the government simply trades off the cost-effectiveness of collection with bribes multiplied by a constant representing these social costs, this multiplier would need to have a minimum magnitude of 15 to choose state agents over chiefs (Table A29).[115] Put differently, the government would need to weight the social cost of $1 paid in bribes 15 times higher than the value of $1 in net revenues to prefer Central over Local.[116]

## 9.1 Combined Team — Central X Local

Might combined teams — pairing chiefs and state agents together — have promise for raising revenues? This question touches on issues of team production and peer effects, which are beyond the scope of this paper. We therefore approach this question in a reduced-form way to shed light on whether pairing one chief and one ministry agent together could provide a policy-relevant package. Reasoning that the chief would contribute local information to the team, while the ministry agent would contribute better monitoring and enforcement, we expected that "Central X Local" (CXL) would

[114] For this analysis, we re-estimate the treatment effects on views of the government and chief studied in Table 5 and interact the treatment dummy with tax or bribe payment, respectively (Table A27). Payment is an outcome, so these interactions are difficult to interpret. But the lack of meaningful heterogeneity nonetheless provides suggestive evidence that payers did not update negatively about the government or chief.

[115] Cost-effectiveness estimates use data on collector transport and compensation (cf. Section A3.2).

[116] If chiefs were paid via mobile money, obviating trips to the ministry, this multiplier would increase to 35.

outperform Central and Local.

However, CXL neighborhoods had tax compliance in between that of Central and Local. Figure A6 documents a compliance trend over time that approximates a linear combination of that for Central and Local.[117] We observe no complementarities or positive peer effects between the chief and state collector. Anecdotally, both types of collectors reported coordination issues in this treatment arm. For instance, chiefs and state agents complained of having problems meeting one another at the time specified, and disagreements over who should be in charge of the receipt printer and tax funds.[118]

In sum, this CXL treatment arm achieved lower revenues than Local, yet it had higher costs (because of greater transport costs for state agents). In this setting, delegating tax collection to chiefs appears preferable on most measurable dimensions compared to a hybrid collection model involving collectors of each type.

## 9.2 The Limits to Codifying Local Information

Information is a pillar of state capacity. States must render society "legible" in order to raise revenue and pursue other state building projects (Scott, 1998). This paper provides direct evidence of the value of local information possessed by city chiefs in raising tax compliance. When equipped with local information, state collectors raised 31.7% more revenue.

However, the results also highlight the limits of the state's ability to codify and harness local information. Some information possessed by chiefs and useful for tax collection appears to have been simply uncodifiable. This conclusion stems from the combination of two observations: (i) Local realized higher tax compliance than CLI, and (ii) chiefs did not exhibit greater persuasive power. The remaining gap likely reflects the uncodifiable information of the chief that is relevant for tax collection, including "tacit knowledge" about payment propensities of households (Polanyi, 1958).[119]

[117] Table A28 summarizes these results in table form. CXL had higher compliance than Central, though the effect on revenues is less robust. Local still outperforms CXL. As noted when discussing CLI and Local, the change in coefficients for CXL in Columns 1 and 6 derives from the change in the definition of the time period fixed effects described in Section 5, which are defined based on the start and end date of the treatments being compared. Thus, when Local is included in the comparison, the time period definition changes to account for trends in compliance over the full period under examination. Figure A6 provides a visual depiction of trends and the distribution of observations across time.

[118] These coordination problems are reminiscent of the challenges encountered in the hybrid targeting strategy examined in Alatas et al. (2012).

[119] Polanyi (1958) (ch. 4) coined the term tacit knowledge for abilities like facial recognition or language learning that cannot be easily expressed as the sum of explicit, codifiable facts. Williamson (1979) draws on this idea when discussing the appropriate governance structures in markets high in idiosyncratic transaction-specific human capital. Ober (2008) emphasizes the social value of political institutions capable of integrating technical and tacit knowledge.

What aspects of local information are uncodifiable? If such information were truly akin to tacit knowledge, then by definition we could not perfectly characterize it. However, we can compare characteristics of households who were visited after registration in Local and CLI and examine where they diverge. Overall, the characteristics of households visited in CLI are closer to those visited in Local than Central, on both observable and unobservable dimensions (Figure 1).[120] Comparing CLI to Local, the clearest difference concerns liquidity (Figure A16), with CLI collectors somewhat less likely to have visited above-median liquidity households ($p = 0.089$). The uncodifiable component of chiefs' information may thus concern household liquidity. For instance, one possibility is that chiefs received signals about the *timing* of households' liquidity constraints that enabled them to better target tax visits on the time dimension of payment propensity as well as on time-invariant dimensions (e.g., households' underlying tax morale). Such knowledge would have been difficult to convey in a one-off consultation with state collectors.

An alternative interpretation is that chiefs possessed other (codifiable) information that they simply chose not to share during consultations with state collectors in CLI. Although we cannot rule it out entirely, this interpretation appears unlikely given that the households recommended by chiefs in CLI resemble closely the households that chiefs themselves targeted in Local neighborhoods.[121] Moreover, anecdotal evidence from state collectors and program supervisors confirms that chiefs were sincerely engaged during CLI consultations.[122] All told, the results suggest that, in urban settings of low state capacity, the government can achieve better outcomes — from the perspective of the state coffers as well as that of citizens — by delegating collection responsibilities to local elites rather than by trying to integrate their local information into state collection.

# 10 Conclusion

We examined the classic tradeoff between deploying state agents to collect taxes or delegating such responsibilities to local elites in the context of property taxation in a

[120] The similarity between the implied targeting functions of collectors in CLI and Local (rather than Central) provides further evidence about the compositional shift in targeting that led state collectors in CLI to achieve higher compliance than those in Central, as discussed in Section 8.

[121] The co-movement of CLI and Local in terms of tax visits and their correlations with household characteristics is evident in Figure 1 as well as Table 8.

[122] For instance, as noted above, chiefs suggested adding "willingness to pay" — in addition to "ability to pay" — as a field on the form state collectors' filled out during the consultations. They felt an important dimension about households' payment propensity was not reflected in the codification of their knowledge, and unprompted they suggested an amendment to the protocol.

low-state-capacity setting. Chief tax collection raised compliance and revenues compared to state collection. Chief collection did not undermine, and may have even enhanced, the perceived legitimacy of the government, suggesting that engaging with these local elites can complement, not substitute for, the capacity of the formal state. Chiefs appear to have outperformed state collectors by using local information to more efficiently target households likely to pay. Under reasonable assumptions about the social cost of bribes — also higher in Local than Central — the government would almost certainly prefer chief to state collection in this setting.

Rich countries, which collect a much higher share of their GDP compared to poor countries, typically have highly centralized tax collection apparatuses. What, then, are the longer-run policy implications of the observed superiority of chief collection in a setting like the DRC? This paper provides evidence that low-capacity states could benefit in the short run from collaborating with chiefs and other types of local elites as they seek to raise revenue in urban and peri-urban areas.[123] In the longer run, we suspect that state collection will become more attractive.[124] In particular, as there is more third-party information available to tax ministries — because of the expansion of the formal sector (Jensen, 2018) and increasing financial development (Gordon and Li, 2009) — then chiefs' informational advantages will dissipate and will eventually be surpassed by the state. But, in the meantime, local elites are important allies for fragile states seeking to establish rudimentary fiscal capacity.

[123] We make no claim of generalizability in rural areas. Rural elites would likely have more power and discretion as tax collectors compared to the urban elites studied in this paper due to high costs of monitoring and limited footprint of the formal state in rural areas.

[124] Section A3.1.5 discusses more formally how contextual differences and future government policies could alter chiefs' advantages.

# 11 Tables and Figures

**TABLE 1: COMPONENTS OF THE TAX CAMPAIGN AND ITS EVALUATION**

| Activity | Actor | Timing | N | J |
|---|---|---|---|---|
| **Tax campaign** | | | | |
| Property registration | Collectors | May-Dec 2018 | 45,162 | 356 |
| Tax visits | Collectors | May-Dec 2018 | 45,162 | 356 |
| **Evaluation** | | | | |
| Baseline survey | Enumerators | Jul-Dec 2017 | 4,343 | 356 |
| Midline survey | Enumerators | Jun 2018-Feb 2019 | 35,650 | 356 |
| Endline survey | Enumerators | Mar-Sep 2019 | 3,950 | 356 |

*Notes*: N = number of observations, J = number of clusters (neighborhoods). The property register has more observations per neighborhood than the midline survey because the former includes information on all compounds, including (exempted) government buildings, churches, and empty lots, while the midline survey was only conducted with privately owned plots liable for the property tax. The primary tax outcomes result from merging official property tax records with data from the property register. The mechanics of the tax campaign and data sources are discussed, respectively, in Sections 2.1 and 4.

**TABLE 2: TREATMENT ALLOCATION**

| Treatment | Central | Local | CLI | CXL | Control |
|---|---|---|---|---|---|
| Neighborhoods | 110 | 111 | 80 | 50 | 5 |
| Properties | 14,489 | 14,383 | 9,422 | 6,071 | 797 |

*Notes*: This table shows the numbers of neighborhoods (clusters) and properties assigned to each treatment arm. In Central, state agents hired by the provincial tax ministry collected property taxes, while in Local, neighborhood chiefs collected. CLI is short for Central + Local Information, a treatment arm in which tax ministry agents consulted with chiefs before making tax visits. In CXL, or Central X Local, one agent of the tax ministry and one chief worked together on the campaign. In Control, citizens received tax letters informing them of their responsibility to pay at the tax ministry (rather than paying to collectors), as was the status quo declarative system in Kananga until 2016. We discuss these treatments in Section 3.1. We also discuss the reason for differential allocation of clusters across treatment arms in Section 3.1.

## TABLE 3: RANDOMIZATION BALANCE

| | N | Central Mean | Local | CLI | CXL |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| *Panel A: Property Owner Characteristics* | | | | | |
| Years of Education$^B$ | 3614 | 10.56 | -0.07 | -0.03 | -0.60* |
| | | | (0.24) | (0.27) | (0.32) |
| Electricity$^B$ | 3627 | 0.13 | 0.01 | 0.002 | 0.02 |
| | | | (0.01) | (0.02) | (0.02) |
| Log HH Monthly Income$^B$ | 3594 | 10.53 | 0.07 | -0.07 | -0.21 |
| | | | (0.16) | (0.19) | (0.25) |
| Trust of Chief$^B$ | 3613 | 3.07 | 0.05 | 0.10 | 0.19 |
| | | | (0.06) | (0.07) | (0.08) |
| Trust of National Government$^B$ | 3436 | 2.51 | 0.04 | -0.0004 | 0.02 |
| | | | (0.06) | (0.07) | (0.09) |
| Trust Provincial Government$^B$ | 3459 | 2.41 | 0.08 | 0.04 | -0.0005 |
| | | | (0.06) | (0.07) | (0.08) |
| Trust of Tax Ministry$^B$ | 3423 | 2.36 | 0.04 | -0.02 | -0.07 |
| | | | (0.06) | (0.07) | (0.08) |
| Sex$^M$ | 22221 | 0.77 | 0.01 | 0.001 | -0.01 |
| | | | (0.01) | (0.01) | (0.01) |
| Age$^M$ | 19874 | 54.35 | 0.45 | 0.12 | 0.56 |
| | | | (0.48) | (0.59) | (0.64) |
| Majority Tribe$^M$ | 22625 | 0.77 | 0.02 | 0.002 | 0.02 |
| | | | (0.02) | (0.01) | (0.02) |
| Employed$^M$ | 24298 | 0.74 | 0.01 | 0.003 | -0.01 |
| | | | (0.01) | (0.01) | (0.02) |
| Salaried$^M$ | 24299 | 0.25 | 0.01 | -0.01 | -0.01 |
| | | | (0.01) | (0.01) | (0.01) |
| Works for Government$^M$ | 24299 | 0.15 | 0.01 | 0.01 | -0.01 |
| | | | (0.01) | (0.01) | (0.01) |
| Relative Works for Government$^M$ | 26996 | 0.23 | 0.003 | 0.02 | 0.01 |
| | | | (0.01) | (0.01) | (0.02) |
| *Panel B: Property Characteristics* | | | | | |
| House Quality$^M$ | 28362 | 0.004 | -0.01 | 0.14 | -0.07 |
| | | | (0.10) | (0.09) | (0.11) |
| Distance to State Buildings and City Center$^R$ | 44087 | 1.5 | 0.06 | -0.001 | 0.04 |
| | | | (0.05) | (0.06) | (0.07) |
| Distance to Health Institutions$^R$ | 44087 | 0.33 | 0.02 | 0.04 | 0.004 |
| | | | (0.02) | (0.02) | (0.03) |
| Distance to Education Institutions$^R$ | 44087 | 0.65 | 0.03 | 0.04 | 0.01 |
| | | | (0.03) | (0.04) | (0.04) |
| Distance to Roads$^R$ | 43468 | 0.41 | 0.03 | -0.02 | 0.04 |
| | | | (0.04) | (0.05) | (0.06) |
| Distance to Eroded Areas$^R$ | 43468 | 0.12 | 0.002 | 0.01 | 0.03 |
| | | | (0.01) | (0.01) | (0.01) |
| *Panel C: Neighborhood Characteristics* | | | | | |
| Per Capita Property Tax Revenues in 2016$^B$ | 351 | 145.37 | 25.88 | -34.28 | -32.83 |
| | | | (39.36) | (40.84) | (39.66) |
| Affected by Conflict in 2017$^B$ | 351 | 0.02 | 0.01 | 0.003 | 0.04 |
| | | | (0.02) | (0.02) | (0.03) |
| *Panel D: Attrition* | | | | | |
| Baseline to Endline | 4,186 | 0.1 | -0.02 | -0.02 | -0.04*** |
| | | | (0.01) | (0.01) | (0.01) |

*Notes:* This table reports the coefficients from balance tests estimated by regressing baseline and midline characteristics for property owners (Panel A), properties (Panel B), and neighborhoods (Panel C) on treatment indicators, including randomization stratum fixed effects and clustering standard errors at the neighborhood level. Panel D shows differences in attrition from baseline to endline surveying. The Central arm is the omitted category, and Pure Control neighborhoods are excluded. Superscripts $B$, $M$, and $R$ denote variables from baseline, midline, and registration, respectively. The results are discussed in Section 3.3. Variables are described in Section A2.6. Balance tests for bilateral treatment comparisons are shown in Table A2. We discuss these results in Section 3.3.

**TABLE 4: LOCAL V. CENTRAL: COMPLIANCE AND REVENUES**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| *Panel A: Compliance* | | | | | |
| Local | 0.023** | 0.032*** | 0.032*** | 0.033*** | 0.040*** |
| | (0.008) | (0.007) | (0.008) | (0.007) | (0.008) |
| Observations | 28872 | 27764 | 213 | 27764 | 23803 |
| Clusters | 221 | 213 | | 213 | 213 |
| Central Mean | .068 | .063 | .065 | .063 | .073 |
| *Panel B: Revenues* | | | | | |
| Local | 57.215** | 79.870*** | 82.709** | 69.177** | 82.384*** |
| | (25.939) | (23.063) | (38.738) | (20.849) | (23.889) |
| Observations | 28872 | 27764 | 213 | 27764 | 23803 |
| Clusters | 221 | 213 | | 213 | 213 |
| Mean | 195.583 | 184.65 | 212.274 | 184.65 | 211.361 |
| Time FE | No | Yes | Yes | Yes | Yes |
| House FE | No | No | No | Yes | Yes |
| Stratum FE | Yes | Yes | Yes | Yes | Yes |
| Exempt Excluded | No | No | No | No | Yes |

*Notes*: This table reports estimates from Equation 1, comparing property tax compliance in Local and Central (the excluded category). The two panels show estimates from separate regressions of compliance and revenues on treatment, respectively. All regressions include fixed effects for randomization strata and cluster standard errors at the neighborhood level. Column 1 regressions do not include time period fixed effects described in Section 5 while those in other columns include them. Regressions in Columns 1–3 do not include house fixed effects. Column 3 shows results when the data are collapsed to the neighborhood level. We use robust standard errors and assign the minimum value for time period fixed effects to a neighborhood. Regressions in Column 4 exclude exempted properties. The data include all properties registered by tax collectors merged with the government's property tax database. We discuss these results in Section 6.1.

## TABLE 5: LOCAL V. CENTRAL: MISMANAGEMENT AND VIEWS OF GOVERNMENT, CHIEFS, AND TAXES

| *Dependent variable* | $\hat{\beta}$ | SE | $R^2$ | N | $\bar{x}_{Central}$ |
|---|---|---|---|---|---|
| *Panel A: Property Assessments* | | | | | |
| Assigned Exemption | 0.039* | 0.021 | 0.055 | 13772 | 0.266 |
| Incorrect Exemption | 0.012 | 0.007 | 0.020 | 13771 | 0.044 |
| Assigned High Band | 0.030 | 0.021 | 0.230 | 27764 | 0.114 |
| Incorrect Assignment | -0.013** | 0.006 | 0.041 | 27764 | 0.031 |
| *Panel B: Bribes* | | | | | |
| Paid Bribe (Midline) | -0.001 | 0.003 | 0.007 | 18596 | 0.016 |
| Gap Self v. Admin (Midline) | 0.016* | 0.009 | 0.018 | 14309 | 0.077 |
| Paid Bribe (Endline) | 0.018* | 0.009 | 0.049 | 1169 | 0.014 |
| Other Payments (Endline) | 0.031** | 0.014 | 0.041 | 2407 | 0.094 |
| *Panel C: View of government* | | | | | |
| View of government (index) | 0.023 | 0.049 | 0.100 | 2411 | 0.011 |
| Trust in government | 0.127** | 0.057 | 0.075 | 2286 | 0.028 |
| Responsiveness of government | -0.049 | 0.045 | 0.099 | 2282 | 0 |
| Performance of government | -0.060 | 0.052 | 0.060 | 2179 | -0.014 |
| Integrity of government | 0.043 | 0.047 | 0.058 | 2313 | 0.016 |
| *Panel D: View of chief* | | | | | |
| View of chief (index) | 0.052 | 0.049 | 0.093 | 2386 | 0.017 |
| Trust in chief | 0.040 | 0.053 | 0.116 | 2372 | 0.022 |
| Responsiveness of chief | -0.058 | 0.057 | 0.111 | 1681 | -0.008 |
| Performance of chief | 0.067 | 0.059 | 0.081 | 1342 | 0.007 |
| Integrity of chief | 0.056 | 0.056 | 0.081 | 1888 | 0.011 |
| *Panel E: View of taxation* | | | | | |
| Perceived tax compliance on avenue | 0.100* | 0.055 | 0.073 | 1851 | 0.026 |
| Trust in tax ministry | 0.085 | 0.061 | 0.073 | 2259 | 0.025 |
| Property tax morale | 0.075 | 0.047 | 0.057 | 2343 | 0.014 |
| Fairness of property taxation | -0.004 | 0.053 | 0.046 | 2407 | 0.003 |
| Perception of enforcement | -0.019 | 0.058 | 0.070 | 2379 | 0.015 |

*Notes*: Each row summarizes an OLS estimation of Equation 1, comparing Local and Central, with the dependent variable noted in the first column. $\hat{\beta}$ is the coefficient on the treatment indicator, followed by the cluster-robust standard error, $R^2$, number of observations, and $\bar{x}_{Central}$ the Central group mean. In Panel A, row 1 shows differences in whether the collector designated the property exempt from taxes. Properties owned by the elderly, widows, government pensioners, and handicapped individuals, among others, are legally supposed to be exempted. Row 2 shows differences in whether an independent enumerator disagreed (in either direction) with the exemption status of a given property. Row 3 shows differences in whether a property was assigned to the high-value category, and row 4 shows whether enumerators' independent evaluations diverged with the collectors' designation. In Panel B, the outcomes in rows 5 and 7 are self-reported bribe payment as measured during the midline and endline surveys, respectively. The outcome in row 6 indicates property owners who reported paying the tax but who were not recorded as having paid in the administrative data. The outcome in row 8 is self-reported payment of any informal fees at endline. We discuss the results from Panels A and B in Section 6.2. In Panels C–E, for endline outcomes we also measured at baseline — all variables except for *Perceived tax compliance* and *Fairness of property taxation* — we control for the baseline value. Each dependent variable, described briefly in Section 6.2 and in detail in Section A2.6, is standardized to facilitate interpretation of coefficient magnitude. We discuss the results in Panels C–E in Section 6.2. In all panels, regressions include fixed effects for randomization strata, and cluster standard errors at the neighborhood level. Regressions estimating effects on midline and property assessment outcomes include time period fixed effects described in Section 5 and house type fixed effects. We do not include house type fixed effects for endline outcomes to maximize the analysis sample, as discussed in Section 5. The number of observations varies across regressions due to (i) outcomes being drawn from different surveys, and (ii) non-response for specific survey questions.

## TABLE 6: LOCAL V. CENTRAL: TAX VISITS

| | Visited by Collector | Number of Visits by Collector | Other Contact with Collector | Instances of Other Contact |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Local | -0.007 | 0.017 | 0.008 | 0.019 |
| | (0.026) | (0.046) | (0.007) | (0.012) |
| Time FE | Yes | Yes | Yes | Yes |
| House FE | Yes | Yes | Yes | Yes |
| Stratum FE | Yes | Yes | Yes | Yes |
| Observations | 18265 | 18254 | 3533 | 3533 |
| Clusters | 209 | 209 | 206 | 206 |
| Mean | .417 | .553 | .025 | .039 |

*Notes*: This table reports estimates from Equation 1, comparing the tax visits collectors made after registration in Local and Central (the excluded category). All regressions include fixed effects for house type, randomization strata, and time periods described in Section 5, and cluster standard errors at the neighborhood level. Columns 1 and 2 report differences in tax visits by — after the registration visit — by the extensive and intensive margins, respectively. Columns 3 and 4 report differences in citizen-reported other contact with collectors outside of the tax campaign, by the intensive and extensive margins, respectively. We discuss these results in Section 7.1.

## TABLE 7: CENTRAL V. CENTRAL + LOCAL INFORMATION

| | Compliance | Revenues | Visited | Visits | Compliance | Compliance |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Central Plus Local Info | 0.024** | 48.325** | -0.016 | -0.026 | 0.026* | 0.022** |
| | (0.009) | (21.466) | (0.028) | (0.044) | (0.014) | (0.009) |
| Local | | | | | | 0.046*** |
| | | | | | | (0.007) |
| Visit Control | No | No | No | No | Yes | No |
| Time FE | Yes | Yes | Yes | Yes | Yes | Yes |
| House FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Stratum FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 20636 | 20636 | 13884 | 13877 | 5283 | 33746 |
| Clusters | 165 | 165 | 163 | 163 | 161 | 267 |
| Central Mean | .051 | 152.399 | .387 | .497 | .097 | .052 |
| Test CLI=Local ($p$-value) | | | | | | 0.007 |

*Notes*: This table compares the Central + Local Information (CLI) arm to the Central arm, which is the excluded category. Columns 1, 5, and 6 report impacts on compliance. Column 2 reports impacts on revenues. Columns 3 and 4 report differences in tax visits by collectors after registration by the extensive and intensive margins, respectively. All regressions include fixed effects for house type, randomization strata, and time periods and cluster standard errors at the neighborhood level. All specifications include time fixed effects defined to maximize overlap between the treatments under comparison, as discussed in Section 5. Column 5 restricts to the sub-sample of properties that received any tax visits after registration. Column 6 includes a dummy for the Local treatment in the regression. The bottom row reports the $p$-value from a test for equality between the CLI and Local. We discuss these results in Section 7.2.

