

**SUPPLEMENTARY DATA AND APPENDIX**  
for  
THE PARTICIPATION DIVIDEND OF TAXATION:  
How Citizens In Congo Engage More  
With The State When It Tries To Tax Them  
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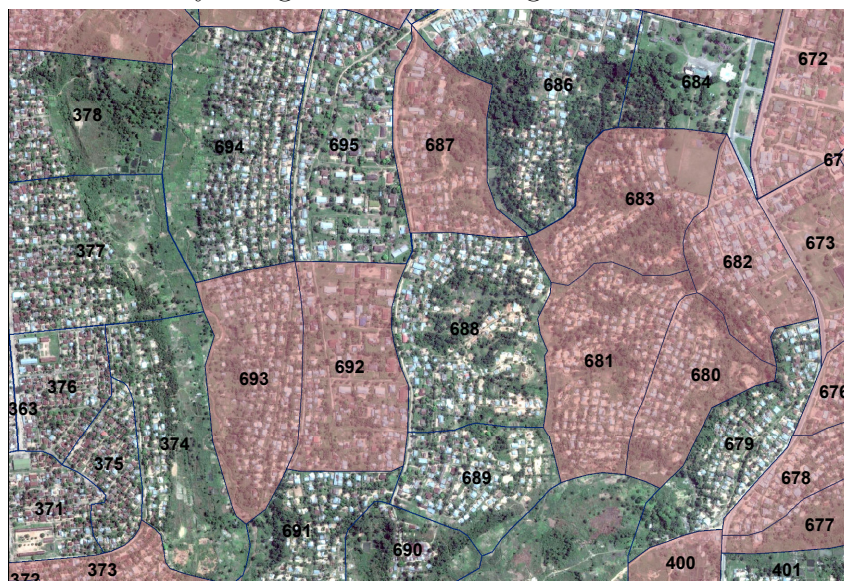
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# A1 Background Information

## A1.1 The 2016 property tax campaign

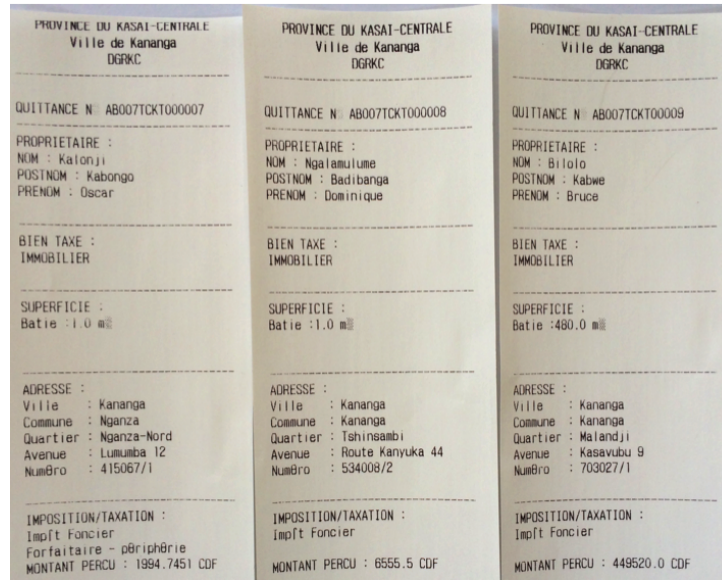
As noted in the paper, random assignment of the 2016 property tax campaign occurred on the neighborhood level. Figure A1 shows a zoomed-in version of neighborhoods in one part of the city, and Figure A7 shows the extent of neighborhoods across the city. Neighborhood boundaries approximate roads, ravines, and other landmarks that are easily recognizable from the ground.



**Figure A1:** Neighborhoods in Kananga (red indicates treatment)

Tax collectors used tablets and handheld receipt printers. Receipts contained the unique tax IDs assigned by collectors during property registration (Figure A2). Only printed receipts were considered valid for the campaign.

Initially, the campaign was meant to include a rental tax, levied on income from residential renters, as well as the property tax. However, the government subsequently dropped the rental tax from the campaign. This decision seemed at first to be caused by the fact that Hologram Identification Systems' tablet application could not support the rental tax. Even after Hologram had fixed this technical issue (two months into the campaign), though, the government did not change campaign protocols to include renters. Thus, the 2016 campaign focused exclusively on the property tax.



**Figure A2:** Examples of printed receipts from the property tax campaign.

## A1.2 Bribe payment

A natural concern about field-based tax collection is bribery (Khan et al., 2015). If collectors collect more bribes than taxes, a door-to-door campaign could fail to be revenue positive; it could also generate backlash among citizens. Such backlash is an important outcome in its own right, and would constitute another possible channel through which the tax campaign might affect participation. However, this section shows that there is little evidence that the tax campaign increased bribe payment. Indeed, levels of bribe payment were low across treatment groups.

Two types of collusive bribes are plausible in this context. First, households could have paid the collector a smaller amount than the tax,  $b < \tau_1$ , where  $\tau_1$  in this case is a flat household liability, say 2,000 Congolese Francs (CF). The collector might have accepted  $b$  in exchange for a promise not to enforce the true liability. The clearest way to measure such bribes is through household self-reports using local codes for bribes. This type of bribe is typically known as paying the “transport” of the tax collector, or making an “arrangement.”<sup>1</sup> The measure of bribe self reports, *Paid bribe*, takes the value of 1 if a participant reported paying a bribe according to these local codes. I examine the intensive margin using self reports of the amount

<sup>1</sup>Other local codes for bribes include giving the collector a “coffee,” “tea,” “beer,” and “water.”



paid, *Bribe amount*.

Second, ‘midrange’ houses, which face a higher liability, could have paid the collector  $\tau_1 + b < \tau_2$ , where  $\tau_2$  here is the flat tax of 6,600 CF.<sup>2</sup> In this case, the owner of a midrange house would have paid the 2,000 CF rate plus a bribe in exchange for “reclassifying” the house to the lower tax rate. To measure reclassification bribes, a property tax expert from the provincial tax ministry, who did not conduct field-based tax collection but who had initially trained all tax collectors, examined photographs of each house in the sample. Examining each photo on a tablet, one at a time, he assigned to each the correct rate, without knowledge of what the field-based collectors assigned (or the identity of the assigned collector). Comparing the two rates enables me to identify mismatches, in which the household should have paid 6,600 CF but actually paid 2,000 CF. Although it is possible that collectors made mistakes when classifying households, this is unlikely because (a) the key distinction between houses that should pay 6,600 CF and 2,000 CF is obvious (based on the type of building material), and (b) collectors were incentivized to tax households at the true rate because their bonus was a constant percentage of what they deposited to the state. *Paid bribe* equals 1 in such cases of reclassification bribes, as well.<sup>3</sup>

Measurement error is always a concern when asking about bribes. However, paying small bribes is not taboo in this context. In another project on bribe payment at Kananga’s roadway tolls, nearly 50% of participants openly told enumerators that they bribed the toll officer to avoid paying the full amount (Reid and Weigel, 2020). Nonetheless, to assuage concerns about self-reported bribes, I also collected two other measures of bribes. First, *Others bribe* is the perceived frequency of bribe payment among other households on the respondent’s street. Second, *Going rate* is the respondent’s estimate of ‘how much would one have to give the tax collector so that he/she will pass to the next house?’ These bribe measures are inspired by Khan et al. (2015).

Table A1 shows the average effects of the tax campaign on bribe payment across five measures from two different samples. No coherent pattern emerges. When examining self-reported bribes in the midline or endline data, the treatment effect is

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<sup>2</sup>Midrange houses make up less than 10% of the properties in Kananga.

<sup>3</sup>Payment of bribes is analogous in control neighborhoods. Households could have paid bribes to field-based collectors who strayed outside treated neighborhoods, or they could have paid bribes at the tax ministry (where all control individuals were in principle supposed to have paid the tax).

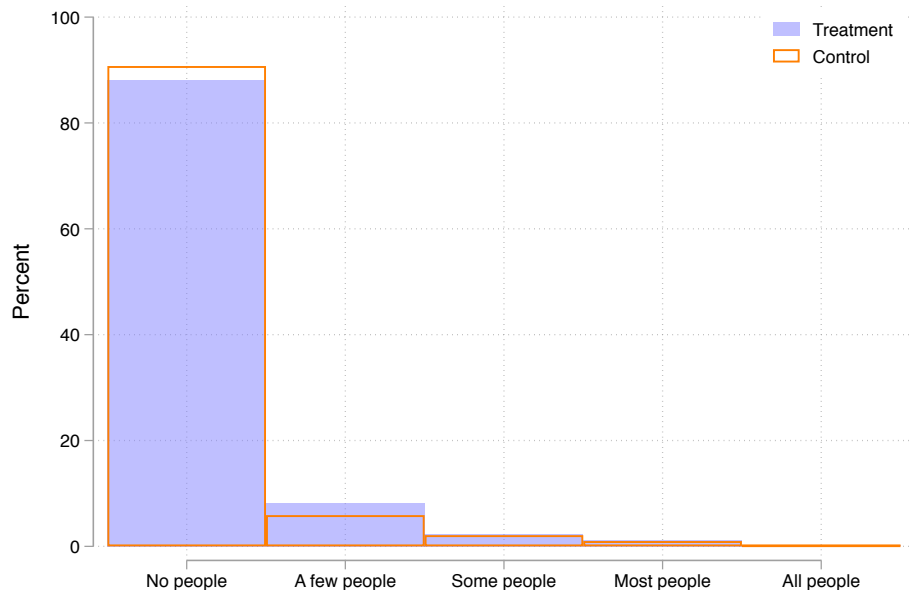
a precisely estimated zero (Columns 1-2). There is a marginally significant *decrease* in reported bribe amounts due to the tax campaign. In contrast, the point estimate is positive and marginally significant when *Others bribe* is the dependent variable. Figure A3 reveals that this effect corresponds with 1.5% of respondents in treatment switching from answering that “none of the people on my street” paid bribes to “a few of the people on my street” paid bribes. In contrast, the corresponding estimate when examining the ‘going rate’ of bribes is of tiny magnitude and not statistically different from zero.

Further evidence comes from considering participants’ self-reported beliefs about why some people in Kananga did *not pay* the property tax. Of the seven possible reasons evaluated by participants, bribe payment was considered the least important (Weigel, 2018), reinforcing the plausibility of the low measured incidence of bribes in this setting.

**Table A1:** Effects of the campaign on bribe payment

	Paid bribe (Midline)	Paid bribe (Endline)	Bribe amount (Endline)	Others bribe (Endline)	Going rate (Endline)
Campaign	-0.001 (0.002)	-0.000 (0.006)	-27.047* (14.054)	0.048* (0.025)	1.903 (53.843)
Covariates	No	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes
$R^2$	0.003	0.017	0.015	0.052	0.071
Observations	18141	2913	2913	2046	2566
Clusters	344	356	356	343	356
Control Mean	0.005	0.026	42.149	1.140	712.500

*Paid bribe* is an indicator for individuals’ self-reported bribe payments using local codes for bribes. It also equals 1 in the case of ‘reclassification’ bribes. *Bribe amount* is self reported amount paid in bribes. *Others bribe* is a standardized variable increasing in the perceived frequency that one’s neighbors are paying bribes instead of the property tax. *Going rate* is the estimated amount of money that it would take to bribe a tax collector in lieu of paying the property tax. Data: midline and endline survey.



**Figure A3:** Perceptions of the frequency at which other households on the street paid bribes in place of the property tax (*Others bribe*).

Some readers familiar with high rates of reported bribe payment in other parts of the DRC (Paler et al., 2016; Sanchez de la Sierra and Titeca, 2019), a paradigmatic ‘kleptocracy,’ might be puzzled by this null result and the low level of bribe payment in general.<sup>4</sup> There are several plausible explanations. First, collectors faced a considerable degree of uncertainty about the government’s plans to audit their work and pursue sanctions if bribes were found to have been paid. This uncertainty existed because this was the first-ever citizen tax campaign in Kananga. Collectors had no precedent on which to base expectations about the probability of government monitoring and punishment. Moreover, during the campaign, the tax ministry suspended an agent for collecting bribes from firms in place of the rental tax. Although the agent was unaffiliated with the property tax campaign, this disciplinary action was explicitly highlighted by tax ministry directors to the collectors working on the 2016 campaign, potentially deterring their future bribe taking. Finally, the tax campaign represented, in most cases, a one-shot game between collector and household. But collusive bribery between bureaucrats and citizens is more likely in repeated games, as when the same collector is responsible for collecting property

<sup>4</sup>I also anticipated an increase in bribe payment, as noted in the pre-analysis plan.

taxes in a neighborhood year after year (Khan et al., 2015).

In light of the negligible impact on bribe payment, I do not consider bribes as a potential mechanism through which the tax campaign could impact participation, as discussed in the pre-analysis plan.

### **A1.3 Kamuina Nsapu militia activity in Kananga**

In August 2016, a clash between the national police and a customary chief in Dibaya territory led to the death of the chief and the beginning of the Kamuina Nsapu militia movement. Although the group’s demands were triggered by local grievances, very shortly the Kamuina Nsapu evolved into a broader protest movement against the national government. In December 2016, President Joseph Kabila did not step down at the end of his constitutionally mandated term. Protests shook Kinshasa and many cities across Congo, and the Kamuina Nsapu took up arms. A cycle of tit-for-tat violence between the army and the Kamuina Nsapu left thousands dead and hundreds of thousands displaced (ReliefWeb, 2017).

These unfortunate events do not affect the internal validity of the research design, as the randomization nets out any broader trends that affect the city as a whole. But endline data collection was more difficult as a result. In particular, the research team could not administer endline surveys in one commune (Nganza), representing approximately 16% of the city’s total neighborhoods, in which the conflict was most acute.<sup>5</sup> Individuals in this commune could not be sampled or invited to participate in the endline survey as a result.

The conflict must also inform the interpretation of the external validity of results concerning attitudes toward the provincial government. Although directives to the military during the conflict came from the national government, citizens may have also blamed the provincial government for the conflict that marred the province.

The instability in Kananga would most likely lead causal effects of the tax campaign on participation to be more muted. Because attending a townhall meeting or submitting a government evaluation required traveling across the city, insecurity would have effectively made these forms of participation more costly. Indeed, the government ultimately issued a shelter in place order and so canceled all townhall

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<sup>5</sup>This commune is home to many migrants from Dibaya, where the Kamuina Nsapu movement originated. As such, it was the focal point of the conflict in Kananga, even though the majority of the violence occurred in rural areas outside the city.

meetings after April 1 due to increased violence in Kananga. Although it is difficult to ascertain precisely how this insecurity interacted with the experiment, it likely would have suppressed participation throughout the city.

## A2 Experimental design and measurement

### A2.1 Cross-randomized interventions

This field experiment also included two cross-randomized interventions intended for a separate paper on bribe payment. First, a citizen information treatment was randomized on the household level in all neighborhoods. This treatment was embedded in the informational fliers distributed in early 2016 announcing the property tax campaign. Fliers were distributed to every fifth household in all neighborhoods, ensuring that a quasi-random set of households spread evenly throughout the city would receive them.<sup>6</sup> Fliers informed citizens that provincial tax collectors were starting property tax collection and that “they could come to your household for this reason in the coming months.” They also specified that the money would be used to “secure the province, to kickstart economic development, and to protect the wellbeing of the population of Kasai Central.” In addition to the basic announcement of the campaign, treatment fliers additionally (1) specified the precise tax rates that households face, and (2) included a photograph of the type of printed receipt households should receive upon payment. An English translation of the flier text is as follows, where items 3-5 only appeared on the treatment version.

1. Please take note of the following information.
2. The DGRKOC collectors will start to collect property and rental taxes this year. They might come to your household for this reason in the following months.
3. The amount due for the property tax is 2,000 CF, unless you live in a large house or a multi-storied house. The property tax should be paid only one time per year. (*Treatment only*)
4. The amount due for the rental tax is 20% of monthly rent, retained at the

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<sup>6</sup>Although I cannot measure precisely which of these households read the fliers, note that about 80% of respondents were able to read in some capacity.

source. The rental tax should be paid each month. (*Treatment only*)<sup>7</sup>

5. The DGRKOC tax collectors should give you a receipt printed by a portable printer in front of you. See the example to the right. You have the right to ask for such a receipt to avoid paying two times. (*Treatment only*)
6. The money that they collect will support the efforts of the provincial government to secure the province, to kickstart economic development, and to protect the well being of the population of Kasai Central.
7. If you have any questions or complaints, please contact 0827316243 or 0974982998. These are the telephone lines of an independent NGO of scientific researchers who will transmit your messages to the leaders of the DGRKOC and to the governor. They will keep your identity confidential.

Second, collectors were informed that half of treated neighborhoods would be ‘audited’ by the tax ministry leadership. Before working in a neighborhood, collectors learned from a list posted at the tax ministry which neighborhoods were to be audited. The effects of the audit and information interventions on bribe payment are discussed in detail in a separate paper pooling data from the 2016 tax campaign with data from subsequent campaigns.

Key baseline covariates were balanced across the relevant treatment and control groups for these cross-randomized interventions, as shown in the pre-analysis plan. Additionally, within the endline sample, receipt of the treatment flier is balanced across individuals in treatment and control ( $t = -0.91$ ). Table A2 summarizes the three interventions (the tax campaign, audits, and informational fliers).

	Treatment		
	Audit		Control
	Audit	No Audit	Control
<b>Info</b>	65	62	88
<b>No Info</b>	60	66	90

**Table A2:** Neighborhoods (clusters) in each treatment cell.

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<sup>7</sup>The campaign initially included the rental tax, but subsequently the government decided to focus exclusively on the property tax, as discussed on p. 3.



## A2.2 Endline sampling and probability weights

This section provides more detail about endline sampling. As noted in Section III in the paper, selection occurred in two steps. First, enumerators conducted a short screening survey of roughly 20 property owners per neighborhood. Second, from those 20 households, 10 were selected and invited to participate.

I conducted a new random sample at endline — rather than tracking baseline respondents — because (a) I needed a considerably larger sample at endline to be powered for the survey-based outcomes, and (b) the baseline sample included a large share of renters, whereas the tax campaign (and thus the endline survey) focused on property owners. I had included renters in baseline sampling because, as noted, the tax campaign had initially included renters but ultimately focused on property owners only (see p. 3). Thus, it was cleanest to take a fresh random sample at endline for the purposes of studying the effects of the campaign on participation.<sup>8</sup>

As with the baseline survey, enumerators randomly sampled households for the screening survey by following a skip pattern until they walked up and down every street in a neighborhood.<sup>9</sup> After enumerators had randomly administered roughly 20 screening surveys, recording basic household attributes and the eligibility of potential participants, I selected a subsample of eligible participants in Stata. First, I took 4 fully random households from the full set. Second, I selected an additional 6 households per neighborhood based on observed house quality. Assigning higher probability to higher quality houses (e.g. those built with modern materials other than mudbricks) focuses on the population most affected by the tax campaign. Tax collectors differentially targeted higher quality houses, paying them more visits and collecting more tax from them. Oversampling these houses for the endline survey means the unweighted estimates concern this targeted population. It also enables analysis of heterogeneity by house quality, as in the robustness checks shown in Section A4, and estimation of ‘reclassification bribes’ (Section A1.2).

