Job Market Paper

BUILDING STATE AND CITIZEN: How Tax Collection in Congo Engenders Citizen Engagement with the State JONATHAN L. WEIGEL<sup>†</sup> November 26, 2017

### Abstract

Does tax collection catalyze citizen engagement with the formal state? I conduct the first field experiment to randomize tax collection: a door-to-door tax campaign in Kananga, D.R. Congo. I use the experiment to test the classic hypothesis that citizens will respond to increased tax collection by participating more in politics. As predicted, the campaign increases costly participation by 5 percentage points (28%): citizens in taxed neighborhoods are more likely to attend townhall meetings hosted by the government or to submit suggestion cards evaluating its performance. I present a model in which citizens participate more because tax collection sends a signal of state capacity, raising the expected benefits to participation. Analysis of respondents' stated beliefs about government capacity supports this mechanism. The paper thus provides field-experimental support for theories positing taxation as the origin of inclusive political institutions as well as evidence of a new mechanism linking tax collection and political engagement.

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<sup>\*</sup>My sincere thanks to James Robinson, Nathan Nunn, Rohini Pande, Horacio Larreguy, Otis Reid, Asim Khwaja, Rema Hanna, Anders Jensen, Vincent Pons, Robert Bates, Jorge Dominguez, Gwyneth McClendon, Evan Lieberman, Pia Raffler, Wilson Prichard, Noah Nathan, Leander Heldring, and Gemma DiPoppa as well as participants at the April 2017 NEWEPS Conference and the June 2017 WGAPE Conference for invaluable comments and suggestions. For outstanding research assistance and program management, I thank Anissa Chabib, Morgan Yucel, and Gabrielle Corrigan as well as Elie Kabue Ngindu and a superb team of enumerators. I am grateful for the collaboration with the Provincial Government of Kasaï Central, the DGRKOC, and Hologram Identification Systems. Many others supported this project, especially Jim Mukenge, James Diderich, Woody Collins, Roger Makombo, and Antonio Martinez. I gratefully acknowledge funding from the Weiss Family Program Fund, the Weatherhead Center for International Affairs, and the Fulbright Scholar Program. AEA pre-registration ID: AEARCTR-0001316. The online appendix can be found at jonathanweigel.com.

## 1 Introduction

Inclusive political institutions are deemed essential for sustained economic development (Acemoglu et al., 2017). But where do such institutions come from? One prominent explanation centers on taxation (Schumpeter, 1918; Besley and Persson, 2009, 2013). When rulers in the early modern period began systematically taxing their subjects to pay for wars, people protested, demanding public goods and representation in return for tax compliance (Tilly, 1985; Bates and Lien, 1985; Levi, 1989). This process triggered the co-evolution of tax compliance, citizen participation in politics, and accountable governance. The slogan "no taxation without representation" captures the intuition that tax collection is essential in the development of inclusive institutions. This paper examines a core assumption in this narrative: that tax collection increases citizen participation in politics. I call this the taxparticipation hypothesis.<sup>1</sup>

Despite the prevalence of this assumption, it is not obvious that citizens will choose to engage *more* with a state that is seeking to tax them. Political action is individually costly but publicly beneficial; many citizens might prefer to free ride on the efforts of others (Olson, 2009). Some might withdraw from the state altogether (Scott, 2009). Cross-country regressions find a positive relationship between tax revenues and political participation (Prichard, 2015). But selection bias makes observational analysis difficult to interpret.<sup>2</sup> A third factor, such as state capacity, might cause both fiscal reliance on taxation and political participation. Moreover, when governments expand their tax base, they target certain types of individuals — those with easily taxable assets, for instance — who may participate more for other reasons. Given these threats to inference, testing the tax-participation hypothesis requires experimental evidence. However, to date the only such evidence comes from lab and survey experiments — and this evidence is mixed. Some studies find that simulating taxation in the lab increases preferences for government monitoring (Paler, 2013; Martin, 2014), but closely related survey experiments find null results (de la Cuesta et al., 2015).