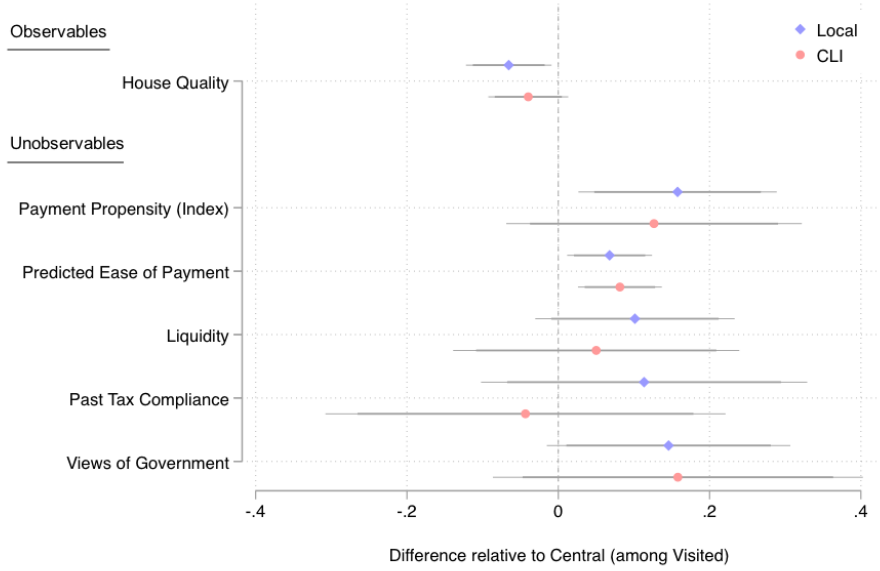
## TABLE 8: THE VALUE OF CHIEFS' INFORMATION

| | Visited (1) | Compliance (2) | Visited (3) | Compliance (4) | Compliance (5) | Visited (6) | Compliance (7) | Visited (8) | Compliance (9) |
|---|---|---|---|---|---|---|---|---|---|
| *Panel A: Ease of payment* | | | | | | | | | |
| Ease of payment | 0.045*** | 0.056*** | 0.029** | 0.044*** | 0.085*** | | | | |
| | (0.012) | (0.007) | (0.014) | (0.008) | (0.012) | | | | |
| Predicted Ease of payment | | | | | | 0.054** | 0.046*** | 0.013 | 0.040*** |
| | | | | | | (0.017) | (0.012) | (0.017) | (0.007) |
| Wall quality | | | 0.025** | 0.021** | | 0.017* | 0.017** | 0.020* | 0.011** |
| | | | (0.012) | (0.007) | | (0.010) | (0.007) | (0.011) | (0.004) |
| Roof quality | | | 0.005 | -0.000 | | 0.003 | -0.004 | 0.018** | -0.010 |
| | | | (0.006) | (0.002) | | (0.006) | (0.004) | (0.008) | (0.006) |
| Erosion threat | | | 0.017 | -0.004 | | 0.002 | -0.008 | 0.000 | -0.005 |
| | | | (0.011) | (0.004) | | (0.011) | (0.007) | (0.010) | (0.004) |
| Observations | 5623 | 8214 | 4599 | 5215 | 2121 | 5828 | 5843 | 5007 | 5013 |
| Clusters | 79 | 80 | 66 | 66 | 77 | 93 | 93 | 80 | 80 |
| Mean | .375 | .072 | .35 | .065 | .129 | .435 | .103 | .41 | .059 |
| *Panel B: Willingness to pay* | | | | | | | | | |
| Willingness to pay | 0.034** | 0.037*** | 0.033** | 0.038*** | 0.058*** | | | | |
| | (0.011) | (0.007) | (0.012) | (0.008) | (0.012) | | | | |
| Predicted Willingness to pay | | | | | | 0.045** | 0.036*** | 0.007 | 0.032*** |
| | | | | | | (0.016) | (0.010) | (0.015) | (0.009) |
| Wall quality | | | 0.022 | 0.021** | | 0.018* | 0.017** | 0.020* | 0.012** |
| | | | (0.013) | (0.009) | | (0.010) | (0.006) | (0.010) | (0.005) |
| Roof quality | | | 0.011 | 0.001 | | 0.004 | -0.004 | 0.018** | -0.010 |
| | | | (0.008) | (0.002) | | (0.006) | (0.004) | (0.008) | (0.006) |
| Erosion threat | | | 0.016 | -0.005 | | 0.002 | -0.008 | 0.000 | -0.005 |
| | | | (0.012) | (0.005) | | (0.011) | (0.007) | (0.010) | (0.005) |
| Observations | 3981 | 5596 | 3977 | 4525 | 1428 | 5828 | 5843 | 5007 | 5013 |
| Clusters | 50 | 50 | 50 | 50 | 48 | 93 | 93 | 80 | 80 |
| Mean | .356 | .062 | .356 | .066 | .108 | .435 | .103 | .41 | .059 |
| Treatment | CLI | CLI | CLI | CLI | CLI | Local | Local | Central | Central |
| Visited Only | No | No | No | No | Yes | No | No | No | No |
| House FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Stratum FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

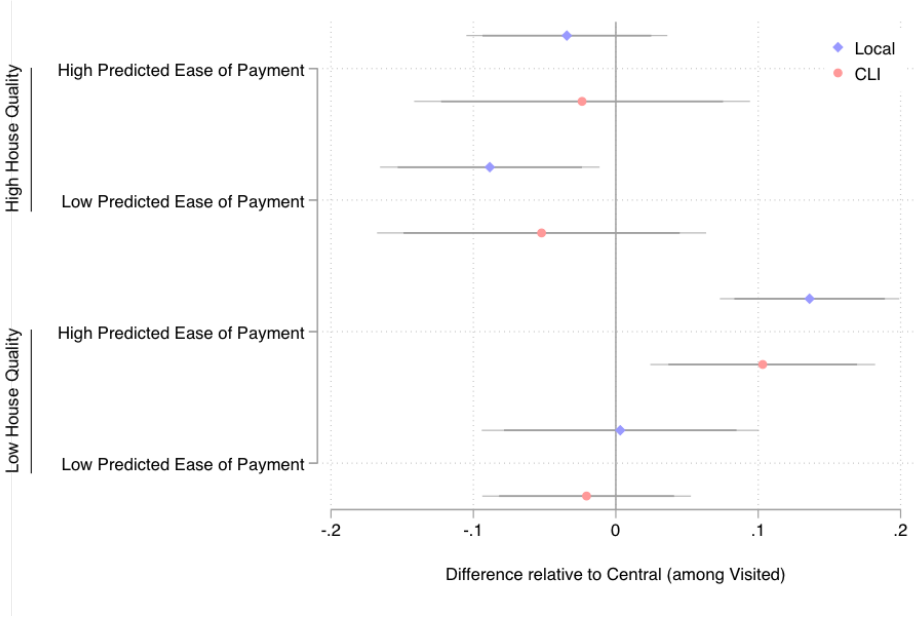
*Notes*: This table explores the extent to which chiefs' recommendations in Central + Local Information (CLI) predict tax visits after registration and tax payment. Columns 1–5 show correlations in CLI between chiefs' recommendations and outcomes. Columns 6–9 report correlations between predicted propensity measures described in Section 7.2 and outcomes in Local (Columns 6 and 7) and Central (Columns 8 and 9). Columns 1, 3, 6, and 8 show correlations between propensity and tax visits; Columns 2, 4, 5, 7, and 9 show correlations between propensity and compliance. Column 5 shows correlations with compliance conditional on receiving a visit after registration. All regressions include house type and randomization stratum fixed effects and cluster standard errors at the neighborhood level. Columns 3, 4, and 6–8 include controls for observable household characteristics. We discuss these results in Section 7.2.

## FIGURE 1: CHARACTERISTICS OF HOUSEHOLDS VISITED BY COLLECTORS AFTER REGISTRATION ACROSS TREATMENTS

A: Observable and Unobservable Characteristics




B: Predicted Ease of Payment and House Quality




*Notes*: This figure reports differences by treatment arm in the characteristics of properties visited by collectors after registration, showing differences in characteristics of visited properties in the Local and CLI arms relative to the Central arm. Panel A shows differences in observable and unobservable characteristics for indices described in Section 8.1. Panel B shows differences in the probability of receiving a visit in the four cells indicated (defined by interactions of high/low dummies for household house quality and predicted ease of payment). Differences are estimated through separate regressions of characteristics on a treatment indicator among visited properties, controlling for the leave-one-out neighborhood mean of the outcome (Panel A) or the neighborhood mean of house quality and ease of payment (Panel B). We include time period, house type, and stratum fixed effects. We cluster standard errors at the neighborhood level. Households that paid during registration are dropped. As a comparison, Figure A11 shows the correlations between tax visits and household characteristics within treatments, rather than differences across treatments. Figures A12 and A13 replicate this analysis while omitting house fixed effects and neighborhood mean controls, respectively. We discuss these results in Section 8.1.

**TABLE 9: LOCAL V. CENTRAL: THE DISTRIBUTION OF THE TAX BURDEN**

| *Outcome:* | Compliance by Prop. Type | | Complier Characteristics | | |
|---|---|---|---|---|---|
| | Low Band Prop. | High Band Prop. | House Quality | Avg. Mon. Income | Liquidity Index |
| | (1) | (2) | (3) | (4) | (5) |
| Local | 0.037*** | 0.002 | -0.148** | 0.002 | -0.063 |
| | (0.008) | (0.013) | (0.057) | (0.042) | (0.167) |
| Time FE | Yes | Yes | Yes | Yes | Yes |
| House FE | No | No | Yes | Yes | Yes |
| Stratum FE | Yes | Yes | Yes | Yes | Yes |
| Observations | 24581 | 3384 | 1324 | 228 | 228 |
| Clusters | 208 | 150 | 157 | 121 | 121 |
| Central Mean | .063 | .062 | .102 | .007 | .118 |

*Notes*: This table reports estimates from a version of Equation 1, comparing property tax compliance in Local and Central (the excluded category). We include fixed effects for house type, randomization strata, and time periods, as described in Section 5, and we cluster standard errors at the neighborhood level. Columns 1 and 2 report estimates of the impact of local collection on compliance for low- and high-band households, respectively. Column 3 reports differences in an index of house quality conditional on the property paying the tax. Column 4 reports differences in monthly household income of properties, averaged across baseline and endline values, in Congolese Francs, conditional on paying the tax. Column 5 reports differences in an index of liquidity measures drawn from baseline (excepting income, which is also included, and uses information from endline) among payers. Columns 3–5 control for the leave-one-out neighborhood mean of the outcome. Figure A24 replicates this analysis excluding house fixed effects. We discuss the interpretation of these results in Section 8.2.

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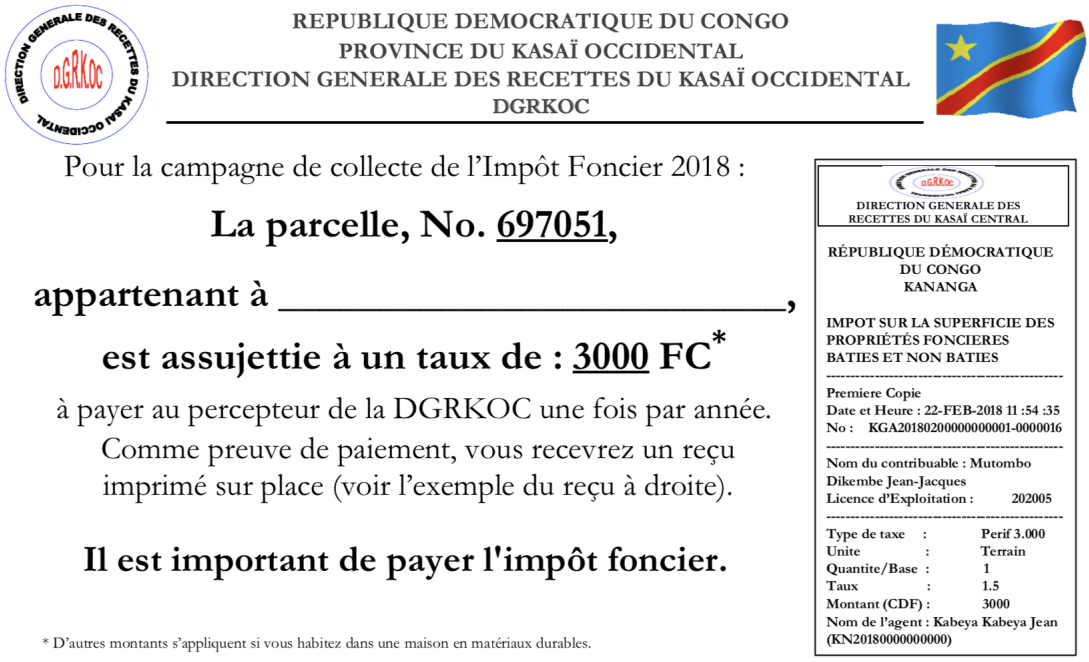
# Supplementary Data and Appendix
## For Online Publication

# A1 Additional Exhibits for the Main Analysis

## A1.1 Additional Exhibits for Paper Section 2 — Setting

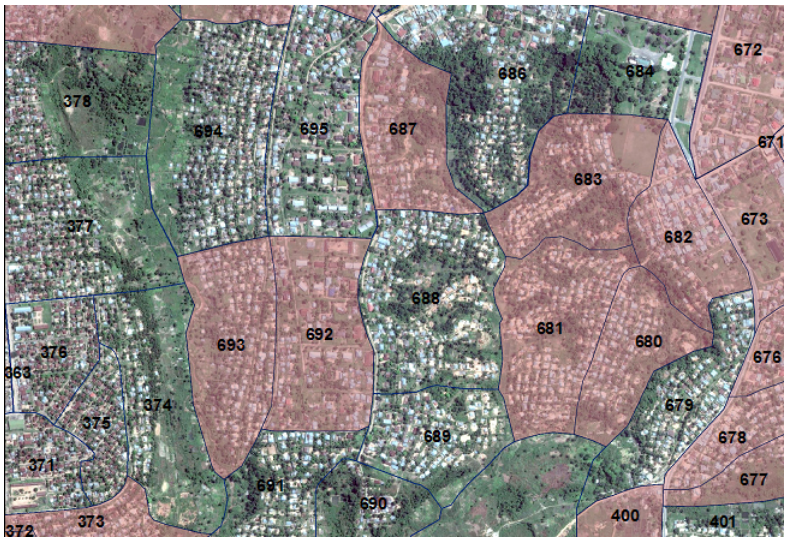
**FIGURE A1: SAMPLE TAX NOTICE**




*Notes*: This figure displays a sample tax notice, which is discussed in Section 2.1. The flier says: "For the 2018 property tax collection campaign: the compound 697051 belonging to [name of owner] is subject to a tax rate of 3000 CF to be paid to a DGRKOC collector once per year. As proof of payment, you will receive a receipt printed on the spot (see example to the right). It is important to pay the property tax." The footnote says "Other amounts apply if you live in a house built of durable materials." This flier contains the Control message ("It is important to pay the property tax"), discussed in the text in Section 7.3 and in detail in Section A2.2. A version of the flier in Tshiluba, the primary local language, was printed on the opposite side. Fliers were identical across treatment arms.

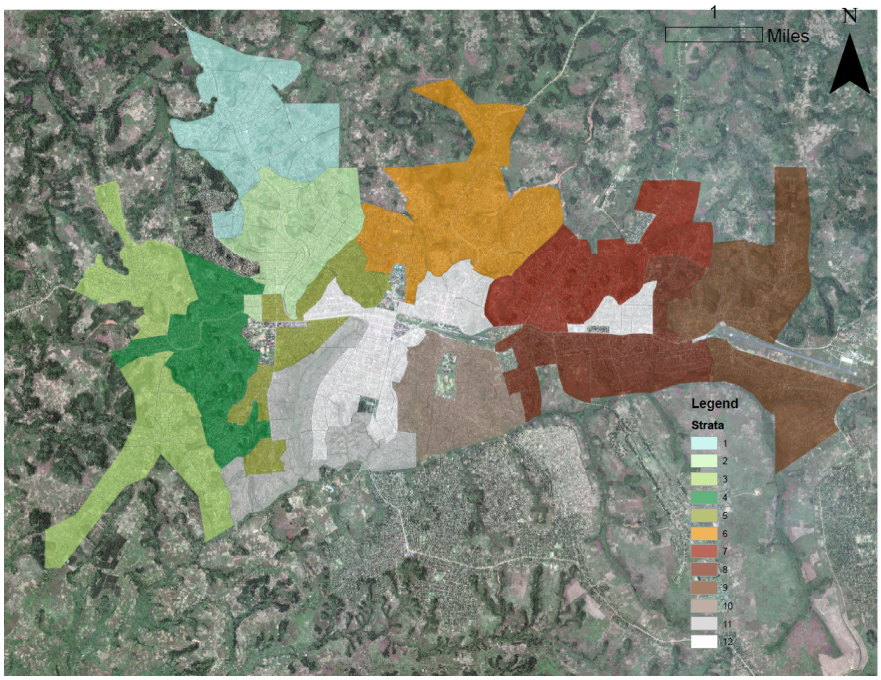
## A1.2 Additional Exhibits for Paper Section 3 — Design

### FIGURE A2: THE UNIT OF RANDOMIZATION: NEIGHBORHOODS OF KANANGA




*Notes*: This figure displays a sample of neighborhood divisions in Kananga, which are discussed in Section A2.1.

### FIGURE A3: GEOGRAPHIC STRATA



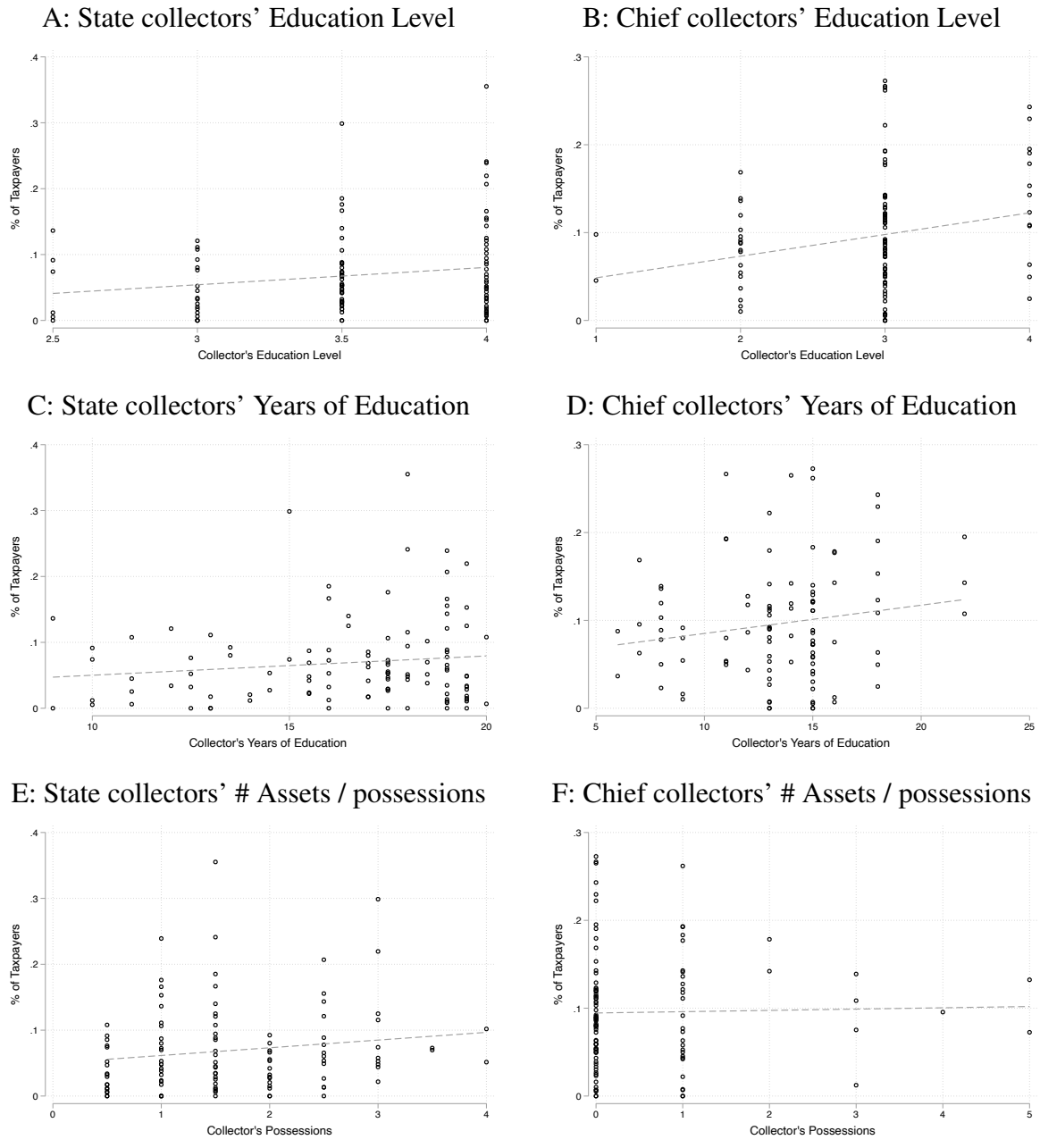

*Notes*: This figure displays the geographic strata of Kananga, which are discussed in Section A2.1.

**TABLE A1: LOCAL V. CENTRAL: COLLECTOR CHARACTERISTICS**

| Variable | State collectors | Chief Collectors | Difference |
|---|---|---|---|
| | (1) | (2) | (3) |
| Age | 30.820 | 58.706 | 27.886*** |
| | (8.103) | (11.132) | (1.756) |
| % Female | 0.060 | 0.046 | -0.014 |
| | (0.240) | (0.210) | (0.038) |
| Born in Kananga | 0.500 | 0.600 | 0.100 |
| | (0.505) | (0.492) | (0.085) |
| Log Monthly Income | 4.283 | 4.027 | -0.256 |
| | (0.896) | (1.156) | (0.187) |
| Wealth (Possessions) | 1.800 | 0.613 | -1.187*** |
| | (1.309) | (1.153) | (0.205) |
| Education (Years) | 17.080 | 13.299 | -3.781*** |
| | (3.368) | (3.505) | (0.593) |
| Math Ability | 0.740 | 0.743 | 0.003 |
| | (0.231) | (0.258) | (0.043) |
| Reading Ability | 1.780 | 1.811 | 0.031 |
| | (0.603) | (0.753) | (0.121) |
| Trust in Government | 3.033 | 2.723 | -0.310* |
| | (0.707) | (1.043) | (0.163) |
| Perceived Government Capacity | 146.148 | 159.915 | 13.767 |
| | (74.372) | (99.130) | (15.706) |
| Preference for Redistribution | 2.673 | 2.745 | 0.071 |
| | (0.561) | (0.584) | (0.098) |
| Preference for Progressive Taxation | 2.576 | 2.468 | -0.108** |
| | (0.279) | (0.309) | (0.051) |
| Observations | 50 | 111 | 161 |

*Notes*: This table compares baseline characteristics of state collectors (Column 1) and chiefs (Column 2). Column 3 reports a simple difference-in-means test. The data come from surveys conducted with tax collectors before the 2018 campaign. The first six variables are the respondent's age, a gender indicator, an indicator for being born in Kananga, log monthly income, wealth (defined as the number of possessions: motorbike, car, radio, TV, generator and sewing machine) and years of education. *Math Ability* and *Reading Ability* are the average score of collectors on a series of quiz-type questions. The last four measures concern attitudes about the government and about redistribution, measured through survey questions with Likert-scale response options. These comparisons are discussed in Section 3.1.

## FIGURE A4: COLLECTOR PERFORMANCE AND EDUCATION / WEALTH




*Notes*: This figure shows the relationship between tax compliance in the neighborhood and tax collectors' education levels (Panels A and B), years of education (Panels C and D), and wealth (Panels E and F). Wealth here is defined as number of possessions among the following: motorbike, car, radio, TV, generator, and sewing machine. The relationships are reported separately for neighborhoods assigned to the Central and CLI treatment arms where tax collection was done by state agents (Panels A, C, and E) and for neighborhoods assigned to the Local treatment arm where tax collection was done by city chiefs (Panel B, D, and F). These comparisons are discussed in Section 3.1

## TABLE A2: RANDOMIZATION BALANCE: BILATERAL TREATMENT COMPARISONS

| | Local | CLI | CXL |
|---|---|---|---|
| | (1) | (2) | (3) |
| *Panel A: Baseline Characteristics* | | | |
| Years of Education | -0.003 | 0.001 | -0.003 |
| | (0.003) | (0.003) | (0.003) |
| Electricity | 0.008 | 0.021 | 0.030 |
| | (0.027) | (0.031) | (0.031) |
| Log HH Monthly Income | 0.006 | -0.003 | -0.006 |
| | (0.005) | (0.005) | (0.005) |
| Trust of Chiefs | 0.012 | 0.026** | 0.026** |
| | (0.012) | (0.012) | (0.013) |
| Trust of National Government | -0.015 | -0.010 | -0.002 |
| | (0.014) | (0.012) | (0.013) |
| Trust of Provincial Government | 0.026 | 0.018 | -0.001 |
| | (0.016) | (0.015) | (0.016) |
| Trust of Tax Ministry | -0.001 | -0.008 | -0.003 |
| | (0.010) | (0.012) | (0.012) |
| Observations | 2117 | 1768 | 1501 |
| Clusters | 221 | 187 | 159 |
| $F, p$ | 1.08, 0.37 | 1.12, 0.34 | 1.15, 0.33 |
| *Panel B: Midline Characteristics* | | | |
| Sex | -0.001 | -0.027** | -0.005 |
| | (0.009) | (0.011) | (0.010) |
| Age | 0.000 | -0.000 | -0.000 |
| | (0.000) | (0.000) | (0.000) |
| Majority Tribe | 0.001 | -0.013 | 0.001 |
| | (0.018) | (0.014) | (0.011) |
| Employed | -0.002 | 0.008 | 0.004 |
| | (0.013) | (0.014) | (0.010) |
| Salaried | 0.003 | -0.032** | -0.025* |
| | (0.016) | (0.014) | (0.015) |
| Works for Government | -0.029 | 0.029 | -0.019 |
| | (0.025) | (0.024) | (0.024) |
| Relative Works for Government | 0.036 | 0.024 | 0.043* |
| | (0.025) | (0.023) | (0.024) |
| House Quality | -0.001 | 0.002 | 0.005 |
| | (0.022) | (0.018) | (0.019) |
| Distance to State Buildings and City Center | 0.061 | -0.470** | 0.048 |
| | (0.158) | (0.156) | (0.199) |
| Distance to Health Institutions | 0.064 | 0.257 | -0.066 |
| | (0.201) | (0.222) | (0.187) |
| Distance to Education Institutions | 0.445* | 0.387 | 0.179 |
| | (0.267) | (0.250) | (0.310) |
| Distance to Roads | -0.171 | 0.035 | 0.197 |
| | (0.145) | (0.133) | (0.133) |
| Distance to Eroded Areas | 0.157 | 0.026 | 0.458 |
| | (0.262) | (0.297) | (0.303) |
| Observations | 10666 | 8500 | 7542 |
| Clusters | 172 | 141 | 123 |
| $F, p$ | 0.98, 0.47 | 2.37, 0.01 | 1.00, 0.46 |
| *Panel C: Neighborhood Characteristics* | | | |
| Per Capita Property Tax Revenues in 2016 | 0.0001 | -0.0001 | -0.0001 |
| | (0.0002) | (0.0001) | (0.0002) |
| Affected by Conflict in 2017 | 0.131 | -0.131 | 0.444** |
| | (0.289) | (0.362) | (0.215) |
| Observations | 221 | 190 | 160 |
| Clusters | 221 | 190 | 160 |
| $F, p$ | 0.39, 0.68 | 0.41, 0.67 | 2.46, 0.09 |
| Stratum FE | Yes | Yes | Yes |

*Notes:* This table summarizes balance tests for bilateral treatment comparisons. Each column compares the noted treatment arm to Central. The bottom row of each panel contains the statistics for tests of the omnibus null hypothesis that the treatment effects for the covariates studied in Table 3 are all zero using parametric $F$ tests. As usual, regressions include stratum fixed effects and cluster standard errors at the neighborhood level. We run separate tests for variables drawn from baseline survey, midline survey, and neighborhood-level data to maximize the number of observations included in each regression. Midline characteristics include the distance characteristics from registration reported in Table 3. We discuss these results in Section 3.3.

