INFORMAL ELITES AS LOCAL BUREAUCRATS:

HOW WORKING AS A TAX COLLECTOR IMPROVES THE PERFORMANCE OF CITY CHIEFS IN THE D.R. CONGO

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Abstract

How does delegating state responsibilities to informal local leaders impact those leaders' legitimacy, integrity, and distribution of resources? We tackle this question by exploiting whether city chiefs in the Democratic Republic of the Congo were randomly assigned to collect property taxes in 2018. To obtain an objective measure of chief behavior, we study a government cash transfer program implemented one year later in which chiefs had discretion over the recipients of development aid. We examine how delegating tax collection to chiefs impacts their (*i*) chosen distribution of aid benefits throughout the neighborhood, (*ii*) diversion of public resources, and (*iii*) perceived legitimacy among the residents they serve. Chiefs who collected taxes allocated more program benefits to poorer households and thus made fewer inclusion and exclusion errors. They were no more or less likely to pocket benefits or allocate them to family. Across a range of measures, citizens appear to have updated positively about chiefs who collected taxes. We provide evidence that collector chiefs were more likely to target the poor because door-to-door tax collection created opportunities to learn which households were in greatest need.

1 Introduction

In settings of low state capacity, governments often delegate responsibilities of the formal state to informal local leaders like chiefs. For example, local leaders often work along the formal state to collect taxes (Balán et al., 2022), allocate land and property rights (Goldstein and Udry, 2008), resolve local disputes (Acemoglu et al., 2019), provide public goods (Acemoglu et al., 2014), and target subsidies (Basurto et al., 2020; Alatas et al., 2019). Most research focuses on the performance of local leaders in *delegated activity*, for instance in allocating fertilizer subsidies (Basurto et al., 2020) or in collecting taxes (Balán et al., 2022). Less is known about how such delegation impacts local leaders themselves — their perceived legitimacy, their integrity, their allocation of scarce resources. Understanding these impacts on local leaders is essential to assessing the full governance implications of delegation as a policy tool of low-capacity states.

By way of illustration, if we judged the colonial-era policy of delegating tax collection to village chiefs in sub-Saharan Africa only based on the revenue it raised compared to collecting tax via a formal bureaucracy, we might conclude that delegation furthered the state's objectives and was thus a policy success.[1] But this analysis would ignore the first-order impacts of working for the colonial regime on chiefs' local leadership, where by some accounts it eroded accountability and sewed 'decentralized despotism' (Mamdani, 1996).

This paper explores how collecting taxes for the formal state impacts the local leadership of city chiefs in Kananga, DRC. We exploit random variation in whether city chiefs or state agents were responsible for property tax collection in 2018 in the city of Kananga. In treated neighborhoods, the resident city chief went door to door registering properties and demanding the annual property tax payment. In control neighborhoods, state agents performed tax collection duties (as they had in previous tax campaigns).[2] In all neighborhoods, chiefs maintained their usual responsibilities — such as dispute resolution, local public good provision, and the targeting of development programs — whether or not they collected taxes. As discussed in a companion paper (Balán et al., 2022), property tax compliance is low in Kananga — only 8.8 percent of property owners paid during the 2018 tax campaign — but chief collection raised tax compliance by 3.2 percentage points, increasing revenue by 44 percent. Regardless of who collected the property tax, all revenue went to the provincial government. This tax campaign therefore generated random variation in

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[1]Herbst (2001), for example, argues that direct administration would have been too costly, and thus indirect rule via local leaders proved a 'cost-effective' strategy for Africa colonial overlords.

[2]Chiefs did not play a role in formal property tax collection in Kananga before 2018.

whether chiefs have been delegated tax collection responsibilities by the formal state. This paper studies how collecting taxes impacts chiefs themselves.

Measuring the quality of informal local leaders' leadership is a challenge. We focus on their perceived legitimacy in the neighborhoods they serve, their actual and perceived corruption levels, and their chosen allocation of scarce state benefits among neighborhood residents. This last outcome is particularly important in clientelistic settings where local elites operate within patronage networks and control access to power and resources. To obtain objective measures, we study a government cash transfer program in which chiefs had discretion over the recipients of development aid.[3] The program, administered by the Provincial Division of Social Affairs (DIVAS), involved (*i*) identifying the poorest quintile of households in each neighborhood, and (*ii*) selecting five cash transfer recipients among them during a public lottery. Beneficiaries received 10,000 Congolese Francs, equivalent to one month of household income for this target population. Because chiefs are embedded in local neighborhoods and thus possess rich information about households, the government charged them with responsibility (*i*): distributing program tickets to the poorest 20% of households in their neighborhood.[4] Thus, each chief had discretion over which households were potential beneficiaries of the cash transfer program. Embedding a measurement strategy in this program allows us to obtain real-world measures of chiefs' diversion of state resources (corruption) and their distribution of development program benefits.

Ex ante, it is not obvious how collecting taxes would impact chiefs' leadership. On one hand, working for the formal state could incentivize chiefs to prioritize the state as their principal — rather than citizens — thus making chiefs less locally accountable and consequently more extractive, corrupt, and less legitimate in the eyes of their neighborhood's residents.[5] On the other hand, collecting taxes locates chiefs at the heart of the social compact between state and citizen and could thus strengthen chiefs' sense of duty in advancing the wellbeing of their neighborhood residents. It could also provide chiefs with better information about households, or simply shift the distributional logic of chief patronage away from kinship ties and instead toward reciprocal exchange with taxpayers.

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[3]The time gap between tax collection and the cash transfer program helps to ensure that any effects we find do not reflect short-term impacts of tax collection, such as being busy with these new responsibilities in addition to normal chief duties.

[4]Chiefs often play a role in targeting government subsidies and development aid due to their local information about the needs and potential marginal treatment effects of households (Basurto et al., 2020).

[5]As noted, a large qualitative literature argues that indirect colonial rule undermined the accountability of local leaders (Mamdani, 1996). Specifically collecting taxes and administering 'native courts' for the colonial state is thought to have co-opted chiefs and sewed 'decentralized despotism' in Africa.

We find that collecting taxes causes chiefs to be more likely to target the poor with program benefits. Specifically, in neighborhoods where chiefs collected taxes, they were less likely to make errors of inclusion — giving a program ticket to households in the top 80% of the neighborhood wealth distribution — or errors of exclusion — failing to give program tickets to households in the bottom 20%. The results are similar when we define errors using a pre-registered wealth index of household assets and property characteristics or using self-reported monthly income. Citizens also perceived chiefs to have made a more pro-poor allocation of program benefits in neighborhoods where the chief collected taxes.

Although governments delegate targeting tasks to chiefs because of their local information, the discretion enjoyed by chiefs creates scope for corruption and capture. Chiefs might have allocated program tickets to family members or coethnics. They also had an opportunity to pocket program monies outright. Yet we find little evidence that tax collection impacted these measures of local capture. If anything, in neighborhoods where chiefs collected taxes, citizens viewed the chief as less likely to to target their family members (though the results are only marginally statistically significant).

We then examine how collecting taxes shaped city chiefs' local legitimacy. We estimate treatment effects on citizens' views of the chief after tax collection but before the cash transfer program. When considering an index of citizens' self-reported trust in, performance, integrity, and importance of the chief, chief tax collection causes citizens to update positively about chiefs by 0.126 standard deviations. There is no effect on citizen demand for the chief's services or the reported activity of the chief.

Why did tax collection cause city chiefs to target poorer households with cash transfer program tickets? We examine several potential mechanisms: collecting taxes might have (i) created opportunities for *learning* which households in the neighborhood are in the greatest need and thus enabled chiefs to realize a more pro-poor allocation of development program benefits; (ii) changed chiefs' *preferences* over the optimal allocation of benefits among their constituents; or (iii) stimulated bottom-up *citizen pressure*, or the anticipation thereof, which might have spurred chiefs to realize a more pro-poor allocation of program benefits in accordance of the constituents' preferences.

To investigate if learning explains our targeting results, we use a quiz-like survey module administered after the property tax campaign but before the cash transfer program. City chiefs were asked to provide the name, education level, and occupation of a set of randomly selected property owners in the neighborhood. We can validate chiefs' responses using detailed survey data about these same owners and thus score each chief's local knowledge

level. Chiefs are considerably better informed about the residents in neighborhoods where they collect taxes.[6] Moreover, chief knowledge correlates with improved targeting of the cash transfer program tickets. City chiefs thus appear to have learned about the needs of their community while collecting taxes and then used this information when allocating program tickets, resulting in fewer errors of inclusion and exclusion.

To provide evidence if collecting taxes might have changed chiefs' preferences, we examine naturally occurring (random) heterogeneity in whether chiefs collected taxes in all or only part of their jurisdiction. In half of the neighborhoods where state collectors collected taxes instead of chiefs (55 out of 110 neighborhoods), the neighborhood's chief in fact collected in another part of their jurisdiction. If learning were the only mechanism, then chiefs would be more pro-poor *only* in parts of their jurisdiction where they taxed. By contrast, if taxation changes chiefs' preferences, they would be more pro-poor throughout their entire jurisdiction. The evidence is more consistent with the former scenario: the effects on pro-poor targeting are concentrated primarily in parts of chiefs' jurisdictions where they taxed. We also find little evidence that the treatment changed chiefs' preferences, as reported in various survey questions about redistribution and chiefs' responsibilities.

Third, to test for a citizen pressure mechanism, we exploit another cross-randomized experiment designed to nudge citizens to make demands of the chief. Specifically, in randomly selected neighborhoods, households received fliers containing *information* about the cash transfer program and an encouragement to seek out the chief and ask for a share of the benefits. In another set of randomly selected neighborhoods, households received the same information plus an invitation to request a meeting to audit the chief — and the chief was informed of this possibility. We call these *information + audit* neighborhoods. We designed these treatments to generate cross-cutting random variation in the (i) actual and (ii) anticipated citizen pressure on chiefs to allocate program benefits in accordance with the preferences of the neighborhood. Households who received fliers in both treatment arms were about eight percentage points more likely to receive program tickets than uninformed citizens. The treatments thus generated the expected 'first stage' of citizen pressure. However, we find no difference in chiefs' responsiveness to this nudge in neighborhoods where

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[6]Our evidence of a learning mechanism suggests that the type of tax collection matters for the impacts on the legitimacy and accountability of local leaders. If local chiefs had not gone door-to-door collecting taxes, they would not have had opportunities to learn about the needs of residents. However, our results are likely to generalize to similar low-income countries with low-capacity states in which tax authorities delegate tax collection to local leaders because in these same settings in-person collection of taxes or distribution of tax bills are widely observed practices (Cogneau et al., 2020; Krause, 2020; Okunogbe, 2021).

they collected taxes relative to neighborhoods where they did not collect.[7] Actual or anticipated citizen pressure thus does not appear to be the mechanism behind the more pro-poor distribution of tickets realized by collector chiefs.

Finally, we rule out several other alternative explanations for the results, including the possibility that chiefs became more obedient to the formal state or improved their organizational skills due to collecting taxes. On net, the evidence is thus most consistent with a learning mechanism: walking door to door in their neighborhoods and asking citizens about their ability to pay taxes appears to have provided chiefs with better information about the economic needs of their constituents, which allowed them to better target development program benefits to the poorest households in the neighborhood.

To our knowledge, this project is the first to examine the causal effect of integrating informal leaders into the formal state on those leaders' local legitimacy and performance. Although states often delegate official responsibilities to local leaders in low-capacity states,[8] examining the impacts of this practice beyond delegated activity has typically been outside the scope of past research in this area. We find that collecting taxes causes chiefs to distribute more cash transfer program benefits to the poorest houses in their neighborhoods and to be viewed more positively by local residents.

Second, we contribute to a growing literature on local leaders in the context of targeting subsidies and antipoverty programs. Alatas et al. (2019) find little evidence of capture of welfare programs by informal local leaders in Indonesia, and Basurto et al. (2020) find that Malawian chiefs use their local information to target fertilizer subsidies to the most productive farmers. We build on this literature by providing experimental evidence of how collecting taxes — a task often performed by such leaders in similar contexts — shapes their targeting of cash transfers. The policy experiment we study thus sheds light on the links between the crucial roles played by local leaders in both the revenue and the expenditure side of the state.

Third, we contribute to the literature on taxation and accountability. A large literature argues that broad-based taxation induces more accountable governance by stimulating participation and demands among citizens for political representation and public goods

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[7]We find that chiefs were more likely to distribute program tickets to taxpayers in neighborhoods where they collected taxes, which is consistent with reciprocity as a potential mechanism. However, taxpayers have above-average wealth. Reciprocity to taxpayers thus works against the fact that collector chiefs allocated more of the program tickets to poorer households.

[8]On local elites working with the tax authorities in Africa, see, for example Mamdani (1996); Boone (2003); Iversen et al. (2006); Baldwin (2015); Sanchez de la Sierra (2019); Jibao et al. (2017); Gottlieb et al. (2020); Cogneau et al. (2020); Van den Boogaard (2021).

spending (Schumpeter, 1918; North and Weingast, 1989; Ross, 2004; Moore, 2008; Paler, 2013; Martin, 2014; Prichard, 2015a; Weigel, 2019). The standard narrative is that taxation fuels bottom-up pressure that leads governments to adopt assemblies and proto-democratic institutions. This paper provides evidence of a complementary effect of tax collection: rendering local leaders more accountable to their populations.[9]

2   Setting

The DRC is the fourth most populous country in Africa and one of the five poorest countries globally. The average monthly household income in Kananga, the capital of the Kasaï Central province and the study site, is about \$106 (Purchasing Power Parity \$168). The DRC is also a low-capacity state, ranking 188 out of 200 countries in terms of fiscal capacity.[10] The tax revenue of the Provincial Government of Kasaï Central is low, roughly \$0.30 per person per year in 2015. Perhaps unsurprisingly, public goods and services are also scarce and of low quality in Kananga. Only 5% of households have access to running water, and 14% have any source of electricity. Similarly, only 9% of the roads are paved, and less than 3% of the streets occasionally benefit from municipal garbage collection. In sum, Kananga resembles the low state capacity, low fiscal capacity, low service provision trap noted by Besley and Persson (2009).

To increase tax revenue, which primarily comes from licenses paid by firms and fees on trade and transport, the provincial government has recently kept with international best practices for local revenue mobilization (Franzsen and McCluskey, 2017) and turned to property taxation.[11] Indeed, since 2016 it has conducted a series of citywide door-to-door property tax collection campaigns (Weigel, 2019; Balán et al., 2022). The randomized policy experiment we study is embedded in the 2018 property tax campaign.

In Kananga, and in other urban areas in Francophone Africa, local order is preserved by informal leaders known as city chiefs. These chiefs are local elites whose normal responsibilities include: (i) helping mediate local disputes, and (ii) maintaining local infrastructures

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[9]This second effect is perhaps analogous to the "short route of accountability" discussed by World Bank (2004), in which the agents of the state themselves become responsive to citizens. This stands in contrast to the "long route of accountability" in which citizens demand more responsive service delivery by voting out bad politicians.

[10]We follow Besley and Persson (2014) and proxy fiscal capacity using the country's tax-GDP ratio. Data available at https://data.worldbank.org/indicator/gc.tax.totl.gd.zs

[11]This decision is consistent with advice from tax experts for a local government in a rapidly urbanizing context in which increases in property values have not been matched with parallel increases in property tax revenue needed to fund urban infrastructure (Fjeldstad et al., 2017). Property taxes are widely considered underexploited in sub-Saharan Africa (Moore et al., 2018).

through an informal institution called *salongo* in which citizens contribute labor toward public goods provision. They are nominated by elders in the neighborhood and rubber-stamped by the city government officials.[12] They have indefinite and often lifelong tenure, which sometimes passes through families.[13] City chiefs do not receive regular salaries and the main benefit of being a city chief is the status that comes with the position. City chiefs share many characteristics with customary chiefs — e.g., dispute resolution and local public goods provision — but are a distinct institution. Urban chiefs frequently play a role in tax collection or in the allocation of subsidies.[14]

2.1 2018 Property Tax Campaign

To study the effect of tax collection on chiefs' performance and local legitimacy, we leverage random variation in whether city chiefs or state agents were responsible for tax collection during the 2018 property tax campaign in Kananga. Before describing the experimental design, we outline key details and procedure of the tax campaign.[15]

Campaign Stages. — In every neighborhood of the city, the campaign was conducted in two stages. First, collectors in teams of two went door to door to conduct a property register. As in many developing countries, the government lacked a complete and up-to-date property valuation roll. Collectors first assessed whether the property was exempt.[16] They then assessed each property's tax liability based on the materials used in the construction of the main house. If the main house was built in non-durable materials, such as mudbricks, the property was assigned to the *low-value band* category (89 percent of properties) and faced an annual official tax liability of 3,000 Congolese Francs.[17] By contrast, if the main

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[12]Chiefs are thus accountable to the people in their jurisdiction and to the state. However, the rules by which they are selected and removed demonstrate that they are primarily accountable to the people living in their jurisdiction.

[13]About 19% of city chiefs reported inheriting their position from a relative.

[14]Beyond conflict resolution, urban chiefs play many complementary roles vis-à-vis the formal state (Henn, 2020). For example taxation, land titling, information campaigns, or subsidies distribution in settings like Senegal, Cote d'Ivoire, Niger, Cameroon, DRC, and elsewhere (de Russel, 1998; Nguema, 2005; de Sardan et al., 2009; De Herdt and Titeca, 2019; Cogneau et al., 2020).

[15]For more details about the stages of the 2018 property tax campaign and the random assignment of neighborhoods to tax collection by the city chief or by state agents during the tax campaign, we refer to Section I and II of Balán et al. (2022).

[16]Exempt properties represent 14 percent of properties in Kananga. They include (i) state-owned properties, (ii) churches, schools, scientific institutions, and philanthropic institutions, (iii) properties owned by the elderly (55 years old and above), (iv) properties with houses under construction.

[17]Rather than facing a property tax schedule that applies marginal tax rates to property value — common in high- and middle- income countries — properties tax in Kananga face a simplified tax instrument: a flat, fixed fee due once per year and determined by the principal's house's construction materials. Due to absent

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house was built in durable materials, such as bricks or concrete, the property was assigned to the *high-value band* category (11 percent of properties) and faced an annual tax liability of 13,200 Congolese Francs. When registering properties, collectors assigned them a unique tax ID and issued official tax notices informing the owner about the tax liability and providing other information about the property tax.

Second, after completing the neighborhood property register, the same team of two collectors returned to households for follow-up tax visits. Collectors used their handheld receipt printers to issue receipts to taxpayers. Each transaction was recorded in the device's memory and downloaded to the government database weekly when they deposited tax revenue.[18] Collectors had one month to complete work in each assigned neighborhood. They typically finished the property registration in the first few days of the month and conducted follow-up tax visits for the rest of the month. Collectors were instructed to revisit households until they paid the property tax during the assigned month. Property owners who did not pay the property tax by the end of the month in theory owed 250% of the original liability and faced the possibility of a court summon. Although such sanctions were rare among resident property owners, citizens' beliefs about enforcement were heterogeneous, potentially explaining part of the individual- and neighborhood-level variations in tax compliance.

