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Local Elites as State Capacity: How City Chiefs Use Local Information to Increase Tax Compliance in the Democratic Republic of the Congo†

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This paper investigates the trade-offs between local elites and state agents as tax collectors in low-capacity states. We study a randomized policy experiment assigning neighborhoods of a large Congolese city to property tax collection by city chiefs or state agents. Chief collection raised tax compliance by 3.2 percentage points, increasing revenue by 44 percent. Chiefs collected more bribes but did not undermine tax morale or trust in government. Results from a hybrid treatment arm in which state agents consulted with chiefs before collection suggest that chief collectors achieved higher compliance by using local information to more efficiently target households with high payment propensities, rather than by being more effective at persuading households to pay conditional on having visited them. (*JEL* D73, D83, H24, H26, H71, O12, O17)

There is growing agreement about the importance of state capacity—including tax capacity—for economic and political development (Besley and Persson 2009, Acemoglu and Robinson 2019, Stasavage 2020). But how fragile states build capacity remains a puzzle. Such states typically operate alongside a range of local

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and traditional elites.[1] Whether these elites are an impediment or an asset to state modernization and development is debated. While local elites at times capture local politics (Anderson, Francois, and Kotwal 2015) and civil society (Acemoglu, Reed, and Robinson 2014), there may be scope for low-capacity states to collaborate with local elites to improve governance and service delivery (Baldwin 2016; Basurto, Dupas, and Robinson 2020). This paper studies if fragile states can increase their fiscal capacity by delegating tax collection to local elites.

A fundamental decision facing rulers is whether to deploy their agents to collect taxes or to delegate collection to local elites.[2] In weak states, local elites are thought to achieve greater enforcement thanks to detailed local information about taxpayers that state agents lack.[3] Collection by local elites is also thought to lower administrative costs, as there is no need to staff a tax office in every province (Levi 1989).[4] The key trade-off is that local elites are harder to control (Johnson and Koyama 2014). This exacerbated principal-agent problem could lead to leakage from total revenues as well as other social costs, especially if it empowers elites to become more extractive (Mamdani 1996). 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, while relying on their own agents when the state is strong.[5] 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.[6] Consistent with this prediction, local elites continue to play an important role in tax collection today primarily in countries with weak states, many of them in sub-Saharan Africa.[7]

This paper investigates the trade-off between local elites and state agents as tax collectors in the Democratic Republic of the Congo (DRC), a low-capacity

[1] These include customary and religious elites (Chaney 2013; Michalopoulos and Papaioannou 2015; Cantoni, Dittmar, and Yuchtman 2018), economic elites (Baland and Robinson 2008), and even rebel groups (Sanchez de la Sierra 2020).

[2] 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 eighteenth century, tax farming was the norm for customs and excise taxes, while wage contracts prevailed for land and other direct taxes (Kiser 1994).

[3] See, e.g., Kiser (1994), Scott (1998), Johnson and Koyama (2014), Mayshar, Moav, and Neeman (2017), and Stasavage (2020).

[4] In seventeenth-century England, Kiser (1994) estimates that tax administration costs amounted to roughly 20 percent of total revenue for state-administered customs taxes, while contracting with local elites reduced this cost to 8 percent (p. 303).

[5] For example, 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 (Levi 1989, p. 79). Local elites frequently collected taxes in the medieval and early modern periods (Ertman 1997), exemplified by land tax collection by English commissioners and justices of the peace (Harriss 1993, Kiser and Karceski 2017, Stasavage 2020). Modern state tax administration then emerged in Europe starting in the eighteenth century (Brewer 1990, Bonney 1995).

[6] 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 (Kleven et al. 2011, Pomeranz 2015, Naritomi 2019, Jensen 2022).

[7] On local and customary elites collecting tax in Africa, see Mamdani (1996); Boone (2003); Iversen et al. (2006); Baldwin (2016); Sanchez de la Sierra (2020); Jibao, Prichard, and van den Boogaard (2017); Gottlieb, LeBas, and Magat (2021); Van den Boogaard (2021).

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 45,162 properties, to "Central" or "Local" tax 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 local notables, selected by elders in the community, who resolve neighborhood disputes and help maintain local infrastructure through an informal labor tax in which citizens contribute to local public goods. They are analogous to the types of local elites whom states have engaged in tax collection historically and in many African countries today.[8]

Aside from the type of collector, all other aspects of tax collection—property registration and assessment, tax liabilities, training and campaign protocols, collector compensation, etc.—were identical across treatments. Collectors first went door to door registering properties, assigning tax IDs, and assessing annual tax liability based on the quality of building materials. Collectors then solicited payment of the property tax, issuing receipts using handheld printers to payers. 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 increased the share of registered property owners who paid the property tax in 2018 from 6.3 percent in Central to 9.5 percent in Local, a 3.2 percentage point increase. This uptick in compliance raised property tax revenue by 44 percent. By comparison, cross-randomized enforcement messages on tax notices caused a percent increase in tax compliance one-fifth as large. Although average compliance may seem low, it is similar to property tax compliance in the capital cities of other low-income countries.[9] We rule out several alternative explanations for this result, including that chiefs collected from properties that should have been exempted, or that awareness of (or competition with) other treatment arms motivated chiefs.

Alongside this increase in tax revenue, city chiefs were about 1.8 percentage points more likely to collect bribes than state collectors, consistent with principal-agent concerns. However, we find little evidence of local mismanagement or backlash on other measurable margins. For instance, 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' tax morale, trust in the government, or increased local labor taxation by chiefs.

Why did chiefs collect more tax than state collectors? We explore three families of mechanisms. First, as residents of the neighborhoods they taxed, chiefs might have had lower effort costs of visiting households and thus conducted *more tax visits* after property registration. This could have increased compliance if households faced time-varying cash-on-hand constraints, or if more visits increased the

[8] 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 and Alou 2009, Honig 2017, De Herdt and Titeca 2019) and often play a role in property taxation (Nguema 2005, Cogneau et al. 2020).

[9] For example, property tax compliance is roughly 7 percent in Haiti (Krause 2020), 7.7 percent in Liberia (Okunogbe 2019), 12 percent in Senegal (Cogneau et al. 2020), and 25 percent in Ghana (Dzansi et al. 2020).

perceived risk of enforcement. Examining treatment effects on reported visits from collectors, however, we find no differences on the extensive or intensive margin.

A second possible mechanism is that, conditional on doing similar numbers of tax visits, chiefs were able to more efficiently *target* their visits thanks to local information about citizens' underlying payment propensities. To investigate this possibility, 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. The meetings endeavored to codify and 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.

CLI 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 collected more tax by changing which households they targeted in response to the chief's information, visiting and taxing those recommended by the chief with higher probability. Indeed, comparing the characteristics of households visited by collectors after registration across treatments, 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 the neighborhood—led to greater compliance gains for state collectors in the CLI treatment arm.

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., if they were more trusted, or had a closer link to public services—or more credibly threaten sanctions for noncompliance.[10] To test this possibility, we examine if chiefs still collected more tax when their targeting ability was neutralized during property registration (when all collectors followed a linear, house-by-house route to issue sequential tax IDs). Tellingly, chiefs did not collect more tax than Central agents during registration. Additional tests also provide little evidence in support of a persuasion mechanism.[11] Ultimately, then, chiefs appear to have collected more tax than state collectors because of informational advantages that enabled them to better target tax visits based on households' underlying payment propensities.

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 houses with high-quality walls and roofs—visible characteristics—and more likely to visit owners with higher ability and willingness to pay—nonvisible characteristics. Correspondingly, the additional households that chiefs brought into the tax net had, on average, slightly lower-quality properties, yet they had ability and willingness to pay similar to taxpayers in Central. Chief collection thus appears more de facto regressive in terms of house quality but not in terms of income or liquidity.

[10] For instance, chiefs may have been able to threaten informal sanctions, such as increased labor taxes.

[11] These include heterogeneity by (i) baseline chief trust and power, and (ii) cross-randomized tax notice messages.

All told, should low-capacity states delegate tax collection to local elites in urban and peri-urban areas? Chief collection raised more revenue—and proved 53 percent more cost-effective—than state collection, but it also increased bribes. We estimate that a revenue-maximizing 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 thus conclude that, in the short run, fragile states seeking to establish rudimentary fiscal capacity could benefit from greater engagement with local elites.

Importantly, such engagement should complement, and not substitute for, investments in the enforcement capacity of the formal state (Besley and Persson 2009). Past work in developing countries finds that such investments—especially in the ability to centralize third-party information (Pomeranz 2015, Naritomi 2019) and to use tax instruments suited to the context (Best et al. 2015)—can pave the way toward considerably higher compliance. In the short run, however, improving enforcement in these ways may require a threshold level of state capacity and revenue that some fragile countries lack.[12] In these countries,[13] we view engagement with local elites in taxation as a complementary short-term approach to raise revenue at the margin and create the fiscal space to invest further in state tax enforcement capacity.[14]

To our knowledge, this paper is the first to examine the trade-off between employing state agents or local elites in tax collection in a randomized policy experiment. While governments have always confronted this trade-off when setting tax policy (Levi 1989, Kiser 1994, Ertman 1997), the provincial government of Kasaï-Central's decision to randomize neighborhoods of Kananga into chief or state tax collection allows us to estimate the causal effects of these approaches on state revenues, tax incidence, corruption, and views of the government.[15] We therefore build on recent work highlighting how tax policy choices 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 may vary in low-income countries as a function of state capacity.