## TABLE A3: RANDOMIZATION BALANCE: INCLUDING CONTROL GROUP

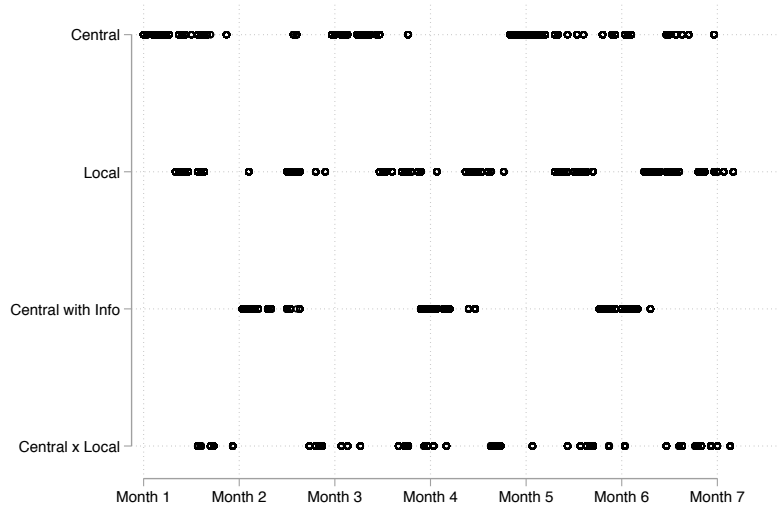
| | N | Control Mean | Central | Local | CLI | CXL |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: Property Owner Characteristics* | | | | | | |
| Years of Education$^B$ | 3667 | 9.75 | 0.81 | 0.71 | 0.40 | 0.41 |
| | | | (1.50) | (1.50) | (1.51) | (1.52) |
| Electricity$^B$ | 3680 | 0.19 | -0.06 | -0.05 | -0.07 | -0.04 |
| | | | (0.09) | (0.09) | (0.09) | (0.09) |
| Log HH Monthly Income$^B$ | 3646 | 10.64 | -0.11 | 0.07 | -0.15 | -0.25 |
| | | | (0.29) | (0.29) | (0.30) | (0.34) |
| Trust of Chief$^B$ | 3666 | 2.91 | 0.16 | 0.23 | 0.31 | 0.36 |
| | | | (0.35) | (0.35) | (0.35) | (0.35) |
| Trust of National Government$^B$ | 3488 | 2.33 | 0.19 | 0.22 | 0.18 | 0.20 |
| | | | (0.18) | (0.18) | (0.18) | (0.19) |
| Trust Provincial Government$^B$ | 3511 | 2.25 | 0.16 | 0.25 | 0.19 | 0.18 |
| | | | (0.20) | (0.20) | (0.21) | (0.21) |
| Trust of Tax Ministry$^B$ | 3474 | 2.37 | -0.01 | 0.02 | -0.04 | -0.08 |
| | | | (0.15) | (0.15) | (0.16) | (0.16) |
| Sex$^M$ | 22699 | 0.84 | -0.07*** | -0.05** | -0.07** | -0.08*** |
| | | | (0.02) | (0.02) | (0.03) | (0.03) |
| Age$^M$ | 20269 | 53.85 | 0.50 | 0.24 | 0.17 | 0.64 |
| | | | (1.16) | (1.14) | (1.27) | (1.30) |
| Majority Tribe$^M$ | 23014 | 0.81 | -0.04 | -0.02 | -0.01 | -0.02 |
| | | | (0.08) | (0.08) | (0.08) | (0.08) |
| Employed$^M$ | 24764 | 0.78 | -0.04 | -0.03 | -0.03 | -0.05 |
| | | | (0.03) | (0.03) | (0.03) | (0.03) |
| Salaried$^M$ | 24765 | 0.23 | 0.02 | 0.01 | 0.01 | 0.01 |
| | | | (0.05) | (0.05) | (0.05) | (0.05) |
| Works for Government$^M$ | 24765 | 0.16 | -0.01 | -0.01 | -0.003 | -0.01 |
| | | | (0.05) | (0.05) | (0.05) | (0.05) |
| Relative Works for Government$^M$ | 27497 | 0.26 | -0.03 | -0.04 | -0.02 | -0.03 |
| | | | (0.05) | (0.05) | (0.05) | (0.05) |
| *Panel B: Property Characteristics* | | | | | | |
| House Quality$^M$ | 28957 | -0.14 | 0.14 | 0.05 | 0.28 | 0.14 |
| | | | (0.31) | (0.31) | (0.31) | (0.32) |
| Distance to State Buildings and City Center$^R$ | 44899 | 1.86 | -0.36** | -0.31* | -0.28 | -0.36* |
| | | | (0.18) | (0.18) | (0.19) | (0.20) |
| Distance to Health Institutions$^R$ | 44899 | 0.38 | -0.05 | -0.02 | 0.01 | -0.06 |
| | | | (0.08) | (0.08) | (0.08) | (0.08) |
| Distance to Education Institutions$^R$ | 44899 | 0.78 | -0.13 | -0.10 | -0.04 | -0.13 |
| | | | (0.21) | (0.21) | (0.21) | (0.21) |
| Distance to Roads$^R$ | 44280 | 0.38 | 0.04 | 0.08 | 0.03 | 0.04 |
| | | | (0.14) | (0.14) | (0.14) | (0.15) |
| Distance to Eroded Areas$^R$ | 44280 | 0.12 | -0.003 | 0.01 | 0.002 | 0.02 |
| | | | (0.02) | (0.02) | (0.02) | (0.02) |
| *Panel C: Neighborhood Characteristics* | | | | | | |
| Per Capita Property Tax Revenues in 2016$^B$ | 351 | 176.48 | -105.93 | -139.42 | -138.83 | |
| | | | (101.43) | (100.48) | (100.68) | (101.85) |
| Affected by Conflict in 2017$^B$ | 356 | 0.20 | -0.18 | -0.16 | -0.16 | -0.14 |
| | | | (0.18) | (0.18) | (0.18) | (0.18) |
| *Panel D: Attrition:* | | | | | | |
| Baseline to Endline | 4,246 | 0.13 | -0.04 | -0.05 | -0.06 | -0.07 |
| | | | (0.05) | (0.05) | (0.05) | (0.05) |

*Notes:* This table reports the coefficients from balance tests estimated by regressing characteristics for property owners (Panel A), properties (Panel B), and neighborhoods (Panel C) on treatment indicators, clustering standard errors at the neighborhood level. Panel D shows difference in attrition from baseline to endline surveying. The Control arm is the excluded category. Randomization stratum fixed effects are not included because Control neighborhoods do not exist in every strata. Superscripts $B$, $M$, and $R$ denote which variables come from baseline, midline, and registration, respectively. Variables are described in Section A2.6. Joint orthogonality tests for specific treatment comparisons are shown in Table A2. We discuss these results in Section 3.3.

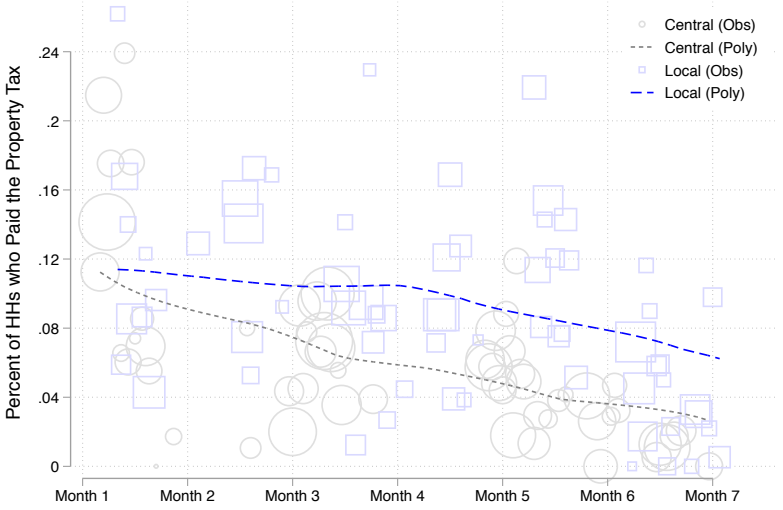
## A1.3 Additional Exhibits for Paper Section 5 — Estimation

**FIGURE A5: STAGGERING OF TREATMENTS AND DECREASING COMPLIANCE OVER TIME — CENTRAL AND LOCAL**

A: Staggering of Treatments (All Treatments)



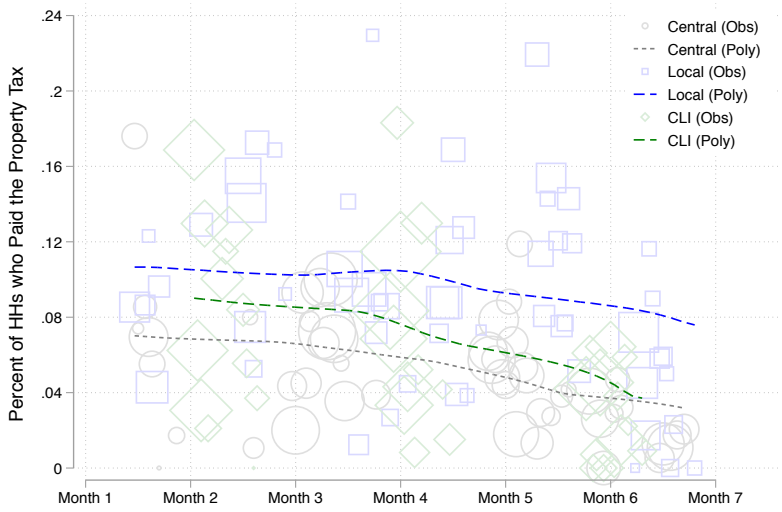

B: Compliance Over Campaign (Central and Local)



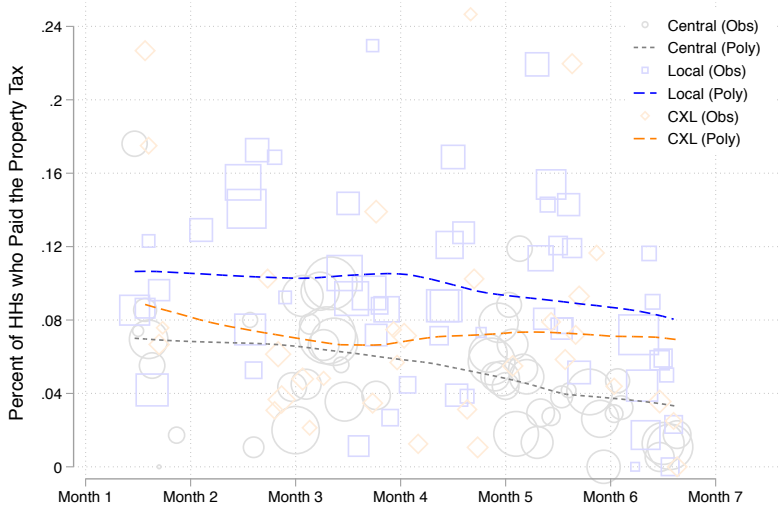

*Notes*: This figure shows the staggering of treatments and decrease in compliance (for Central and Local) over the tax campaign. Panel A shows a rug plot of observations in each treatment arm by day, according to receipts from handheld printers. Panel B shows trends in compliance for Central and Local treatments. Blue squares represent Local observations, gray circles represent Central observations, with size indicating number of observations. Lines — dashed blue for Local, dotted gray for Central — are local linear polynomials estimated using the displayed data, separately by treatment. This figure is discussed in Section 5.

## FIGURE A6: DECREASING COMPLIANCE OVER TIME — CLI, CXL

A: Adding Central + Local Information




B: Adding Central X Local




*Notes*: This figure shows the decrease in compliance (for CLI and CXL, relative to Central and Local) over the tax campaign. Panel A shows trends in compliance for Central, CLI, and Local treatments. Panel B shows trends for Central, CXL, and Local treatments. Blue squares represent Local observations, gray circles represent Central observations, green diamonds represent CLI observations (Panel A), and orange diamonds represent CXL observations (Panel B), with size indicating number of observations. Lines — dashed blue for Local, dotted gray for Central, dashed green for CLI (Panel A), dashed orange for CXL (Panel B) — are local linear polynomials estimated using the displayed data, separately by treatment. This figure is discussed in Section 5.

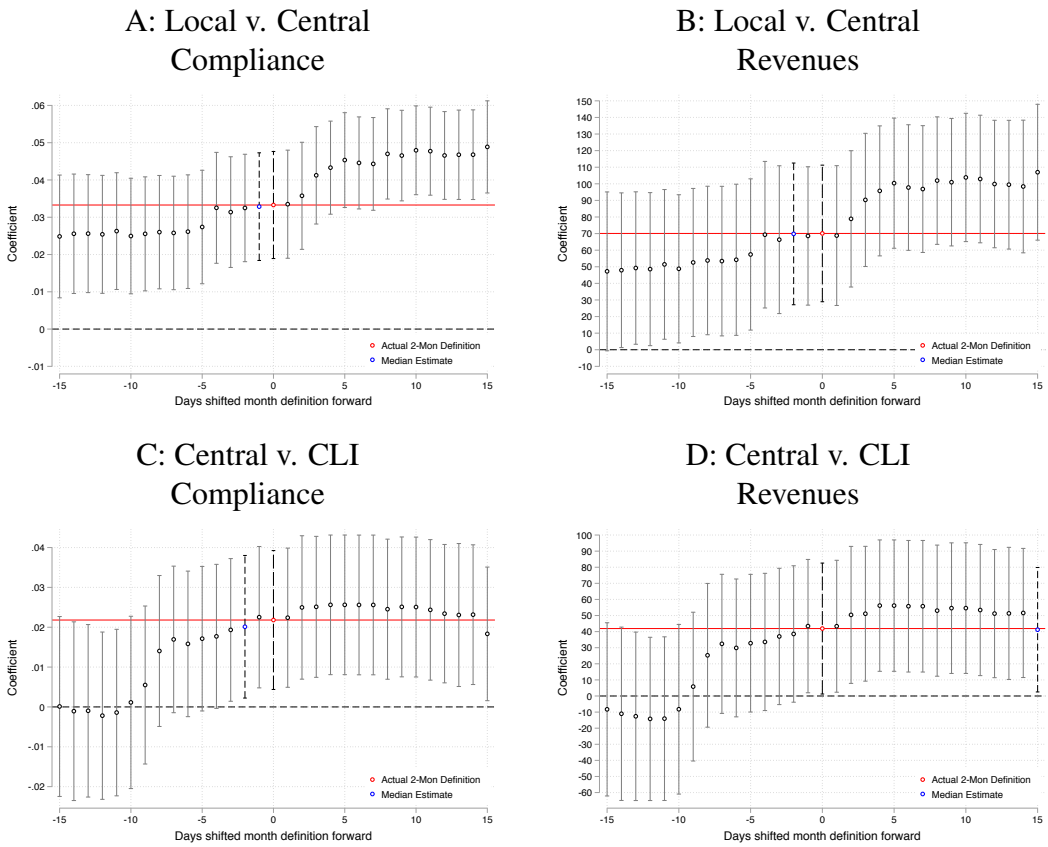
## A1.4 Additional Exhibits for Paper Section 6 — Main Results

### TABLE A4: LOCAL V. CENTRAL ROBUSTNESS: DIFFERENT APPROACHES TO TIME IMBALANCE

| | No Adjustment (1) | Two Month Fixed Effects (2) | Shift Median Two Month Fixed Effects (3) | Interaction Weighted Estimator (4) | One Month Fixed Effects (5) | Time Restriction (6) | Coarsened Exact Matching (7) |
|---|---|---|---|---|---|---|---|
| *Panel A: Compliance* | | | | | | | |
| Local | 0.023** | 0.033*** | 0.033*** | 0.031*** | 0.032*** | 0.042*** | 0.032*** |
| | (0.008) | (0.007) | (0.007) | (0.007) | (0.007) | (0.007) | (0.008) |
| Observations | 28872 | 27764 | 27506 | 37186 | 28872 | 25912 | 26637 |
| Clusters | 221 | 213 | 211 | 221 | 221 | 199 | 203 |
| Central Mean | .068 | .063 | .064 | .063 | .068 | .053 | .068 |
| *Panel B: Revenues* | | | | | | | |
| Local | 46.042* | 70.090*** | 69.822** | 73.932*** | 69.296** | 92.235*** | 78.782** |
| | (23.401) | (20.995) | (21.783) | (18.593) | (22.186) | (20.358) | (31.044) |
| Observations | 28872 | 27370 | 27664 | 36792 | 28872 | 25912 | 26637 |
| Clusters | 221 | 210 | 212 | 221 | 221 | 199 | 203 |
| Central Mean | 195.583 | 186.837 | 187.922 | 186.837 | 195.583 | 160.598 | 195.583 |
| One Month FE | No | No | No | No | Yes | No | No |
| Two Month FE | No | Yes | Yes | Yes | No | No | No |
| House FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Stratum FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes*: This table displays alternate approaches for addressing time imbalance in the comparison of the Local arm to the Central arm, the excluded category, as noted in Section 5 and discussed at length in Section A2.5. Panel A reports impacts on compliance, and Panel B reports impacts on revenues. Column 1 makes no adjustments. Column 2 includes the time period fixed effects described in Section 5. Column 3 includes time period fixed effects defined by selecting the median estimate among all permutations of the start date (Figure A7). Column 4 implements an interaction-weighted estimator, following Gibbons et al. (2018), in which time periods defined as in Column 2 are not included as fixed effects but interacted with the treatment indicator and the estimate is the average of the coefficient on the interaction terms, weighted by the number of observations in each period. Column 5 includes one-month fixed effects. Column 6 trims the sample to periods when both treatment arms were in operation. Column 7 implements coarsened exact matching (Iacus et al., 2012). All regressions include fixed effects for house type and randomization strata and cluster standard errors at the neighborhood level. We discuss these results in Section 6.1.

## FIGURE A7: SHIFTING TWO MONTH FIXED EFFECT START DATE




*Notes:* This figures displays robustness to shifting the start date for defining two month fixed effects 15 days forward and backwards from the start date in our preferred specification. Panels A and B report estimates for Local compared to Central collection for compliance and revenues, respectively. Panels C and D report estimates for Central + Local Information (CLI) compared to Central. The long-dashed red estimate reflects the estimate using the preferred definition of time periods; the short-dashed blue estimate is the median estimate among the shifted estimates. All regressions include fixed effects for house type and randomization strata and cluster standard errors at the neighborhood level. We discuss these results in Section 6.1 and report the median estimate in Table A4.

## TABLE A5: LOCAL V. CENTRAL ROBUSTNESS: FULLY-SATURATED MODEL WITH CROSS-RANDOMIZED TREATMENTS

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| *Panel A: Compliance* | | | | | | |
| Local | 0.033*** | 0.033*** | 0.020** | 0.039*** | 0.036*** | 0.022** |
| | (0.007) | (0.007) | (0.008) | (0.008) | (0.010) | (0.010) |
| Observations | 27764 | 27764 | 27764 | 23618 | 23618 | 23618 |
| Clusters | 213 | 213 | 213 | 213 | 213 | 213 |
| Central Mean | .063 | .063 | .063 | .068 | .068 | .068 |
| *Panel B: Revenues* | | | | | | |
| Local | 69.177** | 69.266** | 65.108* | 82.639*** | 72.985** | 75.423** |
| | (20.849) | (20.853) | (33.960) | (23.953) | (22.582) | (36.272) |
| Observations | 27764 | 27764 | 27764 | 23618 | 23618 | 23618 |
| Clusters | 213 | 213 | 213 | 213 | 213 | 213 |
| Central Mean | 184.65 | 184.65 | 184.65 | 198.695 | 198.695 | 198.695 |
| Tax Rate FE | No | Yes | Yes | No | No | Yes |
| Tax Rate X Local FE | No | No | Yes | No | No | Yes |
| Col. Bonus FE | No | No | No | Yes | Yes | Yes |
| Col. Bonus X Local FE | No | No | No | No | Yes | Yes |
| Time FE | Yes | Yes | Yes | Yes | Yes | Yes |
| House FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Stratum FE | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes*: This table reports estimates from Equation 1, comparing property tax outcomes in Local and Central (the excluded category). The panels show the estimates from separate regressions with the outcome an indicator for compliance (Panel A) and revenues (Panel B), respectively. All regressions include fixed effects for house, time period, and randomization strata, and they cluster standard errors at the neighborhood level. Column 1 shows the preferred specification, including no additional controls. Column 2 includes dummies for tax rate abatement groups. Column 3 adds interactions between the abatement group dummies and the Local indicator. Column 4 includes dummies for collector bonus type. Column 5 adds interactions between the collector bonus type dummies and the Local indicator. Column 6 includes abatement and collector bonus dummies and interactions with the Local indicator. Bergeron et al. (2020b) provides details on abatement and collector bonus treatment groups. We discuss these results in Section 6.1.

## TABLE A6: LOCAL V. CENTRAL ROBUSTNESS: INCLUDING CONTROLS, PILOT NEIGHBORHOODS, EXCLUDING MISASSIGNED NEIGHBORHOOD, AND TOP-CODING

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| *Panel A: Compliance* | | | | | | |
| Local | 0.032*** | 0.030*** | 0.031*** | 0.031*** | 0.033*** | 0.029*** |
| | (0.007) | (0.007) | (0.007) | (0.007) | (0.007) | (0.006) |
| Observations | 27748 | 27748 | 27748 | 28781 | 27655 | 219 |
| Clusters | 213 | 213 | 213 | 219 | 212 | |
| Central Mean | .063 | .063 | .063 | .064 | .063 | .061 |
| *Panel B: Revenues* | | | | | | |
| Local | 66.747** | 61.817** | 63.353** | 68.979** | 69.063** | 68.807** |
| | (20.887) | (21.330) | (21.130) | (20.836) | (20.779) | (20.645) |
| Observations | 27748 | 27748 | 27748 | 27748 | 27655 | 213 |
| Clusters | 213 | 213 | 213 | 213 | 212 | |
| Central Mean | 184.73 | 184.73 | 184.73 | 184.73 | 184.73 | 184.052 |
| *Controls*: | | | | | | |
| Age, Age$^2$, Gender | Yes | Yes | Yes | No | No | No |
| Distance to Schools (Imbalanced) | No | Yes | Yes | No | No | No |
| Employed, Salaried | No | No | Yes | No | No | No |
| Government Job (Self & Fam.) | No | No | Yes | No | No | No |
| Majority Tribe | No | No | Yes | No | No | No |
| *Adjustments*: | | | | | | |
| Includes Pilot Nbhds. | No | No | No | Yes | No | No |
| Excludes Misassigned Nbhd. | No | No | No | No | Yes | No |
| Top-Code 10% Nbhds. | No | No | No | No | No | Yes |
| Time FE | Yes | Yes | Yes | Yes | Yes | Yes |
| House FE | Yes | Yes | Yes | Yes | Yes | No |
| Stratum FE | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes*: This table reports estimates from Equation 1, comparing property tax outcomes in Local and Central (the excluded category). The panels show the estimates from separate regressions with the outcome an indicator for compliance (Panel A) and revenues (Panel B), respectively. All regressions include fixed effects for house, time period, and randomization strata, and they cluster standard errors at the neighborhood level. Column 1 includes controls for age, age-squared, and gender, measured in midline survey. Column 2 adds a control for distance from schools (the one imbalanced covariate when comparing Local to Central in Table A2). Column 3 adds controls for having any job, a salaried job, and a government job, a family member with a government job, and belonging to the majority tribe. When including controls, we replace missing values in control variables with the mean for the entire sample and include a separate dummy (for each control variable) for the value being missing. Column 4 includes pilot neighborhoods, with time period and stratum values that reflect its implementation several months before the campaign and in a remote neighborhood. Column 5 excludes the neighborhood misassigned from CXL to Local during the campaign. Column 6 displays estimates from a regression on mean outcomes at the neighborhood-level, winsorizing the top 10% of neighborhoods, using robust standard errors, and assigning the minimum value for time period fixed effects to a neighborhood. We discuss these results in Section 6.1.

## TABLE A7: LOCAL V. CENTRAL: EXEMPTION CATEGORIES

| | Exempted | Incorrect Exemption | Senior | Widow | Government Pension | Handicap | Exempted (by Coethnic) | Exempted (by Know Col.) |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Local | 0.039* | -0.012 | 0.041*** | -0.006 | 0.005 | 0.003** | 0.041 | -0.026 |
| | (0.021) | (0.007) | (0.014) | (0.012) | (0.003) | (0.001) | (0.032) | (0.024) |
| Local X Coethnic | | | | | | | 0.041 | |
| | | | | | | | (0.040) | |
| Coethnic | | | | | | | -0.080*** | |
| | | | | | | | (0.030) | |
| Local X Knows Collector | | | | | | | | 0.067* |
| | | | | | | | | (0.038) |
| Knows Collector | | | | | | | | 0.064** |
| | | | | | | | | (0.031) |
| | (0.021) | (0.007) | (0.014) | (0.012) | (0.003) | (0.001) | (0.056) | (0.029) |
| Observations | 13772 | 13771 | 13772 | 13772 | 13772 | 13772 | 7288 | 13772 |
| Clusters | 213 | 213 | 213 | 213 | 213 | 213 | 207 | 213 |
| Central Mean | .264 | .956 | .126 | .112 | .013 | .004 | .314 | .031 |
| Time FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| House FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Stratum FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* This table shows differences in the exemption rates of properties by chief and state collectors. Column 1 examines treatment effects on official exemptions. Column 2 reports whether third-party evaluations of exemption status diverged with the official designation. Columns 3–6 correspond to the different exemption categories: being senior (age 65+) in Column 3, being a widow in Column 4, receiving a government pension in Column 5 and being handicapped in Column 6. Columns 7 and 8 report exemptions by treatment and coethnicity between collectors and property owners and whether the collector and property owner know one another, respectively. All regressions include randomization stratum fixed effects and house fixed effects as well as the time fixed effects described in Section 5 and standard errors are clustered at the neighborhood-level. These results are discussed in Section 6.1.

## TABLE A8: LOCAL V. CENTRAL: AWARENESS OF OTHER TREATMENTS

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Local | 0.033*** | 0.030** | 0.035*** | 0.029* | 0.033*** | 0.033*** | 0.034** | 0.034** |
| | (0.007) | (0.013) | (0.009) | (0.016) | (0.008) | (0.010) | (0.012) | (0.012) |
| Local X # Adjacent in Other Treatment (Strict) | | 0.003 | | | | | | |
| | | (0.008) | | | | | | |
| # Adjacent in Other Treatment (Strict) | 0.004 | 0.003 | | | | | | |
| | (0.005) | (0.008) | | | | | | |
| Local X # Adjacent in Other Treatment (Broad) | | | | 0.003 | | | | |
| | | | | (0.006) | | | | |
| # Adjacent in Other Treatment (Broad) | | | -0.001 | -0.003 | | | | |
| | | | (0.004) | (0.005) | | | | |
| Local X Length of Border Shared with Other Treatment (Strict) | | | | | | -0.002 | | |
| | | | | | | (0.030) | | |
| Length of Border Shared with Other Treatment (Strict) | | | | | 0.007 | 0.008 | | |
| | | | | | (0.015) | (0.029) | | |
| Local X Length of Border Shared with Other Treatment (Broad) | | | | | | | 0.004 | 0.004 |
| | | | | | | | (0.018) | (0.018) |
| Length of Border Shared with Other Treatment (Broad) | | | | | | | -0.012 | -0.012 |
| | | | | | | | (0.020) | (0.020) |
| # Adjacent (Total) | -0.001 | -0.001 | 0.001 | 0.001 | | | | |
| | (0.003) | (0.003) | (0.003) | (0.003) | | | | |
| Length of Border (Total) | | | | | 0.002 | 0.001 | 0.006 | 0.006 |
| | | | | | (0.008) | (0.008) | (0.009) | (0.009) |
| Observations | 27764 | 27764 | 27764 | 27764 | 27764 | 27764 | 27764 | 27764 |
| Clusters | 213 | 213 | 213 | 213 | 213 | 213 | 213 | 213 |
| Central Mean | .068 | .068 | .068 | .068 | .068 | .068 | .068 | .068 |
| Time FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| House FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Stratum FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes*: This table analyzes potential spillovers due to awareness of other types of tax collectors working in adjacent neighborhoods. The specifications follow Miguel and Kremer (2004) in controlling for the number of adjacent neighborhoods in different treatments (as well as the total number of adjacent neighborhoods). We evaluate two definitions of alternate treatments: the "strict" version codes adjacent neighborhoods as being in the alternate treatment if in Central (for a Local neighborhood) or Local (for a Central neighborhood); the "broad" version codes this as Central, CLI, or CXL (if Local) and Local or CXL (if Central). Due to campaign staggering across neighborhoods, we only consider exposure to treatments in adjacent neighborhoods in which collectors had already worked or were currently working, rather than neighborhoods that had been assigned to a different treatment but had not yet received tax collectors. Columns 1 and 3 report estimates of the impact of Local, controlling for the number of adjacent neighborhoods in the alternate treatment arm and total adjacent neighborhoods, for the strict and broad definitions, respectively. Columns 2 and 4 report estimates of the impact of Local collection with an interaction term for the number of adjacent neighborhoods assigned to the alternate treatment arm, controlling for the total number of adjacent neighborhoods, for strict and broad, respectively. Columns 5 and 7 report estimates of the impact of Local, controlling for length of neighborhood borders (in kilometers) shared with the alternate treatment and total length of borders, for strict and broad respectively. Columns 6 and 8 report estimates of the impact of Local collection with an interaction term for the length of neighborhood borders shared with neighborhoods assigned to the alternate treatment arm, controlling for length of neighborhood borders shared with the alternate treatment and total length of borders, for strict and broad, respectively. We include fixed effects for house type, randomization strata and time periods described in Section 5 and cluster standard errors at the neighborhood level. We discuss these results in Section 6.1.