Randomization. — The government randomly assigned 221 neighborhoods in Kananga to taxation by city chiefs ('Local') or taxation by state agents ('Central').[19] These two treatment arms only differed in the identity of the collectors charged with the property tax campaign responsibilities.[20] Collectors in both treatment arms received the same training, followed the same tax protocol, and used the same technology (handheld receipt printers).

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or incomplete property valuation rolls, simplified property tax schemes like the one used in Kananga have been frequently used in developing countries, including India, Tanzania, Sierra Leone, Liberia, Malawi (Franzsen and McCluskey, 2017).

[18] Collectors were required to account for discrepancies between the tax revenue and the receipt data (rare in practice).

[19] As described in further details in (Balán et al., 2022) there are 356 neighborhoods in the city of Kananga. We only include in our analysis the 111 neighborhoods assigned to Local tax collection and the 110 neighborhoods assigned to Central tax collection. We do not include the neighborhoods assigned to the hybrid 'Central + Local Information' (80 neighborhoods) or 'CentralXLocal' (50 neighborhoods) treatments. Similarly we omit from our analysis the 5 'Control' neighborhoods. These additional treatment arms are described in details in Section II.A of Balán et al. (2022)

[20] Before the campaign, collectors received training by the provincial tax ministry. The sessions were taught by the ministry's chief inspectors and concerned the rules and protocols of property taxation in Kananga, including tax amounts, exemptions, fines for late payments, and the use of handheld receipt printers.

They also received the same compensation for their work as tax collectors.[21] In the 111 neighborhoods assigned to the Local treatment arm, city chiefs were charged with all campaign responsibilities. They worked on each step of the property tax campaign with a local assistant. In the 110 neighborhoods assigned to the Central treatment arm, agents of the provincial tax ministry were charged with campaign responsibilities. State collectors were unsalaried contractors who frequently work for the tax ministry and other branches of the provincial government. They also worked in teams of two so that team size is constant across treatments.

2.2   2019 Cash Transfer Program

To obtain an objective measure of chiefs' local performance after the tax campaign, we study a government cash transfer program in which chiefs have discretion over the list of beneficiaries.

Division of Social Affairs. — We study a program designed by the Provincial Division of Social Affairs (*Division des Affaires Sociales* or DIVAS). The DIVAS is the local branch of the provincial government in charge of administering programs to help vulnerable households. These programs typically involve providing financial or nutritional support to the elderly, orphans, and individuals with disabilities. Following the 2016-2019 Kamuina Nsapu rebellion, some DIVAS programs also focused on the social and economic reintegration of children who had enrolled and fought in the militia.

Cash Transfer Program. — The program we study is a cash transfer program designed by the DIVAS between June and December 2019, i.e., several months after the government completed the 2018 property tax campaign. DIVAS agents and city chiefs implemented the program in all the neighborhoods of Kananga in four steps. First, city chiefs received training from the DIVAS. The sessions, taught by two DIVAS agents, concerned the rules and protocols of the cash transfer program.

Second, city chiefs were tasked with the distribution of the program tickets. They received program tickets corresponding to 20% of the households in their neighborhood (on average, there were 100 households and 23 program tickets per neighborhood). City chiefs were allowed to give up to three tickets per household and had ten days to distribute all the program tickets. When they issued a program ticket, they gave half of the ticket slip to the household head and kept the other half where they wrote the household head's name,

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[21]Collectors across all treatment arms received a piece-rate wage with two components. First, they received 30 Congolese francs (CF) per registered property. Second, they received a piece rate compensation for tax collections equal to 25 percent of the revenue deposited.

address, and property ID code. DIVAS delegated this task to chiefs because of their local knowledge about households' needs. Indeed, for this reason, local elites are often tasked with targeting subsidies and welfare programs (Baldwin, 2015; Basurto et al., 2020).

Third, in the presence of chiefs and other observers, DIVAS agents held a public lottery and selected five cash transfer recipients. Finally, the beneficiaries received the cash transfers. The chief and a DIVAS agent together distributed the cash transfer to the first two beneficiaries (in alphabetical order). The city chief then delivered the remaining three cash transfers without being accompanied by a DIVAS agent.

2.3 Balance

Balán et al. (2022) provides evidence that the randomization of chief taxation duties at the neighborhood level achieved balance along a range of property owner and neighborhood characteristics (Tables 3, A2, and A3). We additionally assess balance for characteristics relevant to the targeting of program benefits by chiefs. These measures were collected during the households surveys conducted after the conclusion of the tax campaign that Balán et al. (2022) study, as described in Section 3.2, and include wall quality, roof quality, erosion threat, accessibility to the neighborhood's main avenue. These variables are included in the pre-specified wealth index that provides our primary measure for assessing differences in targeting across neighborhoods where chiefs collected taxes versus did not. We also consider asset measures in whether the household's compound has a fence, electricity access, and owns a vehicle, which are included in a broader index of wealth and assets that we use to examine differences in targeting in Table A3. Table A1 summarizes balance tests for these measures. We do not find that any of these characteristics differ systematically between chief collection and no collection neighborhoods. We test the omnibus null hypothesis that the treatment effects for the variables are all zero using parametric F-tests for a bilateral treatment comparison and fail to reject the null ($F = 0.612, p = 0.767$).

3 Data

We use administrative and survey data from the 2018 property tax campaign and the 2019 cash transfer program as summarized in Table 1.

3.1 Tax Campaign Data

Administrative data related to the 2018 property tax campaign include information on the assignment of each neighborhood to tax collection by city chiefs (Local tax collec-

tion neighborhoods) or state agents (Central tax collection neighborhoods), as described in Section 2. Additionally, the administrative data includes property registration and tax payment information from the government's tax database for the complete set of registered properties by unique tax IDs. Household survey data includes data from baseline, midline, and endline surveys that collected pre-tax collection data on household and neighborhood characteristics (baseline), post-tax collection experiences with tax collection (midline), and post-tax collection attitudes about chiefs, the provincial government, and tax collection (endline). Balán et al. (2022) describe all these data sources in detail (Section III).

3.2 Cash Transfer Program Data

Administrative Data. Chiefs submitted records of the tax IDs of households to whom they allocated program tickets, covering 4,401 households in the primary sample of Central and Local tax collection neighborhoods. We construct errors of exclusion and inclusion — our main outcomes — by matching ticket records to registration and survey data using tax IDs and observing ticket allocation across household measures of economic well-being. We assess nepotism by comparing household characteristics and connections to the chief among ticket recipients. Cash transfer recipients selected during neighborhood lottery drawings were sampled in the household survey to collect measures related to prize receipt and to evaluate corruption and diversion by chiefs.

Household surveys. After program ticket distribution concluded in each neighborhood, enumerators administered surveys to 6,267 households in the Central and Local tax collection neighborhoods — approximately 28 per neighborhood — from June to December in 2010. In each neighborhood, all households allocated program tickets according to the data submitted by chiefs — approximately 23 households on average — were sampled. In addition, 10 households per neighborhood were randomly sampled from the property register. It was possible, first, for the random sample to overlap with the ticket recipient sample and, second, for enumerators to not succeed in surveying those sampled, thus the average number of surveys completed in a neighborhood does not add up to 33. When assessing errors of exclusion and inclusion by wealth,[22] we weight observations by sampling weights that correspond to the ratio of the total number of households (according to administrative data) to the number of completed surveys in a neighborhood — separately

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[22]For errors by income, consumption, liquidity, and assets, we do not apply sampling weights. The data for these measures is drawn from surveys conducted before the cash transfer program, in which ticket recipients were not more likely to be sampled (see Balán et al. (2022) for details).

for ticket recipients and non-recipients.[23] The survey covered the allocation of tickets and cash transfers, chiefs, and the provincial government. Of the approximately 28 households successfully surveyed in each neighborhood, a subset were selected to complete a longer survey collecting more detailed measures. These households included 5 program ticket recipient households, lottery winners, and the 10 households randomly sampled for surveying. An average of 12 households per neighborhood completed the long survey.

4   Estimation

We use ordinary least squares (OLS) to compare neighborhoods in which local chiefs were responsible for tax collection in the 2018 tax campaign to neighborhoods in which local chiefs were not responsible for collection:

$$y_{ijk} = \beta_0 + \beta ChiefTaxed_{jk} + \mathbf{X}_{ijk}\Gamma + \alpha_k + \varepsilon_{ijk} \tag{1}$$

where i indexes individuals, j neighborhoods, and k randomization strata. Standard errors are clustered at the neighborhood level. The term y_{ijk} is the outcome of interest, α_k are stratum fixed effects, and $\mathbf{X}_{ijk}$ is a covariate vector.[24] The $ChiefTaxed_{jk}$ term is an indicator denoting neighborhoods where chiefs were in charge of tax collection themselves compared to neighborhoods where chiefs were not involved tax collection. In the parlance of Balán et al. (2022), this definition compares neighborhoods in the Local treatment arm to those in the Central treatment arm, and randomization strata in equation (1) correspond to those used to assign these treatments.

In assessing mechanisms, we also examine heterogeneity by whether the cash transfer program was administered in a neighborhood by a chief who *collected taxes in the the same neighborhood*. In the 2018 tax campaign, tax collection duties were randomly assigned to local chiefs at the neighborhood level. Chief jurisdictions, however, include more than one neighborhood on average. We exploit the fact that chiefs charged with tax collection in at least one neighborhood within their jurisdictions then administered the cash transfer in all neighborhoods under their purview. Thus, there exists a set of neighborhoods in which the local chief did not directly collect taxes but was responsible for collection *elsewhere*, in another neighborhood within his or her jurisdiction. Engagement in

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[23]The effects on errors of exclusion and exclusion by wealth we estimate are not sensitive to sample weighting (Tables A6 and A7), though the relative magnitudes understandably differ.

[24]In our preferred specification, we include no covariates — other than stratum fixed effects — as controls but include them in robustness checks.

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tax collection may globally affect a chief's performance in distributing cash transfers, and we therefore additionally compare neighborhoods where chiefs directly collected taxes in 2018 ($ChiefTaxedHere_{jk}$) and neighborhoods where chiefs did not directly collect taxes but did so elsewhere ($ChiefTaxedElsewhere_{jk}$):

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\begin{aligned}
y_{ijk} = {} & \beta_0 + \beta_1 ChiefTaxedHere_{jk} + \beta_2 ChiefTaxedElsewhere_{jk} \\
& + \mathbf{X}_{ijk}\Gamma + \alpha_k + \varepsilon_{ijk}
\end{aligned}
\tag{2}
$$

where the excluded category is neighborhoods where the local chief did not engage in the tax campaign anywhere. The $ChiefTaxedHere_{jk}$ term is identical to the $ChiefTaxed_{jk}$ term in equation (1). The inclusion of the $ChiefTaxedElsewhere_{jk}$ term permits a separate consideration of treatment impacts among neighborhoods indirectly exposed to the engagement of chiefs in tax collection and amounts to removing this set of neighborhoods from the excluded category in equation (1).[25]

5   Results

5.1   Effects on Targeting of Cash Transfers by Wealth and Income

We first compare the targeting of program tickets in neighborhoods where chiefs collected taxes and those where they did not by estimating equation (1) with OLS. Chiefs were instructed to distribute program tickets to the 20% of households with the greatest economic need in each neighborhood within their jurisdiction. We use two primary measures to estimate economic need across households: (*i*) a wealth index constructed from observable

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[25]The definition of where an individual chief collected taxes is imperfect because of chief turnover. While the $ChiefTaxedElsewhere_{jk}$ term captures whether a given chief taxed in another neighborhood within his or her jurisdiction, the $ChiefTaxedHere_{jk}$ term captures both whether the same chief collected in a particular neighborhoods or another chief — who was subsequently replaced — did so. In the majority of cases, the $ChiefTaxedHere_{jk}$ term captures the former case, but for chiefs where there is replacement it captures the latter.

household attributes,[26] and (*ii*) a survey measure of monthly household income.[27,28] We then rank households in each neighborhood by each measure separately and follow Alatas et al. (2012) in defining at the household level an error of exclusion as not distributing a program ticket to a household belonging to the bottom quintile rank. An error or inclusion is defined as distributing a ticket to a household with a rank above the bottom quintile.

Table 2 summarizes the results for the wealth index measure using sample-weighted data.[29] Column 1 provides the estimated difference in total error rates — pooling errors of inclusion and exclusion — among all households, column 2 contains the estimate for errors of exclusion within the bottom wealth quintile, and column 3 the estimate for errors of inclusion within the top four wealth quintiles. Chiefs who collected taxes in a neighborhood commit any error in 25.2% of instances compared to a rate of 31.7% among chiefs who did not collect taxes in a neighborhood, a decrease of 20.5% (Panel A, column 1). This estimated difference in pooled error rates among all households conceals variation in the probability of committing errors of each type. Chief collectors in a neighborhood committed 6.2% (5.6 percentage points) fewer errors of exclusion and 10.4% (1.7 percentage points) fewer errors of inclusion (Panel A, columns 2 and 3).[30,31]

The differences in error rates we estimate are of comparable magnitudes to those es-

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[26]The index is a standardized index of the quality of the walls of structures on the property (whether walls were made of mudbrick vs. cement and in good condition), the quality of the roof (whether roof is made of thatch, mat, bamboo, or palm fronds vs. concrete, tiles, or sheet iron), whether a household compound is threatened by erosion, the quality of the road in front of the property, and how accessible the compound is from the neighborhood's main avenue. In Bergeron et al. (2020) we show that these attributes are positively correlated with property values, the primary asset of households in Kananga.

[27]We consider alternative measures including consumption, liquidity, and assets in Table A5.

[28]An issue with using these measures to estimate errors of exclusion and inclusion among all households is the presence of missing data. However, conditional on missingness being uncorrelated with treatment, this should, given survey procedures were identical across neighborhoods, lead to underestimating error rates rather than introducing systematic bias.

[29]Tables A6 and A7 display results using unweighted data.

[30]The high average error rates are consistent with evidence on the accuracy of targeting methods in Alatas et al. (2016), which finds, for example, that 84% of the poorest households were still excluded by the most successful targeting method they study (self-targeting through ordeals).

[31]Table A2 estimates error rates by the components of the wealth index. Error rates estimated for individual household quality characteristics are consistently negative (with the exception of a small positive difference in exclusion error when considering wall quality alone) but in most cases not statistically significant on their own, suggesting the index measure captures a deeper measure of wealth measured in house quality by combining multiple attributes. Table A1 provides balance tests confirming that households do not differ on average across treatment status along each individual dimension of house quality and assets. We consider a more comprehensive measure of wealth that includes — in addition to the measures in the pre-specified index — assets as captured by indicators for possession of a fence around one's compound, access to electricity, and ownership of a vehicle in Table A3 and find similar results.

timated in other settings but derived from comparisons of alternative methods of targeting public transfers. Alatas et al. (2012) detect 2.9 to 3.1 percentage points higher error rates for hybrid and community-driven targeting of cash transfer relative to a simple proxy means test in Indonesia. Basurto et al. (2020) estimate error rates that are 3.5 to 5.5 percentage points higher when chiefs allocate agricultural input subsidies to poor households compared to the allocation based on a proxy means test in rural Malawi.[32] The similarity in effect sizes suggests that engaging in tax collection offers chiefs in our setting an innovation for improving targeting that is on par with the improvements achieved through targeting transfers based on observable assets (proxy means test) relative to relying on the information of community members or local elites.

To identify the part of the wealth distribution from which the decrease in error rates originate, we consider in columns 4–7 errors by finer increments of wealth. Among poor households (bottom quintile of wealth), we define "very poor" households as those belonging to the bottom decile and "near poor" as those with wealth between the 10th and 20th percentile. Among non-poor households, "middle" wealth households are those with wealth between the 20th and 60th percentile, and "rich" households are those with wealth above the 60th percentile. We then estimate the corresponding errors of exclusion (columns 4 and 5) and inclusion (columns 6 and 7) within each set.[33] Though the difference in error rates is negative across wealth status, we only detect statistically significant differences of 8.2% (7.4 percentage points) among the least wealthy households and of 18.7% (3.1 percentage points) among the wealthiest households (Panel A, columns 4 and 7). We also consider differences in the average wealth of program ticket recipients, which provides a less precise measure of targeting by wealth in that it does not capture the share of households allocated tickets below the poverty threshold determined by the government (i.e., belonging to the bottom quintile). Reassuringly, we find that the average wealth of program ticket recipients is lower in neighborhoods where chiefs collected taxes, though this difference is not significant (Panel A, column 8). Tests for equality of distributions confirms the presence of significant differences in the distribution of the wealth index across treatment groups using both Kolmogorov-Smirnov and Wilcoxon rank sum tests (Table A4,

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[32]In both cases, the differences in error rates translate into higher relative changes in the error rate compared to our setting given the probability of any error in those settings is lower.

[33]The share of total households in each wealth status category does not correspond precisely to the percentile range because households are bunched on certain values of the wealth index measure. Therefore, there are small differences in the correspondence between the percentile thresholds for the wealth status categories in columns 4–7 and the share of households each represents: e.g., the very poor category (bottom decile) includes 15% of all households rather than 10% (Table 2, Panel A, column 1).

rows 1 and 5).[34] These patterns suggest that the lower error of inclusion rate among chief collectors in neighborhoods derives from a reallocation of program tickets from the wealthiest households toward less wealthy households compared to chiefs who did not collect in a neighborhood.[35]

Do these improvements in targeting by chief collectors derive from location-specific factors or general improvements in targeting ability gained through their experience in tax collection? On the one hand, the lower error rate in neighborhoods where chiefs collected taxes may result from mechanisms tied to a specific neighborhood — such as improvements in information about the wealth of households or greater reciprocity with households generated by a chief taxing them directly. On the other hand, tax collection may offer chiefs more general tools or skills that would improve their ability to target poorer households even in areas where they did not directly collect taxes. To assess this question, we estimate equation (2) using OLS to compare neighborhoods where chiefs taxed directly with neighborhoods where the chief taxed elsewhere and with neighborhoods where the chief had no engagement with taxation in the neighborhood or elsewhere. Panel B of Table 2 summarizes the results. We find that the improvements in targeting are driven by neighborhoods where chiefs taxed *directly*. The estimated effects of chiefs taxing elsewhere are in most cases negative, and of comparable magnitude to the average differences estimated in Panel A for the wealthiest households, but we detect no statistically significant differences. We discuss the mechanisms potentially consistent with these results in Section 6.