Second, the paper contributes to work on the value of local information in governance. Despite being a centerpiece in the literature on delegated decision-making (Aghion and Tirole 1997, Mookherjee 2006, Acemoglu et al. 2007), including the targeting of social programs (Alatas et al. 2012; Basurto, Dupas, and Robinson 2020), there remains little direct evidence on the value of local information.[16] We contribute by experimentally illustrating (i) the value of information possessed by

[12] For instance, centralizing and leveraging third-party information to better target tax audits may require computerization across the public and financial sectors. In the DRC, although computerization is a priority for the provincial government, the majority of offices still rely on paper record keeping. The economy is also mainly informal, and financial institutions are weak—meaning that third-party information is scarce. See Ngindu and Weigel (2022) for more on this point.

[13] Our results are most likely generalizable in low-income countries with fragile or very low-capacity states, including the 39 such states identified by the World Bank in 2021 (World Bank 2021).

[14] For instance, new revenues from chief collection could be used to build systems to process third-party information or increase audit probabilities.

[15] Closest in this regard is Khan, Khwaja and Olken (2016), 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.

[16] Important exceptions include Duflo et al. (2018), Dal Bó et al. (2020), and Hussam, Rigol, and Roth (2022), which demonstrate the value of information possessed by environmental regulators, agricultural extension officers, and microentrepreneurs, respectively.

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.[17]

Finally, the paper contributes to literature on local elites in low-capacity states. Scholars have explored the role of such elites in governance and politics,[18] 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, Dupas, and Robinson 2020; Alatas et al. 2019; Voors et al. 2018; Casey et al. forthcoming). Although scholars across the social sciences have studied the role of local elites in tax collection in low-capacity states,[19] this topic has received less attention from empirical economists.[20] While most past work focuses on how elites shape governance outcomes by allocating public resources to clients or by leveraging a legitimacy that formal authorities lack, we identify their local information as a source of state capacity.

I. Setting

The 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 1 to 2 million inhabitants and an average monthly household income of $106 (purchasing power parity $168). The DRC is a low-capacity, "fragile" state, with a 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. To try to raise revenue, the government has turned to the property tax, which currently accounts for about 26 percent of provincial tax revenue.[21] It began to extend the property tax net by launching its first citywide collection campaign in 2016 (Weigel 2020). This paper studies the second such campaign, conducted in 2018.[22]

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 percent have any source of electricity (Table 3). Other public goods typically funded by local taxation, such as road repair, are similarly underprovided. In sum, we study an equilibrium with near-zero tax compliance, very weak state capacity, and minimal service provision. This paper explores the government's attempts to escape this low equilibrium by raising citizen tax compliance.

[17] Our emphasis on local information also complements work on the importance of third-party information in enabling high levels of tax compliance (e.g., Kleven et al. 2011, Pomeranz 2015, Brockmeyer and Hernandez 2016, Naritomi 2019, Jensen 2022).

[18] See, e.g., Michalopoulos and Papaioannou (2013, 2015); Acemoglu, Reed, and Robinson (2014); Anderson, Francois, and Kotwal (2015); Baldwin (2016); Sanchez de la Sierra (2020); Marchais, Henn, and Sanchez de la Sierra (2019); van der Windt et al. (2019); and Henn (2020).

[19] See, e.g., Levi (1989); Mamdani (1996); Boone (2003); Glennerster, Miguel, and Rothenberg (2013); Acemoglu, Reed, and Robinson (2014); Bodea and LeBas (2016); Kiser and Karceski (2017); and Lust and Rakner (2018).

[20] The main exception is Sanchez de la Sierra (2020), which examines nonstate actors collecting taxes in lieu of the state, not in collaboration with the state. Also related is Gottlieb, LeBas, and Magat (2020), which compares the delivery of tax notices by state agents versus marketplace association representatives in Nigeria.

[21] The other largest sources of provincial tax revenue are (i) business licenses and fees paid by firms, and (ii) gatekeeper-style fees on trade and transport.

[22] We therefore study a separate campaign two years after the campaign studied in Weigel (2020). The government did not administer a property tax campaign in 2017 because of a violent insurgency that year.

A. *The 2018 Property Tax Campaign*

The experiment we study was embedded in the 2018 property tax campaign in Kananga implemented by the provincial government of Kasaï-Central. The procedures of the campaign were identical across treatments; what varied was the type of collector.

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, fines for late payments, and the use of handheld receipt printers.

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.[23] When registering households, collectors recorded information about the property owner and assigned a unique tax ID. They delivered tax letters to owners showing the liability and other information about the property tax (online Appendix Figure A1). Collectors assessed each property's tax liability based on the principal house's construction, including whether it was exempt.[24] Household 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 payment of the tax. If households could not pay, 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 were instructed during training to revisit households until they paid the tax during the assigned month.[25] 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.[26]

Timing.—The 2018 tax campaign ran from May to December. Collectors had one month to complete work in each assigned neighborhood. They completed the property register in the first days of the month and conducted follow-up tax visits for the remainder. Collector workload consisted of one to two neighborhoods per month.

[23] 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.

[24] Property tax exemptions, which make up 14 percent of properties in Kananga, include (i) state-owned properties; (ii) schools, churches, and scientific/philanthropic institutions; (iii) properties owned by the elderly (55 years or above), widows or disabled people; and (iv) properties with houses in construction.

[25] Actual revisit rates were at collectors' discretion and vary considerably, as discussed in Section VA.

[26] Although small discrepancies arose occasionally, by the end of the campaign, the total amount in the government account matched the amount in the receipt data.

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	4,246	356
Midline survey	Enumerators	Jun 2018–Feb 2019	36,130	356
Endline survey	Enumerators	Mar–Sep 2019	3,893	356

Notes: N = number of observations, J = number of clusters (neighborhoods). The property register has more observations per neighborhood than the midline survey because of attrition and because the former includes information on all compounds, including (exempt) government buildings, churches, and empty lots, while the midline survey was only conducted with privately owned plots liable for the property tax (see Section IIIB). 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 IA and ID.

Collector Compensation.—Consistent with standard practice at the tax ministry, collectors across all treatments received a piece-rate wage with two components. First, they received 30 Congolese francs (CF) per house registered. Second, they received a piece rate for collections equal to 30 percent of the revenue they deposited to the state on average.[27] Collectors were also reimbursed for transportation expenses incurred while traveling between assigned neighborhoods and the tax ministry.

Tax Rates.—Rather than facing a property tax schedule that applies marginal tax rates to property value—common in high- and middle-income countries (Khan, Khwaja, and Olken 2016, 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 percent) were classified in the *low-value band*, and 4,979 (11 percent) in the *high-value band*.[28] Low-value properties are those in which the principal building is made of nondurable 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 a tax liability of 13,200 CF (roughly \$9).[29] These liabilities represent an average tax rate of 0.32 percent of property value, according to machine learning estimates (Bergeron, Fournier et al. 2020). This is comparable to the property tax rate in certain US states, which ranges from 0.27 percent to 2.35 percent. Simplified property taxation—here, a fixed annual fee—is common in settings of

[27] The magnitude of this wage is analogous to that studied in Khan et al. (2016). Households were randomly assigned to a collector bonus of 30 percent the rate or a flat 750 CF, as discussed in Bergeron, Tourek, and Weigel (2020). We show robustness to controlling for and interacting treatments with household-level collector wages (online Appendix Table A6). In 2018, \$1 was worth roughly 1,500 CF.

[28] 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.

[29] Cross-randomized within these categories, the government assigned certain households to partial rate reductions, the focus of a separate paper (Bergeron, Tourek, and Weigel 2020). For robustness, we control for and interact the main collector treatments with household-level tax rate abatements in online Appendix Table A6.

low state capacity, including India, Tanzania, Sierra Leone, Liberia, Malawi, and elsewhere (Franzsen and McCluskey 2017).[30]

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, citizens' beliefs about enforcement are heterogeneous and a potential mechanism of collector effectiveness we explore in Section VC.

II. Design

After the first property tax campaign in 2016, which only involved agents of the tax ministry, the provincial government of Kasaï-Central reasoned that engaging local city chiefs in collection could further increase revenue.[31] 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 2018 property tax campaign.

A. *Collector Treatments*

1. State Collectors (Central).—In Central neighborhoods, agents of the provincial tax ministry were charged with all campaign responsibilities. State collectors in this arm were unsalaried contractors who frequently undertake work for the tax ministry and other parts of the provincial government.[32] 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.[33] There were 50 such state collectors, who were almost entirely male, with an average age of 31 years and a high school education (online Appendix Table A1). Collectors worked in teams of two, with each team randomly assigned to two neighborhoods per month. Every month collectors were rerandomized into pairs.

2. Chief Collectors (Local).—In Local neighborhoods, city chiefs were charged with campaign responsibilities. These chiefs are local notables whose main responsibilities include (i) mediating local disputes, especially over property; and (ii) helping maintain local infrastructure through an informal labor tax (*salongo*) in which citizens help repair roads, bridges, and other local public goods. Chiefs are nominated by elders in the neighborhood—typically for being longstanding and respected residents—and then rubber-stamped by the government.[34] Chiefs have indefinite and often lifelong tenure, which at times passes through families, and

[30] The United Kingdom and Ireland have also experimented with similar property tax schemes in the recent past.

[31] As noted, such chiefs play a role in property tax collection in many African cities (e.g., Cogneau et al. 2020).

[32] Collectors in this arm are analogous to those who worked on the 2016 campaign, studied in Weigel (2020).