Building on this foundation, I provide the first test of the theorized link between taxation and participation in the context of a randomized policy experiment.<sup>3</sup> From April to December, 2016, the Provincial Government of Kasaï Central launched the first-ever doorto-door property tax collection campaign in Kananga, D.R. Congo. In collaboration with the government, I randomly assigned neighborhoods to treatment and control. Treatment neighborhoods received the tax campaign. Tax collectors went door to door conducting a census of property owners before making in-person appeals for the roughly \$2 property tax,

<sup>&</sup>lt;sup>1</sup>The political resource curse literature also often makes this assumption. When states obtain revenues from natural resources or transfers — instead of taxes — they are thought to be less bound by a social compact to respond to citizens' demands (Ross, 2004; Jensen and Wantchekon, 2004; Brollo et al., 2013; Gadenne, 2017). Only when they rely on taxation, then, do states govern well *because citizens hold them to account*. Meanwhile, in resource-rich states, citizens are politically quiescent precisely because they are not taxed.

 $<sup>^{2}</sup>$ Reverse causality is also a concern: states might expand participation to raise taxes. For example, Lizzeri and Persico (2004) argue that elites extended the suffrage in England to justify increases in taxation to fund public goods countering public health crises in the 19th century.

 $<sup>^{3}</sup>$ To my knowledge, this is the first field experiment to randomize tax collection. Closest in this regard are Dunning et al. (2015), who randomize tax holidays in Uruguay, and Khan et al. (2015), who randomize tax collector incentives.

which they collected on the spot. Control neighborhoods remained in the old declarative system in which citizens were supposed to pay at the bank themselves, but in practice, less than 1% did.<sup>4</sup> Of 431 neighborhoods in Kananga — covering over 100,000 households — I randomly assigned 253 to treatment. The campaign represents a sharp increase in tax enforcement and in the presence of the formal state in Kananga.

Before considering the effects of the campaign on political participation, I examine whether the campaign achieved its goal of raising tax compliance, and thus whether it constitutes a valid test of the "tax-participation hypothesis." One might expect a tax campaign to fail in the DRC. Tax capacity remains weak in many developing countries (Gordon and Li, 2009; Besley and Persson, 2014; Kleven et al., 2016), where high monitoring costs exacerbate the principal-agent problem between governments and tax collectors (Khan et al., 2015). The DRC is a particularly difficult context in which to raise tax compliance because extractive colonial practices, economic decline — the country has lower per capita GDP today than in 1960 — and civil war have weakened state capacity and propagated corruption (Sánchez de la Sierra, 2014; Lowes and Montero, 2016; Henn et al., 2017). Indeed, citizen payment of formal taxes was near zero at baseline: less than 6% of the population knew of the property tax, for example.<sup>5</sup> Will citizens comply when collectors ask them to pay taxes? Will collectors submit taxpayer money to the state?

Despite the difficult circumstances, the campaign raised citizen payment of the property tax by 11 percentage points, up from 0.6% in control neighborhoods — an 18-fold increase.<sup>6</sup> Given that the campaign was a success from the government's point of view, I use its random assignment to test the tax-participation hypothesis: namely, that the tax campaign will increase citizen engagement with the provincial government.

Survey-based measures of political engagement are often unreliable,<sup>7</sup> and elections in Congo have been postponed indefinitely. I therefore worked with the provincial government to create two real-world channels of citizen participation that would provide objective, experimental measures of engagement with the formal state. First, the provincial government agreed to host a series of townhall meetings, in which officials dialogued with citizens about taxation and public spending in Kananga. Second, we set up a suggestion box in the city center in which citizens could submit anonymous evaluations of the provincial government that would be shared with the governor and other top officials. Attending a townhall or submitting a suggestion card exhibits willingness to exert costly effort to have a voice in the provincial government.

Tax collection increases participation on both dimensions: residents of treated neighborhoods were nearly 5 percentage points more likely to attend a townhall meeting or to submit a suggestion card. In percent terms, this is a substantial increase — up 28% from control neighborhoods. During townhall meetings, participants publicly demanded better public

 $<sup>^{4}</sup>$ There are fewer than 300 records of property tax payments made the previous year, and over 86% of these were made by firms, not individuals.

<sup>&</sup>lt;sup>5</sup>See Table 2 for summary statistics from the baseline survey.

<sup>&</sup>lt;sup>6</sup>The program also caused a 64.2 percentage-point increase in the probability of being visited by tax collectors.