We assess the validity of the wealth index measure for identifying households with the greatest economic need in Table 3 by considering differences in error rates by monthly household income. The effects on targeting measured by income mirror those measured by wealth in Table 2.[36] Chiefs who collected taxes in a neighborhood commit any error in 13.1% (4.4 percentage point) fewer instances when the criteria for economic need is income rather than wealth compared to chiefs who did not collect taxes in a neighborhood (Panel A, column 1). Errors of exclusion and inclusion are lower in chief collection neighborhoods, though this difference is only significant ($p = 0.052$) in the case of exclusion (column 2) and not significant at conventional levels ($p = 0.121$) for inclusion (column

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[34]Figure A1 displays the distributions of the wealth index by treatment (Panel A) and treatment and where chiefs directly taxed (Panel B).

[35]In contrast, Alatas et al. (2012) find that differences in targeting accuracy between hybrid and community-based methods, relative to proxy means tests, originate from the the middle of the wealth distribution.

[36]In Table A5 we consider errors by measures of consumption, liquidity, and an index of assets drawn from the endline household survey. Likely due to the much smaller sample sizes available for these measures we detect no differences in error rates by whether the chief was responsible for tax collection in a neighborhood.

3). We again observe that improvements in targeting by income are concentrated among the richest (above 60th percentile) households (column 7).[37] Effects on error rates using income by the location of chief collection — in a neighborhood versus elsewhere — are similar to errors using wealth, with larger negative and significant differences in errors concentrated in neighborhoods where chiefs taxed directly (Panel B). However, we find that chiefs who taxed elsewhere exhibit lower inclusion error rates (column 3), particularly for the highest income households (column 7), compared to neighborhoods where the chief had no engagement with tax collection anywhere. The effect for neighborhoods where the chief collected elsewhere are almost as large as the effect estimated for neighborhoods where chiefs taxed directly.[38]

5.2 Effects on Nepotism, Corruption, and Reciprocity

One concern is that empowering local chiefs to collect taxes may have made chiefs feel that they earned the right to use state resources for their benefit or the benefit of their kin.[39] Additionally, tax collection might have incentivized chiefs to reciprocate to taxpayers — who are typically characterized by higher income and wealth than non-payers — by allocating them program tickets. These effects could have partly offset or worked hand in hand with the assignment of program tickets to poorer households (e.g., allocating program tickets to poorer coethnics or kins while excluding all non-coethnics or non-kins). We assess these hypotheses in this section.

Nepotism. — First we examine whether collecting taxes made chiefs more likely to favor certain groups (e.g., their kins, coethnics, etc.) when allocating program tickets. We first rely on citizens' perceptions about chiefs' allocation of program tickets to specific groups measured at endline. Table 4, columns 1–3, report the results. Citizens in neighborhoods where chiefs collected tax report that the chief was 9.9% (4.4 percentage points) less likely to allocate program tickets to family members relative to those in neighborhoods where the chief did not collect (Panel A, column 1) ($p = 0.067$).[40] However, we do not

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[37]Figure A2 displays the distributions of monthly household income by treatment groups and where chiefs directly taxed. Tests for equality of distributions, however, do not detect significant differences, likely due to smaller sample size relative to the wealth index measure (Table A4).

[38]We assess the robustness of our results by including a neighborhood leave-on-out mean when estimating the impacts of chief taxation on targeting by both wealth (Table A8) and income (Table A9).

[39]Concerns about corruption and mismanagement are noted in the historical literature examining the delegation of tax responsibilities to local actors (Kiser, 1994; Mamdani, 1996).

[40]However, the perception among citizens that collector chiefs targeted family members less becomes nonsignificant when considering where chiefs collected taxes — in a neighborhood or elsewhere (Panel B, column 1).

find significant differences in perceived favoritism when measured using a broader index of connections to the chief (Panel A, column 2).[41] In line with the targeting effects, citizens in chief collection neighborhoods perceive local chiefs as being 11% (5.7 percentage points) more likely to favor the poor in allocating program tickets (Panel A, column 3) ($p = 0.023$). Heterogeneity by where chiefs collected taxes — directly in a neighborhood or elsewhere — indicates that the higher perceptions for collector chiefs that they allocated program tickets to poorer households are driven by neighborhoods where they collected (Panel B, column 3). Chiefs who collected in the neighborhood or in another part of their jurisdiction appear to target non-family connections less than non-collector chiefs, although these differences are only marginally significant (Panel B, column 2). We then validate citizens' perceptions by assessing differences in the likelihood that chiefs allocated program tickets to family members and other connections (tribe, church, and political party). We do so by merging survey data on household links to the chief with the list of program ticket recipients. Table 4, columns 4–5, reports the results. Chiefs in neighborhoods they taxed directly were not significantly less likely to distribute program tickets to either connected group, suggesting that citizens' perceptions are not entirely accurate in this setting.

Corruption. — We next explore whether collecting taxes made chiefs more likely to misuse or divert program resources. Table 5 summarizes tests of several measures of corruption and diversion. First, citizens were asked at endline to report whether the chief requested something in exchange for receiving a program ticket. The reported rate of such a request by the chief is only 3% and does not differ by whether the chief collected in the neighborhood (Panel A, column 1).[42] The last step of the cash transfer program in the neighborhood opened an opportunity for chiefs to pocket state monies. Chiefs were responsible for delivering three of the five transfers to households, providing an opportunity to evaluate their integrity. Lottery winners were asked at endline whether the transfers they received were in an envelope (as a measure of tampering: DIVAS gave transfers to chiefs in sealed envelopes), whether they received a transfer at all, and the amount of the cash transfer missing, defined as the difference between the official amount and the amount received. We find small and statistically insignificant differences across these measures (Table 5,

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[41]The index combines measures of citizens belonging to the same tribe as the chief, attending the same church, being a taxpayer, and belonging to the same political party/coalition.

[42]Though citizens may have incentives to underreport the prevalence of this practice — e.g., due to collusion in sharing potential cash transfers with the chief — the share of households reporting such requests by chiefs is nearly identical to the share reporting paying a bribe to chief collectors (3.2%) during the tax campaign Balán et al. (2022). This suggests — at least in a context where chiefs and citizens are aware a government program is being monitored by enumerators — that scope for engaging in corruption is low.

columns 2–4).[43] In sum, engaging in tax collection does not appear to make chiefs more nepotistic or corrupt in their allocations of program tickets and cash transfers.

Reciprocity. — Finally, we examine whether collecting taxes made chiefs more likely to distribute program tickets to taxpayers due to reciprocal motives. Columns 1 and 4 of Table 6 analyze the allocation of program tickets by chief treatment status and household tax compliance status and show that chiefs who collected taxes were more likely to allocate program tickets to taxpayers.[44] However, taxpayers typically have higher income and wealth than non-payers and allocating them more program tickets could go against the antipoverty objective of the cash transfer program. Columns 2–3 and 5–6 of Table 6 analyze the allocation of program tickets by chief treatment status and household tax compliance status separately for households in the bottom 20% (Columns 2 and 5) and top 80% (Columns 3 and 6) of the wealth distribution. Taxpayers in the top 80% of the wealth distribution were more likely to receive program tickets when the chief collected taxes, but the same did not apply to taxpayers in the bottom 20% of the wealth distribution.[45] Reciprocity toward taxpayers is thus unlikely to explain the allocation of program tickets to poorer households by chiefs who collected taxes. If anything, it might have marginally offset the pro-poor targeting impact of tax collection by chiefs.

5.3 Effects on Attitudes Toward Chiefs

We also consider how collecting taxes shaped city chiefs' local legitimacy by estimating treatment effects on citizens' attitudes about the chief and their performance in the cash transfer program. Table 7 reports comparisons of views of the chief, citizen demands for chief services, and the activity level of the chief as reported by neighborhood residents.[46] Tax collection appears to lead citizens to update positively about chiefs according to an in-

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[43]The report rate of transfers not being received is high (22%), yet this may reflect several factors: (*i*) chiefs being delayed in delivering transfers, (*ii*) individuals concealing receipt of transfers from others in the households, and (*iii*) over-reporting of non-receipt by households for strategic purposes (e.g., in the hopes that the government or our research organization would replenish the transfer). Therefore, we view this estimate as an upper bound on the amount of diversion. We also see no reason why survey response bias would differ across treatments.

[44]It is important to note that the results presented in Table 6 are descriptive correlations since tax compliance is endogenous to tax collection by neighborhood chiefs.

[45]For nonpayers, Table 6 shows that chiefs who collected taxes were more likely to allocate program tickets to households in the bottom 20% and less likely to allocate them to households in the top 80% of the wealth distribution, confirming the results presented in Section 5.1.

[46]We assess differences only by estimating equation (1) rather than also examining heterogeneity by where chiefs collected taxes as the outcomes in Table 7 focus on citizen perceptions of broad measures of chief quality and therefore relate to how citizens' attitudes and perceptions might have been impacted by their local chief taxing them directly.

dex of self-reported trust and the rated performance, integrity, and importance of the chief (Panel A, row 1). We do not find, however, that citizens demand more chief services or perceive the chief as engaging in more activities if the chief collected taxes in a neighborhood (Panels B and C).[47] In sum, tax collection by local chiefs results in more positive views of the local chiefs and a greater perception that they target the poor when allocating program tickets.

6 Mechanisms

Why did tax collection cause city chiefs to target poorer households with program tickets? We examine three key mechanisms: (i) going door to door collecting taxes could have created opportunities for *learning* about the neediest households in the neighborhood; (ii) collecting taxes was a positive shock to the responsibilities of chiefs that may strengthen the social contract they have with the local population and thus amplify chiefs' *preferences* over the optimal allocation of program benefits in the neighborhood; or (iii) tax collection by the chief might have activated bottom-up *citizen pressure*, or the threat thereof, which might have led the the chief to distribute more program tickets to the poorest households in anticipation of the constituents' preferences. The evidence is most consistent with a learning-based mechanism.

6.1 Learning

A first explanation is that collecting taxes could have created opportunities for learning and thus increased city chiefs' knowledge of household needs in their jurisdiction, which improved their ability to target the poorest households when allocating program tickets.

To investigate changes in chiefs' knowledge as a result of tax collection, we use a quiz-like survey module administered after the 2018 property tax campaign and before the 2019 cash transfer program started. City chiefs were shown photos of 12 randomly selected property owners in the neighborhood and asked to provide their names, education level, and occupation. We know the correct answers to these questions from household surveys and can therefore measure chiefs' knowledge of the name, education level, and occupation of property owners in their jurisdiction by combining both sources of information. We also use this information to construct an index of chiefs' knowledge of the education level and occupation of property owners in their jurisdiction. Table 8, columns 1-3 reports the

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[47]Estimated effects on demands for services and chief activities are generally negative, which is potentially consistent with tax duties crowding out — at least in the short run — chiefs' other responsibilities. However, the estimated coefficients are statistically insignificant individually and when combined into index form.

effect of tax collection on city chiefs' knowledge of respondents' name, education level, and occupation. Column 4 present results for the knowledge index and shows that city chiefs scored 0.134 standard deviations higher on this knowledge index in neighborhoods where they collected taxes than in neighborhoods where they did not collect. These results suggest that collecting taxes door-to-door resulted in city chiefs learning about the needs of households in their jurisdiction.[48]

The information acquired by the city chiefs during door-to-door tax collection might have improved their ability to identify the poorest households in the neighborhood when distributing program tickets. We test this hypothesis by investigating whether more informed chiefs make fewer errors — pooling errors of inclusion and exclusion — when allocating the program tickets. Figure 1 and Table A10 present the results. Using the quiz-like survey module, we find that chiefs who are more informed about the name, education level, and occupation of property owners in their jurisdiction make fewer errors when allocating the program tickets. The results are especially significant when using an index of chief's knowledge along these three dimensions (Panel D of Figure 1 and Column 4 of Table A10). Taken together, these results suggest that improvements in knowledge can meaningfully improve targeting and that going door-to-door collecting taxes caused city chiefs to learn about the earning potential and needs of households in their jurisdiction, which improved their ability to identify the poorest households in the neighborhood when allocating program tickets.

6.2 Preferences

A second explanation is that collecting taxes changed chiefs preferences over the optimal allocation of program benefits in the neighborhood. Their preferences could have changed because taxation confronted chiefs with the poverty of their constituents, or because it was a shock to the social compact that strengthened chiefs' feeling of responsibility in promoting the welfare of their constituents.

As a test, we compare the allocation of program tickets in neighborhoods where the city chiefs taxed and in neighborhoods where the city chief did not collect taxes but was involved in tax collection *elsewhere*, i.e., in another part of their jurisdiction. We described

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[48]Theses results also suggests that the way the property tax is collected also matters for the impacts of tax collection on local leader's subsequent performance. For example if local chiefs had not gone door-to-door collecting taxes, they would not have had opportunities to learn about the needs of residents. However, our results are likely to generalize to other low-income countries with low-capacity states, where tax authorities delegate tax collection to local leaders and in-person tax collection is prevalent (Cogneau et al., 2020; Krause, 2020; Okunogbe, 2021).

this approach and the corresponding empirical strategy in Section 4. If learning is the only mechanism, then chiefs would allocate more program tickets to poor households *only* in parts of their jurisdiction where they taxed. By contrast, if taxation changes chiefs' preferences, they would distribute program tickets to poorer households throughout their entire jurisdiction, including where they did not tax. Consistent with the learning effects described above, we find that changes in the allocation of program tickets were more pronounced in neighborhoods where the chief was in charge of tax collection both when defining the error rate using the wealth index (Table 2, Panel B) or household income (Table 3, Panel B). However, we also find suggestive evidence of changes in the allocation of program tickets in neighborhoods where the chief did not tax if the chief taxed *elsewhere*. We detect significant differences only when considering targeting of the wealthiest households when defining error rates using the wealth index (Table 2, Panel B, column 7). However, effects are statistically indistinguishable from the effect of taxation by the city chief in the neighborhood across all wealth categories excluding the poorest.[49] By contrast, we find significant effects when defining error rates using monthly household income instead (Table 3, Panel B) and cannot reject equality of effects for most comparisons. Our interpretation of the difference is that the wealth index measures observable indicators of need and thus maps to what chiefs could have learned during taxation, while reported income is harder to learn and also much less predictable. However, because it is correlated with wealth, we still observe suggestive increases in pro-poor targeting when using the income need measure.

As a further test, we examine if citizens perceive chiefs to be working harder, or to be engaged in other types of activities in their neighborhoods. We don't find evidence that this is the case. City chiefs are not perceived by citizens as more active in terms of organizing salongo (informal taxation), mediating conflicts, providing political representation, or providing personal favors in neighborhoods where they collected the property tax (Table 7, Panel C). City chiefs also do not feel responsible for more tasks as a result of tax collection (Table 9). Lastly, we find no effect of tax collection by city chiefs on the frequency or duration of salongo (Table A11, Panel A), which is one of the chief's primary responsibilities in Kananga. We also don't find evidence that tax collection resulted in changes in the targeting of salongo participants by the city chiefs since we find no effect on how households view salongo (Table A11, Panel B) or on the perceived incidence (Table A11, Panel C) and enforcement (Table A11, Panel D) of salongo. All in all, we find mixed evidence

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[49]The last row of Table 2 and 3 report the p-value of tests of equality of coefficients for neighborhoods where chief taxed directly and neighborhoods where the chief did not tax here but taxed elsewhere.

supporting public-spiritedness as a mechanism. An increase in public-spiritedness might have partly explained that tax collection caused city chiefs to allocate program tickets to poorer households. Still, the increase in public-spiritedness did not have consequences beyond the allocation of program tickets. In particular, it did not increase the frequency of other chief activities such as informal taxation.

6.3 Citizen Pressure

A third family of explanations for collector chiefs' improved targeting lies in greater responsiveness to citizen demands and pressures. This mechanism could operate through several channels. On the one hand, chiefs that assume a role in the collection of provincial revenues may be more receptive to individual demands for access to redistributive benefits among deserving citizens. On the other hand, having stepped into this public role, such chiefs may anticipate greater efforts to hold them accountable in how they target public benefits and proactively adjust their allocation of benefits. To explore these possibilities we partnered with DIVAS to cross-randomize two interventions aimed at increasing citizen demands and accountability pressures before chiefs began distributing program tickets.

Design of Cross-Randomized Experiment. In the first sub-treatment arm (Information), before the program ticket distribution, 20% of households in a neighborhood were randomly selected to receive information about the cash transfer program. During door-to-door visits, enumerators distributed fliers containing information about (*i*) the goal of the cash transfer program, (*ii*) the number of prize recipients in the neighborhood, (*iii*) the name of the chief responsible for distributing program tickets, (*iv*) the time period of ticket distribution (see Figure A3 for an example). Fliers also encouraged citizens to talk to the chiefs by noting that citizens could "see the chief for more information". This treatment was therefore intended to generate random variation within and across neighborhoods in whether citizens were informed about and prompted to engage with the chief regarding the cash transfer program.[50] By making individual demands more likely, the Information arm aimed at testing whether collector chiefs were more responsive to them.

A second sub-treatment arm (Information & Audit) aimed at increasing citizens' collective action capacity and their ability to exert pressure on the chief. In these neighborhoods, before the program ticket distribution, 20% of households were randomly selected to re-

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[50]Although many citizens likely had some knowledge about the lottery, they were unlikely to know precisely who was responsible for distributing tickets and when distributions was scheduled to occur in a particular neighborhood.

ceive the same information fliers as in the Information arm plus an audit meeting request form. The audit form informed citizens that they could request an audit meeting to investigate whether the chief implemented the cash transfer program fairly in their neighborhood (see Figure A4 for an example).[51] Forms specified that the audit meetings would be conducted by well-known and respected local civil society organizations: RIAC (the Network for Transparency and Anti-corruption), which specializes in promoting transparency and fighting corruption, and SOCICO (the Civil Society of Congo), which focused on government accountability in the areas of violence, conflict, and elections.[52] Citizens were informed that RIAC and SOCICO would organize audit meetings in the neighborhoods that submitted the most audit request forms as a share of the neighborhood population. Importantly, whether enough audit request forms were submitted to result in an audit meeting was not revealed to the chief until after the cash transfer recipients were selected by public lottery and received their transfer.[53] By raising the probability of an audit meeting, the Audit arm aimed at testing whether collector chiefs were more responsive to threats of citizen collective action.

A third arm involved no additional interventions (Control). All the sub-treatment arms were randomly assigned at the level of the *chief*, meaning all neighborhoods within a chief's domain received the same intervention — Information, Information & Audit, or Control.[54] Table A12 shows the experimental design. The randomization achieved balance across property owner characteristics (Table A14, Panel A), property characteristics (Table A14, Panel B), neighborhood characteristics (Table A14, Panel C) and the property characteristics used to construct the wealth index (Table A15).[55]

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[51] Citizens could separately request audits of the chief or the DIVAS, the two key actors involved in the cash transfer program. They received two different forms and to request an audit they had to drop each form in a different drop box located in a different location in the city center.

[52] Such community meetings are common in the DRC. Although there are few formal avenues of engagement with the state, local meetings at the neighborhood level were the principal form of political expression discussed by participants in focus group meetings held in Kananga in 2016.