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

[34] To some extent, chiefs have multiple principals: the people in the neighborhood and the state. However, the norms governing their selection and removal make it clear that they are primarily accountable to the neighborhood. Applying the logic of the multiple principals problems, chiefs' split allegiances would likely decrease their effectiveness as collectors relative to state collectors from the perspective of revenue maximization.

deposition is very rare.[35] Chiefs do not receive regular salaries, and most 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.[36]

In the context of tax collection, several qualities of city chiefs are worth noting. First, because they are selected by and embedded in their communities, city chiefs possess a high degree of *local information*. Given the importance of third-party information for tax enforcement (Kleven et al. 2011, Pomeranz 2015), local information may be an asset to chief tax collectors. Second, chiefs have *authority*, stemming from both customary legitimacy—the institution was modeled on the village chieftaincy—and recognition from the formal state. Chiefs thus enjoy high levels of trust and respect in their neighborhoods, which may shape citizens' nonpecuniary motives to pay taxes (Luttmer and Singhal 2014).[37] Chiefs also have power, which may influence citizens' pecuniary motivations to pay taxes (Allingham and Sandmo 1972) if chiefs are more credible in threatening sanctions for noncompliance. We explore whether these qualities of chiefs impact their abilities as tax collectors in Section V.

The 111 chiefs who worked on the tax campaign were 95 percent male.[38] The average chief was 59 years old and had completed 13 years of education. Beyond the qualities noted in the previous paragraph, chiefs' characteristics thus differ in several ways from state collectors (online Appendix 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.[39] Each chief had a local assistant who completed the training and worked on each step of the campaign. Collectors thus always work in teams of two across all treatment arms.

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

[36] Mirroring the literature on rural chieftaincy, urban chieftaincy can be thought of as a second-best institution that increases local surplus in weak states, or as an instance of local elites seeking to extract rents. According to the first view, one solution to the insecurity of property in weak states is that local notables like city chiefs can act as a local arbiter in exchange for the state recognizing their "neo-customary" status (Boone 2014)—as did President Mobutu in 1972 (Nzongola-Ntalaja 1975). This arrangement may reduce property rights insecurity, thereby boosting investment and expanding the tax base for the state, while the chief gets utility from the status conferred by the position (Baldwin 2016, Honig 2017, van der Windt et al. 2019, Mustasilta 2019). According to the second view, city chiefs use their status and their official position to extract rents, and property rights security varies according to one's ties with the chief (Mamdani 1996; Boone 2003; Goldstein and Udry 2008; Acemoglu, Reed, and Robinson 2014). Beyond conflict resolution, urban and rural chiefs play many complementary roles vis-à-vis the formal state (Henn 2020), from taxation to land titling to information campaigns in settings like Senegal, Côte d'Ivoire, Niger, Cameroon, DRC, and elsewhere (de Russel 1998, Nguema 2005, de Sardan and Alou 2009, De Herdt and Titeca 2019, Cogneau et al. 2020).

[37] For instance, citizens may have a higher intrinsic willingness to pay taxes (Dwenger et al. 2016) when the chief collects. Alternatively, reciprocal motives to pay taxes (Besley 2020) may be stronger with chief collectors given their role in local public goods provision.

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

[39] These demographic and attitudinal differences should work against chiefs' effectiveness as tax collectors, given that collectors with more education, wealth, and positive views of the government tend to collect more tax (online Appendix Figure A4). Indeed, controlling for these collector characteristics magnifies our estimates (online Appendix Table A8).

3. Central + Local Information (CLI).—This arm is identical to Central, with one addition. After completing property registration, but before follow-up tax visits, state collectors consulted with the neighborhood's chief 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, and collectors recorded the information on the property register. After the meeting, collectors parted ways with the chief and proceeded with tax collection. This treatment arm endeavored to codify the chief's information and transmit it to state collectors to reveal 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 reassigned 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. Because of space constraints and this arm's policy orientation, we provide results from CXL and further discussion in online Appendix A3.2.

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. These letters were identical to those distributed elsewhere, except that they instructed property owners to pay 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 they were intended to shed light on mechanisms and have policy relevance, respectively. Only five neighborhoods were allocated to Control because evidence from the 2016 campaign suggested compliance would be near zero (Weigel 2020).[40]

B. *Randomization*

The unit of randomization is the neighborhood (online Appendix 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 eight that were the site of a logistics pilot several weeks before the campaign launch (see online Appendix A2.4), leaving 356 neighborhoods for the randomization.[41] We use

[40] Due to an implementation error, one neighborhood randomly assigned to CXL received the Local treatment. We use the de facto assignment throughout and show robustness to dropping this neighborhood in online Appendix Table A7.

[41] 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.

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 number 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, city chiefs collected. CLI is shorthand 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 IIA. We also discuss the reason for differential allocation of clusters across treatment arms in Section IIA.

a block-randomized design and stratify on (i) geographic location, (ii) treatment status in the previous property tax campaign, and (iii) past experience of the city chief with tax collection.[42] 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.[43]

C. *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.[44] In panel B, we consider property characteristics, as measured in the property register and in the midline survey. In panel C, we consider neighborhood characteristics. Overall, only one variable (years of education) varies systematically between Local and Central based on simple t-tests, as one would expect under random assignment.[45] In online Appendix 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 ($F = 0.39$, $p = 0.68$).[46] In this comparison, one covariate (distance to schools) is imbalanced at the 10 percent level.

[42] Section IIB contains detailed descriptions of these variables used to construct randomization strata.

[43] These include neighborhood-level baseline averages in terms of (i) education, (ii) proximity to a ravine, (iii) quality of house walls, (iv) knowledge of the chief, (v) perceived responsiveness of the chief, (vi) tax compliance in 2016, (vii) conflict-affectedness, and (viii) the number of chiefs active in the neighborhood.

[44] We provide more details on the baseline and midline survey in Section III.

[45] In online Appendix Table A7, we reestimate the main results controlling for years of education. Online Appendix Table A3 alternatively reports balance tests relative to the pure Control arm.

[46] 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 versus 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 versus Central comparisons controlling for imbalanced covariates in online Appendix Table A19.

Table 3—Randomization Balance

	Observations (1)	Central mean (2)	Local (3)	CLI (4)	CXL (5)
Panel A. Property owner characteristics					
Years of education^B	3,614	10.56	−0.07	−0.03	−0.60
			(0.24)	(0.27)	(0.32)
Electricity^B	3,627	0.13	0.01	0.002	0.02
			(0.01)	(0.02)	(0.02)
Log household monthly income^B	3,594	10.53	0.07	−0.07	−0.21
			(0.16)	(0.19)	(0.25)
Trust in chief^B	3,613	3.07	0.05	0.10	0.19
			(0.06)	(0.07)	(0.08)
Trust in national government^B	3,436	2.51	0.04	−0.0004	0.02
			(0.06)	(0.07)	(0.09)
Trust in provincial government^B	3,459	2.41	0.08	0.04	−0.0005
			(0.06)	(0.07)	(0.08)
Trust in tax ministry^B	3,423	2.36	0.04	−0.02	−0.07
			(0.06)	(0.07)	(0.08)
Sex^M (1 = male)	22,221	0.77	0.01	0.001	−0.01
			(0.01)	(0.01)	(0.01)
Age^M	19,874	54.35	0.45	0.12	0.56
			(0.48)	(0.59)	(0.64)
Majority tribe^M	22,625	0.77	0.02	0.002	0.02
			(0.02)	(0.01)	(0.02)
Employed^M	24,298	0.74	0.01	0.003	−0.01
			(0.01)	(0.01)	(0.02)
Salaried^M	24,299	0.25	0.01	−0.01	−0.01
			(0.01)	(0.01)	(0.01)
Works for government^M	24,299	0.15	0.01	0.01	−0.01
			(0.01)	(0.01)	(0.01)
Relative works for government^M	26,996	0.23	0.003	0.02	0.01
			(0.01)	(0.01)	(0.02)
Panel B. Property characteristics					
House quality^M	28,362	0.004	−0.01	0.14	−0.07
			(0.10)	(0.09)	(0.11)
Distance to state buildings and city center^R	44,087	1.5	0.06	−0.001	0.04
			(0.05)	(0.06)	(0.07)
Distance to health institutions^R	44,087	0.33	0.02	0.04	0.004
			(0.02)	(0.02)	(0.03)
Distance to education institutions^R	44,087	0.65	0.03	0.04	0.01
			(0.03)	(0.04)	(0.04)
Distance to roads^R	43,468	0.41	0.03	−0.02	0.04
			(0.04)	(0.05)	(0.06)
Distance to eroded areas^R	43,468	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)	(0.02)
Baseline replacement	3,437	0.15	0.01	0.01	0.02
			(0.02)	(0.02)	(0.02)
Registration to midline	44,365	0.21	0.02	−0.01	−0.06
			(0.03)	(0.03)	(0.03)

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, replacement at endline of baseline respondents, and attrition from registration to midline 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. Variables are described in online Appendix A2.6. Balance tests for bilateral treatment comparisons are shown in online Appendix Table A2. We discuss these results in Section IIC.

III. Data

We use administrative tax data as well as three household surveys (see Table 1).

A. *Administrative Data*

Property registration data, covering 45,162 potential taxpayers, include tax ID numbers, geographic coordinates, owner names, property classifications (see Section IA), exemptions, tax rates, and payment during registration. The handheld receipt printers used by collectors stored details of each transaction—the neighborhood number, tax ID, property value band, tax rate, amount paid, time stamp, and collector name—in their memory, uploaded to the government's tax database each week.[47] 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.

B. *Household Surveys*

Enumerators working for the research team administered baseline surveys to 4,246 households from July to December in 2017. To achieve a representative sample, enumerators visited 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, and views of and engagement with chiefs.

Enumerators then administered a midline survey at every property 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 36,130 complete midline surveys.[48]

Finally, from March to September, 2019, enumerators successfully tracked 3,893 baseline respondents to complete the endline survey. Attrition from baseline to endline was 8.3 percent and is balanced across the Central, Local, and CLI treatments (Table 3).[49] In cases in which the baseline respondent was traveling or unavailable to complete the endline survey for three or more weeks, enumerators surveyed another member of the household (12 percent of respondents).[50] The topics were analogous to the baseline survey.

[47] 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.