**TABLE A9: LOCAL V. CENTRAL: FISCAL EXTERNALITIES**

| *Dependent variable* | $\hat{\beta}$ | SE | $R^2$ | N | $\bar{x}_{Central}$ |
|---|---|---|---|---|---|
| *Panel A: Informal Labor Taxes* | | | | | |
| Salongo Extensive (Midline) | -0.031 | 0.032 | 0.057 | 13952 | 0.376 |
| Salongo Intensive (Midline) | -0.240 | 0.247 | 0.025 | 13568 | 1.659 |
| Salongo Extensive (Endline) | 0.005 | 0.028 | 0.063 | 2413 | 0.404 |
| Salongo Intensive (Endline) | 0.459 | 0.445 | 0.051 | 2358 | 3.996 |
| *Panel B: Other Formal Taxes* | | | | | |
| Vehicle Tax | 0.013 | 0.008 | 0.049 | 2405 | 0.031 |
| Market Vendor Fee | 0.057*** | 0.017 | 0.046 | 2409 | 0.128 |
| Business Tax | 0.008 | 0.010 | 0.044 | 2409 | 0.043 |
| Income Tax | 0.037*** | 0.014 | 0.031 | 2406 | 0.095 |
| Obsolete Tax | 0.003 | 0.005 | 0.025 | 2387 | 0.014 |

*Notes*: Each row summarizes an OLS estimation of Equation 1, comparing Local and Central, with the dependent variable noted in the first column. $\hat{\beta}$ is the coefficient on the treatment indicator, followed by the cluster-robust standard error, $R^2$, number of observations, and $\bar{x}_{Central}$ the Central group mean. In Panel A, rows 1 and 2 (3 and 4) report *salongo* contributions along the extensive margin and intensive margin of hours, respectively, at midline (endline). In Panel B, the outcomes are self-reported payment of other formal taxes at endline. Obsolete tax is a poll tax, which existed in the past but does not currently exist, to test the reliability of self-reports. All regressions include fixed effects for randomization strata, and cluster standard errors at the neighborhood level. Regressions using midline data include house type fixed effects, while those using endline data do not, as discussed in Section 5, because this affords analysis in a larger endline sample. The number of observations varies across regressions due to (i) outcomes being drawn from different surveys, and (ii) non-response for specific survey questions. We discuss these results in Section 6.1.

**TABLE A10: LOCAL V. CENTRAL: INFORMAL LABOR TAX SUBSTITUTION**

| | Salongo (Midline) | Salongo Hours (Midline) | Salongo (Endline) | Salongo Hours (Endline) |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Local | -0.026 | -0.207 | 0.000 | 0.490 |
| | (0.032) | (0.254) | (0.030) | (0.454) |
| Local X Paid Tax | -0.075** | -0.262 | -0.051 | -1.387 |
| | (0.035) | (0.226) | (0.070) | (1.039) |
| Paid Tax | 0.061** | -0.128 | 0.038 | 0.757 |
| | (0.029) | (0.167) | (0.052) | (0.796) |
| Time FE | Yes | Yes | No | No |
| House FE | Yes | Yes | Yes | Yes |
| Stratum FE | Yes | Yes | Yes | Yes |
| Observations | 13953 | 13569 | 2330 | 2278 |
| Clusters | 206 | 205 | 221 | 221 |
| Central Mean (No Pay) | .372 | 1.685 | .406 | 4.008 |

*Notes*: This table shows estimates from versions of Equation 1, comparing the Local arm to the Central arm (excluded group), where we include an interaction with verified property tax payment. The outcome is informal labor tax (*salongo*) participation as measured in the midline and endline surveys. Columns 1 and 2 report *salongo* contributions along the extensive margin and intensive margin (hours contributed), respectively, at midline. Columns 3 and 4 report the same at endline. All regressions include fixed effects for house type and randomization strata and cluster standard errors at the neighborhood level. Columns 1 and 2 include time period fixed effects because they analyze midline data, as discussed in Section 5. We discuss these results in Section 6.1.

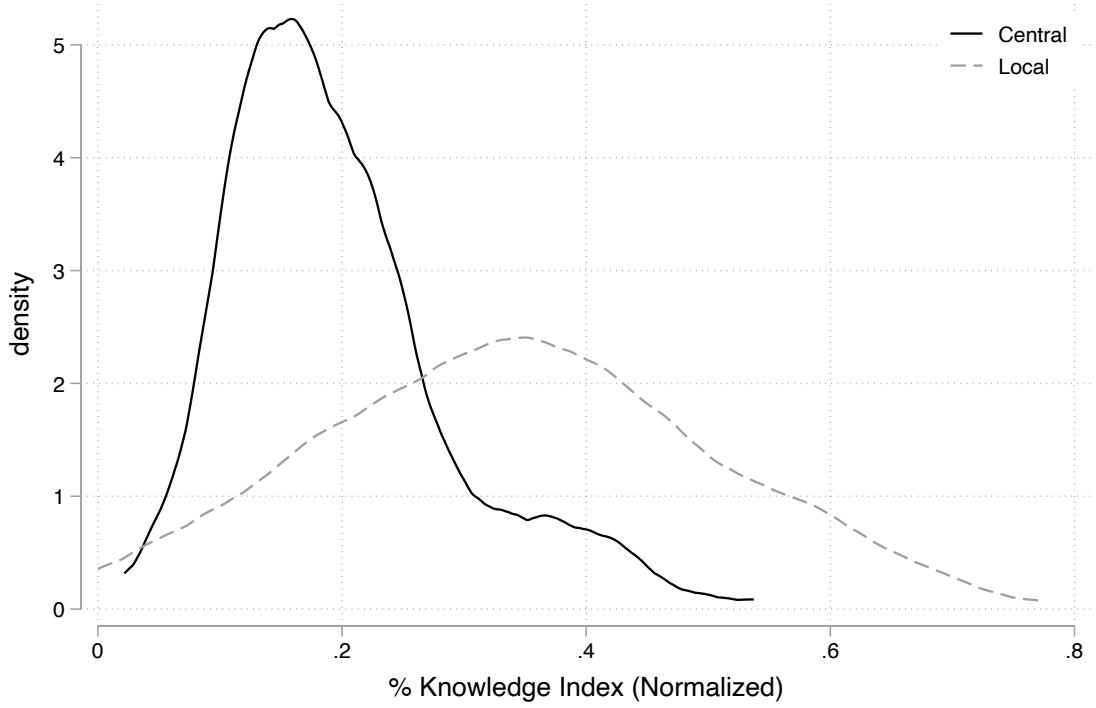
## A1.5 Additional Exhibits for Paper Section 7 — Mechanisms

**TABLE A11: LOCAL V. CENTRAL: TAX VISITS — NO HOUSE FIXED EFFECTS**

| | Visited by Collector | Number of Visits by Collector | Other Contact with Collector | Instances of Other Contact |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Local | -0.006 | 0.020 | 0.008 | 0.019 |
| | (0.026) | (0.047) | (0.007) | (0.012) |
| Time FE | Yes | Yes | Yes | Yes |
| Stratum FE | Yes | Yes | Yes | Yes |
| Observations | 18265 | 18254 | 3533 | 3533 |
| Clusters | 209 | 209 | 206 | 206 |
| Mean | .417 | .553 | .025 | .039 |

*Notes*: This table reports estimates from Equation 1, comparing the tax visits collectors made after registration in Local and Central (the excluded category). All regressions include fixed effects for randomization strata and time periods described in Section 5, and cluster standard errors at the neighborhood level. Columns 1 and 2 report differences in tax visits by collectors — after the registration visit — by the extensive and intensive margins, respectively. Columns 3 and 4 report differences in other contact with collectors outside of the tax campaign, as reported by citizens, by the intensive and extensive margins, respectively. We discuss these results in Section 7.1.

## FIGURE A8: KNOWLEDGE QUIZ: CHIEFS V. STATE COLLECTORS




*Notes*: This figure shows the distributions of knowledge about citizens for chiefs and state collectors. Knowledge of the inhabitants of the neighborhood is measured by the percentage of correct answers regarding a random sample of property owners in a short quiz-type survey module conducted after tax collection. Questions included the owner's name, education level, and occupation. Chiefs took quizzes for the neighborhoods they work in; central agents took quizzes for randomly selected neighborhoods to simulate the knowledge they would have if assigned to a location before collecting taxes there. We discuss these results in Section 7.2.

## TABLE A12: CENTRAL V. CENTRAL + LOCAL INFORMATION ROBUSTNESS: DIFFERENT APPROACHES TO TIME IMBALANCE

| | No Adjustment (1) | Two Month Fixed Effects (2) | Shift Median Two Month Fixed Effects (3) | Interaction Weighted Estimator (4) | One Month Fixed Effects (5) | Time Restriction (6) | Coarsened Exact Matching (7) |
|---|---|---|---|---|---|---|---|
| *Panel A: Compliance* | | | | | | | |
| Central Plus Local Info | -0.001 | 0.024** | 0.019** | -0.004 | 0.024** | 0.019** | 0.041** |
| | (0.011) | (0.009) | (0.009) | (0.008) | (0.010) | (0.009) | (0.016) |
| Observations | 23911 | 20636 | 19767 | 32754 | 23911 | 18834 | 8575 |
| Clusters | 190 | 165 | 161 | 190 | 190 | 150 | 72 |
| Central Mean | .068 | .051 | .057 | .051 | .068 | .055 | .024 |
| *Panel B: Revenues* | | | | | | | |
| Central Plus Local Info | -10.212 | 41.902** | 41.172** | -29.916 | 59.016** | 38.935* | 53.718 |
| | (26.570) | (20.751) | (19.723) | (22.029) | (21.186) | (20.732) | (35.225) |
| Observations | 23911 | 20176 | 20507 | 31963 | 23911 | 18834 | 8575 |
| Clusters | 190 | 162 | 160 | 190 | 190 | 150 | 72 |
| Central Mean | 195.583 | 157.56 | 140.433 | 157.56 | 195.583 | 158.507 | 61.726 |
| One Month FE | No | No | No | No | Yes | No | No |
| Two Month FE | No | Yes | Yes | Yes | No | No | No |
| House FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Stratum FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes*: This table displays alternate approaches for addressing time imbalance in the comparison of the Central + Local Information (CLI) arm to the Central arm, the excluded category. Panel A reports impacts on compliance, and Panel B reports impacts on revenues. Column 1 makes no adjustments. Column 2 includes the time period fixed effects described in Section 5. Column 3 includes time period fixed effects defined by selecting the median estimate among all permutations of the start date (Figure A7). Column 4 implements an interaction-weighted estimator, following Gibbons et al. (2018), in which time periods defined as in Column 2 are not included as fixed effects but interacted with the treatment indicator and the estimate is the weighted average of the coefficient on the interaction terms, weighted by the number of observations in each period. Column 5 includes one-month fixed effects. Column 6 trims the sample to periods when both treatment arms are in operation. Column 7 implements coarsened exact matching (Iacus et al., 2012). All regressions include fixed effects for house type and randomization strata and cluster standard errors at the neighborhood level. We discuss these results in Section 7.2.

## TABLE A13: CENTRAL V. CENTRAL + LOCAL INFORMATION ROBUSTNESS: CONTROLLING FOR IMBALANCED MIDLINE COVARIATES

| | Compliance | Revenues | Visited | Visits | Compliance | Compliance |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: Including Imbalanced Midline Covariates* | | | | | | |
| Central Plus Local Info | 0.024** | 54.046** | -0.008 | -0.021 | 0.021 | 0.030** |
| | (0.011) | (25.793) | (0.034) | (0.055) | (0.016) | (0.011) |
| Local | | | | | | 0.065*** |
| | | | | | | (0.009) |
| Controls for Imbalanced Covariates | Yes | Yes | Yes | Yes | Yes | Yes |
| Visit Control | No | No | No | No | Yes | No |
| Time FE | Yes | Yes | Yes | Yes | Yes | Yes |
| House FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Stratum FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 10064 | 10064 | 10051 | 10048 | 3864 | 16436 |
| Clusters | 155 | 155 | 155 | 155 | 150 | 253 |
| Central Mean | .059 | 161.639 | .393 | .51 | .1 | .059 |
| Test CLI=Local p-value | | | | | | 0.002 |
| *Panel B: Excluding House Fixed Effects* | | | | | | |
| Central Plus Local Info | 0.024** | 26.177 | -0.018 | -0.029 | 0.027* | 0.023** |
| | (0.009) | (23.716) | (0.028) | (0.044) | (0.014) | (0.009) |
| Local | | | | | | 0.045*** |
| | | | | | | (0.007) |
| Visit Control | No | No | No | No | Yes | No |
| Time FE | Yes | Yes | Yes | Yes | Yes | Yes |
| House FE | No | No | No | No | No | No |
| Stratum FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 20636 | 20636 | 13884 | 13877 | 5283 | 33746 |
| Clusters | 165 | 165 | 163 | 163 | 161 | 267 |
| Central Mean | .051 | 152.399 | .387 | .497 | .097 | .052 |
| Test CLI=Local ($p$-value) | | | | | | 0.01 |

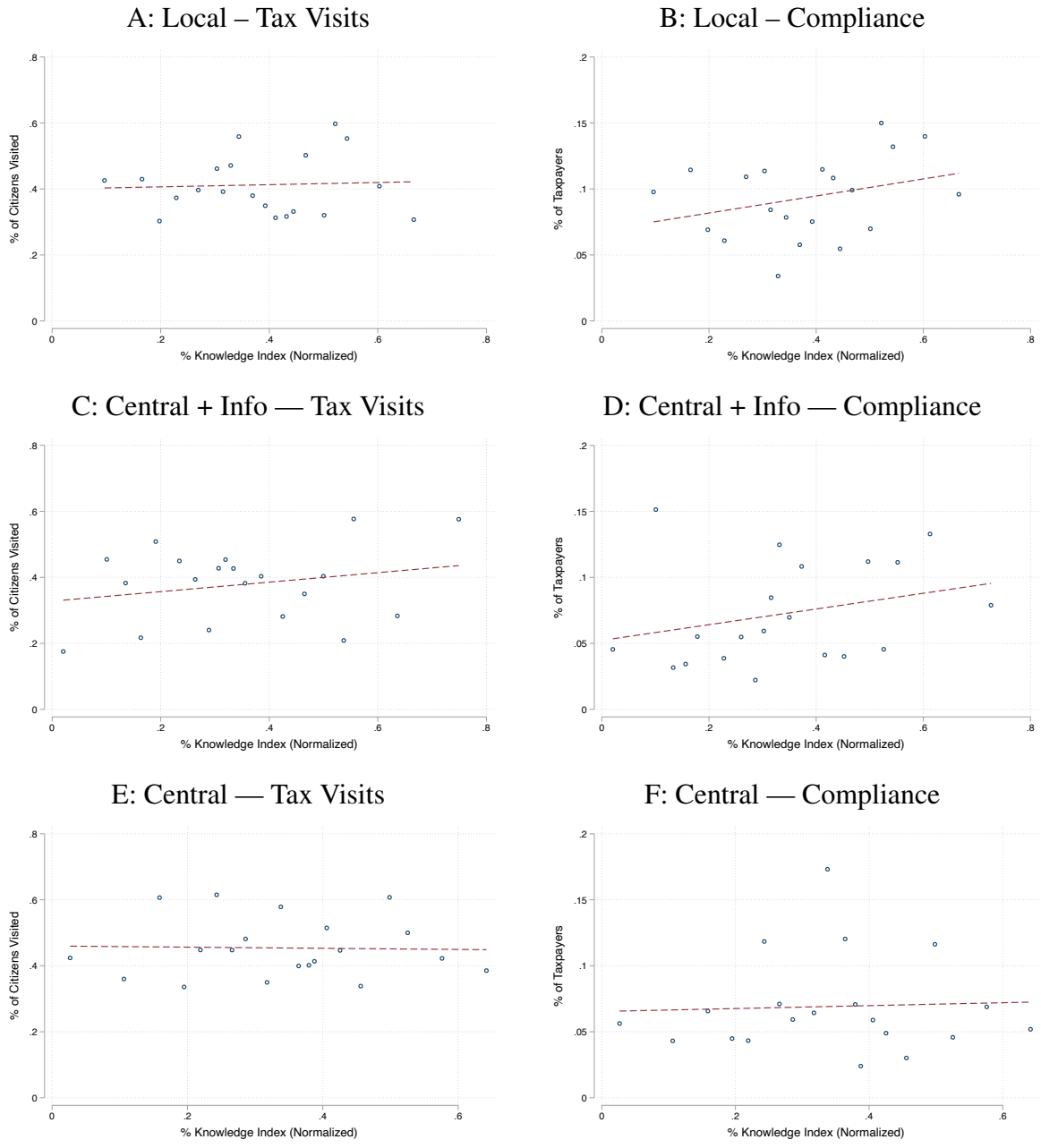
*Notes*: This table compares the Central + Local Information (CLI) arm to the Central arm, the excluded category, controlling for the characteristics imbalanced at midline — sex of property owner, whether property owner is salaried, and distance to state buildings and market — as shown in Table A2 (Panel A) and excluding house type fixed effects (Panel B). Columns 1, 5, and 6 report impacts on compliance. Column 2 reports impacts on revenues. Columns 3 and 4 report differences in tax visits by collectors after registration by the extensive and intensive margins, respectively. All regressions include fixed effects randomization strata and time periods, and cluster standard errors at the neighborhood level. Column 5 restricts to the subsample of properties that received any tax visits after registration. Column 6 includes a dummy for the Local treatment in the regression. The bottom row reports the $p$-value from a test for equality between the CLI and Local. We discuss these results in Section 7.2.

## TABLE A14: THE VALUE OF CHIEFS' INFORMATION — NO HOUSE FIXED EFFECTS

| | Visited (1) | Compliance (2) | Visited (3) | Compliance (4) | Compliance (5) | Visited (6) | Compliance (7) | Visited (8) | Compliance (9) |
|---|---|---|---|---|---|---|---|---|---|
| *Panel A: Ease of payment* | | | | | | | | | |
| Ease of payment | 0.046*** | 0.055*** | 0.029** | 0.043*** | 0.084*** | | | | |
| | (0.012) | (0.007) | (0.014) | (0.008) | (0.012) | | | | |
| Predicted Ease of payment | | | | | | 0.054** | 0.045*** | 0.013 | 0.040*** |
| | | | | | | (0.017) | (0.012) | (0.017) | (0.007) |
| Wall quality | | | 0.027** | 0.017** | | 0.017* | 0.008 | 0.021** | 0.011** |
| | | | (0.012) | (0.007) | | (0.010) | (0.006) | (0.010) | (0.004) |
| Roof quality | | | 0.005 | 0.000 | | 0.003 | -0.004 | 0.018** | -0.010 |
| | | | (0.006) | (0.002) | | (0.006) | (0.004) | (0.008) | (0.006) |
| Erosion threat | | | 0.017 | -0.003 | | 0.002 | -0.007 | -0.000 | -0.005 |
| | | | (0.011) | (0.004) | | (0.011) | (0.007) | (0.010) | (0.004) |
| Observations | 5623 | 8214 | 4599 | 5215 | 2121 | 5828 | 5843 | 5007 | 5013 |
| Clusters | 79 | 80 | 66 | 66 | 77 | 93 | 93 | 80 | 80 |
| Mean | .375 | .072 | .35 | .065 | .129 | .435 | .103 | .41 | .059 |
| *Panel B: Willingness to pay* | | | | | | | | | |
| Willingness to pay | 0.035** | 0.037*** | 0.033** | 0.038*** | 0.058*** | | | | |
| | (0.011) | (0.007) | (0.012) | (0.008) | (0.012) | | | | |
| Predicted Willingness to pay | | | | | | 0.045** | 0.036*** | 0.007 | 0.032*** |
| | | | | | | (0.016) | (0.010) | (0.015) | (0.009) |
| Wall quality | | | 0.025* | 0.017** | | 0.018* | 0.009 | 0.021** | 0.011** |
| | | | (0.013) | (0.008) | | (0.009) | (0.006) | (0.010) | (0.005) |
| Roof quality | | | 0.011 | 0.001 | | 0.004 | -0.003 | 0.018** | -0.010 |
| | | | (0.008) | (0.002) | | (0.006) | (0.004) | (0.008) | (0.006) |
| Erosion threat | | | 0.016 | -0.004 | | 0.002 | -0.006 | -0.000 | -0.005 |
| | | | (0.012) | (0.005) | | (0.011) | (0.007) | (0.010) | (0.005) |
| Observations | 3981 | 5596 | 3977 | 4525 | 1428 | 5828 | 5843 | 5007 | 5013 |
| Clusters | 50 | 50 | 50 | 50 | 48 | 93 | 93 | 80 | 80 |
| Mean | .356 | .062 | .356 | .066 | .108 | .435 | .103 | .41 | .059 |
| Treatment | CLI | CLI | CLI | CLI | CLI | Local | Local | Central | Central |
| Visited Only | No | No | No | No | Yes | No | No | No | No |
| Stratum FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes*: This table explores the extent to which chiefs' recommendations in Central + Local Information (CLI) predict tax visits after registration and tax payment, while excluding house fixed effects as a robustness check. Columns 1–5 show correlations in CLI between chiefs' recommendations and outcomes. Columns 6–9 report correlations between predicted propensity measures described in Section 7.2 and outcomes in the Local (Columns 6 and 7) and the Central (Columns 8 and 9) arms. Columns 1, 3, 6, and 8 show correlations between propensity and visits; Columns 2, 4, 5, 7, and 9 show correlations between propensity and compliance. Column 5 shows correlations with compliance conditional on receiving a visit after registration. All regressions include randomization stratum fixed effects and cluster standard errors at the neighborhood level. Columns 3, 4, and 6–8 include controls for observable household characteristics. We discuss these results in Section 7.2.

## FIGURE A9: TAX VISITS AND COMPLIANCE BY CHIEF KNOWLEDGE OF CITIZENS




*Notes*: This figure shows the relationship between chiefs' knowledge of the inhabitants of the neighborhood and (i) the percent of property owners who received a tax visit after registration (Panels A, C, and E), and (ii) the level of tax compliance (Panels B, D, and F). Chiefs' knowledge of the inhabitants of the neighborhood is measured by the percentage of correct answers when asked to provide the name, education level, and occupation of a randomly selected group property owners. We show these relationships for neighborhoods assigned to Local in Panels A and B as well as neighborhoods assigned to CLI and Central tax collection in Panels C and D, and E and F, respectively. Table A15 analyzes these relationships in a regression framework. We discuss these results in Section 7.2.

**TABLE A15: TAX VISITS AND COMPLIANCE BY CHIEF KNOWLEDGE OF CITIZENS**

| | Central | | CLI | | Local | |
|---|---|---|---|---|---|---|
| | Visits | Compliance | Visits | Compliance | Visits | Compliance |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Chief Info > Median | -0.020 | -0.007 | 0.010 | 0.028* | -0.016 | 0.024* |
| | (0.041) | (0.012) | (0.043) | (0.017) | (0.034) | (0.012) |
| Observations | 110 | 110 | 79 | 80 | 111 | 111 |
| Mean | .454 | .069 | .377 | .073 | .412 | .093 |

*Notes*: This table shows the relationship between city chiefs' knowledge of the inhabitants of the neighborhood and (i) the percent of property owners who received a tax visit after registration (Columns 1, 3, and 5), and (ii) the level of tax compliance (Columns 2, 4, and 6). Chiefs' knowledge of the inhabitants of the neighborhood is measured by the percentage of correct answers when asked to provide the name, education level, and occupation of a randomly selected group property owners. We show these relationships for neighborhoods assigned to (i) Central (Columns 1–2), where state collectors did not consult with chiefs — a placebo check — (ii) Central + Local Information (Columns 3–4), where state collectors did consult with chiefs, and (iii) Local (Columns 5–6), where chiefs themselves collected taxes. We discuss these results in Section 7.2.

**TABLE A16: COLLECTOR OUTCOMES AS A FUNCTION OF DISTANCE TO THEIR OWN NEIGHBORHOODS**

| | State collectors | | Chief Collectors | |
|---|---|---|---|---|
| | Compliance | Revenue (in CF) | Compliance | Revenue (in CF) |
| | (1) | (2) | (3) | (4) |
| Distance (state collector) | -0.007*** | -13.591** | | |
| | (0.002) | (6.018) | | |
| Distance (chief collector) | | | -0.003 | 6.424 |
| | | | (0.019) | (63.335) |
| Time FE | Yes | Yes | Yes | Yes |
| House FE | Yes | Yes | Yes | Yes |
| Stratum FE | No | No | No | No |
| Observations | 22398 | 22398 | 13753 | 13753 |
| Clusters | 183 | 183 | 108 | 108 |
| Mean | .066 | 175.243 | .095 | 255.901 |

*Notes*: This table estimates the relationship between tax compliance (Columns 1 and 3) or tax revenue (Columns 2 and 4) and the distance between collectors' houses and the neighborhoods in which they worked. We estimate this relationship for state collectors in Central and CLI by calculating the average distance for the two randomly assigned collectors (Columns 1 and 2). The relationship for chief collectors is reported in Columns 3 and 4 for completeness, though there is little variation for chief collectors who hailed from the neighborhoods in which they taxed. All regressions include house type and randomization stratum fixed effects as well as the time fixed effects described in Section 5. We cluster standard errors at the neighborhood level. We discuss these results in Section 7.2.