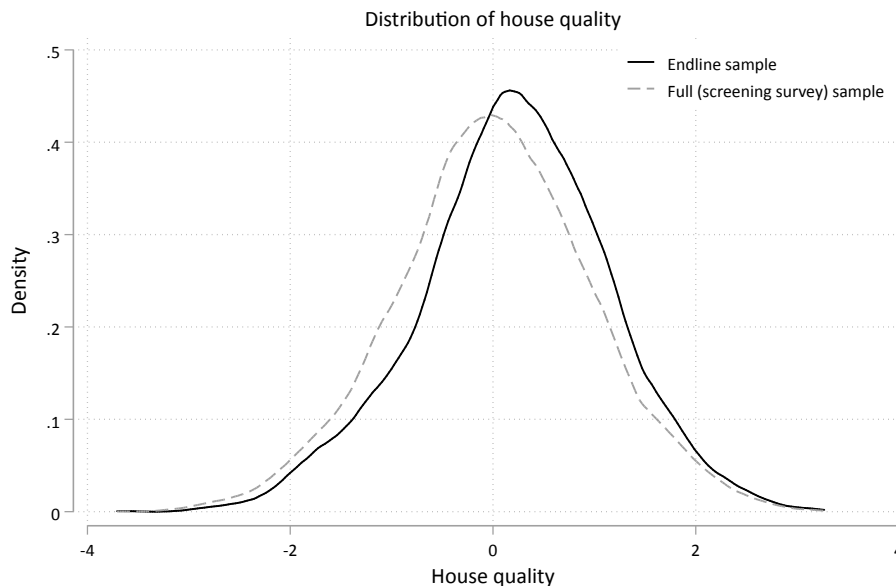
Specifically, for each screening survey respondent, I calculated a house quality

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<sup>8</sup>In addition, enumerators did track 623 property-owner baseline respondents after the tax campaign to enable analysis of heterogeneous treatment effects on tax compliance based on baseline beliefs. This is the subject of a companion paper (Weigel, 2020).

<sup>9</sup>If they finished this exercise but had not yet reached the target of 20 screening surveys, they were instructed to choose a different path through the neighborhood orthogonal to the previous path and continue sampling. If they reached 20 before they had walked down all neighborhood streets, they continued until they reached the end, exceeding the target number of surveys.

index based on characteristics of the compound: the roof, walls, the total number of buildings, and the accessibility to a primary or secondary avenue. The top 6 houses were selected for endline survey enumeration (the “household quality sample”), along with the 4 random draws (the “endline random sample”). The result of this procedure is a slightly higher proportion of relatively higher quality houses in the endline sample. Figure A4 demonstrates this shift graphically: the endline sample is slightly to the right of the full screening survey sample in terms of estimated house quality.



**Figure A4:** Densities of estimated house quality for the endline sample and the full screening survey sample.

Because of this sampling process, the (unweighted) estimates reported in the paper are not representative of the full population of Kananga, but rather of a slightly better-off sub-population of the city that was more targeted by the tax campaign. For completeness, in robustness tables in Section A4, I include specifications with probability weights to make estimates fully representative of Kananga’s population.

I construct weights as follows. First, for each individual  $i$  with house quality  $x$  in neighborhood  $j$ , I estimate a weight,  $w_{ij}^{within}$ , to adjust for the higher selection probability for relatively high quality houses:

$$w_{ij}^{within} = \frac{f_j^{pop}(x_i)}{f_j^{samp}(x_i)} \quad (1)$$

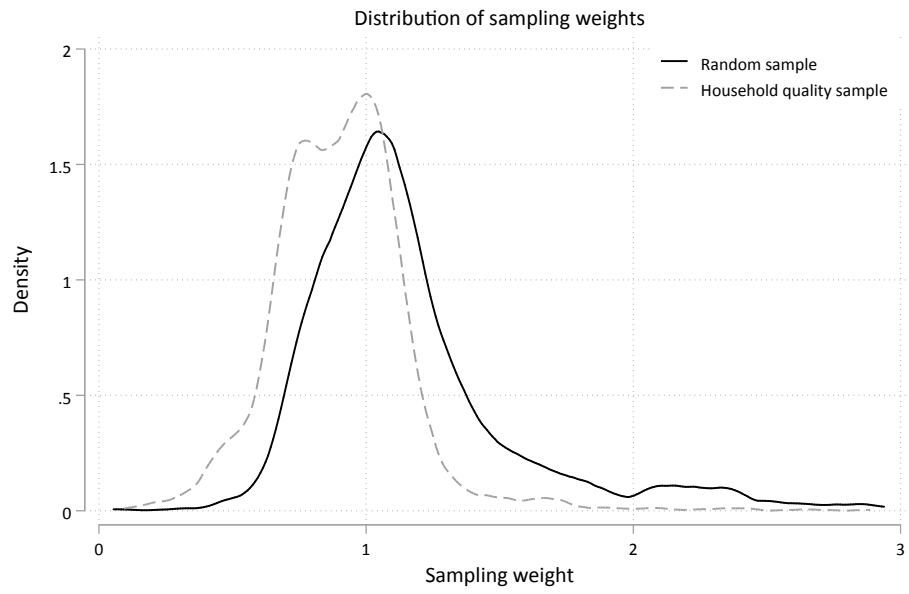
where  $f_j^{pop} \sim N(\mu_j^{pop}, \sigma_j^{pop})$ ,  $f_j^{samp} \sim N(\mu_j^{samp}, \sigma_j^{samp})$ , and the  $\mu_j$ 's and  $\sigma_j$ 's reflect neighborhood-level empirical means and standard deviations, respectively.<sup>10</sup> Second, I construct a between-neighborhood weight to account for the fact that the number of individuals in the endline sample is (roughly) constant across neighborhoods, but neighborhoods themselves have different numbers of property owners. This weight is simply:

$$w_j^{between} = \frac{n_j^{screening} / N}{n_j^{endline} / n} \quad (2)$$

where  $N$  is the total number of property owners in Kananga, and  $n$  is the total number of property owners in the endline sample. The two weights are then multiplied together and normalized to generate a probability weight that is used in regressions in Section A4. Figure A5 shows the distributions of the ultimate weights. As expected, individuals in the “endline random sample” receive the most weight because they are on average of lower house quality and thus underrepresented in the endline sample. By contrast, the “household quality sample” receives the least weight because these high-house-quality respondents are overrepresented in the endline sample. Using these weights in the main analyses does not substantially alter the results.

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<sup>10</sup>I tried using the empirical densities rather than relying on parametric assumptions, but the resulting weights had large numbers of extreme values. Imposing a normal distribution on neighborhood-level house quality is justified by the empirical distributions of house quality taken in the full endline and screening survey samples (Figure A4).



**Figure A5:** Distributions of the weights for endline subsamples.

### A2.3 Townhall meetings and government evaluations

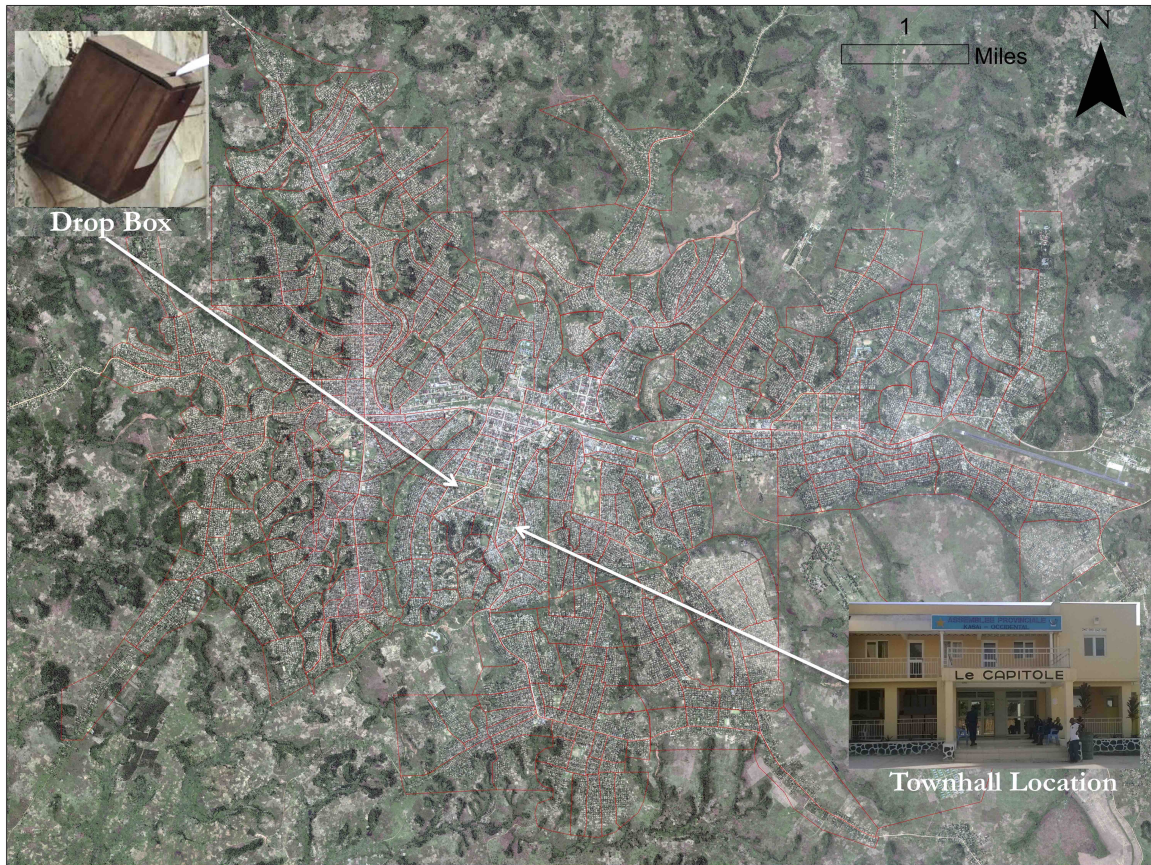
Townhall invitations were distributed evenly in treatment and control neighborhoods. All endline respondents received invitations.<sup>11</sup> Figure A6 shows a sample invitation in French.<sup>12</sup> In addition to receiving the invitation, citizens were read the following verbal prompt: “The meeting will be an opportunity to obtain information about taxation and public spending in Kananga and to ask questions to officials in the provincial government of Kasai Central.”



**Figure A6:** Invitation to townhall meeting held at Kananga’s Provincial Assembly building on February 20, 2017.

<sup>11</sup>The exception, as noted in the paper, is respondents sampled after April 1, when the provincial government discontinued the meetings due to insecurity in Kananga.

<sup>12</sup>In English: “You are invited to a meeting about provincial taxation in the presence of certain members of the Provincial Government of Kasai Central. This meeting will take place on the [DATE] at 10h00 in the Plenary Room of the Provincial Assembly. To ensure the meeting goes smoothly, please arrive by 9h00. Code to present at the entry: [CODE]. Without this invitation, entry to the Plenary Room will not be possible. So please don’t forget to bring this invitation with you.”



**Figure A7:** Locations of provincial assembly building (townhall meeting location) and evaluation form drop box in downtown Kananga.

Townhall meetings took place in the provincial assembly building, shown in Figure A8. The location of the building is shown on the map in Figure A7 in the paper. Individuals deposited their invitations when they arrived, allowing the research team to link participation to household surveys with the unique ID code (unknown to the government). During each meeting, the finance minister and tax ministry director made short introductions, and then the majority of time was devoted to question and answer. The average meeting lasted just over three hours. The atmosphere was formal but confrontational and often tense. Citizens expressed strong dissatisfaction with the quality of the government and demanded better services, more transparency, and more ways to have a voice in politics if they were expected to pay taxes. Evidence about the types of comments citizens made during townhalls is summarized in Section A3.3.3. The topics of citizens' comments were recorded by



enumerators in the back of the room.<sup>13</sup>



**Figure A8:** Left Panel—Provincial Assembly building in Kananga, the location of the townhall meetings (Photo credit: CAID 2016). Right Panel—Townhall meeting on January 30, 2017

Government evaluations were also distributed equally in treatment and control. Endline survey respondents became eligible to receive them after they had finished the survey. They were read the following script concerning the evaluations: “Here is an evaluation form that you can send to the provincial government. The form asks for your opinions about the work of the provincial government. It is your decision whether or not you want to deliver it in a locked box in Biancky [downtown Kananga]. It will be completely anonymous: that’s to say, if you choose to deliver it, the provincial government will never know your identity based on the information on the card. However, an anonymous summary of the information on the card and the cards themselves will be provided to the governor and other officials in the government so they know about your opinions and suggestions.” The drop box was located in downtown Kananga (Figure A9). As promised, the governor ultimately received the evaluation forms along with a report summarizing their contents and containing a typed list of all written-in suggestions. The finance minister and head of the tax ministry also received copies. A summary of the evaluations submitted by citizens appears in Section A3.3.3.

<sup>13</sup>Options on the tablet included: “the property tax / 2016 campaign, provincial taxation in general, the use of tax money in the provincial government / provincial budget, the use of tax money in the national government / national budget, corruption in the provincial government, corruption in the national government, public goods / infrastructure in Kananga (road quality, schools, etc), security in Kananga (militia, etc), politics in Kinshasa (Kabila, the accord, PM Badibanga), other.” When enumerators chose other, they were prompted to write in the topic.

**Kakanda ka matshinkidila** Code :

*Sungula mandamuna awudi musue :*

**A. Tshia kumpala, suaku usungule muenenu uwudi musue:**

- 1 : Ndi nsanka ne bukalenge bua mu polovinse wa Kasai Central ne tshiena musue kushintulula tshintu kampanda ku midimu yende nansha;
- 2 : Ndi njinga ne bukalenge bua polovinse wa Kasai Central buenza mudimu wabu bimpe.

**B. Mpindieu, suaku ungambile ne udi musue menemene, musue, mubenge anyi mubenge moshe ne munu muiku ku malu adi alonda aa.**

**Bukalenge bua polovinse budi ne tshia kuvudija mishindu ya bungi bua didibuinka dia bena ditunga ku mapangadika adidi diangata.**

- Musue menemene
- Musue
- Mubenge
- Mubenge moshe ne munu muiku

**Bukalenge bua polovinse budi ne tshia kuvudija mishindu ya bungi ya dipeta dia ngumu pa ndongamu ya bukalenge bua polovinse**

- Musue menemene
- Musue
- Mubenge
- Mubenge moshe ne munu muiku

**Diela nseke:**

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**Bukalenge bua polovinse budi ne tshia kutula mfranga ya bungi bua kuenza bintu bia musoko kupitshila pa mfranga idibu batula bua bulombodi**

- Musue menemene
- Musue
- Mubenge
- Mubenge moshe ne munu muiku

**Bukalenge bua polovinse budi ne tshia kukeba muaba kampanda munda mua tshimenga undi mua kuikala nya kumanyisha malu pa bintu anyi midimu ya musoko.**

- Musue menemene
- Musue
- Mubenge
- Mubenge moshe ne munu muiku



**Figure A9:** Left Panel—Example of government evaluation form. Right Panel—Evaluation form drop box in downtown Kananga

The full text of the questions on the evaluation form, inspired by (Paler, 2013), is as follows:

First, please choose which of the following options you agree with most:

1. I am satisfied with the Provincial Government of Kasai Central and have no changes to suggest about its operations.
2. I want the Provincial Government of Kasai Central to do a better job.

Now please indicate if you strongly agree, agree, disagree, or strongly disagree with the following propositions.

1. The Provincial Government of Kasai Central should provide more opportunities for public participation in its decisions.
2. The Provincial Government of Kasai Central should provide more access to information about its programs.

3. The Provincial Government of Kasai Central should spend more money on public goods and development and less money on administration.
4. The Provincial Government of Kasai Central should create a place in town where I can report problems with public services.

Comments:

## A3 Additional tables and figures for the paper

### A3.1 Main results (Section V)

This section contains additional figures and tables referenced in the text of Section V in the paper (except for the main robustness checks, which appear in Section A4).

#### Collector visits, taxpayer registration, and payment

This section examines the effects on tax compliance (a) in the endline sample only, and (b) using an alternate measure of property tax payment. Specifically, Table A3 shows estimated treatment effects restricted to the endline sample. The magnitude of the estimated increase in tax payment (Columns 1-2) is analogous to that estimated among the universe of potential taxpayers, as reported in Table III in the paper.

Next the table considers an alternative measure of tax compliance in the endline sample. This robustness exercise is motivated by the fact that the primary measure of compliance also matches on taxpayer IDs — in addition to name matches and possession of valid receipts — but these IDs do not exist in control. There is thus an asymmetry in measurement for the primary household-level tax compliance outcome. Importantly, *the neighborhood-level measure of tax compliance addresses this issue by eliminating the need to merge administrative tax records and household surveys* (Table III, Column 4). Reassuringly, the neighborhood- and household-level tax compliance estimates are very similar.

However, for completeness, I also investigate this measurement issue in the endline sample only. Specifically, I consider an alternative household-level measure that takes a value of 1 only if (i) there is a name match between administrative tax data

and survey data, or (ii) a household presents the enumerator with a valid receipt — the second two conditions of the primary measure. When using this alternative measure of compliance, the magnitude of the treatment effect shrinks from 10.8 to 7.8 percentage points. This decrease likely reflects the fact that the measurement error from name matching causes more downward bias in the estimate of compliance in treatment than it does in control — because control compliance is so low that there are effectively floor effects on the bias induced by measurement error. The logic behind this assertion is as follows.

First, measurement error associated with relying only on name matching and possession of receipts likely causes downward bias on estimated compliance. For the receipt-based condition, households could only lose receipts, leading to underestimation of tax compliance.<sup>14</sup> Similarly, for name matching, measurement error likely biases estimated compliance to zero. This is because married couples in Congo do not share last names (or any names) and nicknames are common. There are thus likely to be ‘false negatives’ after matching government tax records and survey data. On the other hand, ‘false positives’ are possible but less likely given that Stata’s `reclink` function uses a threshold of 0.6 for string matches, and these name matches are conditioned on perfect neighborhood matches. Since neighborhoods contain only 130 compounds on average, it is unlikely that there are many false positives from the matching procedure. On net, then, the receipt-based and name-matching measure likely underestimates the number of payers in treatment and control.

Second, such under-reporting likely causes downward bias on the estimated treatment effect because the rate of compliance cannot go much lower in control. There is essentially a floor on the ability of this measurement error-induced bias to reduce the estimated compliance rate in control. We can be confident that so few households in control paid because in the administrative data, there are only 4 records of property tax payments from control neighborhoods after the randomization of neighborhoods into treatment and control for the 2016 door-to-door campaign started.<sup>15</sup>

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<sup>14</sup>Households could not find or repurpose receipts because enumerators validated receipts using the name and neighborhood number printed on them. Handwritten receipts were not accepted.