[53] After the conclusion of the program, RIAC and SOCICO conducted audit meetings in the 10 neighborhoods that submitted the greatest number of audit meeting request forms as a share of the population. The meetings brought together citizens, the chief, and the DIVAS representatives to discuss the cash transfer program: how the chief decided who to give program tickets to, who received program tickets in practice, who won the cash transfer during the public lottery, and whether the transfers were received by the households selected during the public lottery.

[54] This design was intended to avoid direct spillovers within chiefs: e.g., a chief compensating for pressure in an Information & Audit neighborhood within his or her domain by diverting corruption to a control neighborhood.

[55] Table A14 examines the balance of the cross-randomized experiment using the same measures Balán et al. (2022) uses to assess the balance of the original assignment of tax responsibilities. Of the 44 treatment com-

When comparing outcomes across the cross-randomized interventions, we use OLS to estimate a version of equation (1), interacting the $ChiefTaxed_{jk}$ indicator with indicators for the sub-treatment arms.[56] When considering targeting errors by the wealth index measure we use sample-weighted data as in Table 2. Because the sub-treatments were randomized at the chief level, we replace the randomization strata used in estimating equation (1) with coarser randomization strata used in the assignment of the cross-randomized intervention at the chief level and defined by the geographical location of each neighborhood in Kananga and average tax compliance during the 2018 tax campaign.[57] Standard errors are clustered at the neighborhood level, and our preferred specification does not include covariates.

Response to Individual Demands. We first examine whether chiefs are more responsive to individual citizen demands after collecting taxes from them. Table 10 shows the impacts of informational fliers on citizens' engagement with the chief in the Information and the Information & Audit neighborhoods.[58] Information fliers were successful at encouraging citizens to engage with the chief about the cash transfer program: households that received informational fliers were 87.9% (i.e., 0.051 percentage points) more likely to report asking the chief for information about the program (Column 1) and 46% (i.e., 6.6 percentage points) more likely to receive a program ticket (Column 5). However, information flier recipients were not more likely to ask the chief for information or to receive a program ticket if the chief was responsible for tax collection in 2018 (Columns 2 and 6).[59]

Better targeting could still arise among collector chiefs if the informational flier recipients to whom they granted tickets were more deserving than flier recipients who successfully lobbied non-collector chiefs for tickets. In other words, collector chiefs may have been

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parisons using baseline characteristics, 4 (9.1%) are significant at the 10% significance level, as expected by chance under random assignment. Table A15 assesses balance using household attributes collected in the household surveys described in Section 3. None of the treatment comparisons using these measures are statistically significant.

[56]Specifically, we estimate $y_{ijl} = \beta_0 + \beta_1 ChiefTaxed_{jl} + \beta_2 ChiefTaxed_{jl} * Info_{jl} + \beta_3 Info_{jl} + \beta_4 ChiefTaxed_{jl} * Info \& Audit_{jl} + \beta_5 Info \& Audit_{jl} + \mathbf{X}_{ijl}\Gamma + \alpha_l + \varepsilon_{ijl}$, where all terms are defined analogously to those in equation (1).

[57]These strata were used in the assignment of the cross-randomized interventions and correspond to larger neighborhood sets than those used in equation (1).

[58]We pool these sub-treatment arms when considering the impacts of the informational fliers as randomly selected households in both arms received the fliers. Households in Information & Audit neighborhoods additionally received audit forms, which should increase the bargaining power of citizens when lobbying the chief to receive program tickets.

[59]On the intensive margin, flier recipients asked chiefs for information 89.9% more often (Column 3), though this rate also does not differ across chief tax collection status (Column 4).

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more responsive — in terms of granting program tickets — to needier (i.e., less wealthy) flier recipients while non-collector chiefs may have been less discriminating in their response to the individual demands stimulated by the fliers. However, we find no significant differences in the effect of receiving a flier on the likelihood of receiving a program ticket — nor in requesting information from the chief — across levels of citizen wealth (Figure A5). This pattern suggests that the informational fliers, by encouraging some non-needy citizens to demand access to the program from the chief, might have exerted a countervailing force on the targeting improvements resulting from chiefs engaging in taxation.[60] Comparing targeting outcomes at the neighborhood level, we indeed find that the likelihood of making an error of inclusion or exclusion is higher in Information neighborhoods (relative to No Information neighborhoods) but only in those where chiefs collected taxes (Table 11, Column 1). This effect is driven by collector chiefs in Information neighborhoods being more likely to commit errors of exclusion among the least wealthy (Column 4) and of inclusion among the wealthiest set of households (Column 7). The Information treatment thus appears to undo the targeting improvements collector chiefs attain through other means, offering cautionary evidence against the value of indiscriminately activating citizen demands for access as a means for improving redistribution intended for the poorest.[61]

Response to Collective Action Pressure. We then examine the effect of the Information & Audit sub-treatment arm to explore whether chiefs who collected taxes anticipate more collective action and allocated more program tickets to households in the bottom 20% of the wealth distribution as a result.

We first confirm that the opportunity to hold chiefs accountable was effective at encouraging citizens to submit forms requesting audit meetings. The audit meeting request form submission rate averaged 18%, representing a non-trivial share of households undertaking costly effort to submit their audit meeting request forms at the centrally located drop boxes to request an audit meeting of the chief or the DIVAS. Form submission was skewed: in 37.8% of neighborhoods the submission rate was higher than the average, reaching a max-

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[60]Because the informational fliers were randomly assigned in neighborhoods, the expected value of household wealth corresponds to the neighborhood average. By contrast, chiefs were asked to allocate program tickets to the neediest 20% of households. As a result, the majority of flier recipients were citizens that were unlikely to be among the bottom 20% in wealth terms.

[61]We interpret this result as suggestive evidence as the effect of the Information sub-treatment on the likelihood collector chiefs make an exclusion error is only statistically significant at the 10% level ($p = 0.059$) (Column 2).

imum of 75% (Figure A6).[62]

Are chiefs who collected taxes more likely to anticipate a high demand for an audit meeting? It's been argued that taxation can induce accountable governance by improving citizens' demand and ability to monitor leaders and hold them accountable (Moore, 2008; Prichard, 2015b). Chiefs might anticipate that their role in taxation might have increased citizens' demand to monitor their activity. Table 12 summarizes differences in chiefs' priors about the likelihood of monitoring by citizens across the sub-treatment groups.[63] The Information & Audit sub-treatment increases the perceived likelihood of monitoring and pressure by citizens by 0.5 standard deviations (Column 1). Chiefs working in neighborhoods in the Information & Audit arm on average report expecting 18.8% (9 percentage points) more citizens to monitor and coordinate to complain if unhappy (Column 3), and 0.423 standard deviations higher monitoring and pressure relative to the levels they perceive in other neighborhoods in Kananga (Column 6). The Audit sub-treatment thus appears to have effectively raised chiefs' perceptions about the likelihood of experiencing monitoring of their role in the program.

Yet absent offering citizens an explicit audit opportunity, chiefs who collected taxes report *lower* probabilities of monitoring and pressure. Considering only chiefs (and neighborhoods) assigned to the Control arm, collector chiefs assessed that citizens would exert 0.47 standard deviations less such pressures compared to non-collector chiefs (Column 2). Likewise, these chiefs believe the proportion of citizens that would monitor the program and coordinate complaints to be 30% (15.7 percentage points) lower (Column 4).[64] Surprisingly, tax collection on its own appears to lower the perceived threat of citizen audit in the context of the program. However, the Audit sub-treatment has a stronger effect on collector chiefs' priors, increasing perceived monitoring and pressure by citizens as to be comparable to that of non-collector chiefs in the Audit arm (Column 2), as well as raising the reported proportion of citizens that would monitor and complain to a level slightly higher than that of non-collector counterparts (Column 4). The audit opportunity in effect reversed the negative effect of tax collection on chiefs' beliefs about monitoring.

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[62]Form submission rates overall do not differ significantly across actors, suggesting citizens viewed chiefs and the DIVAS as jointly responsible for the program. Submission rates also do not differ by whether the chief engaged in tax collection in the neighborhood or not.

[63]These measures were solicited before chiefs distributed program tickets but after trainings in which those in the Audit arm were informed of the collective action opportunity.

[64]Ratings of citizen monitoring and pressure relative to other neighborhoods among collector chiefs in the No Information arm is also lower than that for analogous non-collector chiefs but not statistically significant (Column 6).

Nevertheless, the Audit arm resulted in no detectable changes in collector chiefs' targeting of program tickets by household wealth. Like those in the Information arm, we find that chiefs in the Information & Audit arm who collected taxes are more likely to commit errors of inclusion and exclusion (relative to non-collector chiefs in the Control arm), but these differences are not statistically significant (Table 11). Chiefs who collected taxes thus anticipated more collective action in the Information & Audit arm, but did not adjust their targeting strategy as a result.[65]

Together, we find little evidence to support the interpretation that chiefs become more responsive to citizen demands and pressure after collecting taxes. Rather, the results discussed in Sections 6.1 and 6.2 are consistent with tax collection leading chiefs to improve their targeting ability on their own, absent greater citizen accountability efforts.

7 Conclusion

We examined how collecting taxes for the formal state impacts informal leaders' local legitimacy and performance. In neighborhoods where they collected taxes, chiefs were more likely to target cash transfer program tickets to the poorest households in the neighborhood. They made fewer errors of inclusion and errors of exclusion. They were also no more likely to target family members, friends, and coethnics, or to divert state funds altogether. Tax collection appears to have equipped chiefs with better information about the neediest households in the neighborhood, enabling them to allocate more of the cash transfer program benefits to poor households. Additionally, we find suggestive evidence that tax collection increased chiefs' public-spiritedness, leading them to realize an allocation more consistent with the antipoverty program's objective. We find limited support for bottom-up citizen pressure motivating chiefs to distribute more program tickets to the poorest households. Taken together, these results provide experimental evidence that the key role played by local leaders in revenue generation may also strengthen their role in the expenditure side of the state.

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[65] As the Audit sub-treatment combined the opportunity to submit forms requesting audit meetings with the informational fliers provided in the Information arm, it is not possible for us to rule out the possibility that the effect of the Audit arm on collector chiefs' targeting of benefits represents a combination of improvements in targeting (due to higher perceived monitoring through the opportunity to request audits) and allocation of program tickets to wealthier households (due to pressure stimulated by informational fliers). However, we cannot reject that the effects of the two sub-treatment arms among collector chiefs are equivalent.

Tables and Figures

Table 1: Timing of all Activities and Data Collection

Activity	Actor	Timing	N	J
Tax Campaign				
Taxation				
Property registration	Collectors	May–Dec 2018	29,391	221
Tax visits	Collectors	May–Dec 2018	29,391	221
Evaluation				
Baseline survey	Enumerators	Jul–Dec 2017	2,649	221
Midline survey	Enumerators	Jun 2018–Feb 2019	22,430	221
Endline survey	Enumerators	Mar–Sep 2019	2,413	221
Cash Transfer Program				
Program				
Ticket distribution	Chiefs	Jun–Oct 2019	4,401	221
Lottery	Chiefs & DIVAS	Jun–Oct 2019	221	221
Cash transfer distribution	Chiefs	Jun–Oct 2019	1,105	221
Evaluation				
Endline survey	Enumerators	Jun–Dec 2019	6,267	221

Notes: N = number of observations, J = number of clusters (neighborhoods). Taxation and tax campaign evaluation surveys correspond to the sample in Balán et al. (2022) (Table 1) for Central and Local tax collection neighborhoods only. The primary targeting outcomes results from merging official property register records, household survey data from the tax campaign, and ticket distribution data from the cash transfer program. We discuss this table in Section 3.

Table 2: Effects of Chief Tax Collection on Targeting by Wealth

Analysis:		By wealth status		By wealth level				
Outcome:	Any error	Exclusion error	Inclusion error	Exclusion error (very poor)	(near poor)	Inclusion error (middle)	(rich)	Average Wealth
Sample:	Full	0% - 20%	21% - 100%	0% - 10%	11% - 20%	21% - 60%	61% - 100%	Recipients
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Comparing neighborhoods by whether chief taxed there								
Chief taxed here (Local)	-0.065***	-0.056***	-0.017**	-0.074**	-0.044	-0.011	-0.031**	-0.121
	(0.017)	(0.019)	(0.007)	(0.036)	(0.028)	(0.012)	(0.013)	(0.076)
R^2	0.031	0.072	0.006	0.217	0.084	0.019	0.024	0.157
Observations	6267	1000	5267	446	554	2444	2823	4384
Clusters	221	216	221	193	195	218	221	220
Control Mean	0.317	0.898	0.163	0.897	0.899	0.159	0.166	0.065
Panel B: Comparing neighborhoods by whether chief taxed anywhere								
Chief taxed elsewhere (Central)	-0.022	-0.005	-0.011	0.023	-0.032	0.017	-0.040**	-0.089
	(0.025)	(0.020)	(0.011)	(0.038)	(0.035)	(0.017)	(0.020)	(0.114)
Chief taxed here (Local)	-0.077***	-0.058***	-0.023**	-0.064	-0.061*	-0.002	-0.052***	-0.168*
	(0.019)	(0.021)	(0.009)	(0.039)	(0.034)	(0.015)	(0.017)	(0.098)
R^2	0.031	0.072	0.006	0.217	0.085	0.020	0.025	0.158
Observations	6267	1000	5267	446	554	2444	2823	4384
Clusters	221	216	221	193	195	218	221	220
Control Mean	0.328	0.897	0.169	0.891	0.903	0.157	0.181	0.041
p-value test: tax here vs. elsewhere	0.016	0.016	0.202	0.050	0.350	0.183	0.468	0.396

Notes: This table examines the impact of chiefs collecting taxes on their allocation of cash transfer program tickets. Specifically, Panel A compares error rates in neighborhoods where the chief taxed with those where the chief did not tax (the excluded category), and Panel B compares neighborhoods where the chief taxed directly (here), where the local chief did not collect taxes directly but collected taxes in another neighborhood (elsewhere) to neighborhoods where the chief did not collect taxes at all (excluded category). In this table, errors are determined using the pre-specified wealth index and estimated on sample-weighted data, as described in Section 3.2. Columns 1-7 examine errors of inclusion (non-poor households receiving program tickets) and exclusion (poor households failing to receive program tickets). Specifically, in column 1, the outcome is any error (inclusion or exclusion), estimated in the full population of households for which the wealth index measure is available. In columns 2 and 3, the outcome is errors of exclusion among households in the bottom quintile of the wealth distribution and inclusion among households above the bottom quintile, respectively. Columns 4 and 5 consider errors of exclusion for the very poor and near poor. Columns 6 and 7 consider errors of inclusion for the middle and rich categories. Column 8 shows the average wealth level among program ticket recipients. All regressions include tax stratum fixed effects and cluster standard errors at the neighborhood level. We discuss these results in Section 5.1.

Table 3: Effects of Chief Tax Collection on Targeting by Income

Analysis:		By income status		By income level				
Outcome:	Any error	Exclusion error	Inclusion error	Exclusion error (very poor)	(near poor)	Inclusion error (middle)	(rich)	Average Income
Sample:	Full	0% - 20%	21% - 100%	0% - 10%	11% - 20%	21% - 60%	61% - 100%	Recipients
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Comparing neighborhoods by whether local chief was tax collector there								
Chief col. here (Local)	-0.044**	-0.102*	-0.033	-0.130	-0.077	-0.014	-0.054**	-0.037
	(0.019)	(0.052)	(0.021)	(0.107)	(0.066)	(0.029)	(0.027)	(0.062)
R^2	0.022	0.168	0.026	0.308	0.294	0.056	0.048	0.135
Observations	2315	352	1963	128	224	954	1009	604
Clusters	221	213	221	128	181	220	221	207
Control Mean	.337	.721	.268	.738	.712	.288	.249	.085
Panel B: Comparing neighborhoods by whether local chief collected anywhere								
Chief col. elsewhere (Central)	-0.049*	0.051	-0.058*	0.147	-0.013	-0.023	-0.084**	-0.150
	(0.027)	(0.076)	(0.030)	(0.156)	(0.096)	(0.044)	(0.037)	(0.098)
Chief col. here (Local)	-0.070***	-0.078	-0.063**	-0.058	-0.083	-0.026	-0.098***	-0.111
	(0.024)	(0.063)	(0.027)	(0.126)	(0.074)	(0.037)	(0.034)	(0.084)
R^2	0.023	0.169	0.028	0.316	0.294	0.057	0.052	0.139
Observations	2315	352	1963	128	224	954	1009	604
Clusters	221	213	221	128	181	220	221	207
Control Mean	.362	.700	.299	.697	.702	.293	.304	.019
p-value Test Col. here vs. elsewhere	0.350	0.054	0.841	0.145	0.434	0.930	0.657	0.594

Notes: This table examines the impact of chiefs collecting taxes on their allocation of cash transfer program tickets. Specifically, Panel A compares error rates in neighborhoods where the chief taxed with those where the chief did not tax (the excluded category), and Panel B compares neighborhoods where the chief taxed directly (here), where the local chief did not collect taxes directly but collected taxes in another neighborhood (elsewhere) to neighborhoods where the chief did not collect taxes at all (excluded category). In this table, errors are determined using the reported household monthly incomes. Columns 1-7 examine errors of inclusion (non-poor households receiving program tickets) and exclusion (poor households failing to receive program tickets). Specifically, in column 1, the outcome is any error (inclusion or exclusion), estimated in the full population of households for which the income measure is available. In columns 2 and 3, the outcome is errors of exclusion among households in the bottom quintile of the income distribution and inclusion among households above the bottom quintile, respectively. Columns 4 and 5 consider errors of exclusion for the very poor and near poor. Columns 6 and 7 consider errors of inclusion for the middle and rich categories. Column 8 shows the average income level among program ticket recipients. All regressions include tax stratum fixed effects and cluster standard errors at the neighborhood level. We discuss these results in Section 5.1.