[48] We lack midline surveys for 21 percent of registered properties because (i) in 18 percent of cases, no adult was present when the enumerators visited properties, or (ii) the property was an exempt type (e.g., government buildings, churches, and empty lots) that collectors registered but enumerators did not survey (3 percent of cases). Attrition from registration to midline is balanced across treatments (Table 3). There is also variation in missingness across variables from the midline survey due chiefly to imperfect knowledge of midline respondents about the property owner. Such missingness is also balanced across treatments (online Appendix Table A4).

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

[50] Replacement at endline is also balanced across treatments (Table 3).

IV. Estimation and Main Results

We primarily use ordinary least squares (OLS) to compare Local to Central:

$$y_{ijkt} \; = \; \beta_0 + \beta Local_{jkt} + \mathbf{X}_{ijk}\boldsymbol{\Gamma} + \alpha_k + \theta_t + \varepsilon_{ijkt}, \tag{1}$$

where i denotes individuals, j neighborhoods, k randomization strata, and t campaign time periods. Standard errors are clustered at the neighborhood level (356 in total). The term y_{ijkt} is the outcome of interest, α_k are stratum fixed effects, θ_t are fixed effects corresponding to waves of the tax campaign, and $\mathbf{X}_{ijk}$ is a covariate vector. The main analyses contain only dummies for house type (low- or high-value band),[51] and robustness checks include different vectors of covariates, as noted in the preanalysis plan (e.g., online Appendix Table A7). Although our main results table contains a specification without θ_t, our preferred specification when examining tax outcomes includes time fixed effects corresponding to waves of the campaign to net out time trends in tax compliance that occurred during 2018 for reasons unrelated to collector characteristics.[52]

A. *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 III. 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 percent compared to 6.3 percent in Central, a 3.2 percentage point increase. This translates into an additional 79.6 CF per property, a 44 percent increase relative to Central.[53]

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.[54] Moreover, 2018 was only the second time the government had solicited property tax payment from the great majority of citizens. Top tax officials view their goal as the creation of a "fiscal culture" in Kananga, whereby

[51] We exclude house type fixed effects when examining endline outcomes because to do this we would need to merge survey and registration data, which reduces our endline sample size (due to faded tax ID codes).

[52] As we discuss in online Appendix A2.5, 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. Although the staggered rollout ensures considerable overlap in time across treatments, some time imbalance remains and affects our estimates. Including fixed effects corresponding to campaign waves helps restrict the analysis to periods with sufficient overlap among the treatments under comparison. We also consider several alternative strategies to deal with time imbalance, which yield similar estimates to our preferred specification. We do not include time fixed effects when examining outcomes from the endline survey, which were collected in all neighborhoods *after* the tax campaign.

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

[54] For example, property tax compliance is approximately 7 percent in Haiti (Krause 2020), 7.7 percent in Liberia (Okunogbe 2019), 12 percent in Senegal (Cogneau et al. 2020)—which have similar door-to-door campaigns—and 25 percent in Ghana (Dzansi et al. 2020). These estimates come from national capitals; Kananga is the DRC's fourth largest city.

Table 4—Local versus 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	28,872	27,764	213	27,764	23,803
Clusters	221	213		213	213
Central mean	0.068	0.063	0.065	0.063	0.073
Panel B. Revenues					
Local	57.627	79.640	81.830	68.855	81.991
	(25.688)	(22.856)	(38.595)	(20.560)	(23.562)
Observations	28,872	27,764	213	27,764	23,803
Clusters	221	213		213	213
Mean	192.891	182.236	210.134	182.236	208.568
Time fixed effects	No	Yes	Yes	Yes	Yes
House fixed effects	No	No	No	Yes	Yes
Stratum fixed effects	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 (in Congolese francs) 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 IV 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 5 exclude exempt properties. The data include all properties registered by tax collectors merged with the government's property tax database. We discuss these results in Section IVA.

citizens who enter the tax net today will feel obligated to pay taxes again tomorrow.[55] These compliance numbers must then be considered in the context of a fragile state attempting to initiate formal taxation as a source of revenue.

For robustness, we reestimate the results after collapsing the data to the neighborhood level (column 3),[56] and after adding fixed effects for property value bands (column 4). If we exclude exempt properties, the treatment effect increases to 4.0 percentage points (column 5).[57] Further, in online Appendix Table A5, we reestimate equation (1) using each of the adjustments for time imbalance described in online Appendix A2.5, which yield similar estimates to our preferred specification. In online Appendix Table A6, we estimate a fully saturated model with dummies for cross-randomized treatment arms and their interactions with the Local treatment.[58] Finally, we explore a range of additional robustness checks in online Appendix Table A7, including (i) controlling for basic covariates (age, age squared, sex, and years of education), (ii) controlling for basic covariates and proximity to schools (the imbalanced covariate in the Local versus Central comparison), (iii) controlling for further socioeconomic covariates, (iv) reestimating results including

[55] A study of tax holidays in Uruguay indeed finds that paying taxes can be habit forming (Dunning et al. 2017).

[56] Imai, King, and Nall (2009) note that unequal numbers of units within clusters can cause bias in cluster-randomized designs.

[57] Our main specification does not condition on exemptions because they were at collectors' discretion and thus an outcome of treatment.

[58] As noted above, these cross-randomized treatments include property tax rate abatements and collector bonus amounts randomized at the property owner level (see Bergeron, Tourek, and Weigel 2020).

pilot neighborhoods, (v) excluding the neighborhood misassigned to Local, and (vi) reestimating results at the neighborhood level after winsorizing the top 10 percent of outcomes.

As a benchmark, we compare the magnitude of the effect of Local to the effect of a standard enforcement tax letter treatment.[59] As discussed in online Appendix 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. This enforcement message did raise tax compliance (online Appendix Table A28), but it did so one-fifth as much as delegating collection to city chiefs.[60]

One concern is that awareness of other treatments and collector types 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 were designed to limit such comparisons. Collectors in each treatment were trained separately and reported to the tax ministry on different days. During trainings, tax ministry leadership announced that the 2018 procedures, including the collector type by neighborhood, would remain in place for the foreseeable future. Nonetheless, we examine 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 reestimate 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 (online Appendix Table A10).[61] 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. In online Appendix A3.4, we consider additional tests of whether state collector demoralization or exhaustion could explain the results: for instance, restricting Local to chiefs who worked in multiple neighborhoods at once or in sequential waves, or restricting Central to first-time collectors (online Appendix Table A41). There is little evidence that competition motivated chiefs' performance, or that demoralization or exhaustion undermined state collectors' performance.[62]

Did delegating property tax collection to city chiefs crowd in (or out) contributions to other formal or informal taxes? One potential fiscal externality, broadly construed, concerns informal labor taxes (*salongo*), which chiefs themselves administer and to which 38 percent of citizens reported contributing for an average

[59] A large literature studies the effects of embedding enforcement messages in tax letters sent or delivered to taxpayers (e.g., Blumenthal et al. 2001, Pomeranz 2015).

[60] Specifically, assignment to the state enforcement message increased compliance by 58 percent (online Appendix Table A28, column 3). By contrast, in the subsample of respondents who received randomized tax messages, which were introduced in the last phase of the campaign, chief collection increased compliance by 300 percent (online Appendix Table A28, column 1).

[61] 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.

[62] Another alternative explanation is that the more hierarchical nature of collector teams in Local—a chief with an assistant, versus two peer collectors—led to more efficient team production (Alchian and Demsetz 1972). Exploiting across-team variation in state collector differences in several dimensions (age, education, income), we observe no evidence that teams with more dissimilar collectors achieved higher tax compliance (online Appendix A3.3).

4.2 hours over two weeks.[63] When we reestimate equation (1) with self-reported contributions to *salongo* as the outcome, we find no statistically significant treatment effects on the extensive margin, $\hat{\beta} = -0.031$ (0.032), or intensive margin, $\hat{\beta} = -0.240$ (0.247), two weeks after tax collection or eight months after collection (online Appendix Table A11).[64] Although chiefs have no role with other formal taxes, their collection of property tax could have formal fiscal externalities if it shaped tax morale, beliefs about enforcement, or if households have a fixed budget for all taxes. Examining self-reported measures of other formal tax compliance, we find that chief collection increased payment of market vendor fees and the income tax (online Appendix Table A11). To test for experimenter demand effects, we included an obsolete poll tax in the survey for which we observe no treatment effect.[65] There is thus suggestive evidence that delegating property tax collection to chiefs did not interact with informal labor taxation but may have boosted compliance with other formal taxes.

B. *Effects on Mismanagement and Views of the Government*

A key concern in the historical literature (e.g., Kiser 1994, Mamdani 1996) is that delegating collection responsibilities to local elites could fuel corruption and undermine trust in the government—hypotheses we explore in this section.

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 property valuation (i.e., whether a property was classified in the low- or high-value band). For each, we compare collectors' assessments with those of independent enumerators informed of the official rules to identify deviations.[66] According to this measure, chiefs were more likely to (correctly) exempt households (Table 5, rows 1 and 2), and this is driven by more frequent exemptions of the elderly and disabled property owners (online Appendix Table A9).[67] Chiefs were also more accurate in 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.

[63] Indeed, past work finds that formalization can crowd out important functions, such as insurance, of informal institutions (Besley and Coate 1995, Fafchamps and Lund 2003).

[64] To examine further if tax payment and *salongo* participation are substitutes, complements, or neither, we include an indicator for tax payment on the right-hand side and interact it with Local (online Appendix Table A12). Payment is an outcome and thus a "bad control," so we alternatively use a measure of "predicted compliance" estimated through the procedure detailed in Section VB. Paying the property tax and participating in *salongo* are positively correlated in Central but not in Local (panel A). Using the predicted compliance measure suggests a similar pattern, though the interaction term is not significant (panel B). These results are suggestively 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.