<sup>15</sup>Before the campaign, there are 190 records of property tax payments, 90% of which concerned commercial properties, whereas the campaign focused on residential properties. If one wanted to include these commercial payments in estimates of the campaign’s impact on compliance and revenues, they would likely be balanced across treatment and control because they were made before the randomization. Thus, the maximum compliance rate would still be less than 1% in control neighborhoods.

In sum, the combination of (a) verifiably low levels of compliance outside of the campaign, and (b) the fact that receipt possession and name matching likely underestimate actual tax compliance together implies that this alternative estimate of the treatment effect is downward biased.

**Table A3:** Effects on tax compliance in endline sample only and using alternative measure of tax payment

	Property tax compliance			
	(primary measure)		(alternative measure)	
	(1)	(2)	(3)	(4)
Campaign	0.108*** (0.010)	0.111*** (0.010)	0.078*** (0.009)	0.080*** (0.009)
Covariates	No	Yes	No	Yes
Stratum FE	Yes	Yes	Yes	Yes
$R^2$	0.070	0.084	0.062	0.075
Observations	2913	2913	2913	2913
Clusters	356	356	356	356
Control Mean	0.006	0.006	0.006	0.006

*Property tax compliance* indicates property tax payment. Columns 1-2 use the primary measure examined in the paper, e.g. p. 18. Columns 3-4 consider an alternative measure that takes a value of 1 only if (i) there is a name match between administrative tax data and survey data, or (ii) a household presents the enumerator with a valid receipt. It does not, therefore, rely on matching by tax ID, which is only possible in the treatment group. All columns consider estimations conducted in the endline sample only. Data: endline survey merged with government tax database.

## Political participation

This section contains several additional exhibits relevant to the main results about political participation. What follows is a summary of these tables and figures.

- Table A4 demonstrates that the main results on participation are unaffected when controlling for the distance between households and the venues of participation.
- Table A5 shows that the main treatment effects are robust to including on the righthand side: the three imbalanced covariates mentioned in Section II (business owner status, wealth, and the quality of public lighting in the neighborhood at baseline) plus their interactions with treatment. Although the standard errors are larger, the coefficients' magnitude changes little.
- Table A6 explores whether the tax campaign impacted attitudes about politics

or participation at the national or local level. I lack measures of costly engagement at these levels of government, so I consider survey evidence. The outcome in the first column, *Role of citizens in politics*, is an index increasing in views about the importance of citizen monitoring of the government. The second column examines effects on general *Interest in politics*, which combines survey questions about political news consumption, knowledge, and demand for information about the government. The third column examines self-reported *Engagement with national politics*, including current and future participation in national elections, parties, marches, protests, and rallies. The fourth column examines engagement at the local level with city chiefs. These local leaders have two main responsibilities: (1) organizing weekly *salongo*, an informal tax in which citizens contribute labor toward local public goods, such as maintaining neighborhood roads (Olken and Singhal, 2011); (2) mediating local disputes to avoid escalation to the formal court system. Column 5 examines trust in and evaluations of city chiefs. Column 6 examines demand for formalization, measured as citizens' self-reported recent efforts to obtain identity documents, business licenses, and land titles. As always, detailed variable descriptions are located in Section A5.

Reassuringly, treated respondents reported stronger views about the importance of citizens (and their representatives) monitoring the government. They did not report higher overall interest in politics, nor did they report more participation at a national level.<sup>16</sup> Interestingly, individuals in treatment did report engaging less with local city chiefs. Figure A25 shows the effects on the underlying survey questions, revealing in particular that treated individuals reported consulting less with their avenue chief and engaging less in the local labor tax, *Salongo*. Citizens in treatment also did not indicate higher demand for formal documents compared to citizens in control.

- Figure A10 shows the percentage of households that either attended a townhall or submitted an evaluation by neighborhood, separately in treatment and control.
- Figure A11 shows the distribution of the time gap between tax collection and outcome measurement. This time gap is analyzed in Figure I of the paper.

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<sup>16</sup>Figure A24 is a coefficient plot showing results for the individual survey questions.



- Table A7 estimates heterogeneous impacts of the tax campaign on costly participation by a series of moderators pre-specified in the analysis plan.<sup>17</sup> The treatment effect appears slightly more pronounced in neighborhoods with better infrastructure (roads, public lighting). It also appears more pronounced among individuals (i) from the ethnic majority, (ii) who believe the campaign raised compliance considerably, and (iii) who have more overall knowledge of the tax campaign. By contrast, there is little evidence of heterogeneity by self-reported trust in foreign researchers or knowledge of other taxpayers.

**Table A4:** Effects of the campaign on participation controlling for distance from respondents' households to the participation venues

	Townhall meeting attendance (1)	Evaluation form submission (2)	Townhall or evaluation (3)	Townhall and evaluation (4)	Index (townhall & evaluation) (5)
Campaign	0.045** (0.020)	0.024** (0.012)	0.049*** (0.016)	0.028*** (0.009)	0.144*** (0.043)
Distance to townhall	-0.007 (0.014)				
Distance to dropbox		-0.016** (0.008)			
Distance to townhall & dropbox (avg.)			-0.018 (0.011)	0.008 (0.006)	-0.019 (0.028)
Covariates	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes
$R^2$	0.068	0.056	0.072	0.040	0.074
Observations	1934	2913	2913	2913	2913
Clusters	252	356	356	356	356
Control Mean	0.171	0.171	0.164	0.035	-0.077

The outcome variables are analogous to those examined in Columns 1-5 of Table IV in the paper. The three *Distance* variables are the Euclidean distance from respondents' households to the townhall venue (Provincial Assembly building), the evaluation drop box, or to both locations (averaged), respectively. As shown in Figure A7 in the paper, these locations are only 1 kilometer apart in downtown Kananga. Using the average distance is easiest to interpret when examining the outcomes in Columns 3-5, especially considering that all estimations include stratum fixed effects. Data: endline survey merged with participation records and geographic estimates.

<sup>17</sup>The moderators considered in Columns 4-7 are outcomes of treatment, and thus these analyses must be interpreted with caution.

**Table A5:** Effects of the campaign on participation controlling for imbalanced covariates & interactions with treatment

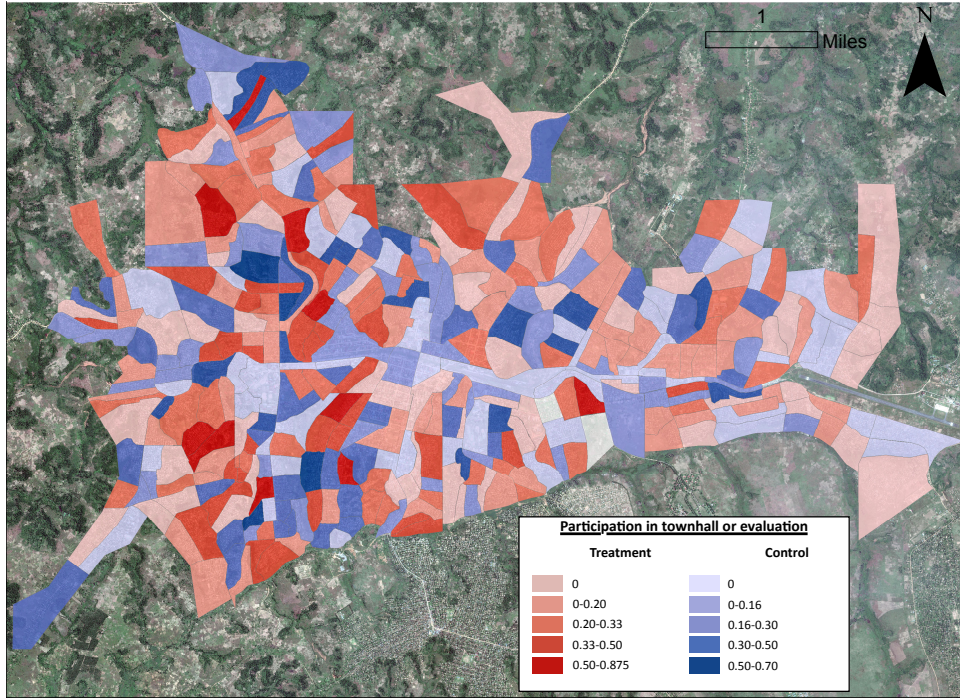
	Townhall meeting attendance (1)	Evaluation form submission (2)	Townhall or evaluation (3)	Townhall and evaluation (4)	Index (townhall & evaluation) (5)	Cost of participation (transport) (6)	Cost of participation (transport & opp.) (7)
Campaign	0.041 (0.025)	0.022 (0.015)	0.050** (0.021)	0.026** (0.012)	0.142*** (0.055)	0.071*** (0.023)	0.093*** (0.029)
Camp. X Bus.	-0.030 (0.039)	-0.010 (0.025)	-0.027 (0.033)	-0.014 (0.016)	-0.078 (0.080)	-0.081** (0.033)	-0.091** (0.042)
Camp. X Wealth	-0.005 (0.021)	-0.003 (0.013)	-0.009 (0.016)	-0.003 (0.009)	-0.021 (0.040)	-0.006 (0.015)	-0.005 (0.020)
Camp. X Light	0.115 (0.099)	0.033 (0.067)	0.045 (0.079)	0.032 (0.042)	0.146 (0.204)	-0.025 (0.059)	-0.019 (0.082)
Business owner	-0.016 (0.030)	-0.011 (0.019)	-0.000 (0.025)	-0.005 (0.011)	-0.009 (0.060)	0.029 (0.025)	0.030 (0.031)
Wealth	0.021 (0.017)	0.019* (0.010)	0.032** (0.013)	0.000 (0.007)	0.060* (0.032)	0.011 (0.010)	0.026* (0.014)
Pub. light qual.	-0.134 (0.081)	-0.007 (0.039)	-0.094* (0.051)	-0.037 (0.027)	-0.246* (0.133)	-0.097** (0.041)	-0.152*** (0.056)
Basic Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$R^2$	0.069	0.055	0.071	0.040	0.074	0.056	0.060
Observations	1934	2913	2913	2913	2913	2913	2913
Clusters	252	356	356	356	356	356	356
Control Mean	0.17	0.099	0.16	0.035	-0.077	0.11	0.16

The dependent variables are the same as those in Table IV. Controls include the three imbalanced covariates noted in paper Section II (business owner status, wealth, and the quality of public lighting in the neighborhood at baseline) as well as interactions with treatment. Data: endline survey merged with participation records.

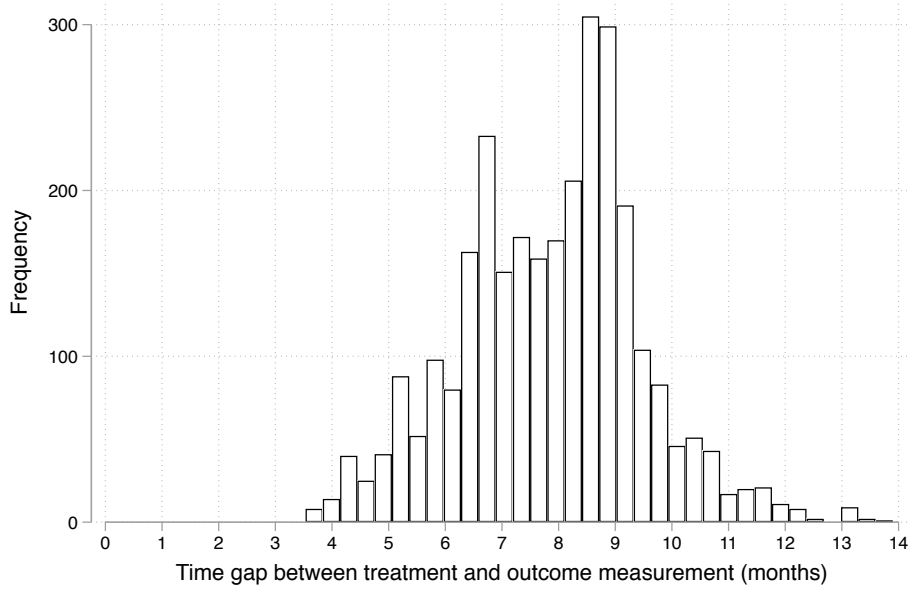
**Table A6:** Effects of the campaign on political attitudes and participation at other levels of government

	Role of citizens in politics (1)	Interest in politics (2)	Engagement in national politics (3)	Engagement with city chiefs (4)	Views of city chiefs (5)	Demand for formalization (6)
Campaign	0.086** (0.042)	0.013 (0.036)	0.025 (0.040)	-0.112** (0.046)	-0.053 (0.048)	0.002 (0.037)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes
$R^2$	0.064	0.269	0.069	0.092	0.067	0.085
Observations	2875	2913	2913	2820	2820	2913
Clusters	356	356	356	356	356	356
Control Mean	-0.033	0.18	-0.015	0.022	-0.0007	0.014

*Role of citizens in politics* is a standardized index increasing in attitudes that citizens and their representatives should actively monitor the government. *Interest in politics* is a standardized index increasing in revealed and self-reported interest in politics. *Engagement with national politics* is a standardized index increasing in self-reported participation in national politics. *Engagement with city chiefs* is a standardized index increasing in the self-reported interactions with and knowledge of avenue chiefs. *Views of city chiefs* is a standardized index increasing in self-reported trust in and evaluations of avenue chiefs. *Demand for formalization* is a standardized index increasing in self-reported recent efforts to obtain identity documents, business licenses, and land titles. Columns 4-5 exclude respondents who are themselves chiefs. See Section A5 for more information about the underlying survey questions in each of these indices. Data: endline survey.



**Figure A10:** Neighborhood-level participation rates (% of HHs that attended a townhall or submitted an evaluation) across Kananga.



**Figure A11:** Distribution of the gap in time between the tax campaign and outcome measurement.

**Table A7:** Heterogeneous treatment effects of the campaign on participation by moderators pre-specified in analysis plan

	Townhall or evaluation						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Campaign	0.026 (0.024)	0.024 (0.022)	0.018 (0.026)	0.048*** (0.017)	0.053*** (0.017)	0.034 (0.023)	0.011 (0.019)
Camp X Road quality	0.051 (0.034)						
Road quality (high)	-0.021 (0.025)						
Camp X Public lighting		0.055* (0.033)					
Public lighting (high)		-0.002 (0.031)					
Camp X Majority ethnicity			0.043 (0.032)				
Majority ethnicity			-0.003 (0.025)				
Camp X Trust researchers				0.019 (0.059)			
Trust researchers				0.006 (0.044)			
Camp X Know payers					-0.040 (0.068)		
Know payers					0.049 (0.059)		
Camp X Perceived compliance						0.031 (0.031)	
Perceived compliance (high)						-0.023 (0.023)	
Camp X Knowledge campaign							0.068** (0.030)
Knowledge of campaign (high)							0.010 (0.022)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$R^2$	0.071	0.073	0.072	0.071	0.073	0.071	0.076
Observations	2913	2913	2913	2913	2849	2913	2913
Clusters	356	356	356	356	356	356	356
Control Mean	0.164	0.164	0.164	0.164	0.164	0.164	0.164
$F$ -test $p$ -value	0.002	0.003	0.007	0.010	0.008	0.006	0.009

The table reports heterogeneous treatment effects of the tax campaign on costly participation by a series of moderators enumerated in the analysis plan. The dependent variable is an indicator for attending a townhall or submitting an evaluation, the same as in Column 3 of Table IV. The first two moderators, *Road quality* and *Public lighting*, come from baseline data; the rest from endline data. All moderators are included as dummy variables, where for continuous variables — all except majority ethnicity and knowledge of taxpayers — the regressors indicate above-median values of the variable. Data: endline survey merged with participation records and baseline public goods measures.

## Beliefs about the government

This section contains the following exhibits concerning beliefs about the government.

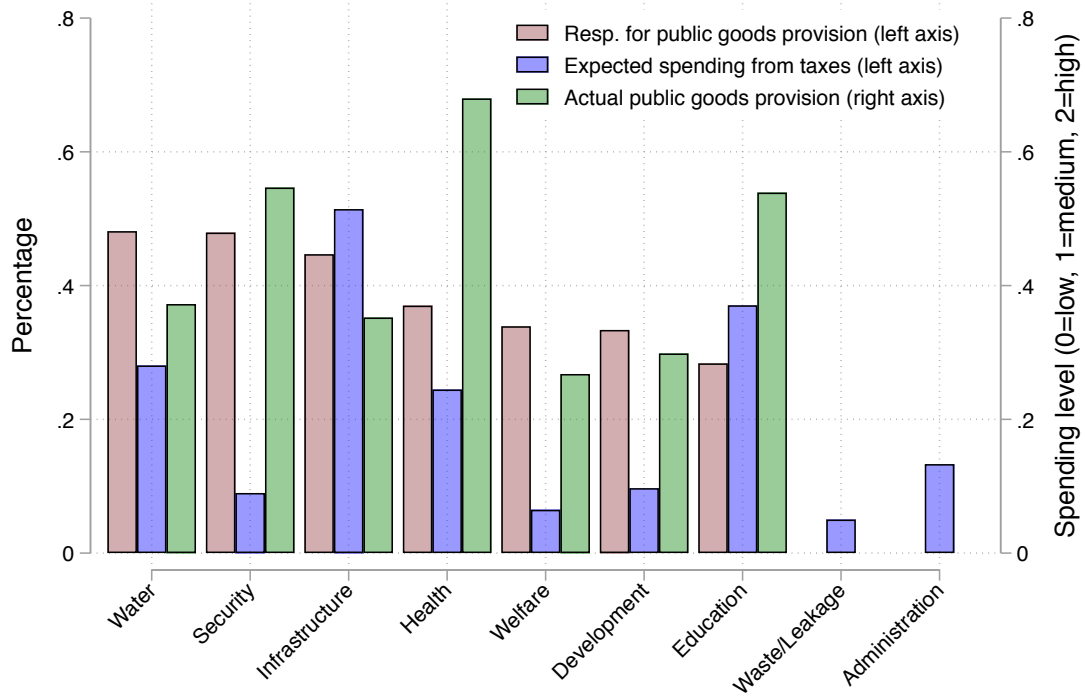
- Table A8 probes further the extent to which the tax campaign caused citizens to update their beliefs about the provincial government’s responsibility in public goods provision. The first column examines the outcome *Resp. for public goods provision*, replicating the result reported in Table V, while the second and third column report results disaggregated by the subindices discussed on p. 21 in the paper. The fourth column then examines citizens’ perceptions of the current level of public goods provision from the provincial government.
- Figure A12 compares: (1) beliefs about the responsibility of the provincial government to provide public goods across a range of sectors (*Resp. for public goods provision*); (2) beliefs about how the revenue from the property tax campaign would actually be spent by the government (*Expected spending from taxes*); and (3) beliefs about the amount of public goods the provincial government was already providing in each sector (*Actual public goods provision*). All data come from the endline survey. Most citizens expected spending on infrastructure, consistent with government messaging that campaign revenues would be used to “promote economic development” (from tax collectors and on pre-campaign fliers). Although infrastructure was among citizens’ spending priorities, they also demanded non-trivial levels of public goods provision from the provincial government across each of these sectors. In particular, there appears to have been a gap between the demand for provincial government spending on security and expectations about spending of property tax revenues. The demand for security likely reflects the Kamuina Nsapu conflict that was ongoing at the time of endline administration (see Section A1.3).
- Figure A13 then examines the accuracy of citizens’ perceptions about the level of compliance with the property tax. In the treatment group, the modal citizen accurately predicted how many people were paying the tax.