**TABLE A17: LOCAL V. CENTRAL: STATE COLLECTORS WORKING NEAR THEIR HOMES**

| | State Collectors Working Near Home | | State Collectors Working Far from Home | |
|---|---|---|---|---|
| | Compliance | Revenue (in CF) | Compliance | Revenue (in CF) |
| | (1) | (2) | (3) | (4) |
| *Panel A: Chiefs v. State Collectors in Central* | | | | |
| Local | 0.027** | 62.778* | 0.034*** | 67.428*** |
| | (0.012) | (32.068) | (0.009) | (24.820) |
| Observations | 17225 | 17225 | 24635 | 24635 |
| Clusters | 142 | 142 | 199 | 199 |
| Central Mean | .069 | 205.113 | .062 | 178.575 |
| *Panel B: Chiefs v. State Collectors in Central and CLI* | | | | |
| Local | 0.031** | 73.889** | 0.038*** | 86.861*** |
| | (0.013) | (34.000) | (0.007) | (18.853) |
| Observations | 17448 | 17448 | 28874 | 28874 |
| Clusters | 153 | 153 | 237 | 237 |
| Central Mean | .055 | 181.043 | .051 | 143.221 |
| Time FE | Yes | Yes | Yes | Yes |
| House FE | Yes | Yes | Yes | Yes |
| Strata FE | No | No | No | No |

*Notes*: This table estimates Equation 1 using as the dependent variable whether households paid the property tax (Columns 1 and 3) and the amount of revenues collected (Columns 2 and 4). It includes state collectors in Central (Panel A) and in Central and CLI (Panel B) as the comparison group. We include Panel B, lumping Central and CLI, to increase the number of state collectors randomly assigned to work near their homes in the analysis. Columns 1 and 2 compare chief collection to state tax collection in cases where at least one assigned state collector lived nearby. We define "nearby" as the maximum distance between a chief's house and the neighborhood in which they taxed, which is 1.59 km in the data. Columns 3 and 4 compare chief collection to state tax collection in cases where no assigned state collector lived nearby. All regressions include house type and the time fixed effects described in Section 5 and cluster standard errors at the neighborhood level. We do not include fixed effects for randomization strata as a large share of strata do not contain a neighborhood from each comparison group (49% of strata include only one treatment when comparing Local to Central near home, 30% include only one when comparing Local to Central and CLI near home). We discuss these results in Section 7.2.

**TABLE A18: LOCAL V. CENTRAL: COLLECTION DURING PROPERTY REGISTRATION**

| | Collection Outcomes during Registration Visit | | | | | |
|---|---|---|---|---|---|---|
| | Compliance | | | Revenues | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Local | -0.001 | -0.001 | -0.000 | -2.564 | -2.850 | -1.593 |
| | (0.002) | (0.002) | (0.001) | (4.278) | (4.334) | (4.059) |
| Time FE | No | No | Yes | No | No | Yes |
| House FE | No | Yes | Yes | No | Yes | Yes |
| Stratum FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 28872 | 28872 | 27764 | 28872 | 28872 | 27764 |
| Clusters | 221 | 221 | 213 | 221 | 221 | 213 |
| Central Mean | .006 | .006 | .006 | 16.116 | 16.116 | 15.657 |

*Notes*: This table estimates Equation 1 using as the dependent variable whether households paid the property tax during the property registration (Columns 1–3) and the revenue collected (Columns 4–6). As described in the text, collectors were instructed to solicit the tax at the end of each registration visit with households. During property registration, collectors followed a linear property-by-property route through neighborhoods, as demonstrated in Figure A10, meaning that collectors could not selectively target taxpayers at this stage of the campaign. All regressions include randomization stratum fixed effects and cluster standard errors at the neighborhood level. Columns 2, 3, 5, and 6 include house type fixed effects. Columns 3 and 6 include time fixed effects described in Section 5. We discuss these results in Section 7.3.

**FIGURE A10: COLLECTORS' ROUTE THROUGH SAMPLE NEIGHBORHOOD DURING PROPERTY REGISTRATION.**


*Notes*: This map shows the linear, property-by-property route taken by collectors in a sample neighborhood in the Quartier of Malanji. Due to error in GPS measures, some points appear slightly outside of the neighborhood (or across the street). This figure is discussed in Section 7.3.

## TABLE A19: HETEROGENEITY BY BASELINE CHIEF CHARACTERISTICS

| | Tax Compliance | | | | |
|---|---|---|---|---|---|
| *Baseline Chief Characteristic:* | Evaluation (Index > Median) | Embeddedess (Index > Median) | Activity (Index > Median) | Remoteness (Index > Median) | Chefferie Indicator |
| | (1) | (2) | (3) | (4) | (5) |
| Local | 0.033*** | 0.023** | 0.023** | 0.026** | 0.041*** |
| | (0.009) | (0.011) | (0.009) | (0.010) | (0.007) |
| Local X Chief Characteristic | -0.001 | 0.019 | 0.028* | 0.011 | -0.043 |
| | (0.014) | (0.016) | (0.016) | (0.015) | (0.026) |
| Chief Characteristic | 0.014 | 0.006 | 0.015 | -0.011 | 0.024 |
| | (0.011) | (0.013) | (0.012) | (0.013) | (0.025) |
| Time FE | Yes | Yes | Yes | Yes | Yes |
| House FE | Yes | Yes | Yes | Yes | Yes |
| Stratum FE | Yes | Yes | Yes | Yes | Yes |
| Observations | 27764 | 27764 | 27764 | 27028 | 27764 |
| Clusters | 213 | 213 | 213 | 212 | 213 |
| Central Mean (Char. Low Value) | .056 | .062 | .057 | .069 | .061 |

*Notes*: This table shows heterogeneity by neighborhood means of chief characteristics as measured at baseline. Column 1 shows results for an index of citizen evaluation of the chief, Column 2 for an index of how embedded the chief is in the community, Column 3 for an index of how active the chief is rated by citizens, and Column 4 shows estimates for a measure of remoteness defined as distance from the city center. In Columns 1–4, the heterogeneity variable is an indicator for the index value being greater than the median value within the relevant treatment arm. Column 5 reports heterogeneity by whether the neighborhood belongs to a *chefferie*, neo-customary zones in which chiefs have more responsibilities and authority. The mean for the below-median group (i.e. a zero value for each indicator variable) within the Central treatment arm is reported at the bottom. All regressions include time, house, and stratum fixed effects and cluster standard errors at the neighborhood level. This figure is discussed in Section 7.3.

**TABLE A20: HETEROGENEITY NEIGHBORHOOD BY REMOTENESS**

| | Trust in Chief | Evaluation of Chief | Honesty of Chief | View of Chief (Index) | Tax Compliance |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Periphery | 0.464** | 1.024*** | 0.330* | 0.723*** | -0.042 |
| | (0.202) | (0.218) | (0.188) | (0.184) | (0.037) |
| Local | | | | | 0.055* |
| | | | | | (0.030) |
| Local X Periphery | | | | | -0.035 |
| | | | | | (0.031) |
| House FE | Yes | Yes | Yes | Yes | Yes |
| Stratum FE | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.062 | 0.072 | 0.074 | 0.071 | 0.018 |
| Observations | 2344 | 1773 | 1862 | 2348 | 29361 |
| Clusters | 221 | 221 | 221 | 221 | 221 |
| Mean | .006 | 3.309 | .014 | .017 | .073 |

*Notes*: This table shows heterogeneity by the remoteness of the neighborhood, as a proxy for chief legitimacy and power. *Periphery* is an indicator for neighborhoods above the median distance from the city center. Columns 1-4 examine the perceived trust levels, performance evaluations, perceived honesty, and overall attitudes, respectively, of chiefs. Column 5 then examines heterogeneity in the treatment effect of chief tax collection, relative to Central, by the remoteness of the neighborhood. All regressions include house and stratum fixed effects and cluster standard errors at the neighborhood level. This figure is discussed in Section 7.3.

**TABLE A21: FLIER MESSAGE EFFECTS ON TAX COMPLIANCE**

| | Tax Compliance | | | Tax Revenue (in CF) | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Local | 0.036*** | | | 109.100*** | | |
| | (0.008) | | | (31.221) | | |
| Central Deterrence | | 0.013* | 0.014* | | 42.715 | 43.394* |
| | | (0.007) | (0.007) | | (25.976) | (25.720) |
| Local Deterrence | | 0.010 | 0.012* | | 15.667 | 19.689 |
| | | (0.007) | (0.007) | | (20.512) | (20.355) |
| Central Public Goods | | 0.005 | 0.005 | | 8.885 | 8.546 |
| | | (0.007) | (0.007) | | (20.910) | (20.487) |
| Local Public Goods | | 0.006 | 0.008 | | 30.113 | 34.374 |
| | | (0.007) | (0.007) | | (25.280) | (24.853) |
| Trust | | 0.010 | 0.011 | | 29.848 | 32.267 |
| | | (0.007) | (0.007) | | (23.055) | (22.938) |
| Observations | 4783 | 6796 | 6796 | 4783 | 6796 | 6796 |
| Mean | .012 | .024 | .024 | 30.326 | 59.64 | 59.64 |
| House FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Time FE | Yes | No | No | Yes | No | No |
| Strata FE | Yes | No | No | Yes | No | No |
| Neighborhood FE | No | No | Yes | No | No | Yes |

*Notes*: This table reports estimates from a regression of tax compliance (Columns 1–3) and tax revenue (Columns 4–6) on indicators for assignment to the Local treatment or the Central arm (Columns 1 and 4), or on indicators for the randomized messages printed on the tax letters distributed at registration (Columns 2–3 and 5–6). Section A2.2 provides descriptions of the central deterrence, local deterrence, central public goods, local public goods, and trust treatment messages. The excluded category in all regressions analyzing fliers is the control message "It is important to pay the property tax." All regressions include type of house fixed effects. Columns 1 and 4 include geographic randomization stratum fixed effects and the time fixed effects described in Section 5. Columns 3 and 6 include neighborhood fixed effects (tax message treatment randomization strata). The data are restricted to the subsample of properties subject to randomized messages on tax laters, which were introduced toward the end of the property tax campaign. We discuss these results in Section 7.3.

## TABLE A22: LOCAL V. CENTRAL: INTERACTIONS WITH FLIER MESSAGES

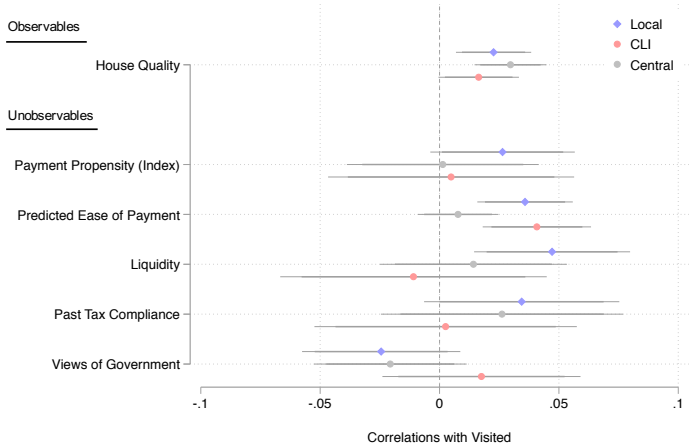
| | Tax Compliance | | Tax Revenue | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Panel A: Central Deterrence Message* | | | | |
| Local | 0.052** | 0.054** | 179.273** | 196.565** |
| | (0.017) | (0.018) | (53.603) | (60.449) |
| Central Deterrence | 0.008 | 0.008 | 17.214 | 16.158 |
| | (0.007) | (0.007) | (13.942) | (14.137) |
| Local X Central Deterrence | 0.008 | 0.010 | 44.815 | 51.255 |
| | (0.015) | (0.016) | (66.115) | (71.207) |
| Observations | 1675 | 1580 | 1675 | 1580 |
| Mean | .034 | .035 | 95.343 | 98.544 |
| *Panel B: Local Deterrence Message* | | | | |
| Local | 0.034** | 0.032* | 68.542** | 65.197* |
| | (0.016) | (0.018) | (30.627) | (33.448) |
| Local Deterrence | 0.008 | 0.008 | 14.587 | 14.602 |
| | (0.008) | (0.008) | (13.365) | (13.347) |
| Local X Local Deterrence | 0.007 | 0.010 | 5.913 | 12.036 |
| | (0.015) | (0.016) | (35.484) | (38.078) |
| Observations | 1682 | 1585 | 1682 | 1585 |
| Mean | .033 | .035 | 78.954 | 82.524 |
| *Panel C: Central Public Goods Message* | | | | |
| Local | 0.043** | 0.043** | 89.765** | 89.487** |
| | (0.013) | (0.015) | (25.922) | (28.285) |
| Central Public Goods | 0.008 | 0.008 | 21.920** | 21.942** |
| | (0.005) | (0.005) | (9.719) | (9.683) |
| Local X Central Public Goods | -0.011 | -0.010 | -42.841 | -40.956 |
| | (0.013) | (0.014) | (35.131) | (37.805) |
| Observations | 1674 | 1581 | 1674 | 1581 |
| Mean | .027 | .028 | 65.591 | 68.185 |
| *Panel D: Local Public Goods Message* | | | | |
| Local | 0.035** | 0.037** | 65.192* | 81.790** |
| | (0.014) | (0.015) | (35.734) | (37.007) |
| Local Public Goods | 0.012 | 0.012 | 66.663 | 65.890 |
| | (0.008) | (0.008) | (47.133) | (47.163) |
| Local X Local Public Goods | -0.010 | -0.008 | -53.038 | -48.424 |
| | (0.017) | (0.018) | (65.423) | (68.030) |
| Observations | 1674 | 1579 | 1674 | 1579 |
| Mean | .03 | .031 | 87.336 | 91.324 |
| *Panel E: Trust Message* | | | | |
| Local | 0.041** | 0.040** | 95.835** | 95.705** |
| | (0.017) | (0.018) | (33.016) | (35.821) |
| Trust | 0.011 | 0.011 | 29.969 | 30.158 |
| | (0.009) | (0.009) | (21.096) | (21.255) |
| Local X Trust | -0.004 | -0.002 | -13.603 | -9.882 |
| | (0.020) | (0.021) | (50.680) | (53.911) |
| Observations | 1689 | 1598 | 1689 | 1598 |
| Mean | .032 | .033 | 80.403 | 83.73 |
| House FE | Yes | Yes | Yes | Yes |
| Time FE | No | Yes | No | Yes |
| Strata FE | Yes | Yes | Yes | Yes |

*Notes*: This table reports estimates from a version of Equation 1, comparing the Local to the Central arm, including interactions with indicators for flier messages printed on tax letters distributed at registration. Section A2.2 provides descriptions of the central deterrence, local deterrence, central public goods, local public goods, and trust treatment messages. The excluded flier message category is the control message "It is important to pay the property tax." The dependent variable is tax compliance in Columns 1 and 2 and tax revenue in Columns 3 and 4. All columns include house fixed effects and randomization stratum fixed effects and Columns 2 and 4 also include the time fixed effects described in Section 5. The data is restricted to the sample of properties subject to randomized messages on tax letters. This figure is discussed in Section 7.3.

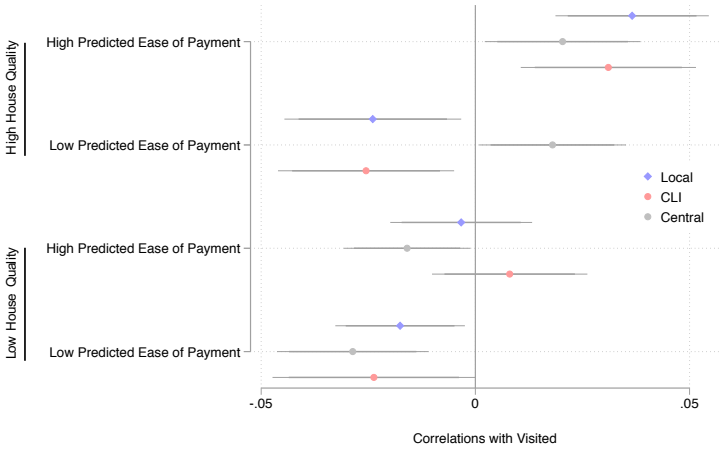
## A1.6 Additional Exhibits for Paper Section 8 — Distributional Impacts

### FIGURE A11: CHARACTERISTICS OF HOUSEHOLDS VISITED BY TAX COLLECTORS AFTER REGISTRATION WITHIN TREATMENTS

A: Observable and Unobservable Characteristics



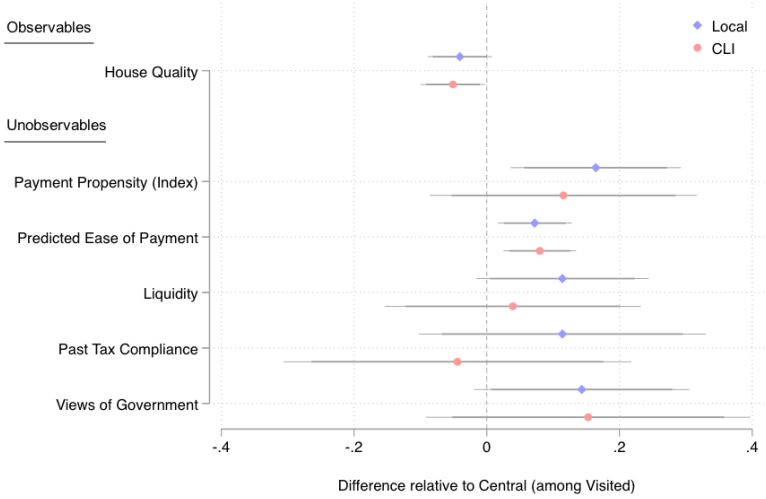

B: Predicted Ease of Payment and House Quality



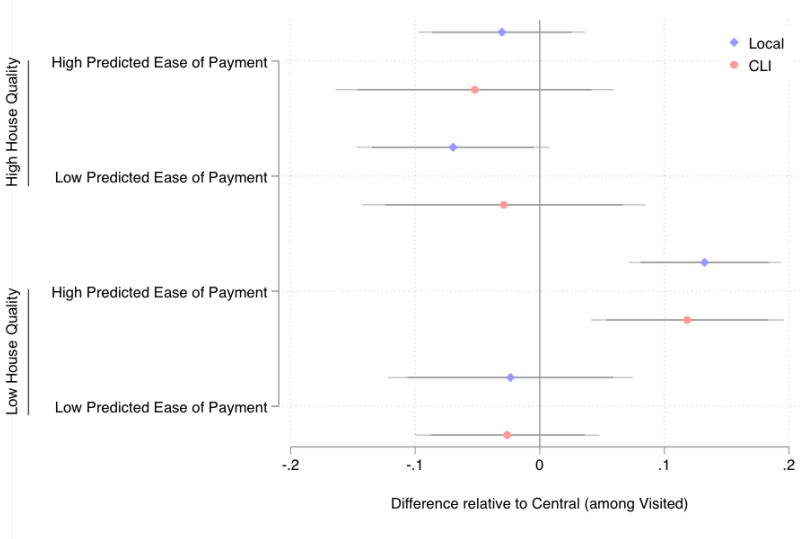

*Notes*: This figure reports correlations by treatment arm in the characteristics of properties visited by collectors after registration. It therefore supplements the analysis in Figure 1, which examines *differences by treatment* in the characteristics of households that received tax visits after registration. Panel A shows correlations with observable and unobservable characteristics for indices described in Section 8.1. Panel B shows correlations with tax visits in the four cells indicated (defined by interactions of high/low dummies for household house quality and predicted ease of payment). Correlations are estimated through separate regressions of characteristics on a treatment indicator among visited properties, controlling for the leave-one-out neighborhood mean of the outcome (Panel A) or the neighborhood mean of house quality and ease of payment (Panel B). We include time period, house type, and stratum fixed effects. We cluster standard errors at the neighborhood level. Households that paid at registration are dropped. This figure is discussed in Section 8.1.

## FIGURE A12: CHARACTERISTICS OF HOUSEHOLDS VISITED BY COLLECTORS AFTER REGISTRATION ACROSS TREATMENTS — NO HOUSE FIXED EFFECTS

A: Observable and Unobservable Characteristics



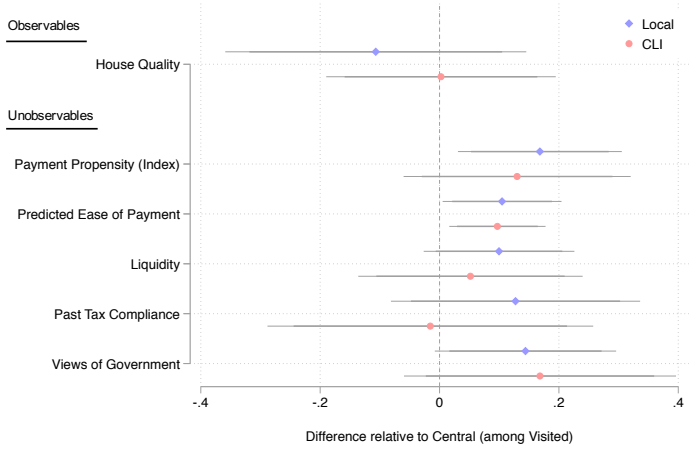

B: Willingness to Pay and House Quality



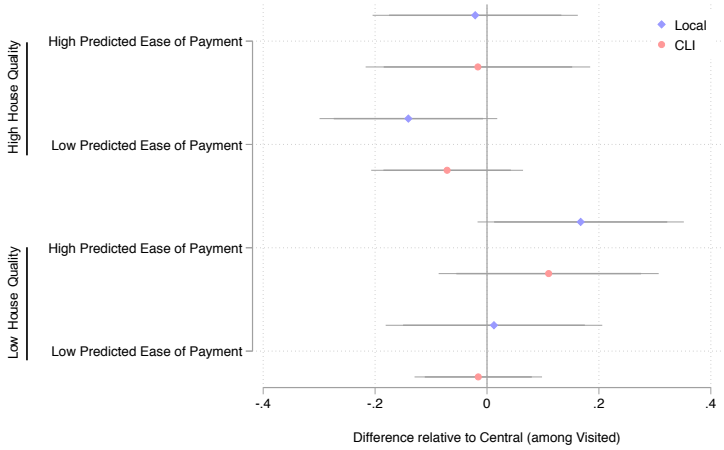

*Notes*: This figure reproduces the results from Figure 1 but excludes house fixed effects as a robustness check. Specifically, it reports differences by treatment arm in the characteristics of properties visited by collectors after registration, showing differences in characteristics of visited properties in the Local and CLI arms relative to the Central arm. Panel A shows differences in observable and unobservable characteristics for indices described in Section 8.1. Panel B shows differences in the probability of receiving a visit in the four cells indicated (defined by interactions of high/low dummies for household house quality and predicted ease of payment). Differences are estimated through separate regressions of characteristics on a treatment indicator among visited properties, controlling for the leave-one-out neighborhood mean of the outcome (Panel A) or the neighborhood mean of house quality and ease of payment (Panel B). We include time period, house type, and stratum fixed effects. We cluster standard errors at the neighborhood level. Households that paid during registration are dropped. We discuss these results in Section 8.1.

## FIGURE A13: CHARACTERISTICS OF HOUSEHOLDS VISITED BY TAX COLLECTORS AFTER REGISTRATION ACROSS TREATMENTS — OMITTING NEIGHBORHOOD MEAN CONTROLS

A: Observable and Unobservable Characteristics



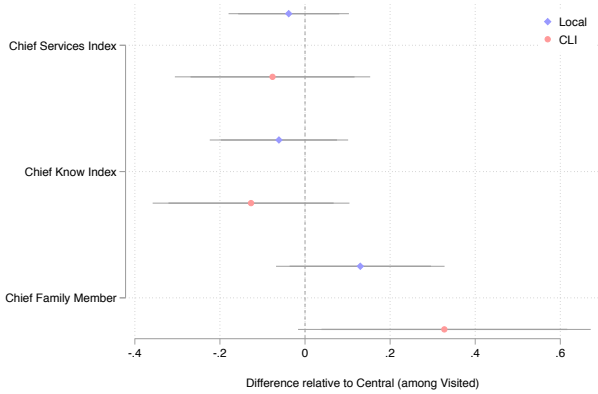

B: Willingness to Pay and House Quality



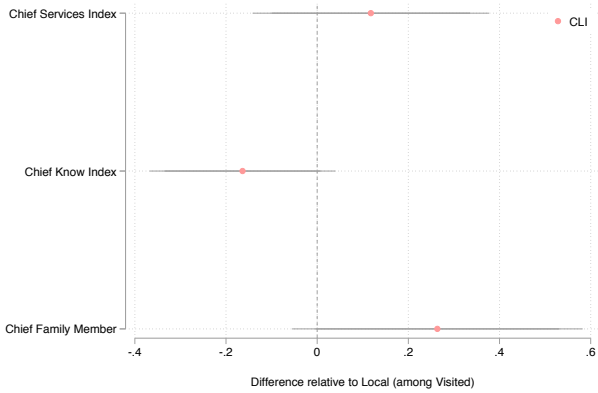

*Notes*: This figure reproduces the results from Figure 1 but omits the neighborhood mean controls as a robustness check. Specifically, it reports differences by treatment arm in the characteristics of properties visited by collectors after registration, showing differences in characteristics of visited properties in the Local and CLI arms relative to the Central arm. Panel A shows differences in observable and unobservable characteristics for indices described in Section 8.1. Panel B shows differences in the probability of receiving a visit in the four cells indicated (defined by interactions of high/low dummies for household house quality and predicted ease of payment). Differences are estimated through separate regressions of characteristics on a treatment indicator among visited properties, controlling for the leave-one-out neighborhood mean of the outcome (Panel A) or the neighborhood mean of house quality and ease of payment (Panel B). We include time period, house type, and stratum fixed effects. We cluster standard errors at the neighborhood level. Households that paid during registration are dropped. We discuss these results in Section 8.1.

## FIGURE A14: CORRELATIONS BETWEEN TAX VISITS AND CHIEF CONNECTIONS

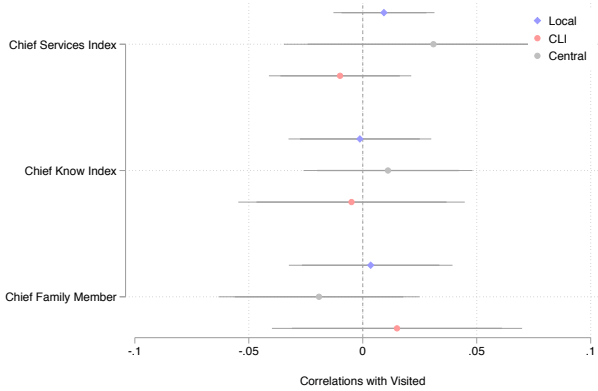
A: Local and CLI v. Central




B: CLI v. Local




C: Correlations by Treatment




*Notes*: This figure reports differences and correlations by treatment arm in the probability of receiving tax visits after registration and households' connections to the chief. Panel A shows differences in terms of the indices described in Section 8.1, comparing Local and CLI to Central. Panel B shows differences comparing CLI to Local. Panel C shows correlations with tax visits by treatment. Differences are estimated through separate regressions of the connection variable on a treatment indicator, controlling for the leave-one-out neighborhood mean. Correlations are estimated through separate regressions of an indicator for receiving a tax visit on a characteristic separately by treatment groups. All regressions control for the leave-one-out neighborhood mean of the connection variable and include time period, house type, and stratum fixed effects and clustering standard errors at the neighborhood level. Households that paid at registration are dropped. We discuss these results in Section 8.1.