Table 4: Nepotism and Fairness

	Citizens' Perceptions of Chiefs' Ticket Allocation			Connections of Ticket Recipients to Chief	
	Family Members of Chief (1)	Index: Other Conn. to Chief (2)	Poor in Nbhd. (3)	Family Member of Chief (4)	Index: Other Conn. to Chief (5)
Panel A: Comparing neighborhoods by whether local chief was tax collector there					
Chief col. here (Local)	-0.044*	-0.042	0.057**	0.042	0.112
	(0.024)	(0.054)	(0.025)	(0.052)	(0.070)
R^2	0.037	0.045	0.039	0.084	0.134
Observations	2723	2723	2723	2520	2532
Clusters	221	221	221	220	220
Control Mean	.446	.048	.521	.053	.006
Panel B: Comparing neighborhoods by whether local chief collected anywhere					
Chief col. elsewhere (Central)	0.048	-0.147*	0.051	0.028	0.010
	(0.037)	(0.076)	(0.038)	(0.080)	(0.098)
Chief col. here (Local)	-0.022	-0.110*	0.080***	0.056	0.117
	(0.029)	(0.065)	(0.031)	(0.063)	(0.090)
R^2	0.038	0.047	0.040	0.084	0.134
Observations	2723	2723	2723	2520	2532
Clusters	221	221	221	220	220
Control Mean	.422	.088	.5	.026	.017

Notes: This table compares a chief's favoring of her connections and citizens' perceptions of a chief's fairness in neighborhoods where the chief taxed with those where the chief did not tax (the excluded category) (Panel A) and in neighborhoods where the chief taxed directly (here), where the local chief did not collect taxes directly but collected taxes in another neighborhood (elsewhere), and where the chief did not collect taxes at all (the excluded category) (Panel B). Columns 1–3 are measures of citizen perceptions of a chief's fairness when allocating tickets. Column 1 is a dummy variable that takes a value of 1 if a chief was perceived to allocate hypothetical cash transfers to her family and friends first or second, before all other households (and 0 otherwise). Column 2 is an index for citizen perceptions of a chief's favoring of her other connections, including people from her tribe, people from her church, taxpayers, and members of her political party/coalition. Each index component takes a value of 1 if a chief was perceived to allocate hypothetical cash transfers to her connections first or second, before all other households (and 0 otherwise). Column 3 takes a value of 1 if a chief is perceived to allocate hypothetical cash transfers to the poorest people in the neighborhood first or second, before all other households (and 0 otherwise). Columns 4–5 are direct measures of a chief's favoring of connections during ticket distribution. Column 4 is an index of a chief's family members, including his or her nuclear family and extended family. Each index component takes a value of 1 if the respondent is a member of the chief's family. Column 5 is an index of other connections to a chief, including members of the same tribe (defined as belonging to the majority language group), people from the same territory, people from the same subtribe (*groupement*), people from the same church, and people who have the chief's phone number. Each index component takes a value of 1 if the respondent is a connection of the chief. All regressions include tax stratum fixed effects and cluster standard errors at the neighborhood level. We discuss these results in Section 5.2.

Table 5: Corruption and Diversion

	Asked Something for Ticket (1)	Cash Not in Envelope (2)	Transfer Not Received (3)	Amt. Cash Missing (CF) (4)
Panel A: Comparing neighborhoods by whether local chief was tax collector there				
Chief col. here (Local)	-0.006	-0.006	0.013	84.262
	(0.014)	(0.021)	(0.049)	(552.583)
R^2	0.073	0.293	0.175	0.170
Observations	2464	311	470	451
Clusters	220	165	200	199
Control Mean	.031	.056	.215	2875.328
Panel B: Comparing neighborhoods by whether local chief collected anywhere				
Chief col. elsewhere (Central)	0.017	-0.029	0.031	250.115
	(0.020)	(0.047)	(0.065)	(709.448)
Chief col. here (Local)	0.003	-0.020	0.029	217.778
	(0.013)	(0.037)	(0.057)	(643.144)
R^2	0.075	0.295	0.176	0.170
Observations	2464	311	470	451
Clusters	220	165	200	199
Control Mean	.016	.077	.206	2828.151

Notes: This table compares measures of corruption and diversion in ticket allocation and cash transfer in neighborhoods where the chief taxed with those where the chief did not tax (the excluded category) (Panel A) and in neighborhoods where the chief taxed directly (here), where the local chief did not collect taxes directly but collected taxes in another neighborhood (elsewhere), and where the chief did not collect taxes at all (the excluded category) (Panel B). Column 1 is a dummy variable that takes a value of 1 if a citizen of the neighborhood reported that the chief asked for something in exchange for allocating the citizen a ticket (and 0 otherwise). Column 2 is a dummy variable that takes a value of 1 if a lottery winner discovered that the cash transfer was not in the envelope given by the chief, suggesting that the chief had stolen the cash (and 0 otherwise). Column 3 is a dummy variable that takes a value of 1 if a citizen reported that it did not win a cash transfer in the lottery even though enumerators' archives suggested that he or she had won, suggesting the chief had stolen or diverted the cash transfer (and 0 otherwise). Column 4 is the amount of the cash transfer in Congolese francs (CF) missing from the prize amount allocated to a winning citizen, calculated by subtracting the amount reported received in surveying from the amount the household was supposed to receive. All regressions include tax stratum fixed effects and cluster standard errors at the neighborhood level. We discuss these results in Section 5.2.

Table 6: Effects of Chief Tax Collection on Targeting of Taxpayers

	Ticket Recipient			Number of Tickets		
	All	Bottom 20%	Top 80%	All	Bottom 20%	Top 80%
	(1)	(2)	(3)	(4)	(5)	(6)
Taxpayer	0.011	0.068	-0.001	0.008	0.040	-0.013
	(0.012)	(0.096)	(0.029)	(0.016)	(0.148)	(0.043)
Chief Taxed	-0.007	0.072**	-0.049***	-0.006**	0.106*	-0.054*
	(0.006)	(0.035)	(0.017)	(0.003)	(0.063)	(0.032)
Chief Taxed * Taxpayer	0.039*	-0.028	0.092**	0.056**	-0.057	0.162**
	(0.021)	(0.121)	(0.044)	(0.027)	(0.184)	(0.074)
Control Mean	.149	.637	.72	.191	.82	.921
Number of Taxpayers		102	719		102	719
Fraction of Taxpayers		.102	.137		.102	.137
Clusters	221	216	221	221	216	221
Observations	29630	1000	5267	29630	1000	5267

Notes: This table summarizes the results for chiefs' allocation of program tickets by household property tax payment status and chiefs'. Row 1 is a dummy variable indicating whether the household paid the property tax in 2018. Row 2 is an indicator for whether the chief collected taxes in the neighborhood during the 2018 property tax campaign. Row 3 is the interaction between the Taxpayer indicator (row 1) and the Chief Taxed indicator (row 2). Columns 1–3 use an indicator for whether the household is a ticket recipient as the outcome and Columns 4–6 use the number of tickets received as the outcome. Columns 1 and 4 consider the entire sample of households while Columns 2–3 and 5–6 restrict the sample to households for which the wealth index measure is available. Wealth is measured by an index of pooled house and neighborhood quality, which is a standardized index including Wall Quality, Erosion Threat, Road Quality, and Accessibility. Using this index, households are split into two groups: households in the bottom 20% of the wealth distribution (Columns 2 and 5) and households in the top 80% of the wealth distribution (Columns 3 and 6). All regressions include tax stratum fixed effects and cluster standard errors at the neighborhood level. We discuss these results in Section 5.2.

Table 7: Attitudes Toward Chiefs

Dependent variable	$\hat{\beta}$	SE	R^2	N	$\bar{x}_{ChiefNotTax}$
	(1)	(2)	(3)	(4)	(5)
Panel A: Views of the chief					
View of chief (index)	0.126**	0.050	0.040	2355	-0.065
Trust in chief	0.050	0.052	0.103	2339	-0.036
Performance of chief	0.029	0.060	0.060	1326	-0.031
Integrity of chief	0.088	0.063	0.055	1505	-0.038
Importance of chief	0.076	0.055	0.027	1879	-0.030
Panel B: Demands for chief services					
Demand for chief services (index)	-0.026	0.047	0.027	2380	0.013
Demand for public goods provision by chief	-0.006	0.041	0.015	2315	0.008
Demand for conflict mediation by chief	-0.039	0.049	0.089	2359	0.023
Demand for political representation by chief	0.017	0.042	0.020	2380	-0.007
Panel C: Activities of the chief					
Activity of chief (index)	-0.074	0.060	0.082	1879	0.054
Overall activity level of chief	-0.034	0.065	0.103	1335	0.026
Frequency of salongo in neighborhood	-0.092	0.061	0.090	1879	0.067
Frequency of chief political representation	-0.019	0.062	0.072	1879	0.024
Frequency of chief conflict mediation	-0.106*	0.056	0.037	1879	0.055
Frequency of chief personal favors	-0.002	0.047	0.023	1879	0.001

Notes: This table compares attitudes toward chiefs in neighborhoods where the chief taxed with those where the chief did not tax (the excluded category). Each row summarizes an OLS estimation of equation (1), comparing neighborhoods where chiefs taxed to those where they did not, with the dependent variable 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 excluded group mean $\bar{x}_{ChiefNotTax}$. Panel A shows estimated differences in citizen-reported ratings of trust in the neighborhood chief and ratings of his or her performance, integrity, and importance, as well as an index combining all individual measures. Panel B shows estimated differences in citizens' reported demands for chief services, including that public goods be provided by the chief, that the chief conduct conflict mediation, and that the chief provide political representation, as well as an combined index. Panel C shows estimated differences in activities the chief has engaged in after the tax campaign in the neighborhood, as reported by citizens, including a rating of overall activity, and the reported frequency with which the chief organizes salongo (informal taxation), provides political representation, mediates conflicts, and provides personal favors, as well as a combined index. All regressions include tax stratum fixed effects and cluster standard errors at the neighborhood level. We discuss these results in Section 5.3.

Table 8: Effect of Tax Collection on Knowledge of Chiefs

Proportion Known:	Name	Educ.	Job	Index
	(1)	(2)	(3)	(4)
Panel A: Comparing neighborhoods by whether local chief was tax collector there				
Chief col. here (Local)	0.058*	0.073***	0.005	0.133**
	(0.030)	(0.024)	(0.021)	(0.056)
R^2	0.080	0.047	0.032	0.056
Observations	2649	2631	2531	2649
Clusters	221	221	221	221
Control Mean	.418	.331	.286	.037
Panel B: Comparing neighborhoods by whether local chief collected anywhere				
Chief col. elsewhere (Central)	0.038	0.059	0.036	0.133
	(0.044)	(0.037)	(0.030)	(0.085)
Chief col. here (Local)	0.077**	0.104***	0.024	0.202***
	(0.037)	(0.031)	(0.026)	(0.071)
R^2	0.080	0.049	0.033	0.058
Observations	2649	2631	2531	2649
Clusters	221	221	221	221
Control Mean	.415	.321	.271	.065

Notes: This table compares city chief's knowledge of 12 randomly selected property owners in the neighborhood households during the quiz-like survey module described in Section 6 in neighborhoods where the chief taxed with those where the chief did not tax (the excluded category) (Panel A) and in neighborhoods where the chief taxed directly (here), where the local chief did not collect taxes directly but collected taxes in another neighborhood (elsewhere), and where the chief did not collect taxes at all (the excluded category) (Panel B). Column 1 uses a dummy variable that takes a value of 1 if the chief knows the name of the owner (an 0 otherwise). Column 2 uses a dummy variable that takes a value of 1 if the chief knows the highest level of education of the owner (and 0 otherwise). Column 3 is a dummy variable that takes a value of 1 if the chief knows the occupation of the owner (and 0 otherwise). Column 4 is a standardized index of chief's knowledge of the owner's name, education, and job. All regressions include tax stratum fixed effects and cluster standard errors at the neighborhood level. We discuss these results in Section 6.1

Figure 1: Error Rate by Chief Knowledge of the Inhabitants of the Neighborhood

Panel A: Knowledge of Name

Panel B: Knowledge of Education

Panel C: Knowledge of Occupation

Panel D: Knowledge Index
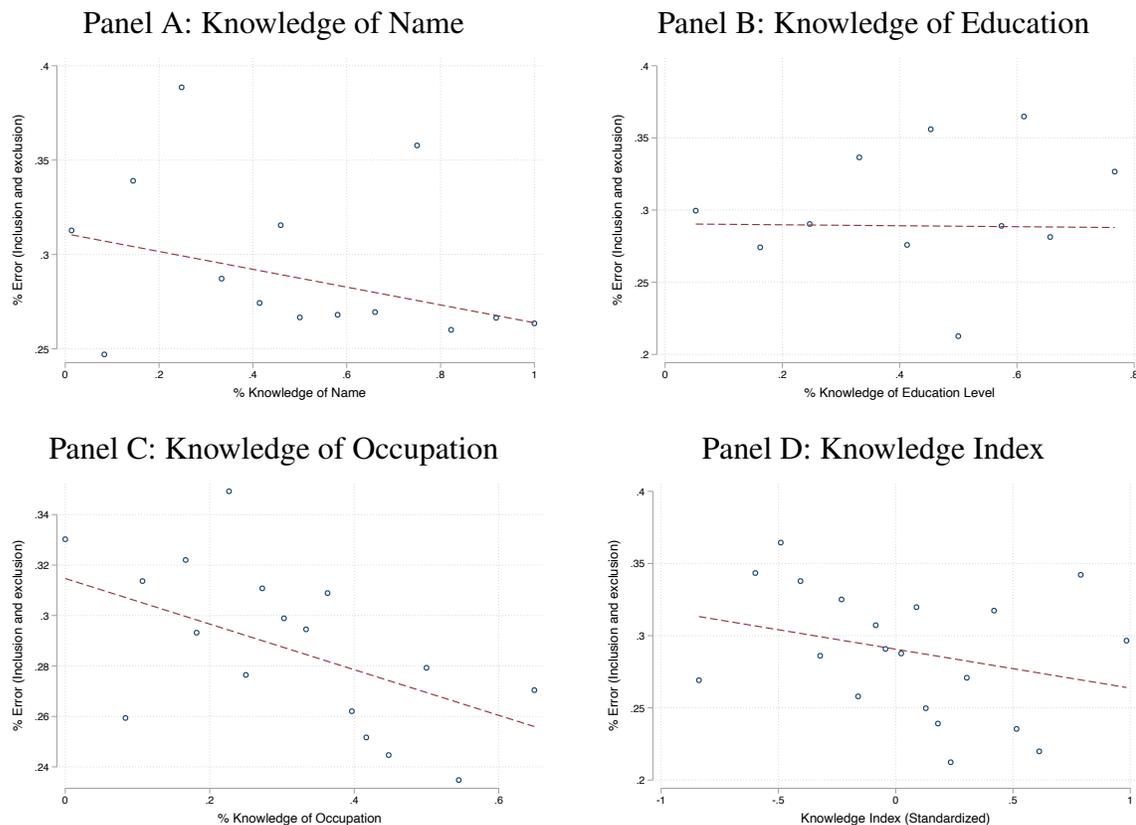
Notes: This figures shows the relationship between chiefs' knowledge of the inhabitants of the neighborhood and the average error rate in the neighborhood. The error rate is defined using a pre-registered wealth index constructed from observable household attributes and estimated on sample-weighted data, as described in Section 3.2. A chief's knowledge of the inhabitants of the neighborhood is measured by the percentage of correct answered when asked to provide the name (Panel A), education level (Panel B), and occupation (Panel C) of a randomly selected group of 12 property owners per neighborhood. In Panel D, chief knowledge is measured using a standardized index of chief's knowledge of the education level and occupation of the respondent. Table A10 analyzes theses relationships in a regression framework. We discuss these results in Section 6.1.

Table 9: Chiefs' Self-Reported Sense of Duty

Index:	Economic Relief	Public Goods Provision	Arbitrate Dispute	All Responsibilities
Panel A: Comparing neighborhoods by whether local chief was tax collector there				
Chief col. here (Local)	0.221	-0.015	0.071	0.127
	(0.139)	(0.130)	(0.140)	(0.141)
R^2	0.315	0.369	0.285	0.327
Observations	221	221	221	221
CompMean	.073	.035	.066	.08
Panel B: Comparing neighborhoods by whether local chief collected anywhere				
Chief col. elsewhere (Central)	0.011	0.116	0.267	0.196
	(0.199)	(0.202)	(0.216)	(0.215)
Chief col. here (Local)	0.227	0.045	0.210	0.228
	(0.200)	(0.161)	(0.182)	(0.185)
R^2	0.315	0.370	0.293	0.330
Observations	221	221	221	221
Control Mean	.097	.219	.188	.225

Notes: This table compares chiefs' self-reported sense of duty in Panel A compares chiefs' self-reported sense of duty for the neighborhoods where the chief taxed and for those where the chief did not tax. Panel B compares chiefs' self-reported sense of duty where the chief taxed directly (here), for those where the local chief did not collect taxes directly but collected taxes in another neighborhood (elsewhere), and for those where the chief did not collect taxes at all. Column 1 is an index of a chief's sense of her economic duties, including (i) ensuring that citizens in her avenue/localité have enough money to survive and (ii) helping citizens in her avenue/localité find jobs. Column 2 is an index of a chief's sense of her duties in providing public goods, including (i) providing citizens in her avenue/localité with water, health care, and other public services and (ii) organizing salongo to help improve public infrastructures in the avenue/localité. Column 3 is an index of a chief's sense of her duties in arbitrating disputes, including (i) resolving disputes among households in your avenue/localité, (ii) resolving crimes that occur in your avenue/localité, and (iii) punishing criminals for crimes that occur in your avenue/localité. Column 4 is an index of a chief's overall sense of duties and includes all aforementioned index components. All index components take a value of 3 if a chief thinks that this task is much more her responsibility than the responsibility of another government agent, 2 if the chief thinks that it is more her responsibility than the responsibility of another government agent, 1 if the chief thinks that it is more the responsibility of another government agent than her responsibility, and 0 if the chief thinks that it is much more the responsibility of another government agent than her responsibility. All regressions include tax stratum fixed effects and cluster standard errors at the polygon level. We discuss these results in Section 6.2.

Table 10: Asking Chief for Information and Program Ticket Receipt by Chief Tax Collection

	Asked Chief for Info.		# Times Asked Chief for Info.		Received Program Ticket	
	(1)	(2)	(3)	(4)	(5)	(6)
Received Info. Flier	0.051***	0.037**	0.098***	0.072**	0.066***	0.059***
	(0.013)	(0.015)	(0.028)	(0.029)	(0.010)	(0.014)
Chief Taxed		-0.015		0.006		-0.007
		(0.015)		(0.033)		(0.007)
Chief Taxed X Received Info. Flier		0.027		0.053		0.015
		(0.026)		(0.055)		(0.019)
R^2	0.015	0.016	0.010	0.011	0.005	0.006
Observations	1969	1969	1969	1969	21239	21239
Clusters	162	162	162	162	162	162
Control Mean	.058	.066	.109	.107	.142	.145

Notes: This table examines the impact of informational fliers on household members' requests for information about the cash transfer program and the chief's allocation program tickets. Twenty percent of households within each neighborhood in the Information and Information & Audit subtreatment arms were randomly assigned to receive an informational flier informing them of the cash transfer program, as described in Section 6.3. The table compares the probability of asking the neighborhood chief for information (Columns 1 and 2), the number of times information was requested (Columns 3 and 4), and receipt of program tickets (Columns 5 and 6) by individual-level exposure to the information fliers. Only neighborhoods in the Information and Information & Audit sub-treatments described in Section 6.3 are included in the sample. Columns 1, 3, and 5 pool neighborhoods where chiefs did and did not collect taxes. Columns 2, 4, and 5 include a dummy and interaction term for a neighborhood being assigned to chief tax collection. All regressions include tax stratum fixed effects using strata for the assignment of cross-randomized arms (assigned at the chief-level) and cluster standard errors at the neighborhood level. We discuss these results in Section 6.3.