<sup>&</sup>lt;sup>7</sup>Measurement error in self-reported political participation can be caused by social desirability bias, time inconsistency, and/or anonymity concerns in repressive settings (Mullainathan and Bertrand, 2001; Krosnick, 1999; Silver et al., 1986).

infrastructure and a more responsive government. "Ravines threaten our neighborhoods, and the government does nothing," asked one individual, "so why should we pay?" Submitted suggestion cards were highly critical — over 90% openly disapproved of the provincial government — with specific requests for greater transparency and accountability.

I rule out three alternative explanations that could also account for increased participation. First, I show that the results are not explained by experimenter demand effects or another artifact of the research design. Second, I demonstrate that the treatment effect is not caused by lower participation in the control group — a 'disappointment effect' — rather than higher participation the treatment group. Third, engagement with the provincial government could be a byproduct of a broader expansion of civic-mindedness and a propensity to participate at all levels of government — national, provincial, and local — rather than an increase in participation with the specific level of government engaged in tax collection, as the theory predicts. Examining self-reported participation at these levels and general interest in politics, I find no evidence to support this view of expanded civic-mindedness. In fact, the campaign erodes the perceived legitimacy of local government brokers (city chiefs). This evidence suggests that formal state building can undermine local power structures.

I next turn to the mechanisms linking tax collection and costly participation. I first document a surprising fact: the increase in participation is not driven by taxpayers alone. Rather, *everyone* in treatment neighborhoods — payers and those who evade payment — is participating at higher levels, compared to people in control neighborhoods. According to instrumental variables estimation as well as simple correlations, the tax collection campaign does not appear to have increased participation through a tax payment channel, as is often assumed (Prichard, 2015; Martin, 2014).

In light of this finding, I argue that a signaling mechanism is at work. In a low-capacity setting, the fact that the state was able to administer a citywide door-to-door tax collection campaign sends a potent signal about its ability to intervene meaningfully in society. This is similar to models emphasizing the signaling role of public projects (Coate and Morris, 1995). To make this argument concrete, I outline a simple decision-theoretical model in which this signal leads citizens to expect greater returns from participation. Realizing the state has higher capacity than they previously thought, citizens exert more effort to have a voice in determining future public policy — whom the government taxes and what services it provides.

To test this signaling model, I examine the effects of the tax campaign on individuals' stated beliefs about the government's capacity. Respondents in treated neighborhoods believe the provincial government has greater *informational capacity*: how much it knows about citizens' properties, earnings, and past tax compliance. They also believe the government has greater *spending capacity*: new public revenues generated by the campaign. These are logical inferences for citizens to have drawn after observing tax collectors going door to door in their local neighborhoods. Thinking the government has higher revenues after the campaign, citizens appear to have sought a voice in determining how the new funds are spent. In the words of one participant, "The provincial government should do more and inform us how this money will be spent on public infrastructure and not wasted on other things."

Finally, I examine whether the public signal sent by the tax campaign also enabled coordi-

nation among citizens, thereby helping them solve the collective-action problem associated with participation. I find suggestive evidence that treated individuals are more likely to coordinate when deciding to attend townhall meetings. But, on the whole, the evidence testing a coordination mechanism is mixed. If anything, coordination appears to play a small, complementary role to the main channel of treated individuals' updating about state capacity and choosing to participate as a result.

This paper makes several contributions. First, it provides the first evidence from a randomized tax collection program in support of the "tax-participation hypothesis." This hypothesis is central to theories positing taxation as the origin of inclusive institutions (Schumpeter, 1918; Besley and Persson, 2009, 2013). It also figures in the political resource curse literature (Ross, 2004; Brollo et al., 2013), which often assumes that untaxed citizens are politically quiescent. Second, the paper outlines and tests a signaling model linking taxation and participation. Although little discussed in the literature, this mechanism is compelling in weak-state, low-tax equilibria, such as those examined in historical accounts of state building (Schumpeter, 1918; Tilly, 1985; Levi, 1989; Brewer, 1990). Third, the paper makes a methodological contribution concerning the measurement of political participation in nondemocracies. Rather than relying on survey- or lab-based measures, it uses two forms of real-world engagement with the provincial government that I helped create in collaboration with government officials.