**Table A8:** Effects of the campaign on the perceived responsibility for public goods provision: analysis of sub-indices and beliefs about current levels of provision

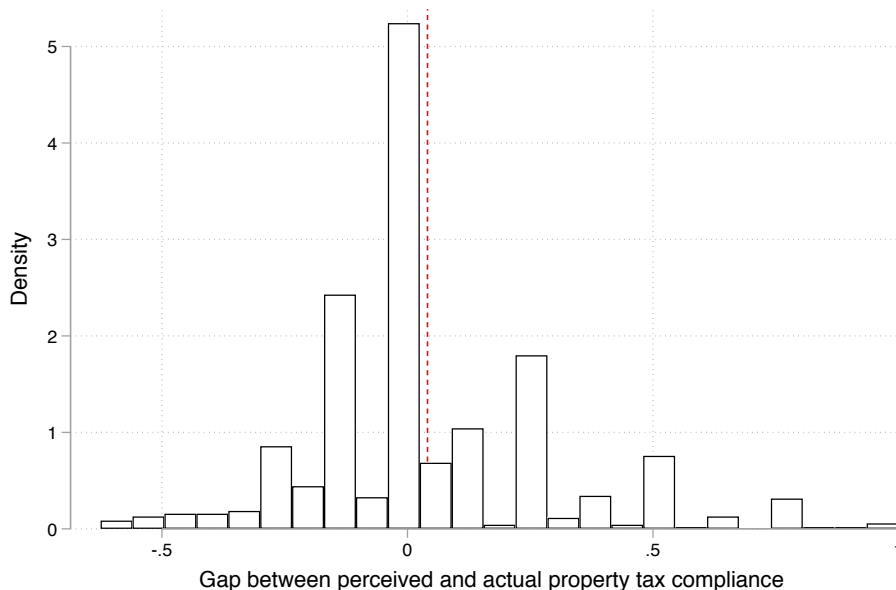
	Resp. for public goods provision			Current public
	(Full index)	(Sector questions)	(Hypotheticals)	goods provision
	(1)	(2)	(3)	(4)
Campaign	0.117** (0.051)	0.088* (0.053)	0.097** (0.041)	0.037 (0.045)
Covariates	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes
$R^2$	0.041	0.043	0.028	0.048
Observations	2913	2813	2900	2910
Clusters	356	356	356	356
Control Mean	-0.063	-0.044	-0.06	-0.042

*Resp. for public goods provision (Full index)* is a standardized index increasing in the perceived responsibility of the provincial government in public goods provision. It is composed of the following two variables. *Resp. for public goods provision (Sector questions)* is composed only of questions asking the respondent who should provide a series of public goods. *Resp. for public goods provision (Hypotheticals)* is composed of questions asking the respondent to choose between viewpoints, one of which espouses the provincial government play a more active role in service provision. *Current public goods provision* is a standardized index increasing in citizens' perceptions of the current level of public goods provision by the provincial government. See p. 62 for the exact survey questions. Data: endline survey.



**Figure A12:** Beliefs about the government’s responsibility for public goods provision, actual level of public goods provision, and expected spending from the property tax campaign revenues.

*Notes:* *Resp. for public goods provision* (left axis) is the share of endline survey respondents who said the provincial government should be principally responsible for providing the indicated public good. *Expected spending from taxes* (left axis) is the share of endline survey respondents who indicated that property tax revenues were likely to be spent in the indicated sector. Each respondent selected the two sectors in which they thought property tax spending was most likely, including waste/leakage and administration. *Actual public goods provision* (right axis) is the average current level of public goods provision perceived by endline survey respondents, where 0 indicates no provision, 1 indicates some provision, and 2 indicates a high degree of provision. The survey questions constituting *Resp. for public goods provision* and *Actual public goods provision* did not ask about administrative costs or waste/leakage, hence the missing data for those categories. Data: endline survey.



**Figure A13:** Difference between perceived and actual neighborhood property tax compliance (treatment group only).

### A3.2 Alternative explanations (Section V.C)

This section contains additional tables and figures referenced in Section V.C of the paper, concerning possible alternative explanations for the reduced-form effect of the tax campaign on participation. The exhibits are as follows:

- Table A9 shows that, across a range of measures discussed on p. 29 in the paper, there do not appear to be differences in the trust in or familiarity with the research team across treatment and control.
- Table A10 examines if flier recipients in control were less likely to participate than non-recipients. Such a decrease would be consistent with a “disappointment effect” hypothesis in which flier recipients in control received a net negative signal (because they expected tax collectors who never came) and lowered their participation. Fliers were distributed quasi-randomly following a skip pattern of “every fifth house” through all neighborhoods of Kananga. As a validation that flier recipients in control indeed learned more about the property tax and collection campaign, the first column of Table A10 summarizes a regression of knowledge about provincial taxation on an indicator

for flier recipients (restricting the sample to the control group). Receiving a flier increases knowledge of taxation by 0.86 standard deviations.<sup>18</sup> The second column then shows that flier recipients and non-recipients in control appear to participate at similar rates. The third column then re-estimates the main results on participation excluding the households in the control group that received informational fliers. If the treatment effect were explained by individuals in control reducing their participation because they expected to receive tax collectors but never did (at least before outcome collection), then we would expect this reduction to be concentrated among those who received informational fliers in control. Thus, excluding control flier receivers would reduce the estimated treatment effect of the campaign. However, there is no such reduction.

- Table A11 offers an alternative test of the ‘disappointment effect’ hypothesis. It shows that, among the 229 individuals in control who were tracked from baseline to endline, views of the provincial government do not appear to have deteriorated over the course of the experiment. This observation reinforces that the treatment effect is unlikely to be explained by declining participation in control rather than increasing participation in treatment.
- Figure A14 shows that there is not an increase in participation (*Townhall or evaluation*) among households in treated neighborhoods close to a border with a control neighborhood, as one would have expected if awareness of the untaxed control was driving participation.

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<sup>18</sup>The outcome variable is an index of five indicator variables capturing citizens’ knowledge of the property tax, tax rate, tax campaign, tax ministry, and the type of receipt for payers of the property tax. Although two of these questions are self-reported and essentially trusting the word of the respondent, three of them are verifiable (and the coding of the variables, as indicated above, captures whether the respondent gave the correct or incorrect answer). Examining each of these questions as separate outcomes yields similar results to the index.

**Table A9:** Effects of the campaign on trust in and familiarity with the research team

	Trusts researchers (1)	Knows researchers (2)	Past Participant (3)	No phone (4)	Fake phone (5)
Campaign	0.056 (0.051)	-0.037 (0.048)	0.021 (0.037)	0.022 (0.019)	-0.014 (0.017)
Covariates	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes
$R^2$	0.036	0.121	0.016	0.123	0.038
Observations	2733	2913	2913	2913	2913
Clusters	356	356	356	356	356
Control Mean	-0.037	0.041	-0.020	0.176	0.077

*Trusts researchers* is a standardized measure of respondents' self-reported trust levels in foreign research organizations. *Knows researchers* indicates respondents who could identify the employer of the enumerator in the endline survey. *Past participant* indicates respondents who self-reported participation in past research activities conducted by our team in Kananga. *No phone* indicates that the respondent did not provide a phone number in the endline survey. *Fake phone* indicates that the respondent provided a bad or fake phone number. Data: endline survey.

**Table A10:** Informational fliers about tax campaign and participation

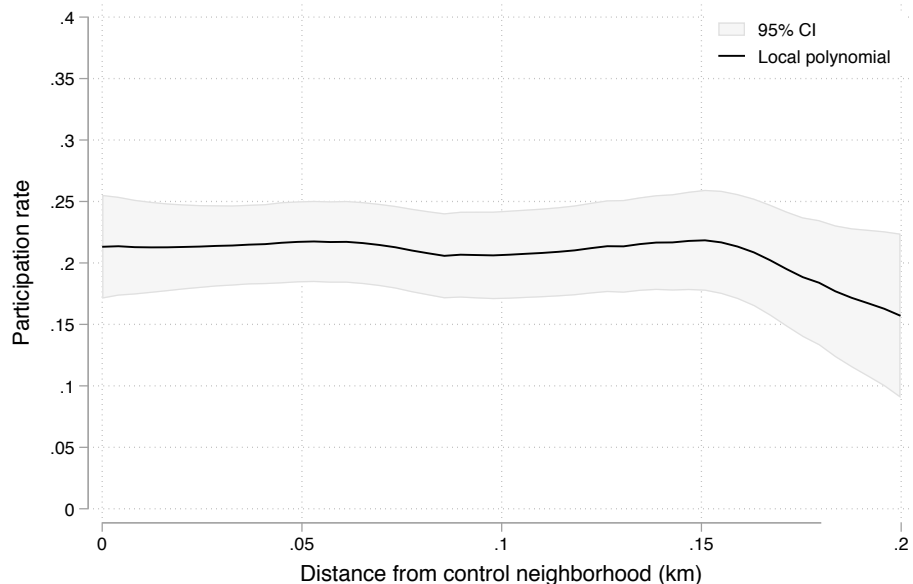
<i>Dependent variable</i>	Knowledge of provincial taxation		Townhall or evaluation	
	<i>Control</i> (1)	<i>Control</i> (2)	<i>Control</i> (2)	<i>Full sample</i> (3)
Flier recipient	0.862*** (0.073)	-0.005 (0.030)		
Campaign				0.049*** (0.017)
Covariates	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes
$R^2$	0.161	0.067	0.071	0.071
Observations	1210	1210	2778	2778
Clusters	145	145	356	356
Control Mean	-0.3	0.16	0.16	0.16

The dependent variable in Column 1 is a standardized index of five survey questions gauging respondents' knowledge of the property tax, tax rate, tax campaign, tax ministry, and the type of receipt for payers of the property tax. The dependent variable in Columns 2-3 indicates attendance at a townhall or submission of an evaluation. The sample in Columns 1-2 is the control group. Column 3 examines the full sample *excluding flier recipients in control*. Data: endline survey merged with participation records.

**Table A11:** Within-individual changes in views of the provincial government from baseline to endline

	Performance of government (1)	Trust in government (2)	Integrity of government spending (3)	Resp. for public goods provision (4)
Endline	0.020 (0.100)	0.001 (0.089)	0.025 (0.092)	-0.126 (0.090)
Individual FE	Yes	Yes	Yes	Yes
Observations	409	450	448	454
Clusters	119	120	120	120
ControlMean	-0.021	0.011	0.007	0.019

*Performance of government* is the respondent’s evaluation of the provincial government. *Trust in government* is self-reported trust in the provincial government. *Integrity of government spending* is the share of tax money respondents expect to be spent well rather than being wasted or stolen. *Resp. for public goods provision* is increasing in the perception that the provincial government should be a primary provider of public goods in Kananga. These outcomes are observed twice for a subsample of baseline participants who were found and re-surveyed after the tax campaign. The sample examined in the table is the 229 of these individuals in the control group. *Endline* indicates data collected during the endline survey (after the tax campaign). All regressions include individual fixed effects, so the analysis concerns changes from baseline to endline within individuals. Data: repeat baseline sample, discussed and analyzed primarily in (Weigel, 2018).



**Figure A14:** Participation (*Townhall or evaluation*) in treatment neighborhoods as a function of distance to control.

### A3.3 Mechanisms (Section VI)

This section contains supplementary tables and figures referenced in Section VI of the paper, examining each of the possible mechanisms in turn.

#### A3.3.1 Entitlement

The first possible mechanism is entitlement, in which the increase in participation is driven by taxpayers who expect reciprocal benefits or experience an endowment effect after paying. The additional exhibits here include the following.

- Table A12 examines coefficient stability in a regression of participation on payment in treatment neighborhoods while sequentially adding controls. Controlling for observables causes the correlation coefficient on the payment indicator to decrease from a magnitude of 0.046 with no controls to a magnitude of -0.004 with a large set of controls.
- Figure A15 plots the needed amount of negative selection on unobservables that would be needed to justify different hypothetical magnitudes of a causal effect of payment on participation. That is, despite the instability of the

correlation coefficient on the payment indicator in Table A12, I examine what amount of selection on unobservables in the opposite direction would be necessary for there to be a true causal effect of payment on participation, following (Oster, 2019). This type of selection would require unobservables that are positively correlated with payment but negatively correlated with participation. For instance, unobservable traits in which individuals have high ability to pay and also high opportunity costs of participation could generate this type of selection.<sup>19</sup> Figure A15 reveals that there would need to be selection on unobservables of -0.16 for there to be a true causal effect of payment on participation with magnitude of 0.05, i.e. a 5 percentage-point difference between payers and non-payers, which is approximately what the no-controls specification in Column 1 of Table A12 implies. This estimate implies a large degree of needed selection, and it increases substantially if we assume larger causal effects of payment. In light of evidence from observables suggesting that most forms of selection appear to push in the opposite direction, these high rates of negative selection on unobservables reinforce the other evidence in the paper that there is not a positive relationship between payment and participation, as one would expect in an entitlement mechanism.

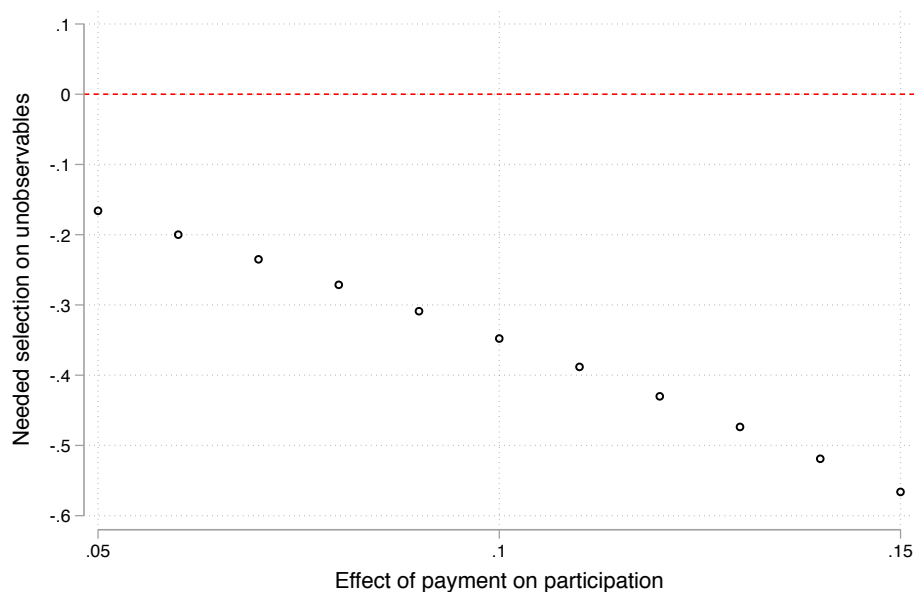
- Figure A16 plots, for each tax collector who worked on the campaign, the correlation between visits and payment, conditional on household covariates and stratum fixed effects. Despite the fact that collecting tax payments was impossible without visiting households (and thus the correlation cannot be negative), the largest correlation coefficient is 0.13, and for only a fifth of collectors is the correlation coefficient statistically different from zero. This considerable variation in the observed effort (visits) and effectiveness (payment) of tax collectors is reassuring for the IV strategy used in Section VI of the paper. Some neighborhoods are randomly assigned to a set of collectors likely to exert high effort; others are assigned to collectors likely to demonstrate high effectiveness.
- Table A13 shows the first stage regressions for the IV strategy.
- Table A14 offers a check of the identifying assumption behind the JIVE strategy by examining the correlation between these instruments and refusals in the midline survey in treatment neighborhoods. The logic is that, if there are

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<sup>19</sup>For what it's worth, Table A12 does not contain empirical patterns that support this intuition from observable proxies (i.e. different proxies of employment and income).



aspects of these underlying collector traits that impact participation through channels other than registration or payment, then that would be reflected in respondents' openness to complete the midline questionnaire, which occurred shortly after the tax campaign with all property owners. Table A14 reveals no statistically significant correlations between the JIVE instrument for payment propensity (or for registration propensity) and the probability of midline respondents' not completing the survey.

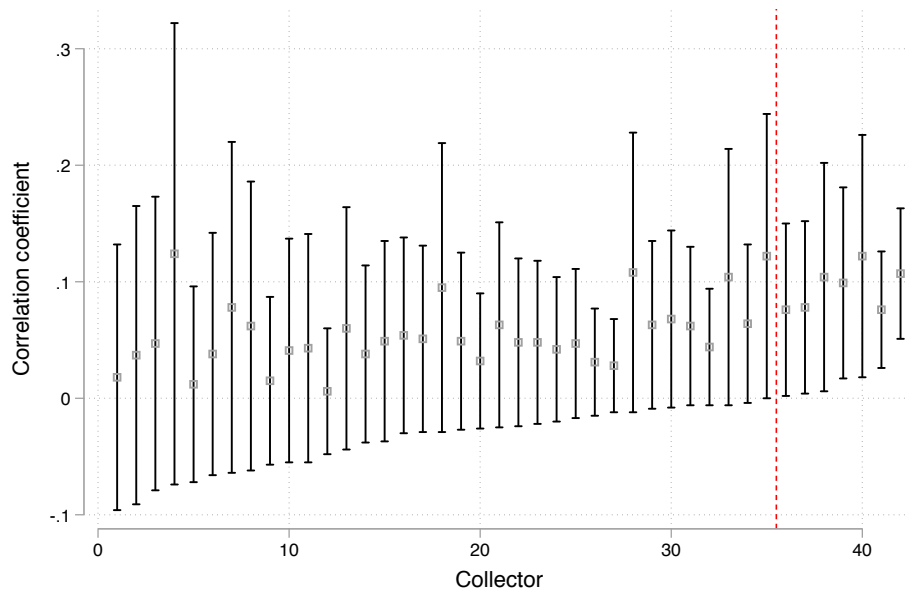


**Figure A15:** Needed selection on unobservables to account for positive causal effects of payment on participation.

**Table A12:** Coefficient stability analysis of the correlation between payment and participation in treatment neighborhoods

	Townhall or evaluation			
	(1)	(2)	(3)	(4)
Property tax compliance	0.046 (0.034)	0.019 (0.034)	0.014 (0.034)	-0.004 (0.037)
Age		0.012*** (0.003)	0.010*** (0.003)	0.011*** (0.003)
Age squared		-0.000*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)
Female		-0.142*** (0.021)	-0.085*** (0.024)	-0.093*** (0.025)
Monthly income (log)			0.018 (0.022)	0.006 (0.022)
Household wealth index			-0.002 (0.011)	0.002 (0.012)
Quality of public lighting			-0.060 (0.071)	-0.070 (0.071)
Education (years)			0.013*** (0.003)	0.012*** (0.003)
Unemployed			0.027 (0.021)	0.030 (0.022)
Owns business of any size			-0.016 (0.023)	-0.012 (0.024)
Government worker			0.064 (0.039)	0.069* (0.042)
Born in Kananga			-0.008 (0.022)	-0.010 (0.022)
Majority ethnicity			0.038* (0.022)	0.035 (0.022)
Integrity of govt. spending				-0.011 (0.010)
Stratum FE	No	Yes	Yes	Yes
$R^2$	0.001	0.076	0.097	0.100
Observations	1703	1703	1699	1596
Clusters	211	211	211	211
Dep. Var. Mean	0.208	0.208	0.208	0.208

*Townhall or evaluation* indicates participation in a townhall or submission of an evaluation. *Property tax compliance* indicates property tax payment validated in the admin data, as described on p. 18 in the paper. Other independent variables are considered in Table II. Data: endline survey merged with government tax database and participation records. The sample is restricted to treated neighborhoods. Sample size decreases in columns 3-4 because of non-response to certain survey questions introduced as controls.