Table 11: Effects of Chief Tax Collection and Cross-Randomized Treatments on Targeting by Wealth

Analysis:		By wealth status		By wealth level				
Outcome:	Any error	Exclusion error	Inclusion error	Exclusion error (very poor)	(near poor)	Inclusion error (middle)	(rich)	Average Wealth
Sample:	Full	0% - 20%	21% - 100%	0% - 10%	11% - 20%	21% - 60%	61% - 100%	Recipients
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Chief taxed here (Local)	-0.076**	-0.071**	-0.021	-0.114	-0.031	0.013	-0.052**	-0.118
	(0.034)	(0.035)	(0.013)	(0.076)	(0.040)	(0.021)	(0.023)	(0.179)
Chief taxed here (Local) X Info	0.077	0.087*	0.008	0.133	0.034	-0.033	0.046	0.128
	(0.050)	(0.046)	(0.019)	(0.088)	(0.052)	(0.034)	(0.031)	(0.237)
Info	-0.011	-0.038	0.023*	-0.067	0.005	0.064**	-0.014	0.074
	(0.035)	(0.027)	(0.013)	(0.044)	(0.036)	(0.026)	(0.024)	(0.180)
Chief taxed here (Local) X Audit	0.005	0.011	0.007	0.059	-0.029	-0.027	0.036	-0.051
	(0.045)	(0.042)	(0.018)	(0.088)	(0.048)	(0.027)	(0.031)	(0.219)
Audit	0.019	-0.008	0.022	-0.044	0.031	0.027	0.015	0.028
	(0.034)	(0.019)	(0.013)	(0.029)	(0.029)	(0.019)	(0.024)	(0.149)
R^2	0.007	0.011	0.002	0.014	0.015	0.004	0.004	0.014
Observations	6267	1000	5267	446	554	2444	2823	4384
Clusters	221	216	221	193	195	218	221	220
Control Mean	.312	.913	.146	.937	.884	.131	.164	.03

Notes: This table examines the impact of chiefs collecting taxes on their allocation of cash transfer program tickets by the cross-randomized sub-treatments described in Section 6.3. Specifically, the table compares error rates in neighborhoods where the chief taxed with those where the chief did not tax by exposure to the cross-randomized information and collective action treatments (the excluded category is neighborhoods where chiefs did not tax and that received no cross-randomized treatment). In this table, errors are determined using the pre-specified wealth index and estimated on sample-weighted data, as described in Section 3.2. Columns 1-7 examine errors of inclusion (non-poor households receiving program tickets) and exclusion (poor households failing to receive program tickets). Specifically, in column 1, the outcome is any error (inclusion or exclusion), estimated in the full population of households for which the wealth index measure is available. In columns 2 and 3, the outcome is errors of exclusion among households in the bottom quintile of the wealth distribution and inclusion among households above the bottom quintile, respectively. Columns 4 and 5 consider errors of exclusion for the very poor and near poor. Columns 6 and 7 consider errors of inclusion for the middle and rich categories. Column 8 shows the average wealth level among program ticket recipients. All regressions include tax stratum fixed effects using strata for the assignment of cross-randomized arms (assigned at the chief-level) and cluster standard errors at the neighborhood level. We discuss these results in Section 6.3.

Table 12: Chief Priors About Audit Threat

Chief Priors:	Monitoring and pressure by citizens		Proportion of citizens who will monitor and come together to complain if unhappy		Monitoring and pressure relative to other nbhds. in Kananga	
	(1)	(2)	(3)	(4)	(5)	(6)
Audit	0.502***	0.164	9.022**	-3.444	0.423**	0.289
	(0.164)	(0.229)	(4.018)	(5.513)	(0.173)	(0.245)
Chief taxed here (Local) X Audit		0.668**		24.707***		0.258
		(0.320)		(7.705)		(0.342)
Chief taxed here (Local)		-0.472*		-15.579**		-0.313
		(0.255)		(6.126)		(0.272)
R^2	0.076	0.104	0.063	0.125	0.057	0.066
Observations	153	153	153	153	153	153
Control Mean	.146	.083	48.053	51.724	.071	.031

Notes: This table examines differences in chiefs' reported beliefs about the likelihood of monitoring and pressure by citizens in advance of program ticket distribution across treatment (chief tax collection) and the Control and Information & Audit sub-treatment arms. The sample is restricted to neighborhoods assigned to only the Control and Information & Audit arms and excludes those in the Information arm. The outcomes are chiefs' priors about the monitoring and pressure that citizens will exert over the cash transfer program (Columns 1–2), the proportion of citizens chiefs believe will monitor and come together to complain if unhappy (Columns 3–4), and how chiefs rated the citizen monitoring and pressure relative to other neighborhoods in Kananga (Columns 5–6). These measures were solicited from chiefs before ticket distribution through chief surveys. All regressions include tax stratum fixed effects using strata for the assignment of cross-randomized arms (assigned at the chief-level) and cluster standard errors at the neighborhood level. We discuss these results in Section 6.3.

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8 Appendix Tables

Table A1: Balance of Wealth and Asset Measures

	N	Central Mean	Local
	(1)	(2)	(3)
Wall Quality	6152	0.045	-0.018
			(0.041)
Roof Quality	6153	0.033	-0.034
			(0.0391)
Erosion Threat	6267	0.007	0.004
			(0.076)
Road Quality	6267	0.056	-0.061
			(0.080)
Accessibility to Main Avenue	6267	0.078	-0.113
			(0.080)
Compound Has Fence	6153	0.053	-0.067
			(0.0675)
Electricity Access	6069	0.055	-0.029
			(0.057)
Vehicle Ownership	6270	0.023	-0.035
			(0.051)
F, p			0.612, 0.767

Notes: This table reports the coefficients from balance tests estimated by regressing wealth and asset measures on a treatment indicator for a neighborhood being taxed directly by the local chief, including randomization stratum fixed effects and clustering standard errors at the neighborhood level. Neighborhoods where chiefs did not directly tax form the omitted category. Measures of characteristics are drawn from households surveys for the sample described in Section 3 and reflect the measures in the pre-specified wealth index as well as measures of assets considered in Table A3. The bottom row contains the statistics for tests of the omnibus null hypothesis that the treatment effects for the covariates studied in the table are all zero using parametric F tests, using regressions that include stratum fixed effects and cluster standard errors at the neighborhood level. Further balance tests are provided in Balán et al. (2022) (Table 3). We discuss these results in Section 2.3.

Table A2: Error Rate by Components of Wealth Index

	Wall Quality			Roof Quality			Erosion Threat			Road Quality			Accessibility		
	Exclusion error	Inclusion error	Mean (ticket recipients)	Exclusion error	Inclusion error	Mean (ticket recipients)	Exclusion error	Inclusion error	Mean (ticket recipients)	Exclusion error	Inclusion error	Mean (ticket recipients)	Exclusion error	Inclusion error	Mean (ticket recipients)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Panel A: Comparing neighborhoods by whether local chief was tax collector there															
Chief taxed here (Local)	-0.009	-0.002	-0.005	-0.326*	-0.002	-0.089*	-0.054	-0.008	-0.042	-0.034	-0.006	-0.094	-0.040	-0.009	-0.134
	(0.024)	(0.006)	(0.048)	(0.193)	(0.006)	(0.049)	(0.038)	(0.007)	(0.081)	(0.043)	(0.007)	(0.086)	(0.027)	(0.007)	(0.087)
R^2	0.115	0.004	0.037	0.501	0.004	0.032	0.112	0.005	0.115	0.145	0.004	0.204	0.090	0.005	0.170
Observations	438	5714	4311	76	6077	4312	432	5835	4384	444	5823	4384	616	5651	4384
Clusters	116	221	220	50	221	220	137	221	220	106	221	220	141	221	220
Control mean	.873	.153	.038	.849	.151	.049	.898	.155	.009	.887	.154	.04	.902	.157	.059
Panel B: Comparing neighborhoods by whether local chief collected anywhere															
Chief taxed elsewhere (Central)	0.042	-0.011	-0.009	0.100	-0.006	-0.001	-0.067	-0.010	-0.142	-0.039	-0.008	0.043	-0.099***	-0.011	-0.161
	(0.048)	(0.009)	(0.060)	(0.250)	(0.009)	(0.045)	(0.055)	(0.009)	(0.125)	(0.042)	(0.010)	(0.125)	(0.032)	(0.010)	(0.130)
Chief taxed here (Local)	0.009	-0.008	-0.010	-0.283	-0.005	-0.090*	-0.088*	-0.013	-0.116	-0.057	-0.010	-0.071	-0.087***	-0.015*	-0.218*
	(0.028)	(0.008)	(0.051)	(0.235)	(0.007)	(0.046)	(0.045)	(0.008)	(0.105)	(0.043)	(0.008)	(0.113)	(0.027)	(0.008)	(0.111)
R^2	0.116	0.004	0.037	0.504	0.004	0.032	0.114	0.005	0.117	0.146	0.004	0.204	0.096	0.005	0.173
Observations	438	5714	4311	76	6077	4312	432	5835	4384	444	5823	4384	616	5651	4384
Clusters	116	221	220	50	221	220	137	221	220	106	221	220	141	221	220
Control Mean	.886	.161	.044	.715	.156	.063	.911	.162	.024	.88	.158	.048	.911	.164	.09
p-value Test here vs. elsewhere	0.422	0.645	0.985	0.083	0.974	0.136	0.652	0.733	0.792	0.732	0.774	0.249	0.732	0.622	0.587

Notes: This table compares the error rate in neighborhoods where the chief taxed with those where the chief did not tax (the excluded category) (Panel A) and neighborhoods where the chief taxed directly (here), where the local chief did not collect taxes directly but collected taxes in another neighborhood (elsewhere), and did not collect taxes at all (excluded category) (Panel B). The outcomes are defined as errors of inclusion or exclusion using components of the house quality wealth index in Table 2. The outcomes are wall quality (columns 1–3), roof quality (columns 4–6), erosion threat (columns 7–9), road quality (columns 10–12), and accessibility (columns 13–15). The first two columns for each outcome are exclusion and inclusion error, respectively, and the third column is the mean difference in the outcome among ticket recipient households. All regressions include tax stratum fixed effects and cluster standard errors at the neighborhood level. We discuss these results in Section 5.1.

Table A3: Effects of Chief Tax Collection on Targeting by Wealth and Assets

Analysis:		By wealth status		By wealth level				
Outcome:	Any error	Exclusion error	Inclusion error	Exclusion error (very poor)	(near poor)	Inclusion error (middle)	(rich)	Average Wealth
Sample:	Full	0% - 20%	21% - 100%	0% - 10%	11% - 20%	21% - 60%	61% - 100%	Recipients
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Comparing neighborhoods by whether chief taxed there								
Chief taxed here (Local)	-0.061***	-0.054***	-0.016**	-0.082***	-0.048	-0.004	-0.030**	-0.102
	(0.017)	(0.017)	(0.007)	(0.028)	(0.032)	(0.012)	(0.013)	(0.067)
R^2	0.027	0.070	0.006	0.152	0.093	0.016	0.021	0.154
Observations	6270	1074	5196	502	572	2614	2582	4385
Clusters	221	219	221	208	194	221	221	220
Control Mean	0.319	0.892	0.161	0.899	0.885	0.162	0.161	0.036
Panel B: Comparing neighborhoods by whether chief taxed anywhere								
Chief taxed elsewhere (Central)	-0.007	-0.018	-0.010	0.006	-0.039	0.022	-0.044*	-0.169
	(0.025)	(0.020)	(0.012)	(0.031)	(0.035)	(0.016)	(0.023)	(0.103)
Chief taxed here (Local)	-0.064***	-0.064***	-0.021**	-0.080***	-0.072*	0.006	-0.054***	-0.191**
	(0.019)	(0.020)	(0.009)	(0.028)	(0.039)	(0.013)	(0.019)	(0.091)
R^2	0.027	0.070	0.006	0.152	0.094	0.016	0.022	0.157
Observations	6270	1074	5196	502	572	2614	2582	4385
Clusters	221	219	221	208	194	221	221	220
Control Mean	0.314	0.889	0.166	0.895	0.882	0.158	0.175	0.066
p-value test: tax here vs. elsewhere	0.009	0.021	0.251	0.019	0.343	0.300	0.505	0.782

Notes: This table examines the impact of chiefs collecting taxes on their allocation of cash transfer program tickets. Specifically, Panel A compares error rates in neighborhoods where the chief taxed with those where the chief did not tax (the excluded category), and Panel B compares neighborhoods where the chief taxed directly (here), where the local chief did not collect taxes directly but collected taxes in another neighborhood (elsewhere) to neighborhoods where the chief did not collect taxes at all (excluded category). In this table, errors are determined using an index of measures from the pre-specified wealth index as well as a individual indicators for possessing electricity, a vehicle, a fence, and a roof of concrete, tiles, or sheet iron (as opposed thatch, straw, or bamboo). Columns 1-7 examine errors of inclusion (non-poor households receiving program tickets) and exclusion (poor households failing to receive program tickets). Specifically, in column 1, the outcome is any error (inclusion or exclusion), estimated in the full population of households for which the wealth index measure is available. In columns 2 and 3, the outcome is errors of exclusion among households in the bottom quintile of the wealth distribution and inclusion among households above the bottom quintile, respectively. Columns 4 and 5 consider errors of exclusion for the very poor and near poor. Columns 6 and 7 consider errors of inclusion for the middle and rich categories. Column 8 shows the average wealth level among program ticket recipients. All regressions include tax stratum fixed effects and cluster standard errors at the neighborhood level. We discuss these results in Section 5.1.
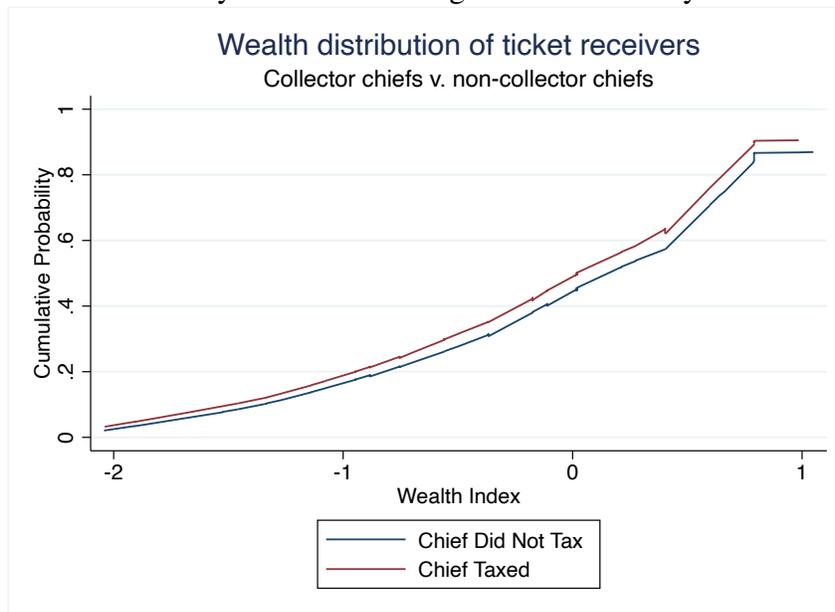
Table A4: Equality of Distribution Tests: Wealth Index and Monthly Household Income

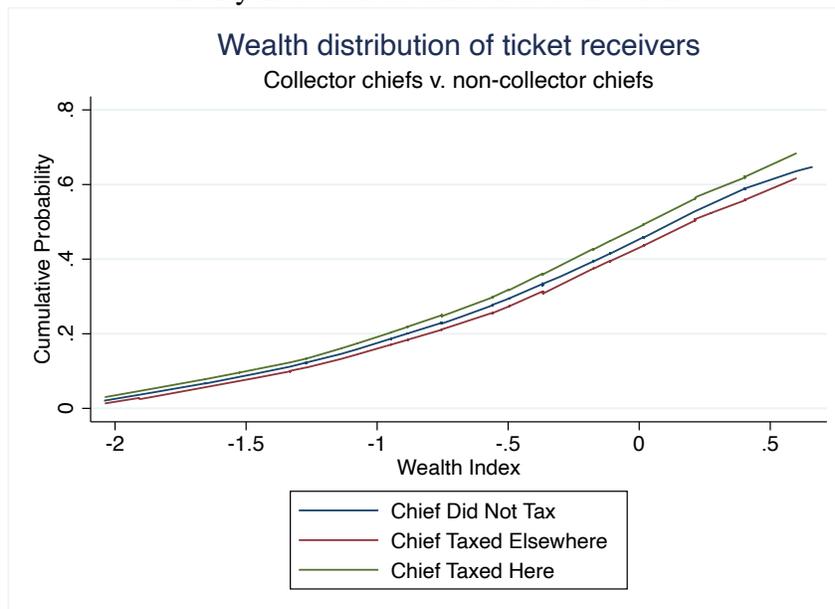|	Pooled Quality	Monthly Income
Panel A: Kolmogorov-Smirnov test		
Chief Taxed vs. Did Not Tax	0.000	0.259
Het: Chief Taxed Here vs. Did Not Tax	0.068	0.149
Het: Chief Taxed Elsewhere vs. Did Not Tax	0.244	0.643
Het: Chief Taxed Here vs. Elsewhere	0.000	0.892
Panel B: Wilcoxon rank sum test		
Chief Taxed vs. Did Not Tax	0.046	0.749
Het: Chief Taxed Here vs. Did Not Tax	0.073	0.278
Het: Chief Taxed Elsewhere vs. Did Not Tax	0.773	0.342
Het: Chief Taxed Here vs. Elsewhere	0.150	0.676

Notes: This table summarizes the results of two tests of the equality of distributions. Panel A reports the p-values of the Kolmogorov-Smirnov test. Panel B reports the p-values of the Wilcoxon rank sum test, and data is clustered by neighborhood. In each panel, Row 1 compares the wealth characteristic distribution of the neighborhoods where the chief taxed with those where the chief did not tax. Row 2 compares the distribution of the neighborhoods where the chief taxed directly (here) with those where the chief did not collect taxes at all. Row 3 compares the distribution of the neighborhoods where the local chief did not collect taxes directly but collected taxes in another neighborhood (elsewhere) with those where the chief did not collect taxes at all. Row 4 compares the distribution of the neighborhoods where the chief taxed directly (here) with those where the local chief did not collect taxes directly but collected taxes in another neighborhood (elsewhere). Each column corresponds to a characteristic of ticket recipients. Column 1 is the pooled quality (wealth) index, which is a standardized index including Wall Quality, Erosion Threat, Road Quality, and Accessibility. Column 2 is monthly income. We discuss these results in Section 5.1.