The paper also contributes to the empirical literature on tax and development. While past work examines how governments can build the tax base in the context of third-party reporting (Pomeranz, 2015; Carrillo et al., 2017; Kleven et al., 2016; Naritomi, 2013; Kumler et al., 2013), tax collector incentives (Khan et al., 2015), lowering tax burden (Waseem, 2013), and reducing bureaucratic barriers to compliance (Kleven and Waseem, 2013; Best et al., 2015), this paper shows that, in a setting with extremely low state capacity and near-zero prior tax enforcement, large gains in individual tax compliance can be achieved by making in-person tax appeals. Finally, in documenting that formal taxation undermined the legitimacy of local government brokers, the paper also contributes to the literature on how formal institutions can crowd out local, informal ones (Lund, 2006; Cheema et al., 2006; Binzel et al., 2013).<sup>8</sup>

The paper reviews the setting (Section 2), experimental design (Section 3), and data collection (Section 4), before turning to hypotheses and outcome measurement (Section 5), estimation strategies (Section 6), and reduced-form results (Section 7). Then, it examines mechanisms (Section 8), considering first payment-based channels, and then introducing and testing a signaling model of tax collection and participation.

## 2 Setting

Kananga, a city of roughly 1 million (the fourth largest in Congo), is the seat of the Provincial Government of Kasaï Central. Although before independence the Belgians planned to turn Kananga (then Luluabourg) into the colonial capital, the province has been deliberately

<sup>&</sup>lt;sup>8</sup>This also contrasts with evidence of local elite capture of government and aid programs (Humphreys et al., 2012; Acemoglu et al., 2014; Anderson et al., 2015).

isolated since independence because it has long been home to the opposition. Additionally, 73% of Kananga's inhabitants are ethnically Luluwa, and there has never been a Luluwa president or prime minister in the DRC. Unlike other Kasaïen cities, such as Mbuji Mayi and Tshikapa, Kananga has not emerged as a center for the diamond trade, nor does it host other large productive enterprises. The city was founded as, and remains, *une ville administrative*, in which the provincial government is the largest employer. Kananga largely escaped the violence that seized the country during the First and Second Congo Wars. But it did not escape the economic decline and institutional decay that afflicted Congo in the past several decades (Young and Turner, 2013). By most accounts, the DRC is poorer today than it was in 1960 (Rodrik, 2016), and Kasaï Central is among the poorest provinces.

Kananga is a good setting in which to test the tax-participation hypothesis for several reasons. First, in light of the obstacles to political development noted in the previous paragraph, Kasaï Central's provincial government has low state capacity. By capacity, I refer mainly to the infrastructural power of the state: the ability to collect taxes, provide public goods, and enforce laws within a given territory (Mann, 2012). The low capacity of Kasaï Central's provincial government makes it comparable to the settings in early modern Europe examined in historical accounts of the origins of participation in politics. These theories discuss incipient states, struggling to fund wars and create a tax bureaucracy (Schumpeter, 1918; Tilly, 1985; Levi, 1989; Brewer, 1990). The Provincial Government of Kasaï Central is at precisely this moment in its political development.

Second, Kananga has low formal tax collection among citizens. With nearly 6 million people in the province, total provincial tax receipts from 2010-2015 were around \$2 million per year. These receipts main came from trade and rental taxes levied on a handful of firms in downtown Kananga, such as mining and mobile-phone companies. Indeed, though there are many taxes on the books, few are enforced among private citizens in Kananga. Only 40% of individuals knew the name of the provincial tax ministry at baseline, and 5.6% of individuals in the sample had heard of the property tax. Before the property tax campaign, the most common taxes that residents of Kananga paid were market vendor fees and a vehicle tax for owners of cars and motorcycles: 3.2% of individuals sampled said they paid one of these taxes.<sup>9</sup> The main national tax to which citizens are in theory liable is the income tax. However, the national tax ministry branch in Kananga only enforces this tax among employees of the government and of large firms. Moreover, many government employees are unaware that their salaries are taxed by the national government before they are accessible at the bank: only 1% of the sample reports having ever paid the income tax. In short, private individuals rarely paid formal taxes before the 2016 property tax campaign. The lack of a broad citizen tax base is a challenge to governments across sub-Saharan Africa.<sup>10</sup>

<sup>&</sup>lt;sup>9</sup>According to the law, landlords are also liable for a rental tax: 20% of monthly rent, deducted before this payment is made from tenant to landlord such that the burden of payment falls on the landlord. However, because most individual renters have informal contracts and may pay rent in kind, this tax is not often enforced among private citizens. Commercial renters, on the other hand, are pursued assiduously by the tax ministry; but firms are not part of the sample for this project. The tax ministry also enforces taxes on transportation, with tolls at the four major roads, the airport, and the train station. Most citizens seldom have occasion to pass by one of these taxed areas.