[65] This now-obsolete tax has a known local name, providing a credible yet fictitious tax as a test of response bias.

[66] The official rules are simple and easy to verify. As noted above, low- and high-value properties are distinguished by the building materials, easily observable to enumerators. Similarly, exemptions are straightforward and verifiable by enumerators speaking with household members. The exemption status of 4.9 percent of properties was determined incorrectly, according to this detection approach, and 2.4 percent of houses were incorrectly assessed.

[67] Additionally, chiefs were not more likely to exempt members of the same tribe (online Appendix Table A9). Chiefs were slightly more likely to exempt property owners who knew them at baseline, but this effect is difficult to interpret because of large baseline differences in knowing collectors by treatment (43 percent in Local, 3 percent in Central).

Table 5—Local versus Central: Mismanagement and Views of Government, Chiefs, and Taxes

	$\hat{\beta}$	SE	R^2	Observations	$\bar{x}_{Central}$
Panel A. Property assessments					
Assigned exemption	0.039	0.021	0.055	13,772	0.266
Incorrect exemption	−0.012	0.007	0.020	13,771	0.044
Assigned high band	0.030	0.021	0.230	27,764	0.114
Incorrect assignment	−0.013	0.006	0.041	27,764	0.031
Panel B. Bribes					
Paid bribe (midline)	−0.001	0.003	0.007	18,596	0.016
Gap self versus admin (midline)	0.016	0.009	0.018	14,309	0.077
Paid bribe (endline)	0.018	0.009	0.049	1,169	0.014
Other payments (endline)	0.031	0.014	0.041	2,407	0.094
Panel C. View of government					
View of government (index)	0.023	0.049	0.100	2,411	0.011
Trust in government	0.127	0.057	0.075	2,286	0.028
Responsiveness of government	−0.049	0.045	0.099	2,282	0
Performance of government	−0.060	0.052	0.060	2,179	−0.014
Integrity of government	0.043	0.047	0.058	2,313	0.016
Panel D. View of taxation					
Perceived tax compliance on avenue	0.100	0.055	0.073	1,851	0.026
Trust in tax ministry	0.085	0.061	0.073	2,259	0.025
Property tax morale	0.075	0.047	0.057	2,343	0.014
Fairness of property taxation	−0.004	0.053	0.046	2,407	0.003
Perception of enforcement	−0.019	0.058	0.070	2,379	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. The column header $\hat{\beta}$ is the coefficient on the treatment indicator, followed by the cluster-robust standard error, R^2, number of observations, and the Central group mean $\bar{x}_{Central}$. 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 exempt. 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 IVB. Panels C and D control for the baseline value except when analyzing *perceived tax compliance* and *fairness of property taxation*, outcomes we only measured at endline. Each dependent variable, described briefly in Section IVB and in detail in online Appendix A2.6, is standardized to facilitate interpretation of coefficient magnitudes. We discuss the results in panels C and D in Section IVB. 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 IV 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 V. The number of observations varies across regressions due to (i) outcomes being drawn from different surveys, and (ii) nonresponse for specific survey questions.

We next examine treatment effects on bribes according to three measures. First, at midline and endline, we asked property owners if they paid "transport" to collectors, a colloquial expression for bribes that is not taboo to discuss in Kananga (Reid and Weigel 2017).[68] According to this measure, just shy of 2 percent of households reported paying bribes to collectors, and essentially all of these payments were made in lieu of, not in addition to, the tax.[69] In other words, these were collusive

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

[69] 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.

bribes, not extortion. Comparing treatment groups, we find that chiefs were more likely to collect bribes (by 1.8 percentage points) according to the endline measure ($p = 0.061$), but not the midline measure (Table 5, panel B). 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 at midline.[70] According to this measure, chiefs were 1.6 percentage points more likely to collect bribes ($p = 0.051$), similar to the endline estimate. As a last measure, the endline survey also asked if households had paid any other informal payments or fees to authorities (not limited to payments made during the tax campaign).[71] 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 principal-agent concerns.[72]

The level of bribes chiefs collected might have been suppressed by awareness of the research team's evaluation. We test for Hawthorne effects by examining the relationship between bribes and baseline chief knowledge of potential sanctions and of our evaluation. Chiefs who at baseline were aware of (i) other chiefs being disciplined, and (ii) the 2016 tax campaign (for which the research team conducted an analogous evaluation) do not appear to have perceived a higher risk of sanctions at endline (online Appendix Table A14, columns 1–3) or to have collected fewer bribes (online Appendix Tables A14, columns 4–6, and A15, panel E). These results are inconsistent with Hawthorne concerns.

Finally, we examine how chief tax collection impacted views of the government and of taxation itself.[73] We again estimate equation (1), this time controlling for respondents' baseline beliefs, where we have repeated measures.[74] Empowering city chiefs to collect taxes does not appear to have undermined the perceived legitimacy of the government (Table 5, panel C). If anything, self-reported trust in the government increased by 0.127 standard deviations. But the effect on an aggregate index of views of the government is not different from zero, so this increase in trust is only suggestive. Regarding views of taxation (panel D), citizens in Local perceived higher compliance of others, mirroring our main results. We find no statistically significant changes in trust in the tax ministry, the perceived fairness of property taxation, tax morale, or enforcement perceptions.[75]

[70] This is an imperfect measure because it includes both 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, estimated treatment effects should be unbiased.

[71] 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.

[72] By predicting likely chief bribe payers in Central and correlating this measure with tax and bribe payment, we provide suggestive evidence that the counterfactual to the increase in bribes paid to chiefs would have been tax payment had Local neighborhoods been assigned to Central (online Appendix Table A16). The increase in bribes in Local is thus most likely a transfer from the government—what Shleifer and Vishny (1993) call "corruption with theft"—rather than a transfer from households. We also estimate the effects of chief collection on the "total tax burden," including taxes, bribes, and *salongo* contributions, in online Appendix Table A13.

[73] Detailed explanations of variables, which we standardize in the analysis to facilitate interpretation of magnitudes, are in online Appendix A2.6.

[74] We have baseline values for all variables except *perceived tax compliance* and *fairness of property taxation*.

[75] For these analyses, we can only rule out effects larger than about 0.1 standard deviations.

In sum, chief collection appears to have increased bribes, but at least according to the margins we are able to measure, there is little short-run evidence that chiefs abused their responsibilities in other ways or damaged citizens' views of the government.

V. Mechanisms

Why did chiefs collect more tax than state collectors? This section considers three potential channels: (i) chiefs made *more tax visits* to households than state collectors; (ii) chiefs could more efficiently *target* their visits to households with higher payment propensity using local information; or (iii) 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 noncompliance.

A. *More Tax Visits*

The first possible mechanism is that chief collectors simply made more follow-up tax visits after property registration than state collectors.[76] Chiefs hailed from the neighborhoods in which they worked, whereas state collectors were dispatched from the tax ministry to assigned neighborhoods by motorbike. Although state agents' transport costs were covered, chiefs may have had lower effort costs of additional tax visits. More visits on the extensive margin—whether collectors ever returned after registration—could have raised compliance as more potential payers were solicited. More visits on the intensive margin—the number of times collectors returned after registration—could have increased compliance by (i) increasing the probability that liquidity constraints were nonbinding at the time of visit, or (ii) causing citizens to update their beliefs about enforcement and to 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, chiefs do not appear to have made more visits on the extensive or intensive margin (Table 6, columns 1 and 2).[77] Could chiefs have encountered citizens and asked them about taxes in ways that would not register as official collector visits? To check, we examine whether citizens reported talking to tax collectors outside of home visits—but find no evidence of more informal contact with collectors in Local on the extensive or intensive margin (Table 6, columns 3 and 4). Chief tax collectors do not appear to have achieved higher tax compliance by making more tax appeals.

B. *Targeting*

Conditional on making a similar number of tax visits, chiefs may possess local information about property owners that enables them to better target those with higher propensity to pay. For instance, imagine that chiefs observe a more accurate

[76] To be clear, follow-up 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.

[77] 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.

Table 6—Local versus Central: Tax Visits

	Visited by collector (1)	Number of visits by collector (2)	Other contact with collector (3)	Instances of other contact (4)
Local	−0.009	0.014	0.008	0.019
	(0.026)	(0.046)	(0.007)	(0.012)
Time fixed effects	Yes	Yes	Yes	Yes
House fixed effects	Yes	Yes	Yes	Yes
Stratum fixed effects	Yes	Yes	Yes	Yes
Observations	18,162	18,151	3,513	3,513
Clusters	209	209	206	206
Mean	0.417	0.552	0.025	0.039

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 IV, and cluster standard errors at the neighborhood level. Columns 1 and 2 report differences in tax visits—after the registration visit—by the extensive and intensive margins, respectively. Columns 3 and 4 report differences in citizen-reported contact with collectors outside of the tax campaign by the intensive and extensive margins, respectively. We exclude property type fixed effects in online Appendix Table A17. We discuss these results in Section VA.

signal about each household's payment propensity compared to state collectors. If both types of collector simply ranked households by payment propensity and visited them in this order, chiefs would achieve higher compliance—assuming (i) they visited the same number of households after registration, as noted above, and (ii) collectors did not visit every household in a neighborhood, which we confirm in the data.[78] We discuss this logic more formally in online Appendix A3.1 and express it visually in online Appendix Figure A17.[79]

As a first test, we consider evidence from the hybrid CLI treatment arm, in which state collectors consulted with chiefs about the ability and willingness to pay of each property owner in the neighborhood. State collectors could then use chiefs' information when targeting their tax visits (conducted without the chief),[80] 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.[81] On average, CLI outperformed Central in compliance and revenues (Table 7). When armed with chiefs' information, state collectors achieved 2.4 percentage point higher compliance and 30.9 percent higher revenues. Importantly, CLI collectors did not conduct more tax visits on the extensive or intensive margin (columns 3 and 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.