Figure A1: Distributions of Wealth Index
A: By Chief Taxed Neighborhood Directly

Wealth distribution of ticket receivers
Collector chiefs v. non-collector chiefs

Cumulative Probability

Wealth Index

— Chief Did Not Tax
— Chief Taxed

B: By Location of Chief Tax Collection

Wealth distribution of ticket receivers
Collector chiefs v. non-collector chiefs

Cumulative Probability

Wealth Index

— Chief Did Not Tax
— Chief Taxed Elsewhere
— Chief Taxed Here

Notes: This figure shows cumulative distribution functions of the house quality index of ticket recipients. The measure is a standardized index including wall quality, erosion threat, road quality, and accessibility. Panel A compares the distributions for the neighborhoods where the chief taxed and for those where the chief did not tax. Panel B compares the distributions for the neighborhoods where the chief taxed directly (here), for those where the local chief did not collect taxes directly but collected taxes in another neighborhood (elsewhere), and for those where the chief did not collect taxes at all. We discuss these results in Section 5.1.

Figure A2: Distributions of Monthly Household Income
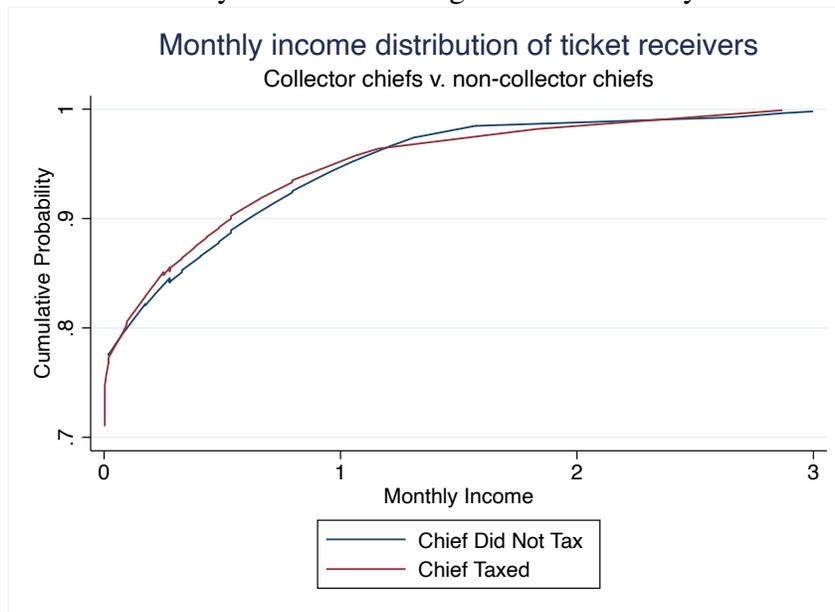A: By Chief Taxed Neighborhood Directly
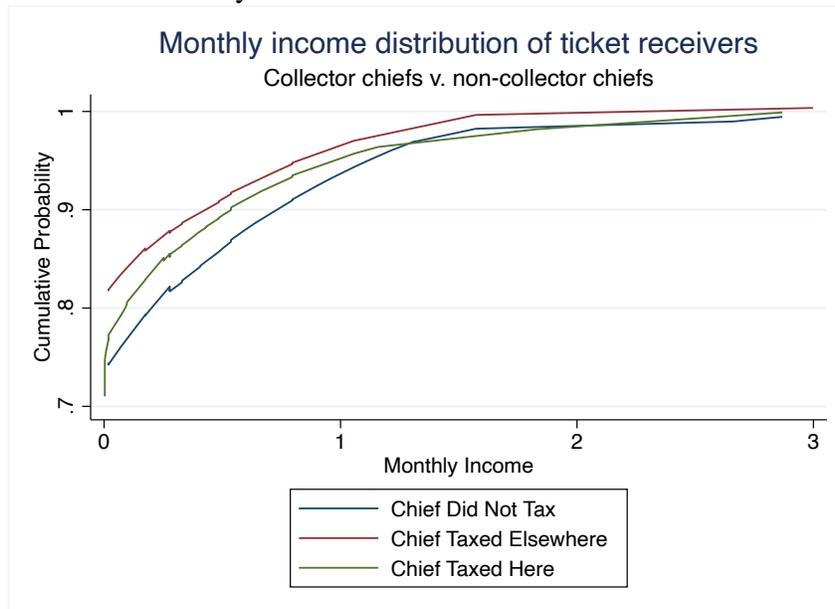

B: By Location of Chief Tax Collection

Notes: This figure shows cumulative distribution functions of the monthly income of ticket recipients. Panel A compares the distributions for the neighborhoods where the chief taxed and for those where the chief did not tax. Panel B compares the distributions for the neighborhoods where the chief taxed directly (here), for those where the local chief did not collect taxes directly but collected taxes in another neighborhood (elsewhere), and for those where the chief did not collect taxes at all. We discuss these results in Section 5.1.

Table A5: Error Rate by Consumption, Liquidity, and Assets

	Consumption			Liquidity index			Asset index		
	Exclusion error	Inclusion error	Mean (ticket recipients)	Exclusion error	Inclusion error	Mean (ticket recipients)	Exclusion error	Inclusion error	Mean (ticket recipients)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Comparing neighborhoods by whether local chief was tax collector there									
Chief taxed here (Local)	0.047	-0.006	-0.004	0.026	-0.013	-0.054	0.143	-0.021	-0.002
	(0.058)	(0.020)	(0.073)	(0.053)	(0.021)	(0.084)	(0.235)	(0.015)	(0.055)
R^2	0.156	0.027	0.102	0.123	0.025	0.174	0.195	0.022	0.114
Observations	245	2126	621	300	2072	621	139	6131	4385
Clusters	106	221	208	146	221	208	25	221	220
Control Mean	.692	.268	.073	.76	.277	.115	.262	.707	.042
Panel B: Comparing neighborhoods by whether local chief collected anywhere									
Chief taxed elsewhere (Central)	0.299***	-0.044	-0.204**	-0.079	-0.081***	0.115	-0.802**	0.001	-0.089
	(0.088)	(0.030)	(0.091)	(0.084)	(0.030)	(0.128)	(0.291)	(0.024)	(0.087)
Chief taxed here (Local)	0.184***	-0.029	-0.105	-0.012	-0.055**	0.002	-0.302	-0.020	-0.048
	(0.068)	(0.027)	(0.102)	(0.064)	(0.027)	(0.099)	(0.291)	(0.020)	(0.083)
R^2	0.185	0.028	0.108	0.126	0.028	0.175	0.222	0.022	0.115
Observations	245	2126	621	300	2072	621	139	6131	4385
Clusters	106	221	208	146	221	208	25	221	220
Control Mean	.657	.298	.019	.769	.314	.022	.342	.714	.018
p-value Test Col. here vs. elsewhere	0.123	0.524	0.123	0.355	0.279	0.312	0.000	0.255	0.475

Notes: This table compares the error rate in neighborhoods where the chief taxed with those where the chief did not tax (the excluded category) (Panel A) and neighborhoods where the chief taxed directly (here), where the local chief did not collect taxes directly but collected taxes in another neighborhood (elsewhere), and did not collect taxes at all (excluded category) (Panel B). The outcomes are defined as errors of inclusion or exclusion using measures of household consumption (Columns 1–3), liquidity (Columns 4–6), and assets (Columns 7–9). The first two columns for each outcome are exclusion and inclusion error, respectively, and the third column is the mean difference in the outcome among ticket recipient households. All regressions include tax stratum fixed effects and cluster standard errors at the neighborhood level. We discuss these results in Section 5.1.

Table A6: Effects of Chief Tax Collection on Targeting by Wealth — No Sampling Weights

	Analysis:	By wealth status		By wealth level				
	Outcome:	Exclusion error	Inclusion error	Exclusion error		Inclusion error		Average Wealth
		Any error		(very poor)	(near poor)	(middle)	(rich)	
	Sample:	Full	0% - 20%	21% - 100%	0% - 10%	11% - 20%	21% - 60%	61% - 100%	Recipients
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Comparing neighborhoods by whether chief taxed there									
Chief taxed here (Local)		-0.042***	-0.071**	-0.040**	-0.074	-0.056	-0.024	-0.056**	-0.120
		(0.016)	(0.034)	(0.017)	(0.054)	(0.045)	(0.024)	(0.022)	(0.076)
R^2		0.018	0.076	0.028	0.172	0.098	0.035	0.051	0.157
Observations		6267	1000	5267	446	554	2444	2823	4384
Clusters		221	216	221	193	195	218	221	220
Control Mean		.663	.36	.72	.348	.372	.715	.725	.065
Panel B: Comparing neighborhoods by whether chief taxed anywhere									
Chief taxed elsewhere (Central)		-0.006	-0.039	-0.003	0.012	-0.099	0.045	-0.046	-0.091
		(0.023)	(0.053)	(0.026)	(0.081)	(0.074)	(0.034)	(0.031)	(0.114)
Chief taxed here (Local)		-0.045**	-0.090**	-0.042*	-0.068	-0.106*	0.001	-0.079***	-0.167*
		(0.020)	(0.043)	(0.022)	(0.064)	(0.059)	(0.031)	(0.028)	(0.098)
R^2		0.018	0.077	0.028	0.172	0.101	0.036	0.052	0.158
Observations		6267	1000	5267	446	554	2444	2823	4384
Clusters		221	216	221	193	195	218	221	220
Control Mean		.668	.363	.727	.336	.388	.708	.744	.043
p-value test: tax here vs. elsewhere		0.040	0.235	0.067	0.257	0.896	0.101	0.206	0.407

Notes: This table examines the impact of chiefs collecting taxes on their allocation of cash transfer program tickets without adjusting for sampling weights. Specifically, Panel A compares error rates in neighborhoods where the chief taxed with those where the chief did not tax (the excluded category), and Panel B compares neighborhoods where the chief taxed directly (here), where the local chief did not collect taxes directly but collected taxes in another neighborhood (elsewhere) to neighborhoods where the chief did not collect taxes at all (excluded category). In this table, errors are determined using an index of measures from the pre-specified wealth index without the application of the sampling weights described in Section 3.2. Columns 1-7 examine errors of inclusion (non-poor households receiving program tickets) and exclusion (poor households failing to receive program tickets). Specifically, in column 1, the outcome is any error (inclusion or exclusion), estimated in the full population of households for which the wealth index measure is available. In columns 2 and 3, the outcome is errors of exclusion among households in the bottom quintile of the wealth distribution and inclusion among households above the bottom quintile, respectively. Columns 4 and 5 consider errors of exclusion for the very poor and near poor. Columns 6 and 7 consider errors of inclusion for the middle and rich categories. Column 8 shows the average wealth level among program ticket recipients. All regressions include tax stratum fixed effects and cluster standard errors at the neighborhood level. We discuss these results in Section 5.1.

Table A7: Effects of Chief Tax Collection on Targeting by Wealth and Assets — No Sampling Weights

	Analysis:		By wealth status		By wealth level				
	Outcome:	Any error	Exclusion error	Inclusion error	Exclusion error (very poor)	(near poor)	Inclusion error (middle)	(rich)	Average Wealth
	Sample:	Full	0% - 20%	21% - 100%	0% - 10%	11% - 20%	21% - 60%	61% - 100%	Recipients
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Comparing neighborhoods by whether chief taxed there									
Chief taxed here (Local)		-0.041***	-0.051	-0.039**	-0.097*	-0.035	-0.028	-0.044*	-0.112
		(0.015)	(0.032)	(0.017)	(0.050)	(0.044)	(0.021)	(0.023)	(0.069)
R^2		0.019	0.072	0.028	0.171	0.092	0.038	0.049	0.149
Observations		6267	1057	5210	483	574	2543	2667	4384
Clusters		221	218	221	204	194	221	221	220
Control Mean		.656	.346	.718	.359	.335	.723	.713	.056
Panel B: Comparing neighborhoods by whether chief taxed anywhere									
Chief taxed elsewhere (Central)		-0.025	-0.072	-0.010	-0.010	-0.139*	0.040	-0.059*	-0.194*
		(0.023)	(0.051)	(0.026)	(0.072)	(0.074)	(0.031)	(0.034)	(0.107)
Chief taxed here (Local)		-0.054***	-0.089**	-0.044**	-0.102*	-0.111*	-0.008	-0.075***	-0.213**
		(0.019)	(0.042)	(0.022)	(0.058)	(0.058)	(0.027)	(0.028)	(0.091)
R^2		0.020	0.075	0.028	0.171	0.099	0.039	0.051	0.152
Observations		6267	1057	5210	483	574	2543	2667	4384
Clusters		221	218	221	204	194	221	221	220
Control Mean		.669	.36	.726	.346	.373	.715	.737	.088
p-value test: tax here vs. elsewhere		0.140	0.668	0.112	0.162	0.629	0.053	0.568	0.815

Notes: This table examines the impact of chiefs collecting taxes on their allocation of cash transfer program tickets without adjusting for sampling weights. Specifically, Panel A compares error rates in neighborhoods where the chief taxed with those where the chief did not tax (the excluded category), and Panel B compares neighborhoods where the chief taxed directly (here), where the local chief did not collect taxes directly but collected taxes in another neighborhood (elsewhere) to neighborhoods where the chief did not collect taxes at all (excluded category). In this table, errors are determined using an index of measures from the pre-specified wealth index as well as a individual indicators for possessing electricity, a vehicle, a fence, and a roof of concrete, tiles, or sheet iron (as opposed thatch, straw, or bamboo), without the application of the sampling weights described in Section 3.2. Columns 1-7 examine errors of inclusion (non-poor households receiving program tickets) and exclusion (poor households failing to receive program tickets). Specifically, in column 1, the outcome is any error (inclusion or exclusion), estimated in the full population of households for which the wealth index measure is available. In columns 2 and 3, the outcome is errors of exclusion among households in the bottom quintile of the wealth distribution and inclusion among households above the bottom quintile, respectively. Columns 4 and 5 consider errors of exclusion for the very poor and near poor. Columns 6 and 7 consider errors of inclusion for the middle and rich categories. Column 8 shows the average wealth level among program ticket recipients. All regressions include tax stratum fixed effects and cluster standard errors at the neighborhood level. We discuss these results in Section 5.1.

Table A8: Effects of Chief Tax Collection on Targeting by Wealth — Including Control for Neighborhood Mean

	Analysis:		By wealth status		By wealth level				
	Outcome:	Any error	Exclusion error	Inclusion error	Exclusion error (very poor)	(near poor)	Inclusion error (middle)	(rich)	Average Wealth
	Sample:	Full	0% - 20%	21% - 100%	0% - 10%	11% - 20%	21% - 60%	61% - 100%	Recipients
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Comparing neighborhoods by whether chief taxed there									
Chief taxed here (Local)		-0.046***	-0.059***	-0.023**	-0.077**	-0.043	-0.013	-0.032**	-0.050**
		(0.016)	(0.021)	(0.009)	(0.038)	(0.030)	(0.013)	(0.013)	(0.020)
R^2		0.011	0.075	0.007	0.217	0.091	0.020	0.024	0.325
Observations		6267	1000	5267	446	554	2444	2823	4384
Clusters		221	216	221	193	195	218	221	220
Control Mean		.317	.898	.163	.897	.899	.159	.166	.065
Panel B: Comparing neighborhoods by whether chief taxed anywhere									
Chief taxed elsewhere (Central)		-0.022	-0.008	-0.010	0.023	-0.024	0.023	-0.042**	-0.033
		(0.024)	(0.024)	(0.014)	(0.039)	(0.039)	(0.019)	(0.021)	(0.028)
Chief taxed here (Local)		-0.072***	-0.064***	-0.028**	-0.066	-0.056	-0.001	-0.054***	-0.082***
		(0.018)	(0.024)	(0.012)	(0.040)	(0.037)	(0.017)	(0.017)	(0.026)
R^2		0.032	0.075	0.008	0.217	0.092	0.021	0.026	0.331
Observations		6267	1000	5267	446	554	2444	2823	4384
Clusters		221	216	221	193	195	218	221	220
Control Mean		.328	.897	.169	.891	.903	.157	.181	.041
p-value test: tax here vs. elsewhere		0.019	0.027	0.120	0.052	0.364	0.123	0.463	0.039
Neighborhood mean control		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table examines the robustness impact of chiefs collecting taxes on their allocation of cash transfer program tickets presented in Table 2 by including in each regression a control for the leave-one-out neighborhood mean of the pre-specified wealth index. The leave-one-out mean value for an individual observations is calculated as the average of the pre-specified wealth index among all other properties in the neighborhood. Panel A compares error rates in neighborhoods where the chief taxed with those where the chief did not tax (the excluded category), and Panel B compares neighborhoods where the chief taxed directly (here), where the local chief did not collect taxes directly but collected taxes in another neighborhood (elsewhere) to neighborhoods where the chief did not collect taxes at all (excluded category). In this table, errors are determined using an index of measures from the pre-specified wealth index and estimated on sample-weighted data, as described in Section 3.2. Columns 1-7 examine errors of inclusion (non-poor households receiving program tickets) and exclusion (poor households failing to receive program tickets). Specifically, in column 1, the outcome is any error (inclusion or exclusion), estimated in the full population of households for which the wealth index measure is available. In columns 2 and 3, the outcome is errors of exclusion among households in the bottom quintile of the wealth distribution and inclusion among households above the bottom quintile, respectively. Columns 4 and 5 consider errors of exclusion for the very poor and near poor. Columns 6 and 7 consider errors of inclusion for the middle and rich categories. Column 8 shows the average wealth level among program ticket recipients. All regressions include tax stratum fixed effects and cluster standard errors at the neighborhood level. We discuss these results in Section 5.1.