<sup>&</sup>lt;sup>10</sup>Tax ministries elsewhere on the continent similarly rely on natural resource rents, trade taxes, and other gatekeeper-style indirect taxes. For an overview of the fiscal landscape of sub-Saharan Africa, see Prichard

This is not to say that individuals are free from petty harassment by the police and other officials. For example, vehicles are often stopped by police seeking bribes; street vendors can dodge the daily market fee by bribing the relevant officer. However, such informal payments are less common in Kasaï than in Kinshasa and other larger Congolese cities (Paler et al., 2016). Only 258 individuals (8.8% of the sample) said they made *any* other payments — formal or informal — to government agents in 2016 (not including the property tax).<sup>11</sup>

The third reason why Kananga is a good setting in which to test the tax-participation hypothesis is that a plausibly exogenous fiscal crisis drove the Provincial Government of Kasaï Central to launch a property tax campaign in 2016. Lizzeri and Persico (2004) argue that the causal chain sometimes runs not from taxation to participation, but from participation to taxation.<sup>12</sup> Assuaging concerns of reverse causality in this setting is the fact that a largely unanticipated national policy triggered the provincial fiscal crisis that led the government to expand enforcement of the property tax. In particular, the splitting — *découpage* — of Kasaï Occidental into two smaller provinces in 2015 shrank the revenue base substantially.<sup>13</sup> In particular, the provincial government in Kananga lost the diamond-rich region around the city of Tshikapa to the new Kasaï province, a large source of revenue. One official estimated that the new province's budget was 40% smaller than that of the old province.<sup>14</sup> This plausibly exogenous fiscal crises that triggered tax collection and state building in Europe, Southeast Asia, and elsewhere (Tilly, 1985; Slater, 2010).

Finally, the nondemocratic nature of the Congolese state is comparable to the autocracies studied in the historical literature on state building (Schumpeter, 1918; Tilly, 1985; Levi, 1989; Brewer, 1990). According to classic accounts, inclusive institutions emerge only as the result of a process of tax bargaining triggered by the state's efforts to extract more revenue from the population. Absent this process, states have little reason to concede representation or channels of direct democratic participation. Political life in Kananga approximates this nondemocratic equilibrium. Although the regime might have once been termed "competitive authoritarian," its cancelation of 2016 elections (without a new date on the calendar) indicates an increasingly autocratic turn. Political parties are not strongly institutionalized in

<sup>(2015)</sup> and Pfister (2009).

<sup>&</sup>lt;sup>11</sup>A survey module asked participants to describe all formal and informal payments to any government agents. It named each of the taxes and fees that focus-group participants identified as most commonly paid in Kananga. See Online Appendix Section 2 for the full procedure and question text.

<sup>&</sup>lt;sup>12</sup>Specifically, they argue that elites in 19th-century England were trapped in a pork-barrel political equilibrium in which raising funds for large-scale public goods provision was near impossible. However, as cholera and other public-health crises hit London, the elites deliberately extended suffrage in order to raise taxes to pay for public services aimed at addressing these crises. According to their argument, then, participation was prior to expanding the tax base in the English case.

<sup>&</sup>lt;sup>13</sup>Découpage, a nationwide decentralization that increased the number of provinces from 11 to 26, was part of President Joseph Kabila's strategy of *glissement* — slippage — that entailed creating administrative holdups to postpone presidential elections. Although this decentralization was mentioned in the 2007 constitution, its sudden announcement in 2015 was a source of surprise to many, as evidenced by the chaos it engendered in provincial-level politics (Wille, 2015).

<sup>&</sup>lt;sup>14</sup>Another explanation for why the government had not enforced property taxes before the 2016 takes the underlying hypothesis of this paper seriously: governments choose not to enforce tax compliance among citizens because they do not want to be held accountable for providing public goods (e.g. Acemoglu (2005)).

the D.R.C., as they have emerged in more consolidated democracies in sub-Saharan Africa.<sup>15</sup> As such, local party brokers do not play a large role in politics.<sup>16</sup>

In this setting, how then do ordinary citizens engage with the government? A focus group on this question revealed two principal means of exerting voice in the nondemocratic government. First, individuals hold local meetings in which they discuss common grievances and public-good failures and then nominate a representative to bring the case before the provincial deputy or another member of the provincial government. Second, individuals or groups of individuals — author formal letters of complaint to the provincial government. The measures of costly participation used in this study were designed to approximate these existing practices of engagement with the state in Kananga.