**Figure A16:** Plotted correlation coefficients (and 95% CIs) between visits and tax compliance for individual tax collectors.

**Table A13:** IV - First stage: Predicting registration and tax payment using randomly assigned tax collector propensities (JIVE instruments) and double collector bonuses

<i>Dependent variable</i>	Registered as taxpayer		Property tax compliance			
	All HHs in treatment		Registered HHs			
<i>Sample</i>	(1)	(2)	(3)	(4)	(5)	(6)
Registration propensity	0.266*** (0.079)	0.175** (0.081)				
Payment propensity			0.115** (0.055)	0.124** (0.059)	0.233** (0.094)	0.230** (0.099)
Double collector bonus	0.340*** (0.032)	0.328*** (0.032)	0.065** (0.032)	0.078** (0.031)	-0.010 (0.038)	0.018 (0.037)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes
Enum FE	No	Yes	No	Yes	No	Yes
Observations	1703	1703	1703	1703	938	938
Clusters	211	211	211	211	210	210
Dep. Var. Mean	0.551	0.551	0.116	0.116	0.210	0.210
<i>F</i> -stat	64.216	55.688	4.241	5.281	3.195	2.753

*Registered as taxpayer* is an indicator for households registered with tax IDs. *Property tax compliance* is an indicator for households who paid the property tax. *Registration propensity* is a leave-one-out (JIVE) estimator that uses randomly assigned tax collectors' observed registration rates in other neighborhoods to predict the probability of households being registered as taxpayers in a given neighborhood. *Payment propensity* is constructed with the same procedure using instead tax collectors' observed payment rates in other neighborhoods to predict the payment rate in a given neighborhood. *Double collector bonus* is an indicator for households randomly assigned to be eligible for a double collector bonus. See p. 37 in the paper for details about these instruments. Data: endline survey merged with government tax database. The sample in all columns includes only the treatment group. Columns 5-6 further restrict the sample to registered households in treatment.

**Table A14:** Correlation between JIVE instruments and respondent refusals in midline survey in treatment neighborhoods

	Respondent refusals in midline survey			
	(1)	(2)	(3)	(4)
Payment propensity	-0.045 (0.042)	-0.012 (0.038)		
Registration propensity			-0.014 (0.036)	-0.021 (0.032)
Covariates	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes
Enumerator FE	No	Yes	No	Yes
$R^2$	0.358	0.517	0.355	0.517
Observations	1703	1703	1703	1703
Clusters	211	211	211	211
Dep. Var. Mean	0.090	0.090	0.090	0.090

*Respondent refusals in midline survey* is an indicator for midline survey respondents who did not complete the questionnaire. *Registration propensity* is a leave-one-out (JIVE) estimator that uses randomly assigned tax collectors' observed registration rates in other neighborhoods to predict the probability of households being registered as taxpayers in a given neighborhood. *Payment propensity* is constructed with the same procedure using instead tax collectors' observed payment rates in other neighborhoods to predict the payment rate in a given neighborhood. See p. 37 in the paper for details about these instruments. Data: endline survey merged with midline neighborhood-level estimates of survey refusals and non-response. The sample is restricted to treatment neighborhoods only.

### A3.3.2 Updating

The second possible mechanism is updating. This section first sketches a simple decision-theoretical framework for the updating mechanism, which has received less attention in the literature. Imagine there is a government and one citizen who is uncertain about the capacity of the government. The government sets a policy  $g(\theta, \lambda)$ , which is a deterministic function of the government's capacity,  $\theta \in \{H, L\}$ , and the citizen's decision to monitor the government,  $\lambda \in \{1, 0\}$ . The citizen incurs a cost  $c$  to participate, and receives utility  $u(g(\theta, \lambda))$  from the policy.

Government capacity ( $\theta$ ) is meant generally. It could be 'extractive capacity,' i.e. ability to collect taxes, or 'productive capacity,' i.e. ability to provide public goods and enforce contracts (Besley and Persson, 2009). A signal of either type of capacity triggers participation because the citizen believes the government will be more likely to affect his future wellbeing — through tax collection or public goods

provision — and thus he has an incentive to try to influence public policy to be as favorable as possible.

Concretely, the government can provide public goods, which increase the citizen's utility, and extract taxes, which decrease the citizen's utility. The citizen's preferred policy (high public goods, low taxes) results when the government is high capacity and when the citizen participates. To simplify notation, call this policy  $g^+$ . When the government is low capacity, the government always provides the same policy (low public goods, low taxes) regardless of citizen participation:  $g(L, 1) = g(L, 0)$ . In this case, the citizen has no incentive to participate. Call this policy  $g^0$ . When the government is high capacity and the citizen does not participate, however, the policy is worse for the citizen than  $g^0$  because the government collects taxes without providing public goods. Call this least-preferred (by the citizen) policy  $g^-$ . To summarize:

$$u(g^+) \geq u(g^0) \geq u(g^-) \quad (3)$$

Before the tax campaign, the citizen believes the government is high capacity with probability  $p \sim F(\cdot)$ . If he participates, his expected utility is:

$$EU_1 = p(u(g^+) - c) + (1 - p)(u(g^0) - c) \quad (4)$$

If he doesn't participate, his expected utility is:

$$EU_0 = p(u(g^-)) + (1 - p)(u(g^0)) \quad (5)$$

The citizen chooses the action that maximizes expected utility. There is a threshold point  $p^*$  of indifference between participating and not participating:

$$p^* = \frac{c}{u(g^+) - u(g^-)} \quad (6)$$

In this expression, the quantity  $(u(g^+) - u(g^-))$  is the participation dividend, which we might term  $d$ . The derivative with respect to  $d$  is negative:

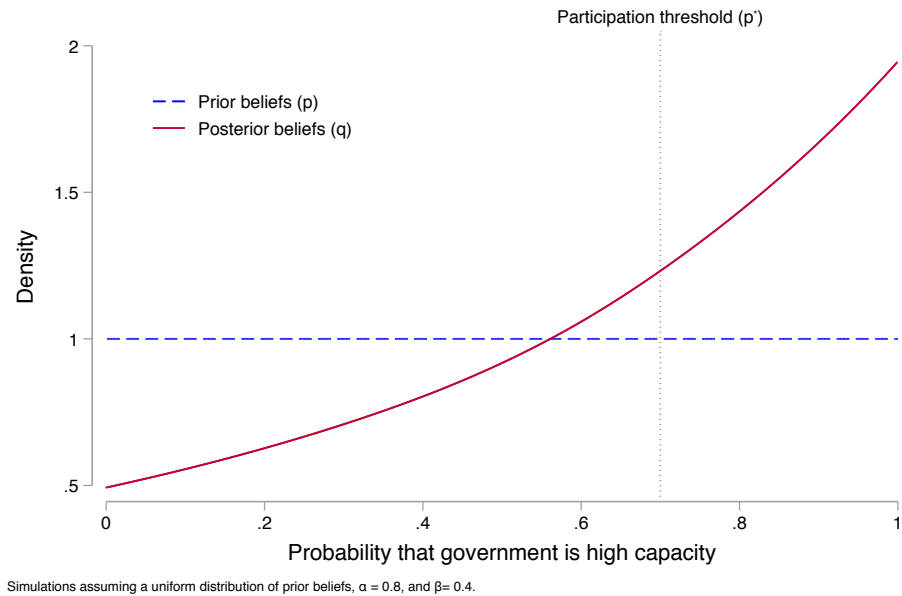
$$\frac{\partial p^*}{\partial d} = -\frac{c}{d^2} < 0 \quad (7)$$

Thus, as the participation dividend increases, citizens can be less confident that the government is high capacity but still choose to participate.

Now assume that the government launches a tax campaign, which sends a signal about its capacity ( $\theta$ ). The citizen knows that a high-capacity government administers a tax campaign with probability  $\alpha$ , and a low-capacity government administers a tax campaign with probability  $\beta$ . Then as long as  $\alpha \geq \beta$ , by Bayes' Theorem, the posterior probability ( $q$ ) that the government is high capacity conditional on administering a tax campaign is:

$$\frac{\alpha p}{\alpha p + \beta(1 - p)} = q \geq p \tag{8}$$

Let  $F(\cdot)$  be a uniform distribution, i.e.  $p \sim U(0, 1)$ , and  $\alpha = 0.8$  and  $\beta = 0.4$ . We can then simulate the distribution of  $q$ , as shown in Figure A17. A threshold ( $p^*$ ) is shown at a value of 0.7. Individuals with values of  $p$  to the right of this threshold participate; those to the left do not. There is more mass to the right of the threshold in the posterior distribution, indicating that individuals with priors to the left of the threshold have shifted in their beliefs to the right, choosing to participate only after receiving the signal sent by the tax campaign. Thus, the tax campaign catalyzes citizen engagement with the state by conveying information about the capacity of the state.



**Figure A17:** Simulated distributions of prior and posterior beliefs about government capacity.

The rest of this section contains tables and figures examining evidence for such an updating mechanism in the context of the 2016 property tax campaign in Kananga.

- Table A15 explores whether there are differences in the beliefs about the government between the control group and different subsets of the treatment group. The first column shows the main treatment effects in the full sample (identical to Table V in the paper). The second column restricts the treatment group to households that were registered by taxpayers only (while all control households are included). The patterns of updating are broadly analogous. If anything, removing unregistered households in treatment from the sample increases the gap with the control group regarding the perceived responsibility and extractive capacity. The third column then restricts the treatment group to non-payers only — again validating that these forms of updating occur among non-payers as well as payers. Finally, Column 4 restricts the treatment group only to registered households who did not pay. Once more, the pattern is largely the same. Of course, this analysis involves conditioning on outcomes, so it should be interpreted as suggestive and correlational.
- Table A16 examines beliefs in the treatment group by comparing relevant subgroups. The first column reproduces the overall treatment effect, while the next three columns restrict to the treatment group only and correlate beliefs among subgroups. Column 2 compares registered households and unregistered households in treatment. Registered households view the government as having higher extractive capacity. Column 3 then compares payers and nonpayers, finding similarly that payers believe the government has more extractive capacity (across all dimensions) and that tax collectors have more integrity. Column 4 compares participators with non-participators. Participators are similar overall, but as noted in the paper, they are noticeably less positive about the integrity of the government.
- Table A17 examines heterogeneous treatment effects by past exposure to the formal state. I use baseline measures of past state exposure aggregated to the neighborhood level: (1) past visits from government tax collectors, and (2) past engagement in political protests. The former measure captures state activity in the neighborhood, while the latter captures respondents' exposure to the state outside of the neighborhood. Consistent with an updating mechanism, the tax campaign appears to have a larger effect on participation in neighborhoods



with relatively less past exposure to the state, though the standard errors on the interaction terms are too large to make this analysis anything more than suggestive.

- Figure A18 plots the participation rate (*Townhall or evaluation*) in control neighborhoods as a function of distance to the nearest treated neighborhood. This does not reveal an increase in participation close to treatment neighborhoods, consistent with spillover effects.
- Table A18 more formally explores spillover effects of the tax campaign, leveraging its cluster-level random assignment. There is evidence of spillovers in collector visits, which is not surprising given that neighborhood borders were often not clearly indicated on the ground. However, such spillovers in visits did not appear to lead to a (detectable) spillover on tax payment in control neighborhoods. There is also no statistically significant evidence of spillovers on costly participation. I am powered to detect a spillover effect size of 0.002 when analyzing the *Treated neighbors* variable (a count of number of adjacent treated neighborhoods) and of 0.049 when analyzing the *Treated borders* variable.

The interpretation of the first estimate is that an additional treated neighbor would be associated with an increase in participation of 0.2 percentage points. The interpretation of the second is that an additional kilometer of border with a treated neighborhood would be associated with an increase in participation of 4.9 percentage points. Although the second spillover estimate may at first seem large, the fact that the units are kilometers changes that interpretation.<sup>20</sup> The median amount of shared border with a treatment neighborhood is 0.75 kilometers, and the standard deviation is 0.47 kilometers. A one-kilometer increase therefore represents a large increase, the equivalent of more than two standard deviations. It is perhaps more informative, then, to note that the spillover analysis can rule out a maximum effect size of 2.3 percentage points associated with a one standard-deviation increase in the length of borders shared with treatment.

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<sup>20</sup>The variable was initially measured in meters, but the magnitude was too small to read in a table. So I rescaled the variable to make it legible.

**Table A15:** Effects of the campaign on beliefs about the government: correlations in different subgroups

<i>Dependent variable</i>	Full sample (1)	Registered only (2)	Nonpayers only (3)	Registered and nonpayers only (4)
<i>Panel I: Responsibility of government</i>				
Resp. for public goods provision	0.117** (0.051)	0.135*** (0.055)	0.117** (0.052)	0.140*** (0.058)
<i>Panel II: Extractive capacity of government</i>				
Information about citizens	0.147*** (0.044)	0.169*** (0.049)	0.127*** (0.045)	0.136*** (0.051)
Ability to punish evaders	0.046 (0.047)	0.093* (0.049)	0.018 (0.049)	0.062 (0.053)
Perceived citizen compliance	0.343*** (0.053)	0.452*** (0.063)	0.252*** (0.048)	0.291*** (0.060)
Performance of tax ministry	0.122*** (0.047)	0.121** (0.051)	0.094* (0.048)	0.068 (0.053)
<i>Panel III: Productive capacity of government</i>				
Ability to provide public goods	-0.013 (0.053)	-0.071 (0.059)	-0.020 (0.053)	-0.096 (0.061)
Performance of government	0.043 (0.049)	0.048 (0.054)	0.035 (0.050)	0.042 (0.056)
<i>Panel IV: Integrity of government</i>				
Integrity of tax collectors	0.187*** (0.044)	0.228*** (0.049)	0.147*** (0.045)	0.152*** (0.051)
Integrity of government spending	0.109** (0.050)	0.085 (0.055)	0.112** (0.050)	0.087 (0.055)
Transparency of government	0.031 (0.045)	-0.003 (0.053)	0.019 (0.046)	-0.036 (0.055)

Each cell summarizes a separate OLS regression. The first column shows the treatment effect from estimating Equation 1 in the paper with the dependent variable indicated (cf. paper Table V). The next columns restrict the treatment group to (i) only households that were registered by tax collectors, (ii) only non-payers, and (ii) registered non-payers, respectively. Data: endline survey merged with government tax database.

**Table A16:** Correlations between citizens' beliefs about the government and taxpayer registration, payment, and participation

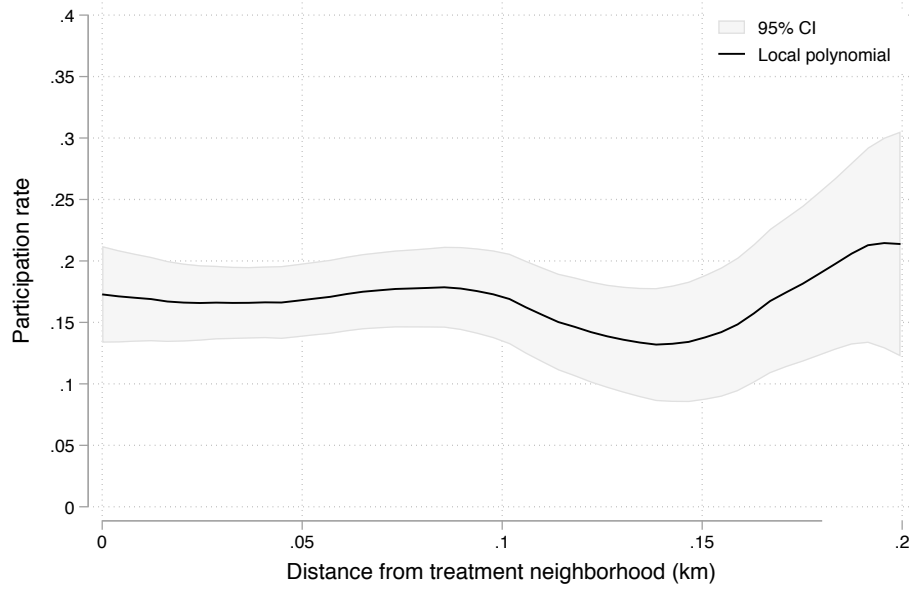
<i>Dependent variable</i>	<i>Full sample</i>	<i>Treatment group only</i>		
	Treatment Effect (1)	Registered vs not (2)	Payers vs not (3)	Participators vs not (4)
<i>Panel I: Responsibility of government</i>				
Resp. for public goods provision	0.117** (0.051)	0.059 (0.044)	0.027 (0.064)	0.071 (0.100)
<i>Panel II: Extractive capacity of government</i>				
Information about citizens	0.147*** (0.044)	0.074 (0.048)	0.189*** (0.073)	-0.093 (0.095)
Ability to punish evaders	0.046 (0.047)	0.098** (0.047)	0.213*** (0.073)	0.210** (0.098)
Perceived citizen compliance	0.343*** (0.053)	0.244*** (0.065)	0.725*** (0.119)	-0.109 (0.118)
Performance of tax ministry	0.122*** (0.047)	0.009 (0.050)	0.331*** (0.071)	-0.018 (0.107)
<i>Panel III: Productive capacity of government</i>				
Ability to provide public goods	-0.013 (0.053)	-0.086 (0.057)	0.082 (0.103)	-0.046 (0.098)
Performance of government	0.043 (0.049)	0.035 (0.050)	0.055 (0.081)	-0.051 (0.118)
<i>Panel IV: Integrity of government</i>				
Integrity of tax collectors	0.187*** (0.044)	0.096* (0.051)	0.463*** (0.084)	-0.249** (0.120)
Integrity of government spending	0.109** (0.050)	-0.046 (0.052)	-0.018 (0.092)	-0.161* (0.083)
Transparency of government	0.031 (0.045)	-0.077 (0.051)	0.104* (0.062)	0.170 (0.122)

Each cell summarizes a separate OLS regression. The first column shows the treatment effect from estimating Equation 1 in the paper with the dependent variable indicated. Columns 2-4 show, within the treatment group only, the correlations between the indicated belief about the government with taxpayer registration, tax payment, and participation, respectively. Data: endline survey merged with government tax database and participation records.