## TABLE A23: LOCAL V. CENTRAL: TAX VISITS AND COMPLIANCE BY COETHNICITY

| | Visited Post-Registration | | | Compliance | | |
|---|---|---|---|---|---|---|
| Match with Collector | Tribe | Subtribe | Lang. Maj. | Tribe | Subtribe | Lang. Maj. |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Local | -0.002 | 0.063 | -0.016 | 0.050*** | 0.026 | 0.049** |
| | (0.031) | (0.044) | (0.039) | (0.011) | (0.019) | (0.017) |
| Local X Match | 0.007 | -0.117** | 0.020 | -0.015 | -0.035 | -0.003 |
| | (0.040) | (0.058) | (0.045) | (0.016) | (0.044) | (0.019) |
| Match | -0.010 | 0.143** | -0.004 | 0.011 | 0.051 | -0.009 |
| | (0.035) | (0.054) | (0.035) | (0.013) | (0.041) | (0.012) |
| Time FE | Yes | Yes | Yes | Yes | Yes | Yes |
| House FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Stratum FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 13628 | 6457 | 13628 | 13752 | 6491 | 13752 |
| Clusters | 210 | 114 | 210 | 210 | 114 | 210 |
| Central Mean (Non-Match) | .438 | .297 | .432 | .072 | .052 | .074 |

*Notes*: This table reports estimates from a version of Equation 1, comparing tax visits and compliance in Local and Central (the excluded category) by whether the collector and property owner are coethnics along a specific dimension. The outcome in Columns 1–3 is whether households reported any tax visits after registration. The outcome in Columns 4–6 is compliance according to administrative data. Match corresponds to an indicator for the chief's or at least one state collector's coethnicity characteristic matching that of the property owner for the characteristics at the top of each column. Columns 1 and 5 show estimates for including an interaction with an indicator for a collector's and property owner's tribe matching, Columns 2 and 6 for subtribe, Columns 3 and 7 for both being members of the language majority, and Columns 4 and 8 for families originating from the same territory. All regressions include fixed effects for time periods described in Section 5, house type, and randomization strata. We cluster standard errors at the neighborhood level. These results are discussed in Section 8.1.

## TABLE A24: LOCAL V. CENTRAL: THE DISTRIBUTION OF THE TAX BURDEN — NO HOUSE FIXED EFFECTS

| *Outcome:* | Compliance by Prop. Type | | Complier Characteristics | | |
|---|---|---|---|---|---|
| | Low Band Prop. | High Band Prop. | House Quality | Avg. Mon. Income | Liquidity Index |
| | (1) | (2) | (3) | (4) | (5) |
| Local | 0.037*** | 0.002 | -0.146** | -0.005 | -0.072 |
| | (0.008) | (0.013) | (0.056) | (0.041) | (0.163) |
| Time FE | Yes | Yes | Yes | Yes | Yes |
| Stratum FE | Yes | Yes | Yes | Yes | Yes |
| Observations | 24581 | 3384 | 1324 | 228 | 228 |
| Clusters | 208 | 150 | 157 | 121 | 121 |
| Central Mean | .063 | .062 | .102 | .007 | .118 |

*Notes*: This table re-estimates the results reported in Table 9 while excluding house fixed effects. Specifically, it reports estimates from a version of Equation 1, comparing property tax compliance in Local and Central (the excluded category). We include fixed effects for randomization strata and time periods, as described in Section 5, and we cluster standard errors at the neighborhood level. Columns 1 and 2 report estimates of the impact of local collection on compliance for low- and high-band households, respectively. Column 3 reports differences in an index of house quality conditional on the property paying the tax. Column 4 reports differences in monthly household income of properties, averaged across baseline and endline values, in Congolese Francs, conditional on paying the tax. Column 5 reports differences in an index of liquidity measures drawn from baseline (excepting income, which is also included, and uses information from endline) among payers. Columns 3–5 control for the leave-one-out neighborhood mean of the outcome. We discuss the interpretation of these results in Section 8.2.

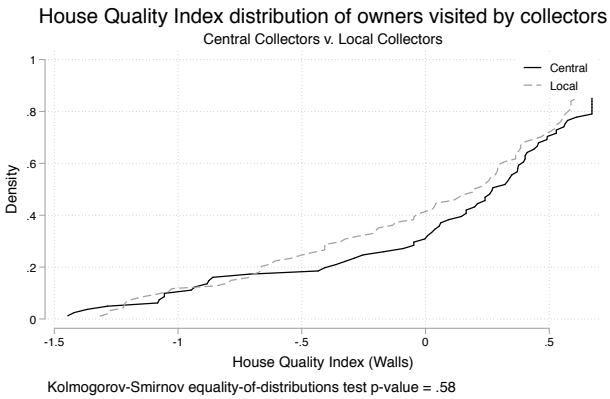
**TABLE A25: LOCAL AND CLI V. CENTRAL: INCIDENCE BY COMPLIER CHARACTERISTICS — NO NEIGHBORHOOD MEAN CONTROLS**

| *Outcome:* | Complier Characteristics | | | | | |
|---|---|---|---|---|---|---|
| | Local v. Central | | | CLI v. Central | | |
| | House Quality | Avg. Mon. Income | Liquidity Index | House Quality | Avg. Mon. Income | Liquidity Index |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Local | -0.220 | 0.002 | -0.053 | | | |
| | (0.156) | (0.041) | (0.174) | | | |
| CLI | | | | 0.134 | 0.015 | 0.183 |
| | | | | (0.126) | (0.053) | (0.211) |
| Time FE | Yes | Yes | Yes | Yes | Yes | Yes |
| House FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Stratum FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 1310 | 224 | 224 | 833 | 140 | 140 |
| Clusters | 156 | 120 | 120 | 115 | 86 | 86 |
| Mean | .102 | .007 | .118 | .099 | .017 | .201 |

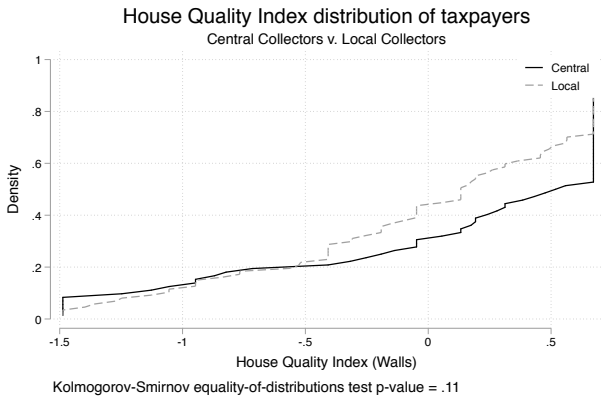
*Notes*: This table re-estimates the results reported in Columns 3–5 of Table 9 while excluding controls for the neighborhood mean. Columns 1–3 examine the distribution of the noted characteristics among taxpayers in a comparison of Local v. Central, while Columns 4–6 compare CLI v. Central. Column 1 and 4 report differences in an index of house quality conditional on the property paying the tax. Columns 2 and 5 report differences in monthly household income of properties, averaged across baseline and endline values, in Congolese Francs, conditional on paying the tax. Columns 3 and 6 report differences in an index of liquidity measures drawn from baseline (except income, which is also included, and uses information from endline) among payers. We include fixed effects for house type, randomization strata, and time periods, as described in Section 5, and we cluster standard errors at the neighborhood level. We discuss the interpretation of these results in Section 8.2.

## FIGURE A15: HOUSE QUALITY, INCOME, AND LIQUIDITY DISTRIBUTIONS AMONG VISITED AND PAYING HOUSEHOLDS BY TREATMENT

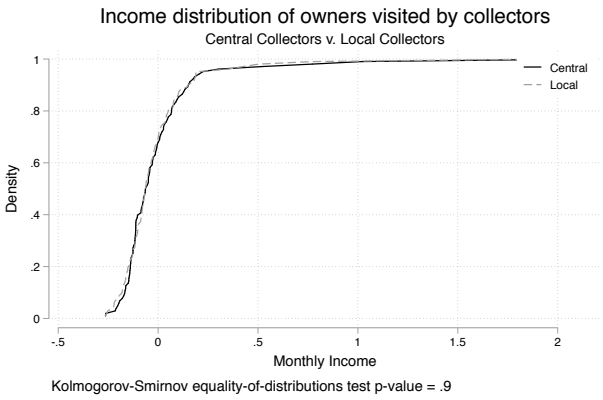
A: House quality - Visited owners



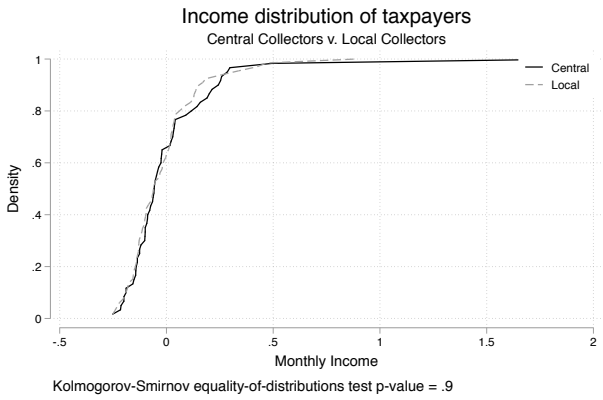

B: House quality - Taxpayers



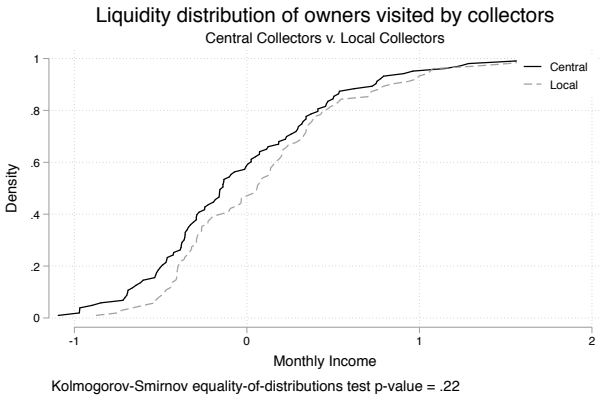

C: Income - Visited owners



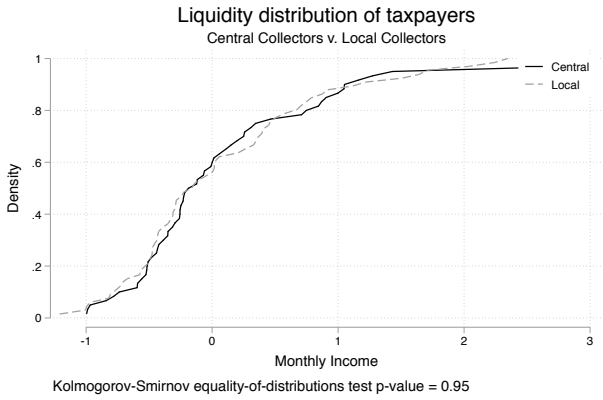

D: Income - Taxpayers




E: Liquidity - Visited owners



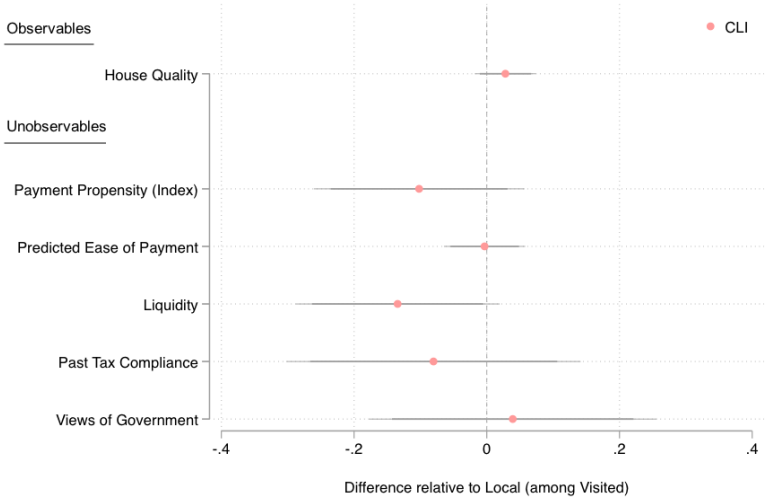

F: Liquidity - Taxpayers




*Notes*: This figure shows cumulative distribution functions of house quality and income by treatment and separately among households that received tax visits after registration (Panels A, C, and E) and that paid the tax (Panels B, D, and F). In Panel B, the taxpayer distribution has considerable mass at the maximum value of the house quality index in Central, making the CDF somewhat difficult to read. Kolmogorov-Smirnov equality of distributions test $p$-values are reported at the bottom. We discuss these results in Section 8.2.

## A1.7 Additional Exhibits for Paper Section 9 — Discussion and Policy Implications

### FIGURE A16: LOCAL V. CENTRAL + LOCAL INFO: DIFFERENCES IN TARGETING OF TAX VISITS BY HOUSEHOLD CHARACTERISTICS




*Notes*: This figure reports correlations by treatment arm in the characteristics of properties visited by collectors after registration. The figure shows differences in observable and unobservable characteristics for indices described in Section 8.1. Correlations are estimated through separate regressions of an indicator for receiving a tax visit on a characteristic separately by treatment groups, controlling for the leave-one-out neighborhood mean of the outcome, including time period, house type, and stratum fixed effects and clustering standard errors at the neighborhood level. Households that paid at registration are dropped. We discuss these results in Section 9.2.

## TABLE A26: LOCAL V. CENTRAL: IMPACTS ON HOUSEHOLD WELL-BEING

| | Monthly Income | Weekly Transport | Bed hungry last month | Bed hungry last month num. days | Lacks 3000 CF cash today | Lacks 3000 CF cash this month | Lacks 3000 CF cash this month num. days |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| *Panel A: Reduced Form* | | | | | | | |
| Local | -2300.525 | -37.852 | -0.015 | -0.017 | -0.014 | -0.003 | 0.105 |
| | (7800.918) | (438.961) | (0.023) | (0.077) | (0.023) | (0.027) | (0.176) |
| Observations | 2277 | 2329 | 2330 | 2330 | 2330 | 2330 | 2330 |
| Mean | 144789 | 4456 | .516 | .993 | .675 | .652 | 1.29 |
| *Panel B: Instrumenting for Taxes Paid* | | | | | | | |
| $\widehat{\text{Taxes Paid}}$ | -1.34e+05 | -2574.310 | -1.054 | -1.181 | -0.942 | -0.180 | 7.147 |
| | (4.86e+05) | (30047.563) | (1.954) | (5.270) | (1.802) | (1.827) | (12.946) |
| Observations | 2277 | 2329 | 2330 | 2330 | 2330 | 2330 | 2330 |
| Mean | 144789 | 4456 | .516 | .993 | .675 | .652 | 1.29 |
| *Panel C: Instrumenting for Taxes or Bribe Paid* | | | | | | | |
| $\widehat{\text{Taxes or Bribe}}$ | 33221.221 | -1.49e+04 | -0.366 | 0.770 | -0.079 | -0.115 | 3.098 |
| | (1.90e+05) | (19209.285) | (0.603) | (1.615) | (0.529) | (0.634) | (3.169) |
| Observations | 1260 | 1287 | 1287 | 1287 | 1287 | 1287 | 1287 |
| Mean | 150899 | 5174 | .482 | .863 | .67 | .63 | 1.1 |
| House FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Stratum FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes*: This table reports estimates from a version of Equation 1, endline measures of well-being in Local and Central (the excluded category). We include fixed effects for house type and randomization strata and cluster standard errors at the neighborhood level. Columns 1 and 2 report differences in monthly household income and weekly transport (a measure of spending). Columns 3 and 4 report differences in whether the household went to bed hungry at least one day in the last month and how many days, respectively. Columns 5, 6, and 7 report differences in whether the household lacked 3,000 Congolese Francs to be able to make a payment at the date of survey, sometime in the last month, and how many times in the last month, respectively. Panel A reports the reduced form results of a regression of outcomes on an indicator for the Local treatment. Panel B regresses outcomes on an indicator for tax payment instrumented by an indicator for the Local treatment. Panel C regresses outcomes on an indicator for paying a tax or bribe with an indicator for the Local treatment. We discuss these results in Section 9.

## TABLE A27: LOCAL V. CENTRAL: VIEWS OF GOVERNMENT AND CHIEFS BY TAX AND BRIBE PAYMENT

| | Provincial Government | | | | Neighborhood Chief | | | |
|---|---|---|---|---|---|---|---|---|
| | Views of govt. (index) (1) | Trust in govt (2) | Responsiveness of govt. (3) | Performance of govt. (4) | Views of chief (index) (5) | Trust in chief (6) | Responsiveness of chief. (7) | Performance of chief. (8) |
| *Panel A: Interaction with Paid Tax* | | | | | | | | |
| Local | 0.036 | 0.153** | -0.057 | -0.036 | 0.070 | 0.057 | -0.039 | 0.085 |
| | (0.052) | (0.060) | (0.046) | (0.055) | (0.053) | (0.056) | (0.059) | (0.063) |
| Local X Paid Tax | -0.090 | -0.288* | 0.148 | -0.184 | -0.155 | -0.143 | -0.326** | 0.057 |
| | (0.118) | (0.151) | (0.137) | (0.138) | (0.132) | (0.136) | (0.150) | (0.120) |
| Paid Tax | 0.082 | 0.065 | -0.101 | 0.173 | 0.116 | 0.028 | 0.261** | -0.123 |
| | (0.089) | (0.109) | (0.108) | (0.107) | (0.095) | (0.100) | (0.115) | (0.085) |
| Observations | 2329 | 2207 | 2205 | 2102 | 2303 | 2291 | 1637 | 1302 |
| Central Mean (No Pay) | -.009 | .004 | -.009 | .009 | -.01 | -.016 | .029 | -.013 |
| *Panel B: Interaction with Paid Bribe (Endline)* | | | | | | | | |
| Local | 0.082 | 0.227** | -0.010 | -0.121* | 0.113* | 0.137* | -0.067 | 0.108 |
| | (0.065) | (0.088) | (0.064) | (0.064) | (0.067) | (0.079) | (0.080) | (0.087) |
| Local X Paid Bribe | 0.321 | -0.531 | 0.842* | 0.287 | 0.154 | -0.428 | -0.246 | 0.805 |
| | (0.461) | (0.405) | (0.487) | (0.497) | (0.490) | (0.506) | (0.473) | (0.539) |
| Paid Bribe | -0.466 | 0.522* | -0.500 | -0.689* | -0.236 | 0.112 | 0.235 | -0.097 |
| | (0.391) | (0.308) | (0.375) | (0.411) | (0.390) | (0.413) | (0.282) | (0.179) |
| Observations | 1124 | 1073 | 1063 | 1021 | 1121 | 1114 | 789 | 645 |
| Central Mean (No Pay) | -.081 | -.052 | -.06 | -.047 | -.062 | -.075 | -.021 | .01 |
| Baseline Control | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| House FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Stratum FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes*: This table shows estimates from versions of Equation 1, comparing the Local arm to the Central arm (the excluded category). The outcomes are views of chiefs and government as defined in Table 5. Panel A shows estimates by interactions with and indicator for paying the tax according to the administrative data. Panel B shows estimates by interactions with an indicator for paying a bribe to the collector at endline (self-reported). All regressions include fixed effects for house type and randomization strata and cluster standard errors at the neighborhood level. We discuss these results in Section 9.

## TABLE A28: CENTRAL V. CENTRAL X LOCAL

| | Compliance | Revenues | Visited | Visits | Compliance | Compliance |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Central X Local | 0.018* | -9.439 | 0.019 | 0.065 | 0.029** | 0.013 |
| | (0.010) | (27.748) | (0.037) | (0.061) | (0.014) | (0.010) |
| Local | | | | | | 0.044*** |
| | | | | | | (0.007) |
| Visit Control | No | No | No | No | Yes | No |
| Time FE | Yes | Yes | Yes | Yes | Yes | Yes |
| House FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Stratum FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 18211 | 18211 | 12476 | 12464 | 5030 | 32496 |
| Clusters | 142 | 142 | 141 | 141 | 140 | 252 |
| Central Mean | .053 | 158.493 | .396 | .518 | .102 | .053 |
| Test CXL=Local ($p$-value) | | | | | | 0.002 |

*Notes*: This table compares the Central X Local (CXL) arm to the Central arm, which is the excluded category. Columns 1, 5, and 6 report impacts on compliance. Column 2 reports impacts on revenues. Columns 3 and 4 report differences in tax visits by collectors after registration by the extensive and intensive margins, respectively. All regressions include fixed effects for house type, randomization strata, and time periods and cluster standard errors at the neighborhood level. All specifications include time fixed effects defined to maximize overlap between the treatments under comparison, as discussed in Section 5. Column 5 restricts to the subsample of properties that received any tax visits after registration. Column 6 includes a dummy for the Local treatment in the regression. The bottom row reports the $p$-value from a test for equality between the CXL and Local. We discuss these results in Section 9.1.

# A2 Additional Details on the Tax Campaign and its Evaluation

## A2.1 Block-Randomized Design

In the randomization of the main tax collector treatments, we used a block-randomized design, stratifying on three variables.

> **1. Geographic strata.** We use 12 geographic strata corresponding to different city regions (Figure A3). Two encompass the city center; the rest correspond to what the tax ministry calls "the periphery." Blocking on these strata ensures balance on a number of geographic characteristics, including (i) the local legitimacy of the chief (higher in the periphery), and (ii) the intensity of prior tax enforcement (lower in the periphery).
>
> **2. Treatment status in the 2016 tax campaign.** We also block on treatment status in the 2016 property tax campaign, randomly assigned on the neighborhood level (Weigel, 2020). Treated neighborhoods received visits from tax ministry agents (similar to the Central arm), while control neighborhoods did not (similar to the pure control arm). Stratifying on this variable ensures balance on past door-to-door property tax collection.
>
> **3. Past experience of chiefs in tax collection.** Finally, we block on a measure of whether chiefs reported ever having been involved in tax collection in the past, which was the case for 22% of chiefs.[125] Incorporating this variable into our strata assures balance on this important chief-level characteristic.

We first created strata using the first two variables. Then, for each, we created two substrata based on the third variable.[126]

## A2.2 Tax Letter Message Treatments

As shown in Figure A1, the tax letters distributed by collectors during registration in all treatment arms contained cross-randomized messages, as in Blumenthal et al. (2001), Pomeranz (2015), and Scartascini and Castro (2007). Collectors were supposed to read the entire flier out loud to property owners during registration. The tax letters provided basic information about the tax campaign, including the compound number, the compound-specific tax rate for the year, to whom the tax should be paid (state or chief tax collector, or either). In addition, the tax letters contained one of the following messages, randomized on the household level:

> **I1. Central deterrence.** This message says that refusal to pay the property tax entails the possibility of audit and investigation by the provincial tax ministry.

[125] Of those who responded affirmatively, 79% reported collecting the property tax, 10% the rental tax (a property tax levied on renters), and the remainder reported having collected other taxes.

[126] We split at the median level of experience with tax collection if a neighborhood had more than one chief, possible in larger neighborhoods with multiple main avenues.

**I2. Local deterrence.** The Local version of the deterrence message says that refusal to pay the property tax entails the possibility of audit and investigation by the neighborhood chief (*chef de quartier*). Note that this is the highest-ranking city chief, to whom other city chiefs seek counsel when needing to resolve problems.

**I3. Central public goods.** This message says that the provincial government will be able to improve infrastructure in the city of Kananga only if citizens pay the property tax.

**I4. Local public goods.** The Local version of this message is exactly the same, except that it mentions each citizens' locality instead of Kananga.[127]

**I5. Trust.** The trust message reminds citizens that paying the property tax is a way of showing that they trust the state and its agents.

**I6. Control.** Control letters say "It is important to pay the property tax."

Finally, there is one last cross-randomized element of the tax letters: some contain an image of a legal receipt along with a phrase noting that the payer should receive a printed receipt. On other letters, there is no copy of the receipt, nor mention of the printed receipts. This treatment, intended for a separate paper on bribe payment, aims to enable citizens to hold tax collectors accountable to following the protocol of the campaign.

## A2.3 Chief Jurisdiction Mapping

The provincial government did not have a precise map of chiefs' jurisdictions. Thus, before the tax campaign, we conducted a mapping survey to identify all chiefs in Kananga and their jurisdictions. In cases where there were multiple chiefs within the same neighborhood, i.e. in charge of two different avenues, we ranked chiefs whom the government could choose for the tax campaign as follows. First, we estimated the spatial extent of each chief's domain by calculating a 20-meter buffer around each avenue they were in charge of. Second, we overlaid these domains with population data from Weigel (2020). We then ranked each chief within a neighborhood according to the share of total population in their domain. The resulting ranking of chiefs therefore estimates the chiefs who have the largest population-weighted jurisdiction in the neighborhood. In certain cases, top-ranked chiefs were unable to collect due to disability, travel during the campaign, or other reasons, and in these cases, we recommended the second highest rank chief, etc.

## A2.4 Logistics Pilot

A logistics pilot, conducted in March-April 2018, had two main goals. First, it tested a new handheld receipt printer and validated that neighborhood chiefs — who are often older and less skilled with technology — would be able to work with the receipt printers. Second, it tested the tax letters and other procedures of the campaign to be sure they could be easily understood by citizens. The pilot was conducted in eight neighborhoods of Kamilabi, a quartier in northwest Kananga that is isolated from the rest of Kananga by a series of steep ravines.

[127] Localities are the smallest administrative unit in Kananga.

This area was selected strategically due to its remote location to minimize potential informational spillovers. We exclude the pilot neighborhoods from our main estimations. But in Table A6, we show that the main results are robust to including these pilot neighborhoods.

## A2.5 Time Imbalance

This section discusses in detail the time imbalance arising from the fact that not all treatments occurred simultaneously (cf. Section 5). As noted, there was a secular decline in compliance over the course of the study (Figure A5). This decline presents a problem for our analysis because the treatments were rolled out in a staggered fashion over time due to logistical constraints at the tax ministry. Although the staggered implementation greatly helps reduce the degree of imbalance by time, there remains imperfect time overlap of treatments. In short, some treatments were implemented in periods with higher compliance, which introduces artificial differences in tax outcomes when comparing treatments. For example, the Central treatment started first and therefore is the only treatment to include observations at a point in time when compliance was highest. Had the Local treatment started at the same time it would have likely registered even higher levels of compliance, according to the trends extrapolated from data collected during the rest of the campaign. Therefore, pooling all data across time would artificially inflate estimates of compliance in treatment arms with (randomly) more observations earlier in time relative to treatment arms with more observations later in time.