[78] On average, 43 percent of households reported any tax visits after registration.

[79] 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 their other responsibilities.

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

[81] Table 7 shows estimates from specifications with time fixed effects delineated by the midpoints between the start and end of each treatment under comparison to maximize time overlap (see Section IV) and house type fixed effects. Online Appendix Tables A18 and A19 show alternative specifications and the inclusion of imbalanced midline covariates for robustness.

Table 7—Central versus CLI

	Compliance (1)	Revenues (2)	Visited (3)	Visits (4)	Compliance (5)	Compliance (6)
CLI	0.024	46.566	−0.016	−0.026	0.026	0.022
	(0.009)	(21.200)	(0.028)	(0.044)	(0.014)	(0.009)
Local						0.046
						(0.007)
Visit control	No	No	No	No	Yes	No
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
House fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Stratum fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	20,636	20,636	13,884	13,877	5,283	33,746
Clusters	165	165	163	163	161	267
Central mean	0.051	150.66	0.387	0.497	0.097	0.052
Test CLI = Local (p-value)						0.007

Notes: This table compares the CLI arm to the Central arm, which is the excluded category. Columns 1, 5, and 6 report effects on compliance. Column 2 reports effects 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 IV. Column 5 restricts to the subsample of properties that received any tax visits after registration. Column 6 includes a dummy for the Local treatment. The bottom row reports the *p*-value from a test for equality between the CLI and Local. We discuss these results in Section VB.

If targeting were the only mechanism, and if chief consultations perfectly transmitted all relevant information to state collectors, then CLI would have completely closed the gap between Central and Local. This was not the case: chiefs still collected more tax than "informed" state collectors in CLI (Table 7, column 6).[82] There may thus have been other dimensions of chiefs' information useful for targeting tax visits that were not transmitted during consultations,[83] or other mechanisms also at work.[84]

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 and 2).[85] This positive association is robust to controlling for visible house characteristics (columns 3 and 4), such as the quality of roof and walls, which (uninformed) state collectors could also use when targeting tax visits.[86]

[82] The gap between CLI and Local is also evident in online Appendix Figure A5.

[83] For instance, as noted in online Appendix A3.6, we find suggestive evidence that chiefs also have information about the optimal *timing* of tax visits. According to receipt data, chiefs appear more likely to collect taxes later in the day when liquidity constraints may be less likely to bind (online Appendix Figure A20).

[84] Another potential explanation is that chiefs may have had an advantage in scheduling future tax visits during property registration because CLI collectors did not yet know high types worth targeting at that stage. (Consultations occurred *after* registration.) However, we do not observe differentially higher compliance in Local among properties where the owner was present during registration (online Appendix Table A20).

[85] When asked about the consultations at endline, 82 percent of state collectors said meeting the chief was very helpful or helpful, and 79 percent said they changed their targeting strategy in line with the chief's recommendations. In fact, 38 percent said they "only targeted households recommended by the chief."

[86] Recommended households also appear to have had higher payment propensities than other visited households. Among those visited after registration, a one point increase in the chief's ability-to-pay (willingness-to-pay) ranking is associated with an 8.3 (5.8) percentage point increase in the probability of payment. This analysis should be taken with a grain of salt because it involves conditioning on an outcome (tax visits).

Table 8—The Value of Chiefs' Information

	Visited (1)	Compliance (2)	Visited (3)	Compliance (4)	Visited (5)	Compliance (6)	Visited (7)	Compliance (8)
Panel A. Ease of payment								
Ease of payment	0.045	0.056	0.029	0.044				
	(0.012)	(0.007)	(0.014)	(0.008)				
Predicted ease of payment					0.039	0.041	0.004	0.027
					(0.021)	(0.012)	(0.016)	(0.009)
Wall quality			0.025	0.021	0.011	0.015	0.025	0.012
			(0.012)	(0.007)	(0.011)	(0.007)	(0.011)	(0.005)
Roof quality			0.005	0.000	0.006	0.001	0.018	−0.010
			(0.006)	(0.002)	(0.008)	(0.004)	(0.008)	(0.006)
Erosion threat			0.017	−0.004	−0.003	−0.011	−0.002	−0.005
			(0.011)	(0.004)	(0.012)	(0.007)	(0.010)	(0.005)
Observations	5,574	8,135	4,551	5,150	4,980	4,994	4,820	4,826
Clusters	79	80	66	66	93	93	80	80
Mean	0.376	0.072	0.352	0.065	0.435	0.103	0.41	0.059
Panel B. Willingness to pay								
Willingness to pay	0.034	0.037	0.033	0.038				
	(0.011)	(0.007)	(0.012)	(0.008)				
Predicted willingness to pay					0.037	0.032	0.016	0.026
					(0.020)	(0.011)	(0.016)	(0.009)
Wall quality			0.022	0.021	0.012	0.015	0.025	0.012
			(0.013)	(0.009)	(0.011)	(0.007)	(0.011)	(0.005)
Roof quality			0.011	0.001	0.006	0.001	0.018	−0.010
			(0.008)	(0.002)	(0.008)	(0.004)	(0.008)	(0.006)
Erosion threat			0.016	−0.005	−0.003	−0.011	−0.002	−0.005
			(0.012)	(0.005)	(0.012)	(0.007)	(0.010)	(0.005)
Observations	3,933	5,521	3,929	4,461	4,980	4,994	4,820	4,826
Clusters	50	50	50	50	93	93	80	80
Mean	0.357	0.062	0.357	0.066	0.435	0.103	0.41	0.059
Treatment	CLI	CLI	CLI	CLI	Local	Local	Central	Central
House fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stratum fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table explores the extent to which chiefs' recommendations in CLI predict tax visits after registration and tax payment. Columns 1–4 show correlations in CLI between chiefs' recommendations and outcomes. Columns 5–8 report correlations between predicted propensity measures described in Section VB and outcomes in Local (columns 5 and 6) and Central (columns 7 and 8). Columns 1, 3, 5, and 7 show correlations between propensity and tax visits; columns 2, 4, 6, and 8 show correlations between propensity and compliance. All regressions include house type and randomization stratum fixed effects and cluster standard errors at the neighborhood level. Columns 3, 4, and 5–8 include controls for visible household characteristics. We show results excluding house fixed effects in online Appendix Table A21. We discuss these results in Section VB.

Moreover, the properties recommended by chiefs in CLI resemble the properties that chiefs themselves visited after registration when working as collectors in Local neighborhoods.[87] For this analysis, we predict properties that chiefs would have recommended in Local and Central using a propensity score approach on a set of household characteristics measured in surveys.[88] These predicted chief recommendations align closely with the households that chiefs did in fact visit and collect from in

[87] In fact, the characteristics of visited households in CLI resemble those in Local more closely than those in Central, as we discuss further in Section VIA and visualize in Figure 1.

[88] Following Alatas et al. (2012), we regress chiefs' payment propensity scores on a range of household characteristics. We store the coefficients for all 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 property owner's age, sex, employment status, salary (dummy), government job status (dummy), and ethnic group. We then bin this predicted measure into a 1–3 rank to be analogous to the CLI measure and correlate it with tax visits and tax compliance in columns 5–8 of Table 8.

Local, even when controlling for visible house characteristics (Table 8, columns 5–6). By contrast, predicted chief recommendations are uncorrelated with visits in Central (column 7), highlighting again the different set of households targeted by informed (CLI) and uninformed (Central) state collectors. Yet the predicted chief recommendations do correlate with tax compliance in Central (column 8).[89] Thus, if state collectors in Central happened upon one of these high-propensity households, the owner would still be more likely to pay; but, absent chiefs' information, state collectors were not more likely to visit high-propensity types than others in the neighborhood.

Third, if the transfer of local information to state collectors explains the gap between CLI and Central, then consulting with more informed chiefs should have led to larger treatment effects. To rank chiefs' local information, we use a quiz-like survey module in which chiefs were asked factual questions about a set of 12 randomly selected residents from their neighborhoods (see online Appendix A3.5). State collectors who consulted chiefs with above-median knowledge, according to this quiz, achieved 2.8 percentage point higher tax compliance (significant at the 10 percent level) than those who consulted with less informed chiefs (online Appendix Table A23, column 2, and online Appendix Figure A7). By contrast, if we correlate chiefs' knowledge and tax compliance in Central—a placebo check since collectors in these neighborhoods did not consult with chiefs—there is no association, $\hat{\beta} = -0.007$ (0.012) (column 4).[90] More informed chiefs appear to have indeed made better consultants, consistent with a targeting mechanism.

Finally, if local information enables better targeting of taxpayers, then state collectors should have collected more tax when randomly assigned to work near their own homes. Consistent with a local informational advantage, an additional kilometer between a neighborhood's centroid and the assigned collectors' houses is associated with a 0.6 percentage point decrease in payment (online Appendix Table A24). However, state collectors working near their houses still achieved lower compliance than chiefs (by 2.7 percentage points), even when distance from collectors' houses is held constant (online Appendix Table A25).[91] This could be explained by the fact that chiefs' information is superior due to their leadership position and history in the neighborhood, or it could be consistent with other possible mechanisms, as we examine the next section.[92]

Could state collectors outperform chiefs if they simply visited all properties again after property registration? In a sense, a strict interpretation of this mechanism would suggest as much: visiting all properties multiple times would offset much of the chief's informational advantage. However, this policy would not likely be viable in a low-capacity setting like Kananga because (i) conducting additional tax visits is costly, and (ii) the tax authority is limited in its ability to motivate collectors to

[89] We observe similar results if instead we use the predicted measures of chief recommendations in both CLI and Central, enabling a direct comparison of targeting across treatments (online Appendix Table A22).