Table A9: Effects of Chief Tax Collection on Targeting by Income — Including Control for Neighborhood Mean

	Analysis:		By income status		By income level				
	Outcome:	Any error	Exclusion error	Inclusion error	Exclusion error (very poor)	(near poor)	Inclusion error (middle)	(rich)	Average Income
	Sample:	Full	0% - 20%	21% - 100%	0% - 10%	11% - 20%	21% - 60%	61% - 100%	Recipients
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Comparing neighborhoods by whether local chief was tax collector there									
Chief taxed here (Local)		-0.057**	-0.105*	-0.034	-0.130	-0.081	-0.011	-0.051*	-0.040
		(0.026)	(0.054)	(0.022)	(0.107)	(0.067)	(0.035)	(0.027)	(0.062)
R^2		0.030	0.169	0.026	0.308	0.295	0.067	0.049	0.135
Observations		2314	352	1963	128	224	954	1009	603
Clusters		220	213	221	128	181	220	221	206
Control Mean		.337	.721	.268	.738	.712	.288	.249	.085
Panel B: Comparing neighborhoods by whether local chief collected anywhere									
Chief taxed elsewhere (Central)		-0.068*	0.051	-0.059*	0.147	-0.009	-0.029	-0.084**	-0.146
		(0.037)	(0.078)	(0.031)	(0.156)	(0.097)	(0.053)	(0.036)	(0.098)
Chief taxed here (Local)		-0.093***	-0.080	-0.064**	-0.058	-0.085	-0.026	-0.094***	-0.111
		(0.034)	(0.064)	(0.028)	(0.126)	(0.075)	(0.044)	(0.034)	(0.084)
R^2		0.032	0.170	0.028	0.316	0.295	0.067	0.052	0.139
Observations		2314	352	1963	128	224	954	1009	603
Clusters		220	213	221	128	181	220	221	206
CompMean		.362	.700	.299	.697	.702	.293	.304	.019
p-value Test Col. here vs. elsewhere		0.395	0.054	0.813	0.145	0.407	0.949	0.728	0.632
Neighborhood mean control		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table examines the robustness impact of chiefs collecting taxes on their allocation of cash transfer program tickets presented in Table 3 by including in each regression a control for the leave-one-out neighborhood mean of the monthly income. In this table, errors are determined using the reported household monthly incomes. The leave-one-out mean value for an individual observations is calculated as the average of the monthly income among all other properties in the neighborhood. Panel A compares error rates in neighborhoods where the chief taxed with those where the chief did not tax (the excluded category), and Panel B compares neighborhoods where the chief taxed directly (here), where the local chief did not collect taxes directly but collected taxes in another neighborhood (elsewhere) to neighborhoods where the chief did not collect taxes at all (excluded category). Columns 1-7 examine errors of inclusion (non-poor households receiving program tickets) and exclusion (poor households failing to receive program tickets). Specifically, in column 1, the outcome is any error (inclusion or exclusion), estimated in the full population of households for which the income measure is available. In columns 2 and 3, the outcome is errors of exclusion among households in the bottom quintile of the income distribution and inclusion among households above the bottom quintile, respectively. Columns 4 and 5 consider errors of exclusion for the very poor and near poor. Columns 6 and 7 consider errors of inclusion for the middle and rich categories. Column 8 shows the average income level among program ticket recipients. All regressions include tax stratum fixed effects and cluster standard errors at the neighborhood level. We discuss these results in Section 5.1.

Table A10: Error Rate by Chief Knowledge of the Inhabitants of the Neighborhood

Chief Information:	Name	Education level	Occupation	Index
	(1)	(2)	(3)	(4)
Chief Info > Median	-0.026	-0.025	-0.028	-0.040**
	(0.019)	(0.019)	(0.019)	(0.019)
R^2	0.010	0.009	0.011	0.023
Observations	221	221	221	221
Mean	.276	.276	.276	.276

Notes: This table shows the relationship between chiefs' knowledge of the inhabitants of the neighborhood and the average error rate in the neighborhood. The error rate is defined using a pre-registered wealth index constructed from observable household attributes and estimated on sample-weighted data, as described in Section 3.2. Chief's knowledge of the inhabitant of the neighborhood is measured by the percentage of correct answered when asked to provide the name (Column 1), education level (Column 2), and occupation (Column 3) of a randomly selected group of 12 property owners per neighborhood. Column 4, measures chief knowledge using a standardized index of chief's knowledge along these three dimensions. All regressions include an indicator for whether the city chief collected taxes in the neighborhood and robust standard errors. We discuss these results in Section 6.1.

Table A11: Salongo Participation

Dependent variable	$\hat{\beta}$	SE	R^2	N	$\bar{x}_{ChiefNotTax}$
	(1)	(2)	(3)	(4)	(5)
Panel A: Contributions to Salongo					
Any contribution to Salongo	-0.007	0.028	0.066	2380	0.404
Hours contributed to Salongo	1.263	0.853	0.069	903	10.187
Panel B: Views of Salongo					
Fairness of Salongo	0.013	0.051	0.034	2376	-0.007
Importance of Salongo	-0.024	0.059	0.043	2380	0.004
Obligation to do Salongo	-0.028	0.068	0.059	2380	0.008
Obligation for taxpayers	-0.041	0.072	0.048	2380	0.018
Obligation for nonpayers	-0.062	0.076	0.062	2380	0.028
Panel C: Incidence of Salongo					
Who contributes - men not women	-0.008	0.060	0.046	2380	0
Who contributes - poor not rich	0.003	0.063	0.042	2380	0.008
Panel D: Sanctions for non-contributors to Salongo					
Likelihood of sanctions for non-contributors	0.030	0.047	0.052	2380	-0.018
Severity of sanctions for non-contributors	0.020	0.047	0.048	2380	-0.012

Notes: This table compares salongo participation in neighborhoods where the chief taxed with those where the chief did not tax (the excluded category). Each row summarizes an OLS estimation of equation (1), comparing neighborhoods where chiefs taxed to those where they did not, with the dependent variable 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 excluded group mean $\bar{x}_{ChiefNotTax}$. Panel A shows estimated differences in citizen-reported contributions to salongo on the extensive margin (an indicator for participating in salongo in the past month) and on the intensive margin (the number of hours contributed to salongo in the past month). Panel B shows estimated differences in citizens' reported views of salongo, including whether (i) it is fair that household must contribute to salongo, (ii) salongo is important for the development of the neighborhood, (iii) salongo is an obligation for all households in the neighborhood, (iv) salongo is an obligation for households who paid the property tax this year, (v) salongo is an obligation for housholds who did not pay the property tax this year. Panel C shows estimated differences in the perceived incidence of salongo including whether (i) women are more solicited for salongo than men, (ii) poor rather than rich household are more solicited for salongo. Panel D reports estimated differences in households' perceived sanctions for non contribution to salongo both in terms of the likelihood and severity of sanctions. We discuss these results in Section 6.2.

Table A12: Experimental Design: Cross-Randomized Arms

	No Info	**Info**	**Info + Audit**
Chiefs do not collect taxes	T1 (28)	T2 (32)	T3 (50)
Chiefs collect taxes	T4 (31)	T5 (29)	T6 (51)

Notes: The number of clusters (neighborhoods) are shown in parentheses. We discuss this table in Section 6.3.

Table A13: Timeline: Cross-Randomized Arms

Activity	Actor	Timing	N	J
Anti-poverty program		Jun-Oct 2019		
1. Flier and audit form distribution	Socico		4,317	162
2. Audit form submission	Citizens		2,706	101
3. Ticket distribution	Chiefs		4,401	221
4. Lottery	Chiefs & DIVAS		221	221
5. Cash transfer distribution	Chiefs		1,105	221
6. Community audit meetings	Socico	Dec 2019-Feb 2020	1,658	11

Notes: Notes: N = number of observations, J = number of clusters (neighborhoods). We discuss this table in Section 6.3.

Figure A3: Information Form Example

REPUBLIQUE DEMOCRATIQUE DU CONGO
PROVINCE DU KASAÏ OCCIDENTAL
MINISTERE DES AFFAIRES SOCIALES, ACTION HUMANITAIRE ET
SOLIDARITE NATIONALE (DIVAS)

Announcement : Development Program

The Ministry of Social Affairs of the Provincial Government of Kasaï Central has the honor of announcing a development program that will take place in this avenue in the coming weeks. For this program, several residents will receive cash transfers with the aim of boosting local development. Unfortunately, there is not enough for everyone. Thus, there will a lottery to choose 5 winners on this avenue.

[Chief] will distribute lottery tickets to inhabitants of this avenue from **[date training]** to **[date_draw]**. See the chief for more information.

Informational flier for property owner **[name]** in the compound **[compound]**. Note : this flier does not guarantee receipt of a ticket.

Figure A4: Audit Form Example

Do you want an audit and verification meeting?

As part of this program, you and other people in your avenue can **request an audit and verification meeting** organized by a civil society organization in Kananga. This is an opportunity for you as a [citizen/taxpayer] to learn more about this program and whether it was implemented properly and fairly. The meeting can focus on the actions taken by the **Division of Social Affairs**, by your **avenue chief**, or **both** in this development program.

IMPORTANT : The civil society organization will only organize a meeting for your avenue if many residents request one.

➢ To **request an audit meeting of [Actor1]**, submit the [COLOR] form to the [COLOR] drop box located at [ADDRESS1].
➢ To **request an audit meeting of [Actor2]**, submit the [COLOR] form to the [COLOR] drop box located at [ADDRESS2].

To request meetings involving **both** actors, submit both forms to the correct boxes. Everything you write will be kept confidential from the concerned parties. All forms must be submitted by **[date]**.

The avenues that submit the most requests (as a share of all households) will get top priority to receive an audit meeting. Your action is important!

REQUEST MEETING of the **[DIVAS/Chef].**	REQUEST MEETING of the **[DIVAS/Chef].**
To request a meeting of the [DIVAS/Chef], please **deposit this form into the locked box at :**	To request a meeting of the [DIVAS/Chef], please **deposit this form into the locked box at :**
[LOCATION].	**[LOCATION]**.
The box will have show this colored stamp:	The box will have show this colored stamp:
[COLOR STAMP]	[COLOR STAMP]
Request of the compound: [Code]	Request of the compound: [Code]

Table A14: Cross-Randomization Balance: Balán et al. (2022) Characteristics

	N (1)	No Info Mean (2)	Info (3)	Coll. Act. (4)
Panel A: Property Owner Characteristics				
Years of Education[B]	2319	10.489	-0.079 (0.325)	-0.138 (0.325)
Electricity[B]	2329	0.126	0.022 (0.021)	0.001 (0.020)
Log HH Monthly Income[B]	2307	10.695	-0.247 (0.199)	-0.241 (0.194)
Trust of Chief[B]	2319	3.096	0.198** (0.080)	0.029 (0.078)
Trust of National Government[B]	2193	2.534	0.091 (0.081)	-0.028 (0.070)
Trust Provincial Government[B]	2209	2.480	0.051 (0.085)	-0.076 (0.074)
Trust of Tax Ministry[B]	2189	2.374	-0.015 (0.080)	-0.104 (0.073)
Sex[M] (1 = male)	14134	0.768	0.008 (0.016)	-0.007 (0.014)
Age[M]	12554	54.648	-0.681 (0.627)	0.928* (0.551)
Majority Tribe[M]	14582	0.773	0.027 (0.036)	0.022 (0.030)
Employed[M]	15627	0.730	0.002 (0.020)	-0.018 (0.018)
Salaried[M]	15628	0.246	-0.004 (0.016)	0.012 (0.013)
Works for Government[M]	15628	0.147	-0.000 (0.012)	0.020** (0.009)
Relative Works for Government[M]	17376	0.229	0.026 (0.017)	0.030** (0.014)
Panel B: Property Characteristics				
House Quality[M]	17719	0.001	0.004 (0.133)	0.031 (0.111)
Distance to State Buildings and City Center[R]	28598	1.445	0.083 (0.121)	-0.128 (0.108)
Distance to Health Institutions[R]	28598	0.316	0.048 (0.035)	-0.007 (0.028)
Distance to Education Institutions[R]	28598	0.605	0.078 (0.057)	0.004 (0.051)
Distance to Roads[R]	27984	0.385	-0.029 (0.064)	0.011 (0.062)
Distance to Eroded Areas[R]	27984	0.133	-0.006 (0.017)	-0.009 (0.017)
Panel C: Neighborhood Characteristics				
Per Capita Property Tax Revenues in 2016[B]	221	169.070	-77.185 (82.604)	-90.249 (77.007)
Affected by Conflict in 2017[B]	221	0.020	-0.025 (0.033)	-0.008 (0.030)

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 indicators for the cross-randomized treatment arms, including randomization stratum fixed effects and clustering standard errors at the neighborhood level. The control arm is the omitted category. Superscripts B, M, and R denote variables from baseline, midline, and registration, respectively, from Balán et al. (2022). Balance tests for wealth and asset characteristics are shown in Table A15. We discuss these results in Section 6.3.

Table A15: Cross-Randomization Balance of Wealth and Asset Measures

	N	No Info Mean	Info	Audit
	(1)	(2)	(3)	(4)
Wall Quality	6152	0.0312	-0.0008	0.0122
			(0.0620)	(0.0533)
Roof Quality	6153	0.0093	0.0204	0.0367
			(0.0573)	(0.0465)
Erosion Threat	6267	0.0182	0.0290	-0.0157
			(0.1094)	(0.0979)
Road Quality	6267	-0.0371	0.1560	-0.0133
			(0.1241)	(0.1206)
Accessibility to Main Avenue	6267	0.0397	0.1156	-0.1213
			(0.1282)	(0.1094)
Compound Has Fence	6153	-0.0027	0.1075	0.0065
			(0.1050)	(0.0879)
Electricity Access	6069	0.0132	0.0303	0.0551
			(0.0857)	(0.0790)
Vehicle Ownership	6270	0.0346	-0.0411	-0.0195
			(0.0830)	(0.0780)
F, p (vs. No Info)			1.149, 0.336	1.131, 0.345
F, p (Info vs. Audit)			1.540, 0.147	

Notes: This table reports the coefficients from balance tests estimated by regressing wealth and asset measures on indicators for the cross-randomized treatments, including randomization stratum fixed effects and clustering standard errors at the neighborhood level. Control neighborhoods form the omitted category. Measures of characteristics are drawn from households surveys for the sample described in Section 3 and reflect the measures in the pre-specified wealth index as well as measures of assets considered in Table A3. The bottom row contains the statistics for tests of the omnibus null hypothesis that the treatment effects for the covariates studied in the table are all zero using parametric F tests, using regressions that include stratum fixed effects and cluster standard errors at the neighborhood level. We discuss these results in Section 6.3.

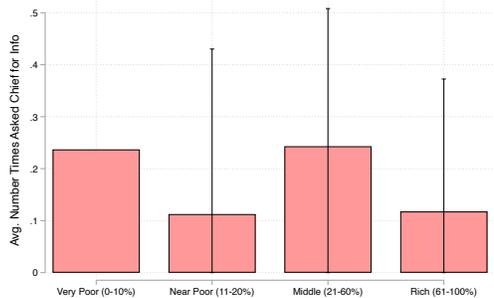# Figure A5: Asking Chief for Information and Receiving Program Ticket by Wealth Status
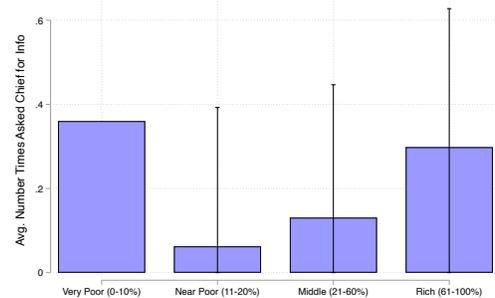

A. Chief Did Not Tax — Asked for Info.
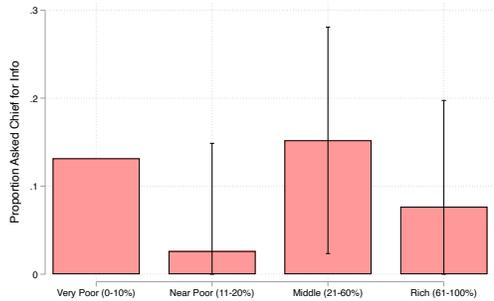
B. Chief Taxed — Asked for Info.
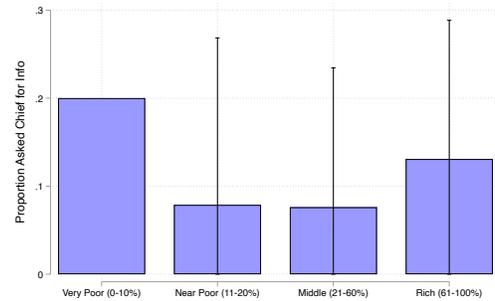
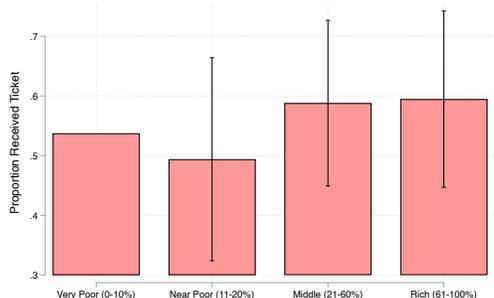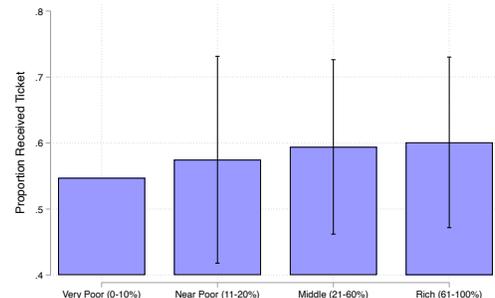C. Chief Did Not Tax — # Times Asked for Info.

D. Chief Taxed — # Times Asked for Info.

E. Chief Did Not Tax — Received Program Ticket

F. Chief Taxed — Received Program Ticket

Notes: This figure shows levels of engagement with the chief for the measures described in Table 10 by household wealth groups. These measures include the likelihood of asking the chief for information about the program (Panels A and B), the number of times asked (Panels C and D), and whether a household received a program ticket (Panels E and F), separately by whether a chief collected taxes (in blue: Panels B, D, and F) or not (in red: Panels A, C, and E). The figure in each panel plots the mean level of the outcome across wealth groups. Vertical bars represent 95% confidence intervals and are truncated at zero for readability. We discuss these results in Section 6.3.

Figure A6: Audit Form Submission Rates by Neighborhood

A: Chiefs

B: DIVAS
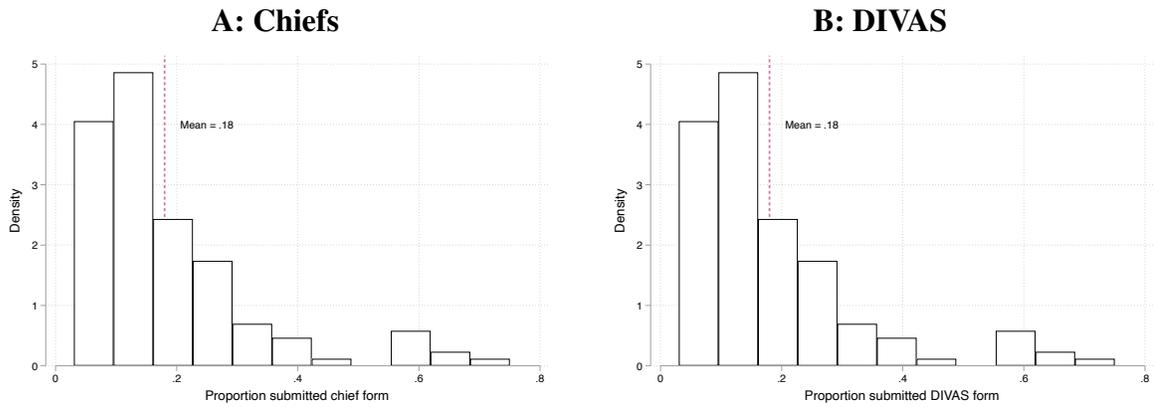
Notes: This figure shows the distributions of rates of audit form submission at the neighborhood level for neighborhoods in the Audit arm. Panel A shows the distribution of the rate of submission for requesting audit meetings for chiefs, and Panel B shows the distribution of the rate of submission for DIVAS. We discuss this table in Section 6.3.