Importantly, the decline in compliance over time does not reflect collectors choosing to work in "easy" neighborhoods first because the timing in which they received collectors was random. That is, within the tax ministry's overall schedule alternating between collection treatments, which neighborhoods appeared in different monthlong waves of the campaign was randomly assigned. This decline is also likely unrelated to collector characteristics, as evidenced by the fact that it impacts all treatment arms in a similar fashion. Instead, we suspect the downward trend in compliance reflects growing dissatisfaction with the government as the December 2018 election approached. The unpopular President Joseph Kabila had managed to avoid facing election two years running, and in 2018 protests were erupting across the country, to which the government responded with repression and violence. In survey outcomes we collected, there is a similar decline in attitudes toward the government and tax morale during this time period.

### A2.5.1 Preferred Specification and Robustness Tests

In our preferred specification we include fixed effects for two month periods of the tax campaign. This ensures that we are comparing treatments within similar time periods with sufficient overlap in treatment observations to permit valid comparisons within time periods. Because it maximizes time balance on both ends, our preferred fixed effects begin on the midpoint between the first days of the two treatments being compared, and end on midpoint between their last days. However, strictly speaking when a two-month period starts (and ends) is arbitrary for the purposes of including time fixed effects, so as a first robustness check, we also run and report our main estimations using fixed effects defined at every possible start date (Figure A7).

We then implement robustness tests using five other approaches: (1) including two month fixed effects defined by shifting the start date of the two month fixed effect period definition in our preferred specification backwards and forwards 15 days and selecting the shifted ver-

sion that yields the median estimate among all shifts, (2) using the interaction weighted estimator from Gibbons et al. (2018),[128] (3) including one-month fixed effects, (4) trimming observations on either end of the campaign if comparison treatments were not also active, (5) estimating effects with coarsened exact matching on time to identify clusters of comparable observations within relevant treatment arms (Iacus et al., 2012). Below we describe these alternative tests:

**1. Median of Shifts in Two-Month Fixed Effect.** Because the start and end points of the two-month fixed effects are arbitrary, we shift these cutoffs 30 times — 15 days backwards and forwards in time— then redefine two-month intervals and re-estimate Equation 1 for each shifted fixed effect version. Figure A7 shows the results of this exercise and displays the median estimate, which we report as a robustness test for our preferred fixed effect definition. This approach (and the preferred approach) addresses trends over the campaign but not trends within two-month periods.

**2. Interaction Weighted Estimator.** This estimator takes the weighted average of estimates from interaction terms of treatment with two-month dummies (defined by the preferred version of two-month fixed effects), weighting by the number of observations in each group. This approach addresses inconsistency in the presence of heterogeneous treatment effects across groups (Gibbons et al., 2018).

**3. One-Month Fixed Effects.** One-month rather than two month fixed effects to allow for finer comparisons across time. This approach may better address trends over the campaign though not trends within one-month periods. However, due to staggering, it will also mean many observations do not contribute towards the estimated effect at all because, for a given treatment comparison, there is no overlap with other treatments in time (Figure A5).

**4.Trimming Observations.** Dropping observations collected before the start (after the end) of other treatment arms, so that only observations collected between the same start and end dates are considered. This addresses problems of overlap at the start and end of the campaign but does not address those in between campaign stages.

**5. Coarsened Exact Matching.** This approach from Iacus et al. (2012) allows for matching on a continuous variable with imperfect overlap across treatments — this matching variable is "coarsened" and then used to match observations across treatments. Such matching offers potentially the best method for dealing with the time confound, as it keeps only the observations closest in time in the treatment groups being compared; however, due to the nature of staggering in the campaign it results in much smaller estimation samples given near matches cannot be found for all observations. This is especially true when comparing the Central and Central + Local Information treatment arms.

Ultimately, we prefer the approach using two-month fixed effects as it addresses the key time imbalance by comparing observations collected relatively close in time, while allowing

[128]This approach estimates treatment effects interacted with time period dummies and then averages those estimates weighted by the observations in each time period.

us to retain the majority of the sample. We use the version defined using the midpoints between the start and end dates of the treatments being compared as it maximizes time balance on both ends and our tests confirm that this choice is robust to shifting the start and end dates. Tables A4 and A12 display the results of these robustness tests for the main estimations. The estimates are remarkably similar across specifications, which we interpret as validation for our preferred approach.

## A2.6 Detailed Survey-based Variable Descriptions

This section provides the exact text of the questions used to construct the survey-based variables considered in the paper.[129]

1. *House Quality.* This standardized variable is increasing in the quality of the house of the respondent, as indicated by the quality of its walls. The exact survey prompt to enumerators is as follows:

    - 'Observe the principal material of the walls of the main house.' [Sticks/ Palms, Mud brick - bad condition, Mud brick - good condition, Bricks, Cement]

2. *Average Monthly Income.* This variable is the self-reported (logarithm of) income of the respondent averaged over the baseline and endline surveys. It was recorded in both the baseline and the endline surveys in response to the question: 'What was the household's total earnings this past month?'

3. *Education.* This variable measures the years of education of the respondent, standardized to facilitate interpretation of magnitudes. The exact survey questions are as follows:

    - 'What is the highest level of school you have reached? [Never been to school, Kindergarten, Primary, Secondary, University]
    - 'What is the last class reached in that level?' [1, 2, 3, 4, 5, 6, >6]

4. *Erosion Threat.* This standardized variable is increasing in the enumerator's perception that the respondent's property is threatened by a ravine, which are caused by erosion and are widespread in Kananga. Properties that lie close to ravines are considerably less valuable. This variable was recorded in the baseline survey in response to the enumerator prompt: 'Is this compound threatened by a ravine?' [Yes - gravely threatened, Yes - somewhat threatened, No]

5. *Has Electricity.* This variable equals 1 if the household reports in the baseline survey that they have access to electricity. The exact question text is: 'Do you have any source of electricity at your home?'

[129] The main variables, such as payments and views, chief characteristics, and household characteristics, are discussed in the paper in Sections 6.2, 7.3, and 8, respectively.

6. *Chief Family Member.* This variable equals 1 if the local chief is a member of the family of the respondent. The exact survey question from baseline is: 'Is the avenue chief a member of your family?'

7. *Chief Know Index.* This is a standardized index increasing in respondents' knowledge of and ties with the local chief. It is composted of the following baseline survey questions:

   - 'Do you know the name of your avenue chief? If yes, what is it?'
   - 'Do you have the phone number of your avenue chief?'
   - 'Do you attend the same church as your avenue chief?'

8. *Chief Services Index.* This is a standardized index increasing in the services and help the respondent has received from the local chief in the past. The exact baseline survey question is as follows: 'In the past six months how many times did your avenue chief helped you deal with any of the following issues?'

   (a) 'Help finding a solution to a problem'
   (b) 'Helped a member of your family get a job'.

9. *Connected to Chief.* This is a standardized index increasing in how close the respondent reports being to the local chief. It is a combination of the Chief Family Member variable and the Chief Know Index.

10. *Trust in Organizations.* This standardized index is increasing in the level of trust the respondent reports having in various organizations. The exact survey question is as follows:

    - Prompt: 'I am going to name a number of organizations. For each one, could you tell me how much confidence you have in them: is it a great deal of confidence, quite a lot of confidence, not very much confidence, or none at all?'
    - Organizations:
      (a) 'NGOs'
      (b) 'Local leaders'
      (c) 'The national government (in Kinshasa)'
      (d) 'The provincial government'
      (e) 'The tax ministry'
      (f) 'Foreign research organizations'.

    The values were reversed to code this variable.

11. *Liquidity Index.* This is a standardized index increasing in the estimate liquidity of the household. It includes multiple questions about the income, employment, consumption, and possessions of the respondent reported in the baseline survey as well as cash-on-hand reported in the endline survey. As above, the measure of income used is the average of baseline and endline values. The exact survey questions about employment and earnings are as follows:

- 'Are you the owner of this compound, or do you rent?'
- 'What type of work do you do now?' [Unemployed-no work, Medical assistant, Lawyer, Cart pusher, Handyman, Driver (car and taxi moto), Tailor, Diamond digger, Farmer, Teacher, Gardner, Mason, Mechanic, Carpenter, Muyanda, Military officer/soldier or police officer, Fisherman, Government personnel, Pastor, Porter, Professor, Guard, Work for NGO, Seller (in market), Seller (in a store), Seller (at home), Student, SNCC]
- 'What was the household's total earnings this past month?'

The exact survey questions about the household's consumption are as follows:

- 'How much money have you spent on transport in the past seven days'
- 'Do you have any source of electricity at your home?'

The exact survey question about the household's possessions is as follows:

- 'In your household, which (if any) of the following do you own: motorbike, car or truck, radio, television, electric generator, sewing machine, none.'

The exact survey questions about cash-on-hand from the endline survey are as follows:

- 'In the past 30 days, has your household had to go to bed hungry because you haven't had enough money on hand?'
- 'On what dates did you find yourself short of cash for these expenditures?' [1-30, All parts of month were the same]
- 'Imagine that today you learn that you need to pay an additional 3000 FC for a school fee in order for your child to continue in school. Could you find this money in the next 4 days? '
- 'In the past 30 days, were there days in which you could not have paid this fee? Which days could you NOT have paid this fee?' [1-30, I could never pay this fee any day]

12. *Government Morale Index.* This is a standardized index increasing in the respondent's evaluation of and trust in the government. The underlying survey questions are as follows:

    - 'I am going to name a number of organizations. For each one, could you tell me how much confidence you have in them: is it a great deal of confidence, quite a lot of confidence, not very much confidence or none at all?'
    - Organizations (values reversed to code these variables):
        (a) 'The national government (in Kinshasa)'
        (b) 'The provincial government'
        (c) 'The tax ministry'

- 'How would you rate the performance of the provincial government in Kananga?' [Excellent, Very good, Good, Fair, Poor, Very poor, Terrible] The values were reversed to code this variable.
- 'Now I would like to ask you what you think the provincial government will do with the money it receives from this 2016 property tax campaign. Imagine that the provincial government of Kasai Central receives $1000 thanks to this campaign. How much of this money will be put to good use, for example providing public goods?'

This is also referred to as the index of household Views of the Government.

13. *Past Tax Compliance.* This variable equals 1 if the household reports in the baseline survey that they have paid property tax in the past. The exact question text was: 'Have you ever paid the property tax?'

14. *Payment Propensity Index.* This index is a combination of the Liquidity Index, the Government Morale Index, and Past Tax Compliance.

15. *Ease of Payment.* This variable is derived from chief consultations in the CLI arm and equals 1 if the chief believes that the household can very easily afford the payment of the property tax. The exact survey question is as follows: 'Does the household head have the financial means to pay the tax?' [Hardly, Easily, Very easily]

16. *Predicted Ease of Payment.* This is a predicted value of the household's ease of payment using household characteristics, as described in Section 8.1. It comprises data collected in the midline survey about the age of the respondent, his sex, his tribe and his employment status from the baseline survey. The exact midline survey questions are as follows:

    - 'Is the owner a man or a woman?'
    - 'How old is the owner?'
    - 'What is his tribe?'

    The exact baseline employment survey questions for employed and, separately, salaried are described in the Liquidity Index entry above. This predicted variable also takes into account whether or not the respondent works for the government. The exact question is as follows: 'Do you work for the government in any capacity? If yes, please describe the job.' [Teacher, Military/ Police, Construction/ Maintenance of infrastructure, Lawyer, Doctor/ Nurse/ Lab Tech, Secretary, Driver, Functionary, Local chief (avenue, quartier), SNCC, Political appointee]

17. *Salongo Contributions.* This is a variable reporting the household's contributions to the *salongo*. The exact midline and endline survey questions are as follows:

    - 'Did someone from your household participate in *salongo* in the past 30 days?' (Extensive margin)

- 'For how many hours in total did they participate in *salongo*? Please add together the time contributed by each member of your household in the past 30 days.' (Intensive margin)

18. *Trust in Government.* This is a variable increasing in the respondent's level of trust in both the provincial and national government. This variable is coded as an average of the answers to the question from the standardized index 'Trust in Organizations' about the national and provincial government.

19. *Responsiveness of Government.* This is a variable reporting the respondent's perception of how responsive the provincial government is. The exact survey question was asked in both the baseline and the endline survey as follows: 'To what degree does the provincial government respond to the needs of your avenue's inhabitants?' [Very hard working, Hard working, Somewhat hard working, Not hard working] Values reversed to code this variable.

20. *Performance of Government.* This is a variable reporting the respondent's perception of the overall performance of the provincial government. The exact survey question was asked in both the baseline and the endline survey as follows: 'How would you rate the performance of the provincial government in Kananga?' [Excellent, Very good, Good, Fair, Poor, Very poor, Terrible] Values reversed to code this variable.

21. *Integrity of Government.* This is a variable reporting the respondent's perception of the integrity of the government, i.e. the opposite of corruption. The exact endline survey question is as follows: 'Now I would like to ask you what you think the provincial government will do with the money it receives from this 2018 property tax campaign. Imagine that the provincial government of Kasai Central receives $1000 thanks to this campaign. How much of that money do you think was misappropriated/wasted?' The integer provided by the respondent was subtracted from 1000 to code the variable.

22. *View of Government (index).* This index is a combination of the following variables: Trust in Government, Responsiveness of Government, Performance of Government, and Integrity of Government.

23. *Trust in Chief.* This is a variable increasing in the respondent's level of trust in the chief. The variable uses the answer to the question from the standardized index 'Trust in Organizations' about the chief.

24. *Responsiveness of Chief.* This is a variable reporting the respondent's perception of how responsive the chief is to the needs of people in the neighborhood. The exact survey question, asked in both the baseline and the endline survey, is as follows: 'To what degree does the chef respond to the needs of your avenue /locality's inhabitants?' [Very hard working, Hard working, Somewhat hard working, Not hard working] Values reversed to code this variable.

25. *Performance of Chief.* This is a variable reporting the respondent's perception of the overall performance of the chief. The exact survey question was asked in both the

baseline and the endline survey as follows: 'Overall, how would you rate the performance of the chef?' [Excellent, Very good, Good, Fair, Poor, Very poor, Terrible] Values reversed to code this variable.

26. *Integrity of Chief.* This is a variable reporting the respondent's perception of the integrity of the chief. The exact endline survey question is as follows: 'Imagine that the chief is in charge of doing a public project on your avenue. He receives $1000. How much of this money will they put in their pockets?' The integer provided by the respondent was subtracted from 1000 to code the variable.

27. *View of Chief (index).* This index is a combination of variables: Trust in Chief, Responsiveness of Chief, Performance of Chief, and Integrity of Chief.

28. *Perceived Tax Compliance on Avenue.* This is a variable reporting the respondent's perception of what share of their neighbors have paid their property tax in 2018. The exact survey question was asked in the endline survey as follows: 'In your opinion, out of 10 compounds on your avenue, how many actually paid the property tax in 2018?'

29. *Trust in Tax Ministry.* This is a variable increasing in the respondent's level of trust in the tax ministry. The variable uses the answer to the question from the standardized index 'Trust in Organizations' about the tax ministry.

30. *Property Tax Morale.* This is a variable reporting the respondent's perception of how acceptable it is not to pay one's property tax. The exact survey question asked in both baseline and endline surveys is as follows: 'Now, imagine that next week a tax collector from the government comes and visits one of your neighbors. Imagine he absolutely refuses to pay the property tax. In your opinion, how acceptable is this?' [It's acceptable, It could be acceptable under some circumstances, It is not acceptable]

31. *Fairness of Property Taxation.* This is an index increasing in the respondent's evaluation of how fair property taxation is. The underlying endline survey questions are as follows:

    - 'In your opinion, how fair is it that households in your neighborhood must pay the property tax?'
    - 'In your opinion, how fair was the amount demanded for the property tax in 2018?'
    - 'In your opinion, how fair were the collectors who worked on the property tax campaign of 2018?' [Very fair, Fair, Unfair, Very unfair] Values reversed to code this variable.

32. *Perception of Enforcement.* This is a variable reporting the respondent's perception of how likely it is that one gets sanctioned for not paying property tax. The underlying baseline and endline survey question is as follows: 'Imagine that next week a tax collector comes and visits one of your neighbors. Imagine he absolutely refuses to pay the property tax. In this case, what is the probability that the government will pursue and enforce sanctions? Choose one of the following options: He will definitely be

pursued and punished; He is very likely to be pursued and punished; He is unlikely to be pursued and punished; He is very unlikely to be pursued and punished.' The values were reversed to code this variable.

33. *Paid Bribe.* This is a variable providing the respondent's self-reported bribe payments. The underlying exact midline and endline survey questions are as follows:

    - 'Did you (or a family member) pay the transport of the collector?'
    - 'Apart from the amount that you paid, did the collector ask you for another small sum on the side (for example, for his transport)?'

34. *Other Payments.* This is a variable providing the respondent's self-reported informal payments to officials. The underlying exact midline and endline survey question is as follows: 'Now, I'd like to talk about small payments made to officials such as small amounts paid for transport, water, tea, etc. In the past 6 months, did you make any such payment?'

35. *Vehicle Tax.* This variable equals 1 if the household reports that they have paid a vehicle tax in 2018. The exact question text was: 'Let's discuss the vehicle tax. Did you pay this tax in 2018?'

36. *Obsolete Tax.* This variable equals 1 if the household reports that they have paid the obsolete poll tax in 2018. The exact question text was: 'Let's discuss the poll tax. Did you pay this tax in 2018?'

37. *Market Vendor Fee.* This variable equals 1 if the household reports that they have paid the market vendor fee in 2018. The exact question text was: 'Let's discuss the market vendor fee. Did you pay this tax in 2018?'

38. *Business Tax.* This variable equals 1 if the household reports that they have paid a business tax in 2018. The exact question text was: 'Let's discuss the companies' register. Did you pay this tax in 2018?'

39. *Income Tax.* This variable equals 1 if the household reports that they have paid an income tax in 2018. The exact question text was: 'Let's discuss the income tax. Did you pay this tax in 2018?'

# A3 Further Analysis

## A3.1 Conceptual Framework

This simple conceptual framework describes a government's decision between collector types in administering a tax collection campaign in a low-compliance setting. We discuss the inputs to the government's choice and the assumptions we make for each. We then discuss how this framework maps to our context and discuss contextual differences and government interventions that could alter the choice between collector types.

### A3.1.1 Setup

**Property owners:** Property owners have intrinsic willingness to comply $\lambda$ with property tax $T$ (normalized to 1) and encounter costs to non-compliance $\theta$ with probability $a$. $\lambda$ is a random variable; cost $\theta$ may represent the fine (plus tax amount) or punishments such as shaming and $a$ the likelihood of incurring such costs.[130] Taxpayer $i$ complies with the tax if $\lambda_i + a\theta \geq T$.

The probability that a taxpayer pays the tax is $\Pr(\lambda_i \geq 1 - a\theta) = 1 - F(1 - a\theta)$. We assume $a$ and $\theta$ are fixed and constant across individuals but can differ by collector type $k$, and define $\rho_k = 1 - a_k\theta_k$. $\Pr(\lambda_i \geq \rho_k) = 1 - F(\rho_k) = 1 - \int_0^{\rho_k} f(\rho_k)d\rho_k$. The low enforcement nature of this setting derives from an assumption that $\rho_k$ is small enough that, for a large share of taxpayers, $\lambda_i + \rho_k < 1$: the sum of intrinsic willingness to pay and cost of non-compliance is less than the amount of the tax.

To visualize how $\lambda_k$ and $\rho_k$ affect the potential number of taxpayers, Figure A17 illustrates distributions of $\lambda$ by collector type, $f(\lambda_L)$ for Local ($L$) and $f(\lambda_C)$ for Central ($C$), for the same population of property owners, and shows values of $\rho$ by collector type. This figure displays a case where $f(\lambda_L)$ is shifted to the right of $f(\lambda_C)$: the intrinsic willingness to pay the tax to type $L$ is higher for most property owners than the intrinsic willingness to pay to type $C$. However, $\rho_C$ is lower than $\rho_L$, reflecting a higher cost to punishment for non-compliance under type $C$ than type $L$. Because in this instance the willingness to pay type $L$ more outweighs type $C$'s enforcement advantage, the proportion of property owners that will pay a collector of type $L$ is greater than the share that will pay a collector of type $C$, as represented by the shaded portions underneath each curve.

The proportion of property owners who will pay the tax (conditional on being visited by a tax collector) thus depends on the intrinsic motivation $\lambda_k$ and the cost of punishment for non-compliance $\rho_k$. This portion will be higher for type $L$ (and vice versa for type $C$) if: (1) the cost of punishment for non-compliance is the same, $\rho_L = \rho_C$, but the intrinsic willingness to pay the tax $\lambda_k$ is higher for type $L$ v. type $C$ (which could be consistent with greater tax morale, trust, reciprocity); (2) the intrinsic willingness to pay is the same across collector types ($\lambda_{Li} = \lambda_{Ci} \forall i$), but the cost of punishment for non-compliance for type $L$ ($a_L\theta_L$) is higher, or $\rho_L < \rho_C$ ($\uparrow a\theta \rightarrow \downarrow \rho$) (which could be consistent with greater unofficial costs to non-compliance such as shaming, withholding of services, or informal tax imposition); or (3) in cases such as those illustrated in Figure A17: higher intrinsic willingness to pay is larger than differences in cost of punishment for non-compliance.

**Tax collectors:** Tax collectors are of type Local ($L$) or Central ($C$). A collector makes visits to property owners and solicits tax payment or bribes.[131] Before outlining the collector's objective function, we first define the inputs to the collector's decision below.

*Average probability of payment among visited*: First, it is necessary to define the average probability of payment, which is generated by visits to property owners. A property owner

[130] In this simple setup, we do not consider other factors that may be relevant to compliance, such as liquidity constraints, but the interpretation of $\lambda_i$ could be expanded to include such factors as long as they would raise willingness to comply independent of the other inputs we specify as informing the taxpayer's compliance decision.

[131] In this section, for simplicity we refer to collectors' "tax visits" simply as "visits." In mapping this framework to the setting in Kananga, these would be visits after property registration in which the collector solicits payment of the property tax.

$i$ only pays — with probability $\Pr(\lambda_{ki} \geq \rho_k)$ — if visited by a tax collector of type $k$. Therefore, the probability of payment for a household $i$ can be expressed as $v_i \cdot \Pr(\lambda_i \geq \rho_k)$ with $v_i \in \{0, 1\}$ being an indicator for a household receiving a visit.[132]

The average probability of payment among property owners visited by collector type $k$, which is a function of the total number of property owners a collector decides to visit $v$, is then:

$$\bar{p}_k(v) = \frac{1}{V} \sum_i v_i \cdot \Pr(\lambda_{ki} \geq \rho_k) \tag{2}$$

where $V = \sum_i v_i$, the number of households visited. $\bar{p}_k(v)$ can be decreasing, flat, increasing, or non-monotonic in $v$ depending on the order — in terms of $\lambda$ — in which collectors visit property owners.[133]

*Collector targeting*: Collector types employ different targeting strategies that are a function of their information about property owners' intrinsic willingness to pay and the cost of punishment for non-compliance specific to their type.

For illustration purposes, we highlight an extreme case: type $L$ collector knows the intrinsic motivation $\lambda_i \forall i$ and $\rho_L$, the punishment probability and cost for collector type $L$, so rank-orders households by $\lambda_i$ as the schedule for making tax collection visits (from highest $\lambda_i$ to lowest). Type $C$ collector knows $\rho_C$ but does not know $\lambda_i$ for any households, so targets visits randomly. In this extreme case, we assume that $E[\lambda_L] = E[\lambda_C]$ and $\rho_L = \rho_C$: the willingness to pay the tax and the cost of non-compliance are the same across collector types, but collector types differ in their knowledge of property owners' $\lambda$'s. However, if the number of visits $v$ a collector makes is less than the total number of households $N$, then $\bar{p}_L(v) > \bar{p}_C(v)$: given type $L$'s ability to rank-order households by willingness to pay, among the households visited by the type $L$ collector the average probability of payment is higher than the average probability among the households visited by the type $C$ collector.[134] This case is illustrated in Figure A18: even for different levels of visits, so long as not all households are visited, $\bar{p}_L(v) > \bar{p}_C(v)$ will hold. Even in a non-extreme case, when type $C$ collectors possess some information about willingness to comply with the tax (curve $\bar{p}'_C(v)$ in Figure A18) — but type $L$ are better informed — the same relationship holds. An alternative way to interpret this difference in strategies is that collectors observe signals about $\lambda_i$, and type $L$ possess a stronger signal than type $C$ that allows for more effective targeting of visits.

This inequality will also hold in the following cases where $E[\lambda_L] \neq E[\lambda_C]$ and/or $\rho_L \neq \rho_C$ (i.e., when collector types differ in information and property owners differ in intrinsic willingness to pay across collector types and/or the punishment cost to non-compliance across collector types): (1) $E[\lambda_L] = E[\lambda_C]$ but $\rho_L < \rho_C$, then $\bar{p}_L(v) > \bar{p}_C(v)$ even if all households are visited: this reflects a case in which the punishment power of type $L$ is higher — the same could hold in reverse; (2) $\rho_L = \rho_C$ but $E[\lambda_L] > E[\lambda_C]$, then $\bar{p}_L(v) > \bar{p}_C(v)$ even if all households are visited: this case could reflect differences in intrinsic motivation to pay that vary by collector type — e.g., type $L$ engenders higher tax morale — the same

[132] In this simple setting we do not consider multiple (re)visits to households, but $v_i$ could also be thought of as number of visits made to a property owner and the same relationships we identify below will hold.

[133] The average probability in Equation 2 can also be expressed using integrals as $\frac{1}{V} \sum_i v_i \cdot \left[1 - \int_0^{\rho_k} f(\rho_k) d\rho_k\right]$.