**Table A17:** Heterogeneous effects of the campaign on participation by past exposure to the formal state

	Townhall or evaluation		
	(1)	(2)	(3)
Campaign	0.050*** (0.016)	0.060*** (0.021)	0.071*** (0.019)
Campaign X Past visits (high)		-0.027 (0.033)	
Past visits (high)		0.027 (0.025)	
Campaign X Past protest (high)			-0.055 (0.037)
Past protest (high)			0.033 (0.023)
Covariates	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes
$R^2$	0.071	0.071	0.072
Observations	2913	2913	2913
Clusters	356	356	356
Control Mean	0.16	0.16	0.16
$F$ -test $p$ -value		0.0092	0.00094

The outcome is an indicator for attending a townhall or submitting an evaluation. *Past visits (high)* indicates neighborhoods above the median level of past visits from government tax collectors reported during baseline. *Past protest (high)* indicates neighborhoods above the median level of past citizen participation in protests reported during baseline. See p. 41 in the paper for further details about these variables. Data: endline survey merged with participation records and neighborhood-level measures from baseline survey.



**Figure A18:** Participation (*Townhall or evaluation*) in control neighborhoods by distance to treatment.

**Table A18:** Spillover effects of the campaign on tax collector visits, compliance, and participation

	Visited by collectors		Paid property tax		Townhall or evaluation	
	(1)	(2)	(3)	(4)	(5)	(6)
Campaign	0.480*** (0.020)	0.480*** (0.020)	0.109*** (0.010)	0.111*** (0.010)	0.049*** (0.016)	0.049*** (0.016)
Treated neighbors	0.017* (0.010)		0.006 (0.005)		0.001 (0.008)	
Total neighbors	-0.011 (0.010)		-0.009* (0.005)		-0.004 (0.007)	
Treated borders		0.063** (0.030)		0.005 (0.015)		0.016 (0.025)
Total borders		-0.037 (0.031)		-0.008 (0.015)		-0.026 (0.023)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes
$R^2$	0.260	0.261	0.086	0.084	0.071	0.071
Observations	2913	2913	2913	2913	2913	2913
Clusters	356	356	356	356	356	356
Control Mean	0.17	0.17	.00058	0.0058	0.16	0.16

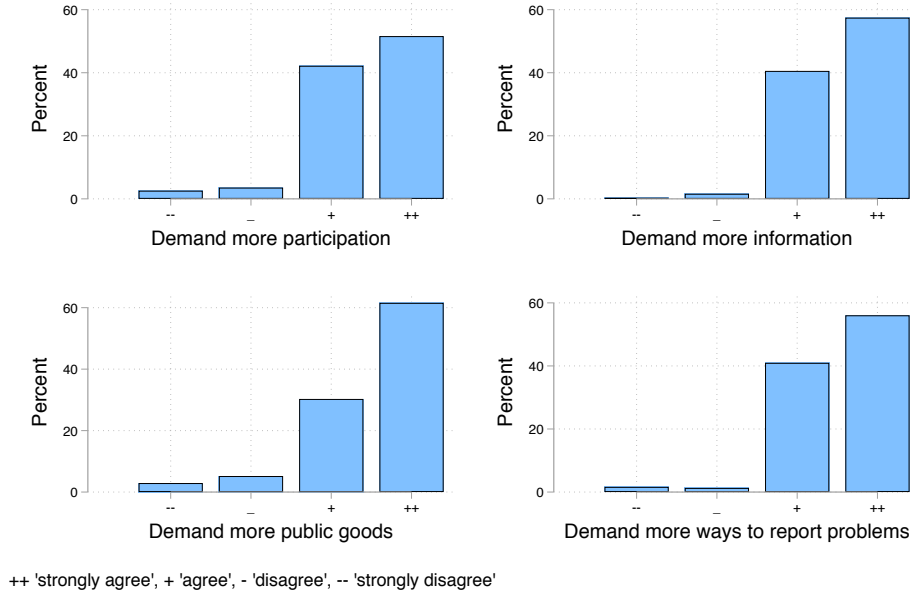
*Treated neighbors* is a count of the number of adjacent treated neighborhoods. *Total neighbors* is a count of the total number of adjacent neighborhoods. *Treated borders* is the total length (in kilometers) of borders shared with treated neighborhoods. *Total borders* is the total length of all neighborhood borders. Data: endline data merged with administrative data on tax payment, participation records, and geographic measures on the neighborhood level.

### A3.3.3 Bargaining

The third possible mechanism is bargaining, in which the tax campaign gave citizens a bargaining chip they could use to demand better governance in exchange for future tax compliance. This section contains several additional pieces of evidence regarding this mechanism.

- Figure A19 summarizes the content of submitted evaluations. It shows that submitted evaluations overwhelmingly demanded more participation, information, public goods, and a location to report problems with public services.
- Table A19 replicates the treatment effects on submission of government evalu-

ations while restricting to evaluation forms that (i) demanded the government do a better job, or (i) contained a written-in demand.



**Figure A19:** Content of submitted evaluations.

**Table A19:** Effects of the campaign on submission of evaluations demanding better governance or containing written-in demands or complaints

	Evaluation demanding better governance (1)	Evaluation with written-in demand / complaint (2)
Campaign	0.021** (0.010)	0.020** (0.008)
Covariates	Yes	Yes
Stratum FE	Yes	Yes
$R^2$	0.049	0.038
Observations	2913	2913
Clusters	356	356
Control Mean	0.078	0.033

*Evaluation demanding better governance* is an indicator variable for individuals who submitted an evaluation demanding the government do a better job (rather than expressing satisfaction). *Evaluation with written-in demand / complaint* is an indicator for respondents who, in the optional comment section at the bottom of the evaluation form, wrote demands of or complaints about the provincial government. Data: endline survey merged with participation records.

### A3.3.4 Coordination

This section examines the plausibility of a coordination mechanism. The intuition behind this mechanism is that a door-to-door tax campaign could increase participation by stimulating communication or shared grievances in treated neighborhoods and thus lowering the costs of coordination. Coordination could have influenced individuals' participation decisions if citizens anticipated being more successful in lobbying the government for public goods spending if more members of the neighborhood attended townhall meetings or submitted evaluations making common demands.<sup>21</sup>

At first glance, this explanation appears unlikely because there were no instances of individuals from the same neighborhood standing up together at townhall meetings to make an overtly joint demand. Also, the intracluster correlation of participating in either the townhall or evaluation submission is relatively low (0.073), and there are not obvious patterns in the spatial distribution of participators within neighborhoods that would suggest a collective action mechanism (Figure A21).<sup>22</sup> However, the campaign could have triggered coordination in more subtle ways.

More formal tests of this mechanism include: (1) examining if treated townhall participants were more likely to arrive at meetings accompanied by other members of the neighborhood; and (2) examining if participants' houses were more clustered geographically within treatment.

The first test is whether individuals in treatment neighborhoods were more likely to travel together to townhall meetings. If the tax campaign lowers barriers to coordination, then one might expect individuals to have shared a motorbike taxi to the townhall meeting venue.<sup>23</sup> They would then arrive together and appear consecutively in participant log. Using data from this log, I define an indicator for individuals who arrived before or after someone from the same neighborhood. I

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<sup>21</sup>Coordination effects would likely be complements to updating about state capacity. Citizens would not only anticipate greater individual-level benefits to participation after observing the tax campaign; they would also be better able to coordinate with their neighbors to lobby the government. That said, Arias et al. (2019) argue that, theoretically, updating and coordination could be complementary, substitutes, or independent mechanisms.

<sup>22</sup>For instance, if participants' houses were more densely clustered in treatment neighborhoods, this might suggest a coordination mechanism, as I explore formally below.

<sup>23</sup>As noted, many individuals in the sample lived far from the venue, such that a taxi ride could cost as much as \$2, the median household's daily income (and the cost of the property tax). Sharing motorbike taxis is very common in Kananga.



then regress this variable on the treatment indicator using the sample of all townhall participants.

The results are summarized in Table A20. Column 1 shows that the coefficient on the treatment indicator is positive, but not statistically significant at conventional levels.<sup>24</sup> Column 2 controls for the total attendance rate in a neighborhood to explore the possibility that joint arrivals are mechanically explained by the higher number of attendees in treatment. Although the neighborhood attendance rate is also an outcome of treatment, and therefore a “bad control” (Angrist and Pischke, 2008), it is nonetheless suggestive that adding this regressor on the right-hand side only slightly depresses the magnitude of the coefficient on the treatment indicator. Column 3 then further controls for the average population density of the neighborhood, which could also impact joint arrivals.<sup>25</sup> Although the sample size is small, this analysis provides little support for the view that townhall participants were coordinating on attending the townhall meetings to a greater degree in treatment compared to control.

The second test of a coordination mechanism is to examine whether treated participants’ houses were more clustered geographically within neighborhoods compared to control. If individuals were making isolated, independent decisions about whether or not to participate, then their houses would have been, on average, spread out evenly throughout the neighborhood, due to the method of random sampling used by enumerators.<sup>26</sup> A stylized depiction appears in the first panel of Figure A20. However, if a coordination mechanism explains the increase in participation caused by the tax campaign, then we might expect individuals in the same vicinity to have also been more likely to attend, such as those within the red circle in the second panel. The third panel indicates a spatial distribution of participator households consistent with a coordination mechanism. The overall spatial distribution of participating households in treatment and control appears in Figure A21.

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<sup>24</sup>The sample size is small in this analysis, so I may well be underpowered to detect an effect even of this non-trivial magnitude.

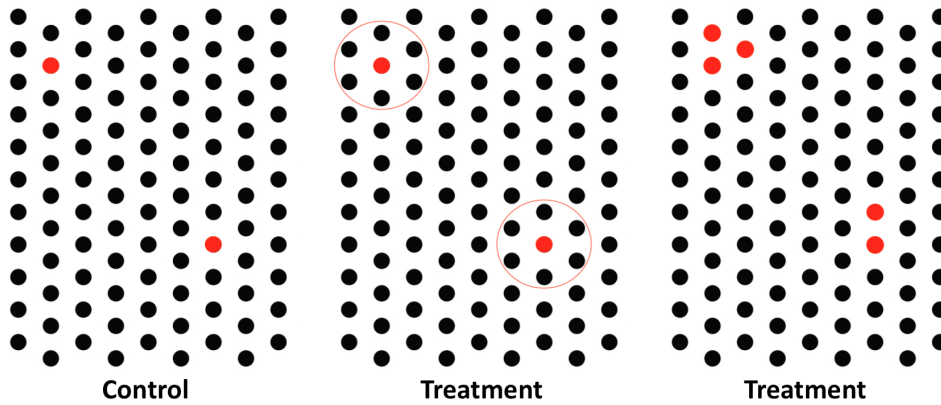
<sup>25</sup>Population density is estimated by dividing the estimated number of households by the total area of the neighborhood.

<sup>26</sup>As noted on p. 11, enumerators followed a skip pattern throughout the whole neighborhood, so respondents are typically spread out.

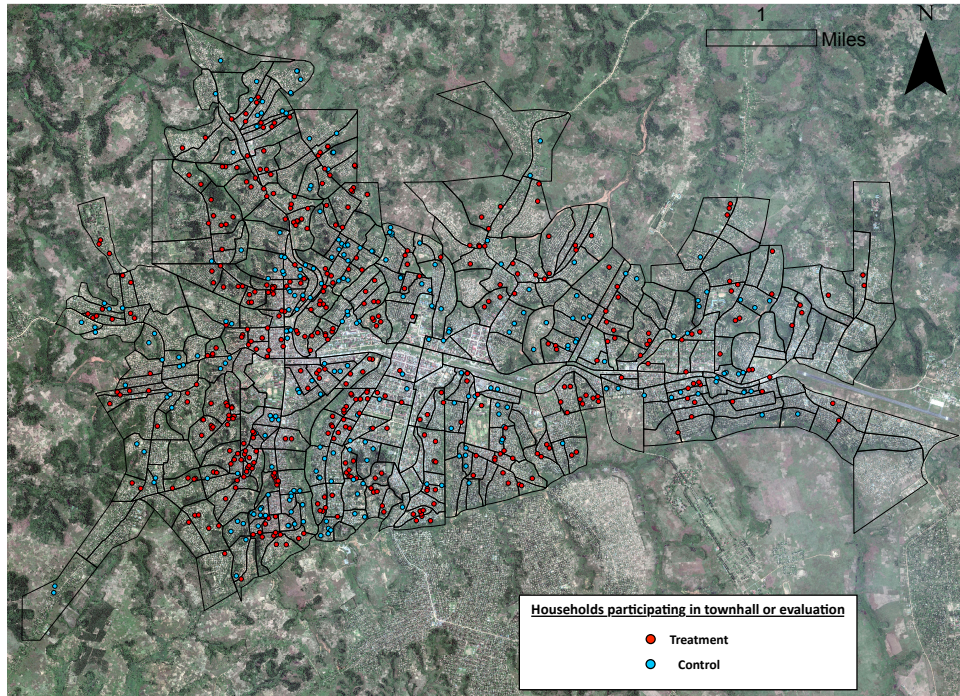
**Table A20:** Effects of the campaign on arriving at a townhall meeting with member of neighborhood

	Arrived at townhall meeting with neighbor		
	(1)	(2)	(3)
Campaign	0.041 (0.047)	0.036 (0.047)	0.029 (0.046)
Neighborhood townhall attendance		0.334** (0.163)	0.313* (0.160)
Neighborhood population density			-0.000* (0.000)
Covariates	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes
$R^2$	0.102	0.120	0.133
Observations	379	379	379
Clusters	184	184	184
Control Mean	.083	.083	.083

The outcome variable is an indicator for townhall participants who arrived immediately before or after another individual from the same neighborhood. *Neighborhood townhall attendance* is the average rate of townhall attendance in the neighborhood. *Neighborhood population density* is the estimated total number of houses in a neighborhood divided by its area. Data: endline survey merged with townhall attendance records. The sample includes all individuals who attended a townhall meeting.



**Figure A20:** Stylized illustration of the spatial distribution of participant households in treatment and control neighborhoods.



**Figure A21:** Locations of treatment and control individuals’ households who attended a townhall meeting or submitted an evaluation.

An observable implication of this hypothesis is that the average distance among the houses of participators within neighborhoods should be *smaller* in treatment relative to control, conditional on the total number of participators per neighborhood. To construct this measure, I first calculate the Euclidean distance between each participant’s household within a neighborhood and then take the average of these distances.<sup>27</sup> I then regress this neighborhood-level measure on the treatment indicator to test if the average distance among participating households is smaller in treatment relative to control. As shown in Table A21, the point estimate on the treatment indicator is not statistically distinguishable from zero. This remains true if we condition on the neighborhood participation level — the number of households participating could mechanically decrease the average distance<sup>28</sup> — and the average population density of the neighborhood. This analysis, therefore, does not provide much support for a coordination mechanism.

<sup>27</sup>The analysis is thus on the neighborhood level, omitting neighborhoods with fewer than two participants.

<sup>28</sup>This is, again, a “bad control,” but included for completeness.

**Table A21:** Effects of the campaign on the average Euclidean distance among participators’ households

	Average distance among participating households		
	(1)	(2)	(3)
Campaign	-0.462 (1.748)	-1.096 (1.749)	-1.257 (1.702)
Neighborhood participation level		12.217*** (3.852)	11.739*** (3.888)
Neighborhood population density			-0.003* (0.001)
Stratum FE	Yes	Yes	Yes
$R^2$	0.265	0.273	0.276
Observations	482	482	482
Control Mean	14	14	14

The outcome variable is the neighborhood-level average Euclidean distance (measured in degrees) among houses of all individuals who either attended a townhall or submitted an evaluation. *Neighborhood participation level* is the average rate of participation (attending a townhall or submitting an evaluation) in the neighborhood. *Neighborhood population density* is the estimated total number of houses divided by the area of the neighborhood. Data are from the endline survey, merged with participation records and geographic measures, collapsed to the neighborhood level. The unit is the neighborhood. The sample includes all neighborhoods with multiple individuals who participated.

In sum, there is little evidence that treated participants arrived together at townhall meetings or were more clustered geographically. It is thus unlikely that a coordination mechanism explains the effect of the tax campaign on participation.

## A4 Robustness checks

This section reports robustness checks for the main estimations in the paper, including the following specifications (variants of Equation 1 in the paper):

1. **No covariates:** This specification includes only strata fixed effects.<sup>29</sup>
2. **Basic covariates:** This specification includes only gender, age, age squared in  $\mathbf{X}_{ijk}$  and nothing in  $\mathbf{X}_{jk}$ .
3. **All covariates:** This specification includes all covariates listed as possible

<sup>29</sup>In the pre-analysis plan, I planned always to include the ‘basic covariates’ below. But for completeness, I have included a no-covariates specification in the robustness tables.

covariates in the pre-analysis plan. On the individual level (i.e. in  $\mathbf{X}_{ijk}$ ), these include gender, age, age squared, years of education, log income, wealth, a dummy for business owners, a dummy for government workers, and a dummy for multiple plot owners. On the neighborhood level (i.e. in  $\mathbf{X}_{jk}$ ), these include baseline measures of average road quality, public lighting, wealth, past reported visits from government tax collectors, past reported payment of taxes, political participation, evaluation of the provincial government, trust in the provincial government, views of government corruption, and views of the importance of the provincial government in public goods provision. I also include dummies indicating deciles of the time lag between the tax campaign and outcome measurement.<sup>30</sup>

4. **Enumerator fixed effects:** This specification includes fixed effects for the 15 enumerators who administered the endline survey.
5. **Sampling weights:** This specification includes the sampling weights discussed in Section A2.2. The resulting estimates can be considered representative of all property owners in Kananga.
6. **House quality heterogeneity:** This specification examines heterogeneous treatment effects of the tax campaign by wealth, as proxied by house quality. This specification was mentioned in the pre-analysis plan as a way to shed light on mechanisms. In particular, the signal about the government sent by the tax campaign may be stronger among individuals with less prior contact with the government. Baseline data reveal that less wealthy individuals are a clearly defined subgroup with less past exposure to the state.<sup>31</sup> Exploring heterogeneity by this dimension complements the analysis of mechanisms in Section VI of the paper. Moreover, it is of general interest to explore if property tax collection is regressive in this context, and whether citizens' political responses vary by wealth. For simplicity, I examine heterogeneous treatment

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<sup>30</sup>I pre-specified these dummies to control for potential impacts of information about the campaign spreading over time.