[90] Chiefs' knowledge is also positively correlated with tax compliance in Local (column 6), consistent with a targeting mechanism, though this is difficult to interpret because chiefs' knowledge was measured after the tax campaign, and chiefs in Local could have become more locally knowledgeable while collecting taxes.

[91] We define "near" as the maximum distance between city chiefs' own homes and their neighborhoods' limits. We thus identify the set of Central neighborhoods with at least one collector living within that distance.

[92] Viewed differently, online Appendix Table A25 estimates a lower bound of the effect of Local versus Central over time, accounting for the local learning that state collectors would do if reassigned to the same neighborhoods in the future.

exhibit the effort needed to implement such a policy.[93] To provide suggestive evidence on this point, we estimate the daily return from tax collection in Central using receipt data to calculate the daily revenues and campaign data on administration costs (transport, collector compensation). After property registration, the return is positive for the first few weeks but becomes negative after day 20 (online Appendix Figure A15, panel A).[94] Because there is a marginal administrative cost of state collectors visiting neighborhoods each day, the government in fact incurs losses when collectors have extinguished the higher-propensity types and try to collect from the remaining noncompliant properties. Thus, in the presence of capacity constraints, the targeting of visits by tax collectors becomes crucial—and this is precisely why chiefs' local information is valuable.[95]

C. *Persuasion*

In a third family of mechanisms, chiefs may have been better able to persuade households to pay, conditional on having targeted them for a tax visit. For instance, chiefs may have been better able to stimulate citizens' tax morale (Luttmer and Singhal 2014). Citizens might have had higher trust in and intrinsic willingness to pay chief collectors (Dwenger et al. 2016), or they might have perceived a clearer taxes-for-services link (Besley 2020). Alternatively, chiefs may have been more credible in threatening sanctions for noncompliers, such as increasing demands for informal taxes or withholding favors and services (e.g., dispute resolution).

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 solicited payment from each household as the last step of the registration protocol. Yet, collectors in all arms followed a linear, house-by-house pattern during registration in order to map the properties in a neighborhood and assign sequential tax IDs.[96] Because collectors' targeting ability was neutralized, 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 (online Appendix Table A26). Although the level of payments during registration is low, these results are inconsistent with persuasion 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

[93] Visiting properties multiple times was precisely the instruction collectors received during training, but nonetheless, only 43 percent of households reported receiving tax visits after property registration. One could even define "fiscal capacity" as a ceiling on the number of tax visits that the state can carry out, similar to how Besley and Persson (2009) operationalize state capacity as a ceiling on the tax rates available to governments.

[94] If we assume that the distribution of tax payments over time is proportional to the distribution of tax visits over time, then we can also compute the return as a function of the share of total properties visited (online Appendix Figure A15, panel B). This analysis suggests that there are positive returns to visiting up to about one-half of the houses in a neighborhood, but going beyond that enters into negative (loss-making) territory.

[95] Moreover, as noted above and discussed in online Appendix A3.6, we find suggestive evidence that chiefs may also target using information about the optimal timing of collection (online Appendix Figure A20), which gives further reason to doubt if increasing state collector tax visits in an untargeted fashion would lead to substantial gains in compliance.

[96] We validate that collectors complied with these instructions using the time stamps and GPS coordinates taken during registration (online Appendix Figure A8).

heterogeneity by chiefs' rank, tenure, age, and method of succession (dynastic or not).[97] We also examine if the treatment effect is larger in neighborhoods in which chiefs were more trusted by and accessible to the population, and in which they were more active in the provision of local services.[98] If chiefs achieved higher compliance through greater powers of persuasion, then the treatment effect should be more pronounced where chiefs were more powerful, trusted, and active in service provision. Yet, we find little evidence of heterogeneity along these dimensions (online Appendix Table A27, panels A–C).[99] The exception is a larger effect in neighborhoods with more active chiefs ($p = 0.078$). While this is consistent with reciprocity driving compliance with chief collectors, it is also consistent with a targeting mechanism: more active chiefs also likely possessed better information about citizens.

Finally, we examine heterogeneity by cross-randomized messages on tax notices designed to interact with the main collector treatments to help isolate mechanisms.[100] These messages included Central and Local versions of standard deterrence and public goods messages, making salient risks of enforcement and the tax-public goods link by the state or the chief, respectively. As noted in our preanalysis plan, the Central (Local) versions of these messages should have been more credible coming from, and thus complemented the efficacy of, state (chief) collectors.[101] However, we find no significant interactions of these flier messages with Local (online Appendix Table A29). These null heterogeneous effects could reflect low literacy, collectors not reading the messages, or simply ineffective message treatments. But we do observe positive overall treatment effects of the deterrence messages on compliance ($p = 0.062$, online Appendix Table A28). Some messages thus appear to have shifted 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.

VI. Distributional Impacts

Given the importance of local information and the enhanced targeting of taxpayers by chiefs it enabled, this section opens the black box of chiefs'

[97] Customary and locality chiefs are higher ranked than avenue chiefs. Congo is a gerontocratic society: older chiefs may enjoy greater authority. Nineteen percent of chiefs inherited their position from their father—and we test if these dynastic chiefs collected more or less tax. For each measure, we calculate baseline averages, then define an indicator for above-median neighborhoods and interact this with treatment.

[98] For these variables, we use data from the baseline household survey. We measure trust in chiefs using an index of citizens' views of the chief (see online Appendix A2.6). We measure accessibility as the share of citizens who knew the chief's name, phone number, and attended the same church. We measure chief activity using questions about the frequency of *salongo*, dispute mediation, and neighborhood advocacy.

[99] The minimum effect size on the interaction term that we can reject at the 10 percent level is 2.3 percentage points.

[100] As noted, different messages were randomly embedded in the official property tax notices (see online Appendix A2.2).

[101] For instance, if chiefs collected more taxes because of greater local sanctioning capacity, there should be a more pronounced treatment effect when tax letters contained the Local deterrence message (rather than Control). Similarly, one would expect analogous heterogeneity with the Local public goods and trust messages, if chiefs collected more tax because of their link with services or the trust they inspire.

information by examining the distribution of tax visits and tax payment by household characteristics.[102]

A. *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 *visible* household characteristics—such as house quality, a signal accessible to both chiefs and state collectors—and *nonvisible* 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).[103] Importantly, this does not mean that chiefs sought out low-quality properties. On the contrary, chief collectors were also much more likely to visit and tax properties with above-median house quality in the neighborhood (online Appendix Figure A9). 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 nonvisible characteristics that predict payment. We examine four such characteristics, drawn from baseline survey data: (i) the predicted ease of payment measure derived from chiefs' consultations in CLI and described in Section VB; (ii) an index of liquidity, which includes cash on hand, income, consumption, employment, and productive assets; (iii) an index of revealed tax morale, proxied by self-reported payments of taxes in the past; and (iv) an index of households' views of the government.[104] Finally, we construct a payment propensity index from these four nonvisible characteristics. According to this index, chiefs were more likely than state collectors to have visited households with nonvisible 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.[105]

[102] This analysis is motivated by the concern that chief collection may be more regressive than state collection, as discussed in historical accounts (Kiser 1994) and recent work on informal taxation (Olken and Singhal 2011).

[103] 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 (online Appendix Figure A11), as does excluding property type fixed effects (online Appendix Figure A10). Online Appendix Figure A13 plots these distributions by treatment.

[104] Each of these indices, and their underlying variables, is explained in detail in online Appendix A2.6. The cash-on-hand measure for the liquidity index is measured at endline and thus posttreatment. We think it is unlikely to be affected by treatment given that on average eight months passed between tax collection and endline enumeration. We also find no significant differences in cash on hand between Local and Central at endline (online Appendix Table A36.)

[105] Examining within-neighborhood correlations—rather than comparing across treatment arms—also 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 (online Appendix Figure A9).