[134] If all households are visited by each collector type, then $\bar{p}_L(v) = \bar{p}_C(v)$.

could hold in reverse;[135] or (3) in cases that depend on relative differences in $E[\lambda_k]$ and $\rho_k$ and collection strategies when all households are not visited: one can imagine cases where, for $E[\lambda_L] = E[\lambda_C]$, $\rho_C < \rho_L$ (cost of non-compliance with type $C$ higher), but type $L$ can target higher-$\lambda$ property owners in completed visits, while type $C$ targets randomly — in such a case the information advantage of type $L$ overcomes the enforcement advantage of type $C$.[136]

*Corruption*: Collectors also have the ability to solicit bribes in lieu of tax payment. This form of corruption captures the cost from the government's perspective of enlisting collectors who may have different incentives to collect bribes instead of taxes across types.[137]

### A3.1.2 Collector's objective function

Collectors earn a piece-rate wage that is a portion $\delta < 1$ of the tax $T = 1$ they collect. A collector of type $k$ chooses $v$ and $\alpha$ to maximize expected utility:

$$EU = v\bar{p}_k(v)[\delta(1-\alpha) + \alpha\beta(1-r\omega) - b_k(\alpha)] - \gamma_k \frac{v^2}{2} \tag{3}$$

where $v$ is the number of visits the collector makes according to the collection strategy described above and $\alpha$ is the proportion of potential collections diverted to bribes. The number of total visits conducted has payoff $\delta\bar{p}_k(v)$ and cost $\gamma_k \frac{v^2}{2}$. The cost of visits is such that each additional visit generates a cost proportional to the visit squared (de Groot 1988; Dal Bó et al. 2018) and $\gamma_k$ is a weight that reflects differences in the costliness of visits across collector types.[138] $\beta \leq 1$ is the proportion of the tax amount the collector is able to recover in bribes. Cost $r\omega$ is the cost for an additional unit of bribes in terms of punishment costs for the collector: $r$ is the probability the government catches the corruption and $\omega$ is the penalty, which does not differ by collector type. $b_k(\alpha)$ is a cost of corruption that captures the social or psychological costs of corruption (such as increasing negative perceptions of oneself among property owners or guilt over diverting revenues from the state), which may differ by collector type based on how sensitive they are to citizen views or how aligned they are with government's objectives. We again let this cost be increasing in the square of the proportion of collections diverted to bribes such that $b_k(\alpha) = b_k \frac{\alpha^2}{2}$, where $b_k$ is a marginal cost that can differ across collector types.

For simplicity, we do not model the bargaining process between collectors and property owners over whether to pay a bribe instead of the tax. We assume that when a collector decides to solicit a bribe instead of the tax, there is some probability — built into the portion of the tax the collector is able to recover $\beta$ — that the property owner will accept, and that

[135] In this case, type $L$ would generate greater compliance through greater persuasion ability alone, rather than superior targeting, holding constant effort.

[136] Note that knowledge of $\rho_k$ may also generate differences, but in this simple case we assume collectors of type $k$ know the costs of punishment for non-compliance for their own type $\rho_k$.

[137] We exclude the case of bribes that could be extracted on top of tax amounts or in exchange for reductions in tax amounts as these forms of corruption appear less likely to be common in our setting. Among self-reported bribe payers, 91% did not pay the tax accordingly to our midline measure (83% according to the endline measure), indicating that bribes are most likely paid to avoid the tax.

[138] de Groot, "Decentralization in Bureaucracies as a Principal-Agent Problem," *Journal of Public Economics*, 1988, 36, 323–337; Bó, Ernesto Dal, Frederico Finan, Nicholas Y Li, and Laura Schechter, "Government decentralization under changing state capacity: Experimental evidence from Paraguay," Working Paper, 2018

the bribe amount will not exceed the tax amount.[139] Collectors therefore determine whether to collect bribes in lieu of tax payment based on the benefits and costs associated with these actions.

*FOC*: A collector chooses $v$ and $\alpha$ to maximize the objective function, giving first-order conditions:

$$v^* = \frac{\bar{p}(v)W}{\gamma - \bar{p}'(v)W} \tag{4}$$

$$\alpha^* = \frac{\beta(1-r\omega)-\delta}{b_k} \tag{5}$$

where $W = \delta(1-\alpha) + \alpha\beta(1-r\omega) - b_k\frac{\alpha^2}{2}$.

We note the following implications of these conditions.[140] First, $v^*$ is increasing in $\bar{p}_k(v)$: a higher average probability of payment among those visited produces more visits. Because $\frac{\partial \bar{p}_k(v)}{\partial E[\lambda_k]} > 0$, $v^*$ is also increasing in $E[\lambda_k]$, the intrinsic motivation of individuals to pay the tax, and because $\frac{\partial \bar{p}_k(v)}{\partial \rho_k} < 0$, $v^*$ is also decreasing in $\rho_k$: a lower cost of punishment for non-compliance decreases the return to additional visits. Second, $v^*$ is decreasing in $|\bar{p}'_k(v)|$: a higher marginal (negative) change in $\bar{p}'_k(v)$ for an additional visit means less visits. Third, $v^*$ is decreasing in $\gamma_k$: a higher multiplier on cost of effort reduces visits. Finally, $\alpha^*$ is decreasing in $r\omega$ (or: $r$ and $\omega$, separately) and $b_k$ but increasing in $\beta$: higher costs to collecting bribes reduces them, higher payoffs increases them.

### A3.1.3 Government's Decision

The government wishes to maximize value from the taxation campaign net the associated costs with employing a collector type. In deciding which collector to engage in collection, it compares:

$$V_L - V_C = (1-\delta)(q_L - q_C) - (g_L - g_C) - \Gamma(c_L - c_C) \tag{6}$$

where $V_k$ is the value the government realizes from employing a collector type.[141]. Output $q_k = v_k^*\bar{p}_k(v_k^*)(1-\alpha_k^*)$ are the revenues collected by collector type $k \in \{L, C\}$, and cost $g_k$ represents the cost of engaging a particular collector type outside of sharing $\delta$ portion of revenues, such as training, materials, and transportation, but could also represent the costs of monitoring a given collector type to limit corruption.[142] Cost $c_k = \alpha_k^*\beta v_k^*\bar{p}_k(v_k^*)$ is the amount of revenues lost to corruption, and the difference across collector types is valued by $\Gamma$. As the cost of corruption — in terms of lost revenues — is already factored into the first term, this term instead represents the cost to the government of permitting corruption, such as in undermining perceived legitimacy or jeopardizing other programs by generating negative perceptions of government officials. We express the government's decision in value

[139] The median bribe amount measured at midline and endline in our sample is 1000 Congolese Francs, which is 40% of the median tax rate faced by households. Moreover, 95% of reported bribe amounts are equal to or less than the tax rate assigned to a household.

[140] In order to characterize relationships between inputs to collection neatly, we consider only interior solutions.

[141] This is not expressed in purely revenue terms as the government places a negative value on corruption that is separate from revenues lost to bribes.

[142] This second formulation of $g_k$ could represent an "oversight cost" by collector type that could additionally create a wedge between the returns to types.

terms rather than explicit revenue terms to accommodate this non-revenue cost; however, in our discussion in the paper we express $\Gamma$ as a multiplier that coverts these non-revenue corruption costs into revenue terms.

The difference in revenues between collector types is therefore, first, increasing (decreasing) in the square of the average probability of payment for collector type $L$ (type $C$),[143] reflecting differences described above that may derive from: collector strategy (informational advantage), intrinsic willingness to pay by type (tax morale), and cost of punishment for non-compliance (sanctioning power). Second, it is increasing (decreasing) in the number of visits conducted by type $L$ (type $C$). Third, it is decreasing (increasing) in the cost of effort multiplier for type $L$ (type $C$). Fourth, it is decreasing (increasing) in fixed costs to employing type $L$ (type $C$) $g_L$ ($g_C$). Finally, it is decreasing (increasing) in the proportion of collections lost to bribes, both from lost revenues and the cost of permitting corruption separate from the impact of revenues.

### A3.1.4 Discussion

This simple framework of the government's problem captures the primary margins through which we hypothesize collector types may differ in their ability to generate value in our setting. First, differences at the property-owner level affect the likelihood of payment, and these may differ by collector type: e.g., a collector type may engender stronger tax morale or trust that generates a higher willingness to pay the tax compared to another collector type. Likewise, collector types may differ in the costs they can impose for non-compliance: e.g., state agents (type $C$) may be better able to impose official penalties for non-compliance, while chiefs (type $L$) may be able to marshal other forms of punishment such as social sanctions, withholding services, or altering demands for informal taxes. These factors determine the property owner's decision to pay, highlighting that the identity of the collector may itself impact the probability of payment, holding targeting and effort fixed.

Second, differences in collector information about the probability of payment across property owners affects effort (and tax revenues). Though the strategies of collectors described above are relatively deterministic — type $L$ rank orders by $\lambda_{Li}$, type $C$ visits randomly, or operates with less information — this formulation captures the intuition that information differences affect the average probability of payment (among visited households) and therefore may also affect collector effort.[144]

Third, differences in the cost of effort by collector type shape effort, reflecting real-world costs associated with traveling to visit individual property owners. We hypothesize that these are mainly a function of the distance between collectors' home location and where property owners live in our setting, but may also depend on physical ability and the opportunity costs of time spent collecting taxes.

Fourth, we hypothesize that chief collectors may experience lower social or psychological costs to soliciting bribes, given they are less aligned with the government than central agents ($b_L < b_C$), and therefore will be incentivized to collect more bribes relative to type

[143] Also decreasing (increasing) in marginal reduction in average probability of payment $\bar{p}'_k(v)$.

[144] The shape of $f(\lambda_k)$ also matters: if there is more (less) variation in $\lambda_i$'s across citizens, then information is (more) less important. Likewise, how much effort collectors put in will matter more (less): this is reflected in the $\bar{p}'_k(v)$ term, which captures how much the average probability of payment changes with each visit. If all citizens have the same willingness to comply with the tax, then the informational advantage of collector type $L$ is zero.

$C$.[145] This difference highlights a cost to employing chiefs that may affect government revenue directly: that chiefs are less aligned with the government's objectives of raising public funds, reflected in the lower social or psychological cost type $L$ faces to collecting bribes, but which also may affect the government's choice of collector type through harms to perceptions of the state. The additional cost of corruption lies in what negative value the government places on such actions, in terms of how the level of corruption might undermine government legitimacy. If chiefs collect more taxes but also more bribes, for example, the government must decide to what extent the costs associated with permitting higher corruption when employing chiefs cancel out the higher revenues they bring in compared to central agents.

The last consideration relates to the cost of employing a particular collector type, which may differ in real-world terms, primarily in the form of compensation for transportation, if collectors differ in their location or payments to collectors entail different logistical challenges. In terms of cost-effectiveness, chief collectors may be more attractive because they live where they collect and thus do not require compensation for transportation to neighborhoods like state agents.

Mapping this framework to our setting, and focusing on the mechanisms through which a given collector type may possess an advantage in collection, as motivated by the literature, we hypothesize that chiefs may generate more tax collections than state agents if: the intrinsic willingness of property owners to pay chiefs is higher (tax morale), chiefs can impose greater punishments for non-compliance (sanctioning power), or, holding the aforementioned factors constant across collector type, if chiefs are better informed about property owner willingness to comply (informational advantage) or have lower costs to effort (transaction costs of visits). Conversely, state agents may generate more revenues because the punishment costs to non-compliance are greater when state agents are collecting (sanctioning power).

### A3.1.5 External Validity

The framework outlined above also allows us to consider in a simple way how contextual differences, or the impact of government interventions, could affect the relative value of employing collectors of a particular type. In this section, we discuss how (1) differences or changes in general tax enforcement (through increasing punishment of non-compliance), (2) citizens in a context having higher tax morale or the government raising it through public good provision, (3) access to richer information on citizens, (4) differences or changes in administrative costs, and (4) alignment of collector types with state objectives could affect the government's decision to employ collectors of a particular type.

1. *Higher enforcement*: Increasing enforcement — specifically in terms of punishing property owners for non-compliance by imposing penalties — would shift $\rho_k = 1 - a\theta$. Consider an extreme shift to perfect enforcement such that $a = 1$ ($\theta > 1$). In this case, all property owners will be willing to pay the tax and collector type is irrelevant in terms of informational advantages, and the revenue generation across types will only depend on (a) the relative effort and administrative costs of employing a particular type, and (b) differences in bribe solicitation.

[145] This assumes that costs to bribes in terms of property owners' perceptions about the government are low.

2. *Greater public good provision*: Likewise, increasing public good provision may shift $\lambda_C$ such that $E[\lambda_C] \geq E[\lambda_L]$: citizens will have higher intrinsic willingness to pay the tax to type $C$, and this reduces the advantage of type $L$ in information or lower costs to effort (or administrative costs).

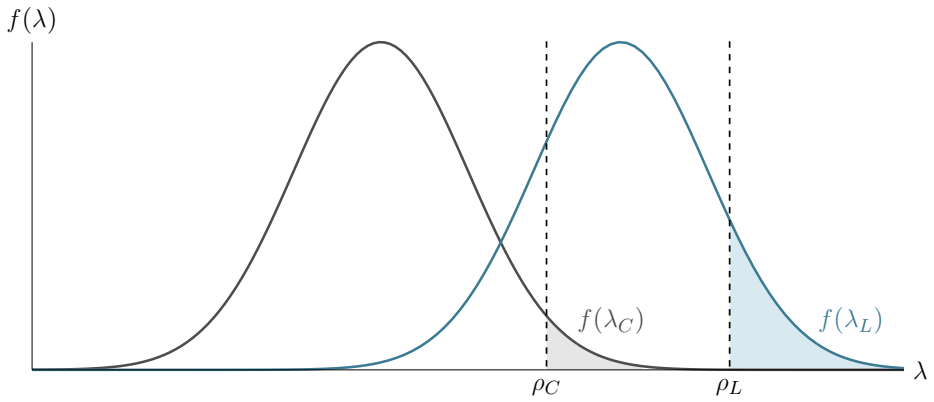
3. *Information on citizens*: Collecting information on property owners relevant to $\lambda_i$'s — through data collection or simply observing past compliance behavior — and transferring this information to type $C$ could neutralize the informational advantage of type $L$. Then differences by collector again would depend more on differences in cost to effort (and administrative costs) and incentives to divert revenues.

4. *Administrative costs*: Reducing administrative costs by collector type (e.g., selecting type $C$ collectors from each neighborhood and assigning them to collect in their own neighborhood — reduces transport costs, and potentially neutralizes type $L$'s informational advantage and equalized property owners' differences in intrinsic motivation to pay — if, for example, driven by trust of the collector).

5. *Alignment of collectors with state*: Reducing mis-alignment of type $L$ in terms of the social or psychological costs of soliciting bribes (through giving type $L$ more of an incentive to care about government revenues, potentially through recognition, greater responsibility in other areas, or providing a salary) may reduce a higher hypothesized prevalence of corruption among type $L$ collectors and therefore make the government's decision more concentrated on the differences in revenue generation between collector types.

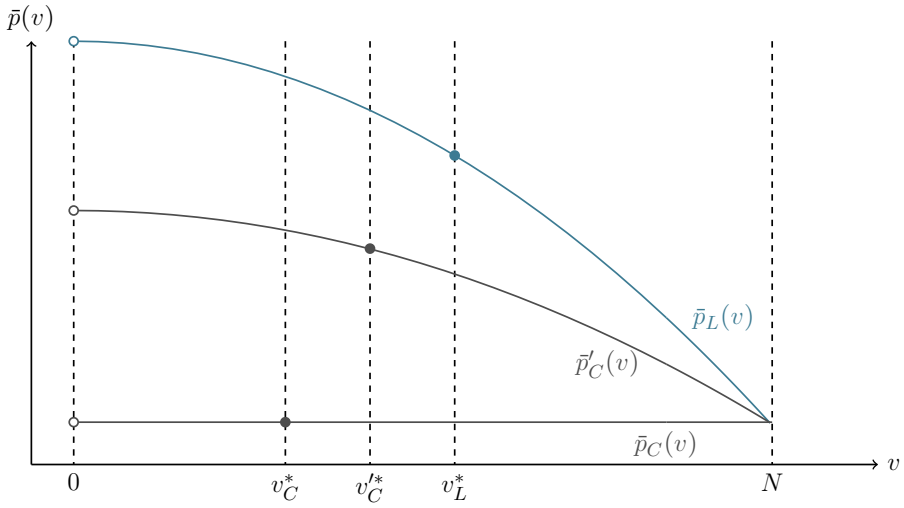
This discussion illustrates the manner in which differences across contexts may change the government's calculus in deciding between collector types or how that calculus may change if the government decides to invest in other inputs to generating tax compliance. In short, the contextual attributes or investments in raising compliance described above would all, in expectation, be positively correlated with the level of development and government resources. This suggests that the tradeoffs we identify, and the salience of the decision between collectors types more generally, is higher in low-enforcement, low-capacity settings, whereas in contexts with higher enforcement or resources for punishing non-compliance, the choice between collector types may be less crucial.

## FIGURE A17: EXAMPLE OF POTENTIAL TAXPAYERS BY COLLECTOR TYPE




*Notes:* Curves $f(\lambda_L)$ and $f(\lambda_C)$ are the distribution of intrinsic willingness, $\rho_L$ and $\rho_C$ the cost of non-compliance, and shaded areas proportion of potential payers by collector type $L$ and $C$. This figure is discussed in Section 7.3 and A3.1.1.

## FIGURE A18: AVERAGE PROBABILITY OF PAYMENT BY VISITS AND COLLECTOR TYPE




*Notes:* Curves $\bar{p}_L(v)$, $\bar{p}_C(v)$, and $\bar{p}'_C(v)$ are the average probability of payment among visited property owners by collector type and informedness. $v^*_k$ are the optimal number of visits selected by collectors, $N$ is the total number of property owners. This figure displays the case where $E[\lambda_L] = E[\lambda_C]$ and $\rho_L = \rho_C$: the only difference across collector types in average payment probability derives from the level of information about $\lambda_i$'s of property owners and number of properties visited. We discuss this figure in Section 7.2 and A3.1.1.

## A3.2 Cost-Effectiveness

To estimate the cost-effectiveness of state and chief tax collection, we examine campaign data on the marginal costs of tax administration, including transport costs and collector compensation.[146] State collectors were reimbursed for motorcycle taxis from the provincial tax ministry to their assigned neighborhoods. Chief collectors, by contrast, did not incur such costs because they worked near their homes. They were, however, reimbursed for weekly trips to the tax ministry to deposit their tax receipts and receive their bonus. The other key marginal cost was collectors' compensation, which was constant across treatments.

The marginal costs associated with Central and Local are summarized in Figure A19 (Panel A). Chief tax collection has roughly 30% lower administrative costs than state collection. Panel B shows back-of-the envelope estimates of the treatments' cost-effectiveness. The return on $1 is 53% higher in Local compared to Central due to the higher revenues achieved as well as the decreased administrative costs. Moreover, while Local was cost-effective, Central on average was not.[147] Further, this analysis reveals heterogeneity that could guide future policy. State collectors were similar to chiefs in cost-effectiveness when working in the city center, whereas they were much less cost-effective in the city's peripheries (Figure A20). Depending on its assessment of the social cost of bribery (cf. Section 9), governments could opt for collection strategies involving state agents in the city center and chiefs in the periphery.

Although the revenue returns to tax administration costs were low, this is a setting of near-zero prior citizen compliance in which the government is making initial investments in fiscal capacity that it hopes will lead to higher revenues in time. Tax officials often discuss their objective of inculcating a "fiscal culture" in Kananga over time. In other words, the government expects positive inter-temporal spillovers that make the expected future return higher than our calculations. In Section A3.1.5, we discuss how contextual differences and broader fiscal capacity investments could alter the choice of collector type. Yet even low-cost investments, such as mobile remittance of taxes by collectors (already on the tax ministry's agenda), could have large revenue impacts.[148] If chief collectors did not have to make weekly (or biweekly) trips to the government to deposit collections and receive their compensation, we estimate that $1 spent on chief collection would generate $3.2, as shown in Panel B of Figure A19.

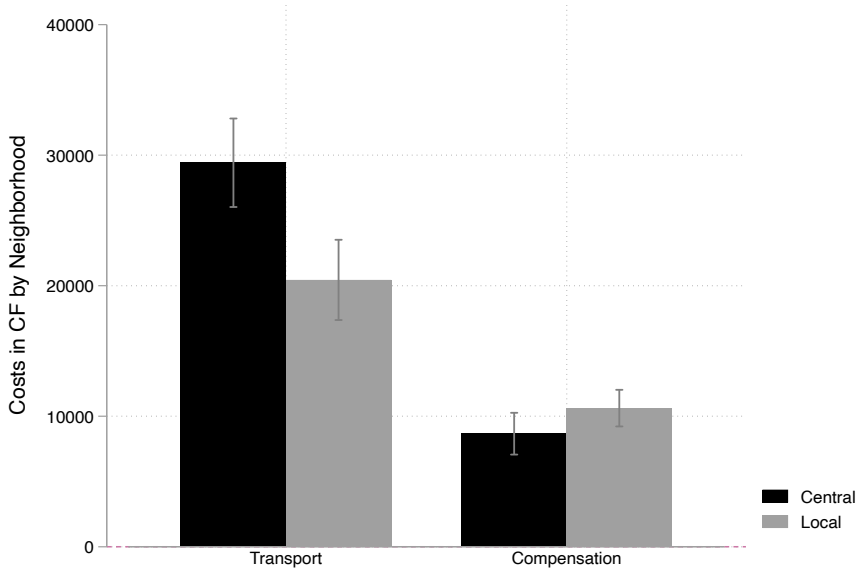
[146] Transportation costs, in particular, are emphasized in theoretical work on the tradeoffs between centralized collection and taxation by local elites (Azabou and Nugent, 1988; Levi, 1989).

[147] At the outset of the campaign, state collection was also cost-effective. But the secular decline in tax compliance over 2018 meant that over the course of the campaign, administration costs exceeded tax revenues.

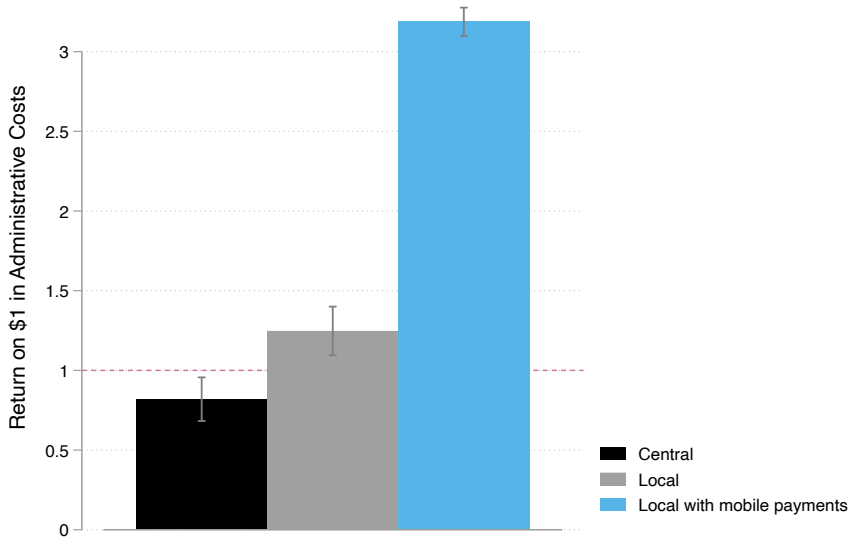
[148] Mobile banking and money transfer services are already widely used in Kananga.

## FIGURE A19: COSTS AND COST-EFFECTIVENESS ACROSS TREATMENTS

A: Costs of Tax Collection Methods

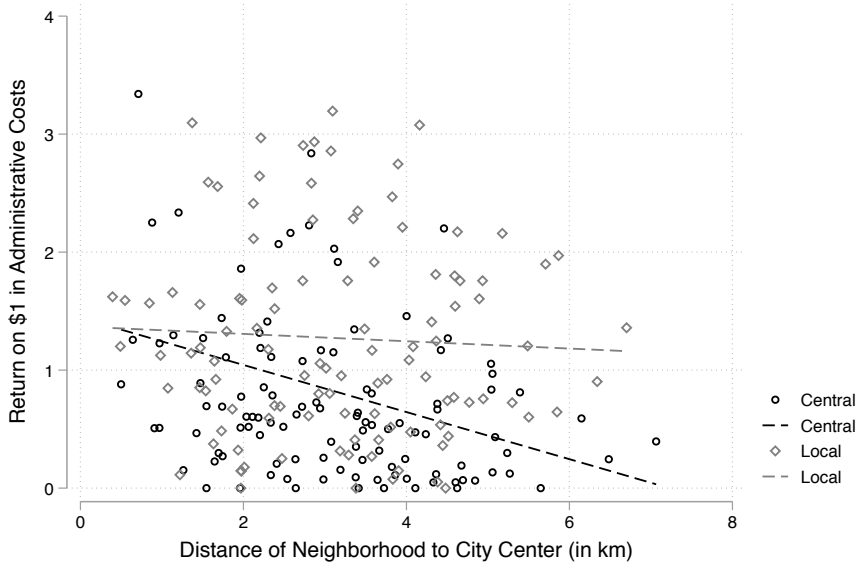



B: Cost-Effectiveness of Tax Collection Methods




*Notes*: This figure reports estimated costs (Panel A) and cost-effectiveness (Panel B) for the Central and Local treatments. In Panel A, costs are broken down by transport and compensation. In Panel B, cost-effectiveness is the return of an additional $1 spent on collection in particular treatment, and the hypothetical cost-effectiveness of Local with mobile payments is shown at far right. Estimates are the mean value of each measure averaging across neighborhoods. Confidence intervals are shown by the vertical bars. We discuss these results in Section A3.2.

## FIGURE A20: COST-EFFECTIVENESS OF LOCAL AND CENTRAL BY REMOTENESS




*Notes*: This figure reports estimated cost-effectiveness for the Central and Local treatments as a function of the distance from downtown Kananga. We discuss these results in Section A3.2.

## TABLE A29: LOCAL V. CENTRAL: BRIBE MULTIPLIER

| | Central | | | Local | | | |
|---|---|---|---|---|---|---|---|
| | Revenues | Costs | Bribes | Revenues | Costs | Bribes | Bribe Multiplier |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Campaign Amounts | 2,851,400 | 4,207,300 | 117,998 | 3,550,500 | 3,197,900 | 228,488 | 15.46 |
| With Mobile Money Payment | | 4,207,300 | | | 1,086,950 | | 34.57 |

*Notes*: This table reports measures from the tax campaign of total revenues collected and costs incurred for the Central and Local treatment arms. Columns 1 and 4 report revenues collected by treatment arm. Columns 2 and 5 report costs, which include bonuses paid to tax collectors and compensation for transportation. The second row reports costs under a hypothetical system in which chief collectors were paid (and remit tax collections) via mobile money rather than visiting the tax ministry to receive bonuses (and deposit collections). Costs for Central under this alternative system would remain the same. Columns 3 and 6 show the amounts of bribes collecting according to the measure at endline, scaled by the number of individuals surveyed at endline relative to the neighborhood population of households. All amounts are in Congolese Francs. Column 7 reports the implied multiplier on bribe payments that would be required for the government to weakly prefer employing state collectors instead of chief collectors: $\Gamma = ((R_L - R_C) - (C_L - C_C))/(B_L - B_C)$. This formula is discussed in more detail in Section A3.1.1. We discuss these results in Section A3.2.