From January to May 2017, the Kasaï region has been afflicted by a cycle of tit-for-tat violence between a local militia group and the national government.<sup>17</sup> Although these unfortunate events do not affect the internal validity of the study — because the randomization nets out citywide trends — it does impact the external validity. As with all randomized controlled trials, the reported results are valid only in the current setting; further research will be needed to determine generalizability in other contexts.

## 3 Experimental design

The treatment, randomly assigned at the neighborhood level, is the door-to-door property tax collection program, which ran from April to December in 2016. I defined the unit of randomization, the 'neighborhood,' by dividing a satellite map of the city into 431 polygons that approximate — but are not perfectly coterminous with — *localités*, the lowest administrative unit in the city. Polygon borders mimic natural boundaries like roads, ravines, or other features easily identifiable from the ground.<sup>18</sup> Among the 431 polygons, 253 were selected randomly to receive the tax program.<sup>19</sup>

Before the tax program, enumerators distributed informational fliers in *all* neighborhoods announcing that (i) the government was launching the campaign in the months ahead, and (ii) money collected would be used to 'promote the economic development of the province.' Individuals selected for the interview were read this information in French or Tshiluba (the most widely spoken African language in Kananga); other individuals were simply handed the fliers. The goal of the fliers was to hold constant the spread of information about the

 $<sup>^{15}</sup>$ Only 26% of individuals in the baseline survey report current or past affiliation with a political party.

<sup>&</sup>lt;sup>16</sup>For instance, less than 10% of the population reported having consulted with their avenue chief — the lowest link in the government bureaucracy and in theory intermediaries between the people and the state — to discuss a problem and make a demand.

 $<sup>^{17}\</sup>mathrm{See}$  Online Appendix Section 2.3 for details on the conflict.

<sup>&</sup>lt;sup>18</sup>Appendix Figure 12 shows all polygons in the city, and Online Appendix Figure 1 shows a close up of treatment and control polygons.

<sup>&</sup>lt;sup>19</sup>Two other anticorruption interventions were cross randomized on the neighborhood level: independent audits and an information intervention. For simplicity of explication, these interventions will be ignored in the following analysis, though more information can be found in Online Appendix Section 2.

taxation campaign.<sup>20</sup>

Randomly assigned in shifting groups of three, the 59 tax collectors working on the property tax campaign completed two tasks in each neighborhood.

- 1. **Census**: First, collectors completed a brief census to identify all individuals liable to pay the property tax in the neighborhood. Collectors received a printed copy of the census information before tax collection. Collectors also assigned a unique code to each house in the neighborhood, written in chalk on the wall or door. These codes were printed on receipts to identify households that paid in the administrative data. The census was verified by members of the research team who had received GPS training to make sure the collectors respected neighborhood boundaries.<sup>21</sup>
- 2. Tax collection: Upon completion of the census in a treated neighborhood, collectors began door-to-door tax collection. They had roughly two weeks to complete each neighborhood. When an individual paid the tax, collectors used a tablet application to generate a receipt, printed on the spot with portable printers.<sup>22</sup> Collectors left the receipt with the taxpayer, with an electronic copy saved in the tablet's memory. Collectors then deposited the money at the bank, where the tablet data was automatically downloaded, enabling program supervisors to check that the amount deposited equaled the amounts on all receipts issued.

The treatment is a bundle of these two components of the program. When examining mechanisms (Section 8), I will briefly consider the effects of tax payment separate from effects of tax collector visits for the census. However, the main analysis considers the reduced-form impact of the program as a whole, which is the theory-relevant quantity of interest in the signaling model introduced in Section 8.2.

Roughly 95% of the population faces a flat annual property tax rate of 2,000 Congolese Francs (CF), about  $2^{23}$  While this might seem like a small amount, 2 is the median household's daily income in the sample. Larger houses in the city center (less than 5% of the sample) face a flat rate of 6,600 CF. Finally, 'villas', typically Belgian-built compounds with a garage (less than 1% of the sample), must be measured, and their owners typically are supposed to pay more than 25,000 CF.