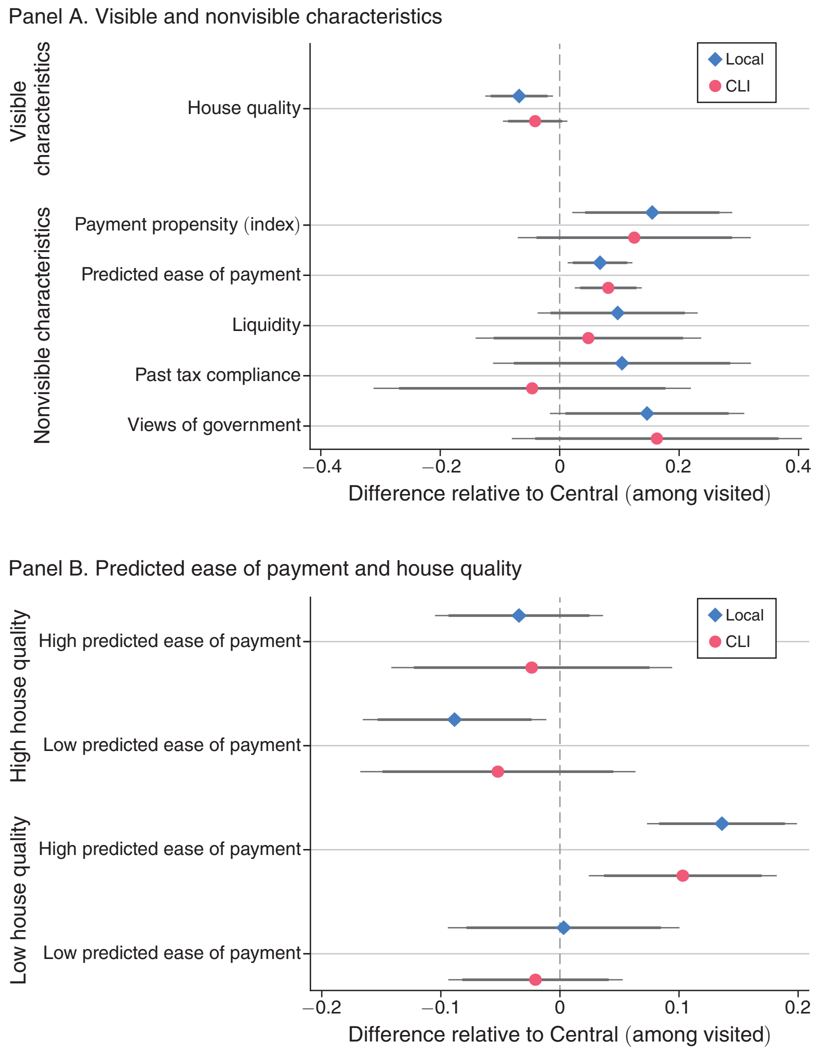

FIGURE 1. CHARACTERISTICS OF HOUSEHOLDS VISITED BY COLLECTORS AFTER REGISTRATION ACROSS TREATMENTS

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 visible and nonvisible characteristics for indices described in Section VIA. Panel B shows differences in the probability of receiving a visit in the four cells indicated (defined by interactions of high/low dummies for 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, online Appendix Figure A9 shows the correlations between tax visits and household characteristics within treatments, rather than differences across treatments. Online Appendix Figures A10 and A11 replicate this analysis while omitting house fixed effects and neighborhood mean controls, respectively. We discuss these results in Section VIA.

To capture the key difference between chief and state targeting, we bin households based on the median values of (i) visible house quality, and (ii) nonvisible ease of payment and examine correlations in the four cells of this 2x2 matrix. According to this partitioning, chiefs were (i) less likely than state collectors 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). Chiefs thus appear to have targeted tax visits using households' underlying payment propensities rather than exclusively relying on external property characteristics like state collectors.

B. *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 compliance varies by treatment across the value bands of the property tax schedule. As noted in Section IA, low-value properties (facing a \$2 rate) are those constructed with nondurable materials, such as mudbricks, while high-value properties (facing a \$9 rate) are constructed with concrete or other durables.[106] Compliance by band thus provides a coarse measure of incidence. Reestimating 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? According to the familiar house quality index, taxpayers in Local were 0.146 standard deviations less wealthy on average compared to Central (Table 9, column 3). However, using survey data on respondents' monthly income and estimated liquidity, we find no differences between taxpayers in Central and Local (columns 4 and 5).[107] Although the sample size in this analysis is small—restricted to tax compliers in the endline sample—this pattern is consistent with collectors' different targeting strategies (Figure 1). Chief collection appears to bring into the tax net property owners with slightly lower-quality houses but with ability to pay similar to tax compliers in Central neighborhoods. In other words, chief tax collection appears more de facto regressive in terms of house quality, but not in terms of income and liquidity.

VII. Conclusion and Policy Implications

Should low-capacity states delegate tax collection responsibilities to local elites in urban and peri-urban areas? On the one hand, chief collection raised revenue and did not undermine citizens' views of the government. It was also more cost-effective: the return on \$1 in tax administration was 53 percent higher in Local compared to Central, due to higher revenues and lower administrative costs.[108] On the other hand, chief collection increased bribes and was more de facto regressive by house quality (but not by income or liquidity). In online Appendix A3.1, we discuss these trade-offs in detail.

[106] In Bergeron, Fournier et al. (2020), we verify that these characteristics indeed predict property value.

[107] Income and wealth are only weakly correlated in urban sub-Saharan Africa due to rapid urbanization in the absence of liquid real estate markets (Fjeldstad, Ali, and Goodfellow 2017).

[108] For this calculation, we use administrative data on costs, including collector transport and compensation (see Section A3.7).

TABLE 9—LOCAL VERSUS CENTRAL: THE DISTRIBUTION OF THE TAX BURDEN

Outcome:	Compliance by property type		Complier characteristics		
	Low band property (1)	High band property (2)	House quality (3)	Avg. mon. income (4)	Liquidity index (5)
Local	0.036	0.002	−0.146	0.002	−0.063
	(0.008)	(0.013)	(0.057)	(0.042)	(0.167)
Neighborhood mean control	No	No	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
House fixed effects	No	No	Yes	Yes	Yes
Stratum fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	24,380	3,384	1,310	228	228
Clusters	208	150	157	121	121
Central mean	0.064	0.062	0.099	0.007	0.118

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 IV, and cluster standard errors at the neighborhood level. Columns 1 and 2 report estimates of the effect of Local collection on compliance for low- and high-band households, respectively. Column 3 reports differences in an index of house quality, conditional on 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. For robustness, we reestimate these results excluding (i) property type fixed effects (online Appendix Table A32) and (ii) leave-one-out neighborhood mean controls (online Appendix Table A33). We also estimate (iii) an interacted version of the house type regressions in columns 1 and 2 (online Appendix Table A34) and (iv) an alternative version of columns 3–5 in which tax compliance is regressed on indicators for complier characteristics above the median value in the sample, a Local treatment indicator, and the interaction between the two (online Appendix Table A35). Online Appendix Figure A13 (panel B) shows the distribution of house quality among tax compliers across treatments. We discuss the interpretation of these results in Section VIB.

Here, we consider a revenue-maximizing government and estimate the social cost of bribery that would justify selecting state over chief collectors.[109] By social cost of bribery we do not mean the mechanical negative effect on revenue (leakage) but rather potential negative fiscal externalities of bribes, such as the risk of undermining tax morale or perceptions of enforcement. Imagine that the government simply trades off the net return on tax collection with bribes multiplied by a constant representing these social costs. In this case, 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 (online Appendix Table A45).[110] Given that we find no evidence that chief collection eroded tax morale or enforcement perceptions (Table 5), a revenue-maximizing government would likely prefer chief to state collection in this setting.

What if the government maximized welfare rather than revenue? Although it is beyond the paper's scope to fully characterize the welfare effects of chief collection, we offer reduced-form evidence of impacts on endline income, cash on hand, consumption, and hunger relative to state collection. There are no effects of chief collection on these proxies for welfare according to intent-to-treat and instrumental variable

[109] Revenue maximization is a standard objective function for autocracies (e.g., Olson 1993)—and a reasonable assumption given the weak institutions of political accountability in the DRC and most fragile states.

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

estimates (online Appendix Table A36).[111] Tax and bribe payers were also not more likely to hold more negative endline views of the government or of the chief than nonpayers, as one might expect if such payments had large welfare costs.[112] Though this analysis does not address whether welfare losses of taxation in general are compensated by the value of public funds, it provides suggestive evidence that chief collection did not reduce citizen welfare more than state collection.

We therefore conclude that governments in fragile and very low-capacity settings are likely to benefit from collaborating with local elites in tax collection in urban and peri-urban settings in the short run.[113] In the longer run, however, it is unlikely that chief tax collection offers a road to building a modern "tax state" (Schumpeter 1918). Countries that raise 30–40 percent of their GDPs in tax typically have a much more centralized tax collection apparatus. In particular, if more third-party information becomes available to tax ministries—because of the expansion of the formal sector (Jensen 2022) and increasing financial development (Gordon and Li 2009)—then chiefs' informational advantages would likely dissipate and eventually be eclipsed by the informational capacity of the state.

Our results are therefore most relevant in the set of low-income countries with very low-capacity states.[114] While many developing countries fall outside of this set, fragile states present some of the most vexing development challenges today. By 2030, one-half of the world's extreme poor will be concentrated in fragile states (Collier, Besley, and Khan 2018). Escaping the low-equilibrium trap of low tax compliance, low public goods provision, and low investment in fiscal or legal capacity is difficult but imperative for achieving prosperity (Besley and Persson 2011). Incrementally expanding extensive-margin tax compliance from a low base—as did city chief tax collectors in the DRC—thus represents crucial progress in building basic state capacity.[115] Importantly, working with local elites can complement, not substitute for, the capacity of the formal state (Henn 2020). New revenues could be invested in training tax inspectors, increasing audit probabilities, and developing systems to process third-party information.[116]

[111] For instance, for weekly transport expenditure, $\hat{\beta} = -37.85$ CF (438.96). To capture local average treatment effects on tax or bribe payers, we also report IV estimates instrumenting payment status with assignment to Local (online Appendix Table A36, panels B and C). There are again no clear differences between treatments.

[112] For this analysis, we reestimate 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 (online Appendix Table A37). 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.

[113] 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 (Mamdani 1996, Boone 2003).

[114] The World Bank identified a list of 39 fragile states in 2021 (World Bank 2021).

[115] The importance of extensive margin gains in tax compliance is emphasized by Cui (2021) in studying the expansion of income tax revenue in China.

[116] In some settings, it may be optimal for governments to incorporate local elite tax collectors directly into the formal state, as England did after the Glorious Revolution (Braddick 1996). State building often involves integrating and institutionalizing local elites, who could otherwise become spoilers in the drive to establishing modern fiscal and legal capacity. For instance, De Tocqueville (1866) argues that the *intendant* system in *ancient régime* France failed to incorporate the nobility into the state, fueling social division and state weakness. By contrast, Tudor England created the position of Lord Lieutenant to institutionalize elites into the state (Braddick 2000). After 1688, local elites also assumed land tax collection responsibilities. The Ottomans similarly built capacity by integrating independent judges (*qadis*) into the state (Barkey 1994). Mukhopadhyay (2014) argues that state builders in Afghanistan should adopt a similar approach with local warlords today.

In sum, as their economies modernize and their states develop over time, countries like the DRC will surely find that centralized state tax collection will lead to higher revenues. But in the meantime, local elites are important allies for fragile states seeking to establish rudimentary fiscal capacity.

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