<sup>31</sup>Specifically, individuals who are below the median in a household wealth index (constructed using the observed quality of the roof, toilets, floor, access to electricity, and ownership of a vehicle) are 4.5 percentage points less likely to report ever seeing government agents in their neighborhood. They are 3.4 percentage points less likely to know the full name of the provincial governor, and 10 percentage points less likely to know the name of the provincial tax ministry. These magnitudes increase considerably among even less wealthy segments of the population (e.g. the bottom quartile of the wealth distribution).

effects by interacting the campaign indicator with a binary measure of wealth (*House quality*) that equals 0 if a house is constructed with mud bricks (56% of the sample) and 1 if a house is constructed with fired bricks, cement, or any other modern material (44% of the sample). To be precise, I estimate the following equation:

$$y_{ijk} = \gamma_1 I_{jk}^{Campaign} + \gamma_2 I_{jk}^{Campaign} * House\ quality_{ijk} + \gamma_3 House\ quality_{ijk} + \alpha_k + \mathbf{X}_{ijk}\mathbf{\Gamma} + \mathbf{X}_{jk}\mathbf{\Phi} + \varepsilon_{ijk} \quad (9)$$

In robustness Tables A22 and A23, the estimated treatment effect on households of below-median house quality ( $\hat{\gamma}_1$ ) and the coefficient on the interaction term ( $\hat{\gamma}_2$ ) are reported in the fifth and sixth columns, respectively. The  $p$ -value of an  $F$ -test of equivalence between the  $\hat{\gamma}_1$  and  $\hat{\gamma}_2$  parameters is reported in the seventh column.

Finally, Table A24 shows Average Effect Size (AES) coefficients for all index outcome variables examined in the paper, following Clingingsmith et al. (2009).

**Table A22:** Robustness checks for treatment effects on payment and participation

<i>Dependent variable</i>	No Covariates	Basic covariates	All covariates	Enumerator fixed effects	Sampling weights	House quality heterogeneity		
	$\hat{\beta}_1$	$\hat{\beta}_1$	$\hat{\beta}_1$	$\hat{\beta}_1$	$\hat{\beta}_1$	$\hat{\gamma}_1$	$\hat{\gamma}_2$	<i>F</i> -test <i>p</i> -value
<i>Panel I: Compliance</i>								
Property tax compliance	0.108 (0.010)	0.109 (0.010)	0.111 (0.011)	0.108 (0.010)	0.106 (0.010)	0.092 (0.012)	0.040 (0.018)	0.053 (0.053)
<i>Panel II: Participation</i>								
Townhall meeting attendance	0.036 (0.020)	0.044 (0.020)	0.052 (0.021)	0.054 (0.018)	0.041 (0.020)	0.053 (0.024)	-0.017 (0.035)	0.181 (0.181)
Evaluation form submission	0.020 (0.012)	0.021 (0.012)	0.028 (0.012)	0.025 (0.012)	0.027 (0.013)	0.032 (0.015)	-0.016 (0.023)	0.170 (0.170)
Townhall or evaluation	0.047 (0.016)	0.050 (0.016)	0.049 (0.017)	0.053 (0.016)	0.046 (0.017)	0.057 (0.021)	-0.016 (0.029)	0.110 (0.110)
Townhall and evaluation	0.027 (0.009)	0.028 (0.009)	0.026 (0.010)	0.032 (0.009)	0.030 (0.010)	0.037 (0.012)	-0.021 (0.016)	0.022 (0.022)
Index (townhall & evaluation)	0.140 (0.043)	0.147 (0.042)	0.142 (0.043)	0.158 (0.042)	0.144 (0.044)	0.177 (0.053)	-0.069 (0.074)	0.031 (0.031)
Cost of participation (transport)	0.052 (0.017)	0.054 (0.017)	0.044 (0.016)	0.058 (0.015)	0.049 (0.016)	0.066 (0.021)	-0.033 (0.028)	0.027 (0.027)
Cost of participation (transport & opp.)	0.073 (0.022)	0.075 (0.022)	0.062 (0.021)	0.082 (0.020)	0.070 (0.021)	0.087 (0.028)	-0.035 (0.038)	0.041 (0.041)

In Columns 1-5, each cell summarizes an estimation of Equation 1 in the paper for the indicated outcome variable. The top number is the estimated coefficient on the treatment indicator ( $\hat{\beta}_1$ ), and the bottom number is the cluster-robust standard error. Columns 6-8 summarize estimations of Equation 9, showing heterogeneous treatment effects by house quality. Columns 6 and 7 provide estimates of  $\hat{\gamma}_1$  and  $\hat{\gamma}_2$ , and Column 8 shows the *p*-value from an *F*-test of equivalence of these two parameters. For more information about each of these robustness checks, see p. 56. All dependent variables are identical to those discussed in Section IV.B of the paper.

**Table A23:** Robustness checks for treatment effects on beliefs about the government

<i>Dependent variable</i>	No	Basic	All	Enumerator	Sampling	House quality		
	Covariates	covariates	covariates	fixed effects	weights	heterogeneity		
	$\hat{\beta}_1$	$\hat{\beta}_1$	$\hat{\beta}_1$	$\hat{\beta}_1$	$\hat{\beta}_1$	$\hat{\gamma}_1$	$\hat{\gamma}_2$	<i>F</i> -test <i>p</i> -value
<i>Panel I: Responsibility of government</i>								
Resp. for public goods provision	0.109 (0.052)	0.104 (0.052)	0.117 (0.051)	0.037 (0.041)	0.119 (0.051)	0.105 (0.063)	0.028 (0.086)	0.569 (0.569)
<i>Panel II: Extractive capacity of government</i>								
Information about taxpayers	0.119 (0.045)	0.127 (0.046)	0.139 (0.045)	0.093 (0.039)	0.131 (0.046)	0.107 (0.059)	0.088 (0.078)	0.878 (0.878)
Ability to punish evaders	0.028 (0.049)	0.036 (0.048)	0.043 (0.047)	0.029 (0.037)	0.036 (0.049)	0.036 (0.063)	0.023 (0.085)	0.922 (0.922)
Perceived citizen compliance	0.313 (0.056)	0.318 (0.055)	0.282 (0.054)	0.360 (0.049)	0.355 (0.054)	0.251 (0.063)	0.218 (0.095)	0.813 (0.813)
Performance of tax ministry	0.151 (0.049)	0.149 (0.049)	0.152 (0.049)	0.076 (0.042)	0.136 (0.048)	0.100 (0.062)	0.049 (0.085)	0.708 (0.708)
<i>Panel III: Productive capacity of government</i>								
Ability to provide public goods	0.003 (0.053)	0.002 (0.053)	0.005 (0.055)	-0.032 (0.040)	-0.005 (0.054)	0.000 (0.069)	-0.029 (0.090)	0.845 (0.845)
Performance of government	0.065 (0.051)	0.065 (0.051)	0.073 (0.050)	0.004 (0.040)	0.062 (0.051)	-0.022 (0.066)	0.140 (0.090)	0.258 (0.258)
<i>Panel IV: Integrity of government</i>								
Integrity of tax collectors	0.209 (0.045)	0.206 (0.045)	0.187 (0.044)	0.164 (0.040)	0.187 (0.045)	0.169 (0.057)	0.040 (0.081)	0.305 (0.305)
Integrity of government	0.134 (0.050)	0.129 (0.050)	0.107 (0.048)	0.075 (0.043)	0.141 (0.050)	0.204 (0.063)	-0.204 (0.081)	0.002 (0.002)
Transparency of government	0.072 (0.047)	0.068 (0.048)	0.055 (0.047)	-0.007 (0.039)	0.045 (0.047)	0.058 (0.061)	-0.057 (0.080)	0.376 (0.376)

In Columns 1-5, each cell summarizes an estimation of Equation 1 in the paper for the indicated outcome variable. The top number is the estimated coefficient on the treatment indicator ( $\hat{\beta}_1$ ), and the bottom number is the cluster-robust standard error. Columns 6-8 summarize estimations of Equation 9, showing heterogeneous treatment effects by house quality. Columns 6 and 7 provide estimates of  $\hat{\gamma}_1$  and  $\hat{\gamma}_2$ , and Column 8 shows the *p*-value from an *F*-test of equivalence of these two parameters. For more information about each of these robustness checks, see p. 56. All dependent variables are identical to those discussed in Section IV.B of the paper.



**Table A24:** Robustness check for index variables: AES coefficients

	Townhall & evaluation index (1)	Resp. for public goods provision (2)	Information about taxpayers (3)	Ability to punish tax evaders (4)	Performance of tax ministry (5)	Ability to provide public goods (6)	Performance of government (7)	Transparency of government (8)
Campaign	0.101*** (0.037)	0.064** (0.028)	0.107*** (0.032)	0.039 (0.040)	0.106*** (0.039)	-0.009 (0.043)	0.035 (0.040)	0.025 (0.038)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2913	2913	2910	2883	2791	2484	2795	2890
Clusters	356	356	356	356	356	356	356	356

Estimated AES coefficients for index variables examined in the paper. All estimates are from the basic specification shown throughout the paper. Details on the estimation approach can be found in Clingensmith et al. (2009). Data: endline survey merged with participation records.

## A5 Exact text of survey questions and question-specific results

This section provides the exact text of the questions used to construct all survey-based variables considered in the paper.<sup>32</sup> It also shows question-specific results (in cases when I otherwise used indices).

### A5.1 Variables considered in Section V.

#### A5.1.1 Survey question text

1. *Resp. for public goods provision.* This variable is a standardized index composed of respondents' answers to two sets of survey questions. In the first set, the enumerator first read the prompt and then gave the respondent a laminated list of all the possible providers.<sup>33</sup> Then, the enumerator listed each of the sectors below, asking which provider should be primarily responsible for service provision in that sector.
  - Prompt: 'I am going to list some services/infrastructure many communities have. Tell me who you think should be primarily responsible for providing each one in our community. This does not need to be the current provider of these services/infrastructure.'
  - Sectors: schools, water system/wells, health care, keeping people safe, helping poor people, economic development, road maintenance.
  - Possible providers: national government, provincial government, NGOs, community organizations, religious groups/leaders, traditional leaders/chiefs, international organizations, 'everyone should take care of themselves'.

In the second set of variables, the enumerator read the prompt followed by two points of view asking which the respondent agreed more with. Finally, the enumerators asked if the respondent agreed strongly or just agreed with the statement. Each variable is coded from 0 to 3, where 0 indicates the respondent envisions the least responsibility for the provincial government, and 3 indicates the most responsibility.

- Prompt: 'Now, I'm going to read you several statements of opposing viewpoints. Please tell me with which statement you most agree.'
- Sets of opposing viewpoints:
  - (a) 'Some people say the provincial government should take more responsibility to ensure that everyone is provided for' or 'Other people

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<sup>32</sup>Note that the main variables, such as payment and participation, are discussed in Section IV.B in the paper.

<sup>33</sup>This first set is labeled the 'sector questions' in Table A8, while the second set is labeled 'hypotheticals.'

say that people should take more responsibility to provide for themselves.’

- (b) ‘Foreign aid organizations should provide more services in the health and education sectors in Congo’ or ‘Foreign aid organizations should provide fewer services like health care and education; that is the responsibility of the provincial government.’
- (c) ‘Foreign aid is necessary for Congo to become peaceful and prosperous’ or ‘Foreign aid is not necessary in Congo; only the government can achieve peace and prosperity.’

2. *Information about taxpayers.* This standardized index is increasing in the perception that the government possesses information about citizens. The underlying survey questions are as follows, to which respondents answered using a four-point scale: ‘Yes - I am completely sure, Yes - I am somewhat sure, No - I am somewhat sure, No - I am completely sure.’

- Prompt: ‘Now I’d like to ask you how much information you think the provincial government keeps about citizens in its archives.’
- Questions:
  - (a) ‘Do you think the provincial government knows the address of your house?’
  - (b) ‘Do you think the provincial government knows which of your neighbors did not pay the property tax in 2016?’
  - (c) ‘Do you think the provincial government knows what you do for a living?’
  - (d) ‘Do you think the provincial government knows how much money you make each month?’

3. *Ability to punish evaders.* This standardized index is increasing in the perceived capacity of the government to punish citizens who evade payment of the property tax. The underlying survey questions are as follows.

- (a) ‘Now, imagine that next week a tax collector comes and visits one of your neighbors. Imagine he absolutely refuses to pay. In this case, what is the probability that the government will pursue and enforce sanctions? Choose one of the following options: very likely, likely, unlikely, very unlikely.’
- (b) ‘Now imagine your neighbor pays a bribe instead of paying the tax. What is the probability that the government will pursue and enforce sanctions?’

4. *Perceived citizen compliance.* This variable captures the share of other citizens whom respondents perceive as having paid the property tax in 2016. The exact question read as follows: ‘In your opinion, how many other people in your quartier paid the property tax in 2016?’ [All/most/some/a few/none]

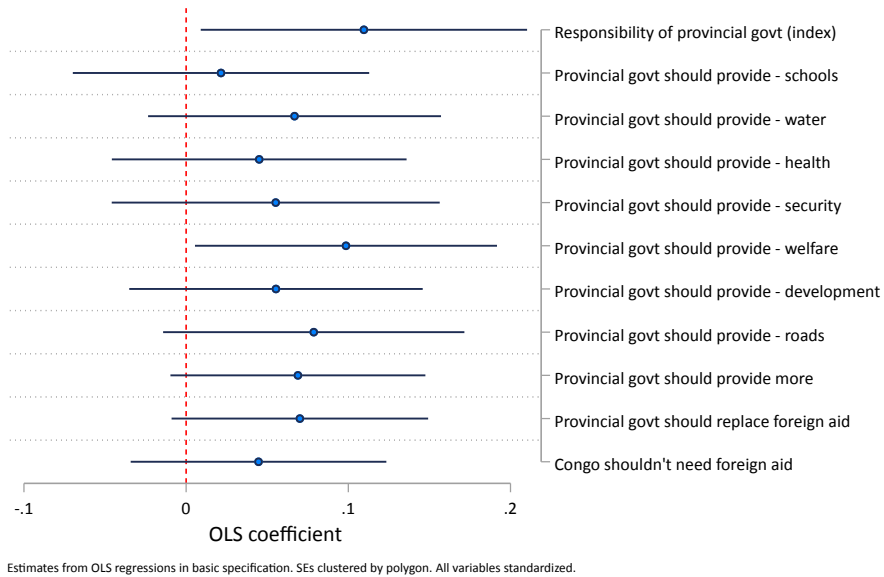
5. *Performance of tax ministry.* This standardized index is increasing in citizens' evaluation of and trust in the tax ministry. The underlying survey questions are as follows.
- (a) 'I am going to name a number of organizations. For each one, could you tell me how much confidence you have in them: is it a great deal of confidence, quite a lot of confidence, not very much confidence or none at all? The tax ministry.'
  - (b) 'Overall, how would you rate the performance of the tax ministry in Kananga?' [Excellent, Very good, Good, Fair, Poor, Very poor, Terrible]
6. *Ability to provide public goods.* This standardized index is increasing in the perceived capacity of the provincial government to provide public goods. The underlying survey questions are as follows.
- Prompt: 'To be successful with its projects, governments need both (1) the desire to do them, and (2) the capacity to execute them. Let's discuss the capacity of the provincial government. Please tell me if you strongly agree, agree, disagree, strongly disagree or are neutral about the following statements.'
  - Questions:
    - (a) 'If the provincial government of Kasai Central wants to improve all of the roads in Kananga, it will do this efficiently and without problems.'
    - (b) 'If the provincial government of Kasai Central wants to provide electricity to everyone in Kananga, it will do this efficiently and without problems.'
    - (c) 'If the provincial government of Kasai Central wants to find and imprison a criminal hiding somewhere in Kananga, it will do this efficiently and without problems.'
7. *Performance of government.* This standardized index is increasing in citizens' evaluation of and trust in the provincial government. The underlying survey questions are as follows.
- (a) 'I am going to name a number of organizations. For each one, could you tell me how much confidence you have in them: is it a great deal of confidence, quite a lot of confidence, not very much confidence or none at all? The provincial government.'
  - (b) 'Overall, how would you rate the performance of the provincial government in Kananga?'
8. *Integrity of tax collectors.* This standardized index is increasing in citizens' confidence that tax collectors will deposit money to the state account. The

underlying survey questions are as follows.

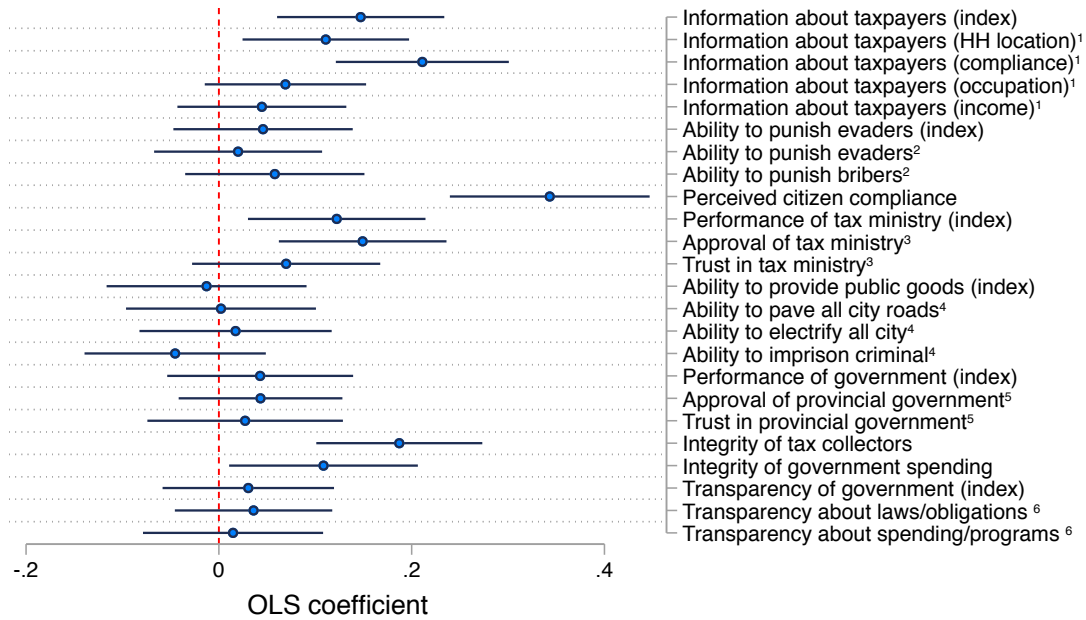
- (a) ‘In general, think of what the tax collectors will do with the money they collect during this 2016 property tax campaign. Imagine the tax collectors collect \$1000 thanks to the campaign. How much of this money will they submit to the state account?’
  - (b) Note: the respondent then answers the question ‘How much of this money will they put in their pockets?’ If the results do not sum to \$1000, the respondent answers again. Only the integer response to the first question is used for this variable.
9. *Integrity of government spending.* This standardized index is increasing in citizens’ confidence that the money received by the provincial government will be spent prudently and not be wasted or stolen. The underlying survey questions are as follows.
- (a) ‘Now I would like to ask you what you think the provincial government will do with the money it receives from this 2016 property tax campaign. Imagine that the Provincial Government of Kasai Central receives \$1000 thanks to this campaign. How much of this money will be put to good use, for example providing public goods?’
  - (b) Note: the respondent then answers the question ‘How much of this money will go to diversion of funds and waste?’ If the results do not sum to \$1000, the respondent answers again. Only the response to the first question is used for this variable.
10. *Transparency of government.* This standardized index is increasing in citizens’ perception of the transparency of the government. The underlying survey questions are as follows.
- Prompt: ‘Please tell me if you strongly agree, agree, neither agree nor disagree, disagree, or strongly disagree with the following statements.’
    - (a) ‘The Provincial Government of Kasai Central frequently and effectively informs the public about the laws and obligations of citizens.’
    - (b) ‘The Provincial Government of Kasai Central is sufficiently transparent and open about its spending and operations.’
11. *Number of collectors.* This count variable is the number of different collectors that visited a household, reported in the endline survey in response to the question: “How many different tax collectors were there who came to your house?”
12. *Number of collector visits.* This count variable is the number of reported visits in the endline survey in response to the question: “How many times did they come to your house in total in 2016?”

13. *Collector time spent (log)*. This variable is the (logged) estimated number of minutes that tax collectors spent at a house over the course of all of their visits. It was recorded in the endline survey in response to the question: “About how much time did they spend at your house in total?”
14. *Observed tablet/printer*. This indicator variable equals 1 if the household reports in the endline survey that they saw collectors using table and/or printer. The exact question text was: “Did you see a tablet or handheld printer being used by the tax collectors?”

## A5.2 Coefficient plots for individual questions used in indices



**Figure A22:** Coefficient plot of treatment effects for individual survey questions in *Resp. for public goods provision* index about the responsibility of the provincial government in the provision of public goods.



**Figure A23:** Coefficient plot of treatment effects for individual survey questions concerning the capacity and integrity of the provincial government.

### A5.3 Additional variables considered in Online Appendix

1. *Role of citizens in politics.* This standardized index is increasing in the view that citizens and their representatives should play an active role in monitoring the government. It is composed of the following questions, with responses on a 4 point scale increasing in agreement with the first viewpoint.
  - (a) ‘Some people say that citizens should have an active role in monitoring the actions of leaders and how the government spends its money’ or ‘Other people say that citizens should have more respect for authority and trust the government to spend its money in the best possible way.’
  - (b) ‘It is more important for citizens to have a voice and some influence in politics, even if that means it makes decisions more slowly’ or ‘It is more important to have a government that make decisions quickly, even if we the citizens have no influence over what it does.’
  - (c) ‘Citizens and their representatives in the Assembly should ensure that the Governor explains to it on a regular basis how his government spends taxpayers’ money’ or ‘The Governor should be able to devote his full attention to developing the country rather than wasting time justifying his actions.’

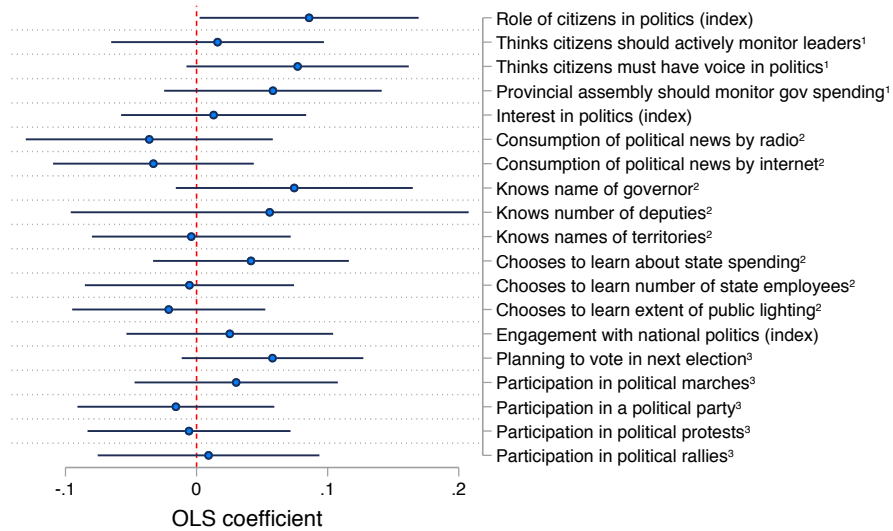
2. *Interest in politics.* This standardized index is increasing in respondents' interest in politics. It is composed of three sets of questions. The first two variables concern self-reported consumption of political news. The next three gauge interest in politics by respondents' knowledge of basic facts about the government and politics. The next three variables present the respondent with a choice between receiving information about the state or about some non-state actor. They are coded as 1 if the respondent chose the state information and 0 otherwise.
- (a) Set 1: consumption of political news.
    - i. 'How often do you listen to the radio about politics?' [5=Multiple times per day, 4=1-7 times per week, 3=1-4 times per month, 2=Less than once per month, 1=Only once ever, 0= Never]
    - ii. 'How often do you read articles on the internet about politics?' [analogous response key]
  - (b) Set 2: knowledge of political facts.
    - i. 'Do you know the name of the current governor of Kasai Central? If yes, what is it?' [2=Knows full name, 1=Knows part of name, 0=Doesn't know name]
    - ii. 'Do you know how many deputies there are in Kasai Central?' [10=correct answer, 9=wrong by 1,..., 0=wrong by 10 or more]
    - iii. 'Can you name the territories that make up Kasai Central? If yes, please name them.' [5=knows all five, ..., 0=knows none]
  - (c) Set 3: choice of political information. Prompt: 'Now I'd like to give you some information. I will give you three sets of choices. For each, tell me which piece of information you would like me to share with you.'
    - i. 'The total spending of the state in Kasai Central in 2016' or 'The total spending of UNICEF in Kasai Central in 2016.'
    - ii. 'The percentage of the population that works for the state' or 'The percentage of the population that is Catholic, Protestant, and Pentecostal.'
    - iii. 'The percentage of Kananga's public lighting that currently functions' or 'The percentage of Kananga's residents who own a diesel generator.'
3. *Engagement with national politics.* This standardized index is increasing in respondents' self-reported past and future participation in national politics. It is composed of the following yes-or-no questions, which I combined into an index (as described on p. 21 in the paper).
- (a) 'Do you plan to vote in the next election?'
  - (b) 'Have you ever been a member of a political party?' and (if not current member) 'Would you consider joining a political party in 2017?'



- i. Responses: 0=never member and not considering, 1=past member but not considering, 2=never member and considering, 3=past member and considering, 4=current member.
  - (c) ‘Have you ever been in a political march?’ and ‘Would you consider being in a political march in 2017?’
    - i. Responses: 0=never participant and not considering, 1=past participant but not considering, 2=never participant and considering, 3=past participant and considering.
  - (d) ‘Have you ever participated in a political protest of any kind?’ and ‘Would you consider participating in a political protest in 2017?’ [analogous response key as for marches]
  - (e) ‘Have you ever attended political rallies?’ and ‘Would you consider participating in a political rally in 2017?’ [analogous response key as for marches]
  
- 4. *Engagement with city chiefs.* This standardized index is increasing in respondents’ self-reported engagement with avenue chiefs. The index is composed of the following questions.
  - (a) ‘Has someone from your household ever gone to your avenue chief to discuss a problem affecting your neighborhood or to complain about the provincial government?’ [1=Yes, 0=No]
  - (b) ‘In 2016, did someone from your house contribute to a public good project in your neighborhood, for example improving a road or building a bridge?’
    - i. This question asks about *Salongo*, an informal labor tax organized by the avenue chief. Participation in *Salongo* is the most frequent form of engagement with avenue chiefs. [1=Yes, 0=No]
  - (c) ‘Do you know the name of your avenue chief? If so, please say the name.’
    - i. This question gauges revealed engagement with the avenue chief. [1=Yes, 0=No]
  
- 5. *Views of city chiefs.* This standardized index is increasing in respondents’ evaluation of and trust in local city chiefs. It is composed of the following questions.
  - (a) ‘To what extent does your avenue chief respond to the needs of the people on your street?’ [Four point response scale increasing in perceived responsiveness]
  - (b) ‘I am going to name a number of organizations. For each one, could you tell me how much confidence you have in them: is it a great deal of confidence, quite a lot of confidence, not very much confidence or none at all? Quartier and avenue chiefs.’ [Four point response scale increasing in confidence/trust]

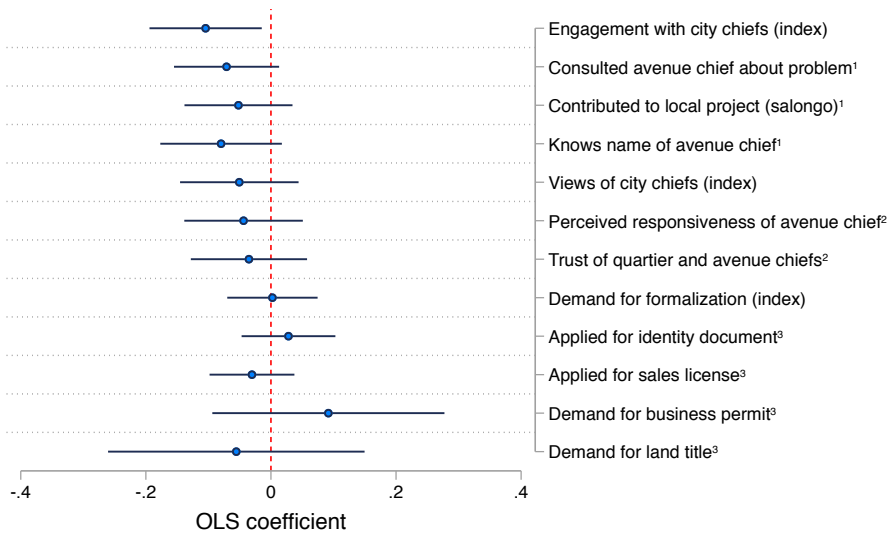
6. *Demand for formalization.* This standardized index is increasing in respondents' reported recent efforts to obtain formal documents. It is composed of the following questions.
  - (a) 'In 2016, did you try to get an identity document like a birth certificate, driver's license, passport or voter's card from the government?' [1=Yes, 0=No]
  - (b) 'In 2016, has someone from your household applied for a license to sell goods in town?' [1=Yes, 0=No]
  - (c) 'Are you considering obtaining a land title in 2017?' [1=Yes, 0=No]
  - (d) 'Are you considering obtaining a business permit in 2017?' [1=Yes, 0=No]
  
7. *Current public goods provision.* This standardized index is increasing in the perceived level of current public goods provision by the provincial government. It is constructed similarly to *Resp. for public goods provision* (sector questions). The enumerator listed a series of sectors, and for each respondents chose whether they thought the provincial government currently provided 'a lot,' 'a little,' or 'nothing' in that sector.
  - (a) Prompt: 'Now let's talk about what services the provincial government currently provides to the citizens of Kananga. In your opinion, what level of public services does the Provincial Government of Kasai Central *currently* provide?'
  - (b) Sectors: schools, water system/wells, health care, keeping people safe, helping poor people, economic development, road maintenance.
  
8. *Trusts researchers.* This variable is increasing in self-reported trust of foreign research organizations. It is constructed like other trust questions already noted. The enumerator prompt was as follows: 'I am going to name a number of organizations. For each one, could you tell me how much confidence you have in them: is it a great deal of confidence, quite a lot of confidence, not very much confidence or none at all? Foreign research organizations.'
  
9. *Knows researchers.* This variable indicates if the respondent was able to identify the employer of the enumerator at the end of the endline survey. The enumerator prompt was as follows: 'Do you remember who I work for? If yes, please say who you think I work for.'
  
10. *Past participant.* This variable indicates participants who report having participated in past activities with our research team (identifiable by referencing props used in lab games from past studies). The enumerator prompt was as follows: 'Have you ever played any games with envelopes and tents with a [research organization] enumerator in the past?'

11. *Expected spending from taxes*. This variable measures the share of endline survey respondents who said they thought property tax revenues were likely to be spent across different sectors. The question was repeated regarding the second most likely sector.
  - (a) Prompt: “Now, think of all the money from the 2016 property tax campaign that the provincial government spends. In what sectors will it spend this money? Choose the sector in which you think it will spend the most money.”
  - (b) Sectors: Schools /education, water system/wells, health care, keeping people safe, helping poor people, economic development, road maintenance, administration, none - they will steal or waste the money, other.
  
12. *Past visits (high)*. This dummy variable indicates neighborhoods with above-median reported past visits from government tax collectors (measured at baseline). I aggregated individual responses for the neighborhood, and then computed this indicator based on all neighborhood-level rates. The baseline survey question was as follows.
  - (a) ‘Has a government tax collector ever come to your neighborhood?’
  
13. *Past protest (high)*. This dummy variable indicates neighborhoods with above-median reported past protest participation (measured at baseline). It is constructed analogously to the previous variable. The baseline survey question was as follows.
  - (a) ‘Have you ever participated in a political protest of any kind?’



Estimates from OLS regressions in basic specification. SEs clustered by polygon. All variables standardized. Superscripts denote constituent questions of the following indices: 1=Role of citizens in politics, 2=Interest in politics, 3=Engagement with national politics.

**Figure A24:** Coefficient plot for individual survey questions in the *Role of citizens in politics*, *Interest in politics*, and *Engagement with national politics* indices.



Estimates from OLS regressions in basic specification. SEs clustered by polygon. All variables standardized. Superscripts denote constituent questions of the following indices: 1=Engagement with city chiefs, 2=Views of city chiefs, 3=Demand for formalization.

**Figure A25:** Coefficient plot for individual survey questions concerning the *Engagement with city chiefs*, *Views of chiefs*, and *Demand for formalization* indices.

## **A6 Data anomalies, noncompliance, and deviations from pre-analysis plan**

### **A6.1 Missing values in house quality data**

Due to a survey programming glitch, there is missing data on house quality for 78 observations (2.7%) in the endline data. Fortunately, I observe several components of the ultimate measure of wealth that is used in the analysis (floor quality, possessions, electricity access), as well as several other strong predictors (weekly and monthly income, amount of money spent on transport in past 7 days, amount of money spent on airtime in past 7 days). It is therefore possible to impute wealth measures following the following steps.

1. Within a neighborhood of three adjacent neighborhoods, estimate the relationship (using OLS) between the missing dependent variable and each of the other related variables.
2. Predict wealth for the 78 missing observations using the coefficients from the previous regression.

In the main analysis, the full sample is used, including these imputed values for the wealth variable. However, the results are robust to dropping these observations in estimations that use the wealth variable.

### **A6.2 Non-compliance among collectors**

One tax collector conducted one day of the property register in a control neighborhood (neighborhood 421) instead of a different treatment neighborhood in the same area. He also collected taxes from two households. This neighborhood was located in Nganza commune, which was ultimately excluded from the analysis because the conflict in Kananga made it too dangerous to conduct the endline survey there. As such, this non-compliance does not affect the analysis considered in the paper.

Although tax collectors were randomly assigned to neighborhoods, at times no tax collectors were able to work during the assigned week. Collector absences were typically due to illness, trips outside of Kananga, or other work (such as petty commerce, which many did on the side). In some extreme cases, the tax ministry non-randomly re-assigned other available collectors to these neighborhoods. This non-random reassignment occurred for 5.5% of treated neighborhoods. The complete list is as follows.

1. Collector 1 re-assigned to neighborhood 111.
2. Collector 18 re-assigned to neighborhoods 579, 212, 558.
3. Collector 31 re-assigned to neighborhoods 368, 639.
4. Collector 7 re-assigned to neighborhood 419.
5. Collector 15 re-assigned to neighborhood 703.

6. Collector 37 re-assigned to neighborhood 676.
7. Collector 17 re-assigned to neighborhoods 539, 675.
8. Collector 19 re-assigned to neighborhood 668.
9. Collector 22 re-assigned to neighborhood 671.
10. Collector 4 re-assigned to neighborhood 242.

### **A6.3 Deviations from pre-analysis plan**

The number of total neighborhoods (356) in the final analysis is less than that anticipated in the pre-analysis plan (431). As described on p. 8, all 71 neighborhoods of the commune of Nganza were dropped before endline enumeration due to insecurity. In addition, four neighborhoods in downtown Kananga were dropped because they contained only non-residential properties (shops, government buildings, churches, etc) and thus survey enumeration could not occur. As noted in Table II, the number of neighborhoods dropped for these reasons is balanced across treatment and control.

In the paper, I focus on the principal reduced-form equation mentioned in the PAP (Equation 1). In the PAP, I also included two more complex specifications to capture the effect of bribes on participation. These specifications were motivated by the concern that the tax campaign would increase bribes, which would create another channel through which treatment could influence outcomes (i.e. essentially an exclusion restriction concern). However, because I observe very low levels of bribery and no treatment effect on bribes, I rely instead on the simpler specifications noted in the analysis plan: Equations 1 and 2 rather than also Equations 3 and 4. The IV analysis is similarly simplified, including only two endogenous regressors rather than also including bribes as an endogenous regressor as in Equation 5 in the PAP. Otherwise, the analysis in the paper is analogous to that described in the PAP. In particular, the PAP makes clear that the primary objective of the anticorruption interventions (audit, information) is to study bribery in a separate paper. The document is also clear that the principal focus is how the tax campaign affects participation and beliefs about the provincial government.

Additionally, I had initially planned to examine two other outcomes that I was not ultimately able to collect. First, enumerators would offer respondents a chance to sign their name to a petition being run by a local civil society organization expressing disapproval of the provincial government, which would eventually be transferred to the governor. However, given concerns about the non-anonymity of respondents, the IRB prevented any researcher involvement with this petition. Second, I had planned to send questions to respondents via SMS and measure who responded. However, during piloting, I found that bulk SMS messages reached only about 40% of the intended recipients due to the low rates of cell phone ownership and use in the context. As a result, I never implemented this measurement strategy.

Although not strictly speaking a deviation from the analysis plan, it is worth noting that the hypotheses concerning the set of households from the baseline survey whom the enumerators were able to track at endline are examined in a separate paper (Weigel, 2018). This paper explores the determinants of tax compliance in detail, exploiting baseline characteristics to do so. Relatedly, questions about the duty of citizens to pay taxes and the analysis noted in Section 4.4 of the PAP are discussed in Weigel (2018).

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