In keeping with standard policy at the tax ministry in Kananga, a small performance-based

<sup>&</sup>lt;sup>20</sup>Half of neighborhoods received additional information: the specific price of the tax and an example of the printed receipts used in the program. This treatment, which aimed to reduce corruption, was crossrandomized and will be analyzed in a separate paper. See Online Appendix Section 2 for more details.

<sup>&</sup>lt;sup>21</sup>Collusion between tax collectors and enumerators is possible but unlikely for several reasons. First, a different enumerator (or enumerators) conducted monitoring surveys with a time lag from verification of census codes; the timing and identity of this individual (or individuals) was unknown to the collector and enumerator during the census. Second, all enumerator surveys were backchecked by a separate team using audio files from interviews and by doing in-field re-interviews. Thus, it would have taken a very sophisticated collusion strategy to avoid detection. Given that the surplus from collusion would be small and uncertain, it is unlikely that the collectors and especially the enumerators would choose to take this risk.

<sup>&</sup>lt;sup>22</sup>See Online Appendix Figure 2 for receipt examples.

 $<sup>^{23}</sup>$ Some individuals are by law exempted from the property tax, including the disabled, the elderly, churches, and active state employees.

bonus was paid out to collectors working on the property tax campaign. Collectors received compensation chiefly based on the amount of taxes they collect.<sup>24</sup> The average weekly bonus was about \$4, though more productive collectors earned more than \$10.

## 4 Data

Data come from four sources: (1) administrative data on property tax payment, (2) a baseline survey conducted before the campaign, (3) a 'monitoring' survey during the campaign, and (4) an endline survey after the campaign. An independent team of enumerators administered all three surveys in both treatment and control neighborhoods following identical sampling and enumeration procedures.

Administrative data come from the government's official tax database. This database is managed by a private company, Hologram Identification Systems, which integrated raw data from collectors' tablets with existing data from the bank.

Baseline survey enumeration occurred in all neighborhoods from March to April in 2016, before the property tax campaign. Enumerators visited a random sample of 5-10 households in every neighborhood to collect pre-program covariates. Random sampling was achieved by assigning enumerators to skip patterns to follow while walking down each avenue in a neighborhood: e.g. visit every  $X^{th}$  house, where X is determined by the estimated number of compounds and a target of 5 households per neighborhood. The questionnaire chiefly concerned past exposure to tax collection and views of the government.

Simultaneously with the campaign, enumerators conducted 'monitoring' surveys to verify the work of tax collectors in all neighborhoods. In treated neighborhoods, enumerators began surveying at least two weeks after collectors had finished work. In control neighborhoods, enumerators similarly waited at least two weeks after an adjacent neighborhood had received tax collectors. Enumerators conducted a very short survey in all compounds, asking whether households were visited by tax collectors and whether they paid the property tax. Some individuals were randomly chosen to participate in a slightly longer survey asking (i) more details about interactions with tax collectors, and (ii) a handful of questions concerning views of the government.

Finally, an endline survey was administered from January to May in 2017, after the conclusion of the tax campaign.<sup>25</sup> Enumerators first conducted a short screening survey of roughly 20

<sup>&</sup>lt;sup>24</sup>Specifically, collectors earned 18% of how much money they deposited. This size bonus is analogous to the incentive pay offered to Pakistani property tax collectors in Khan et al. (2015). Additionally, 40% of property owners in each treated neighborhood were randomly sampled after the census visit for a double bonus: collectors received 36% of the money they collected from these households. The double bonus aimed to (i) encourage tax collectors to work everywhere in the neighborhood, even the harder-to-reach areas, and (ii) create another level of random variation in the likelihood that households receive multiple visits from tax collectors. Whether or not a given household was eligible for the double bonus was indicated on the census lists that collectors received before starting tax collection. The use of this instrument will be discussed in Section 8.1.

 $<sup>^{25}</sup>$ Although the government had planned to continue the collection campaign everywhere in the city beginning in May, the conflict between the Kamuina Nsapu militia and the national army led the provincial

households, randomly sampling participants by following a skip pattern as described for the baseline survey. A subsample of screening survey participants was then randomly selected for the full interview, with houses of higher quality based on observables selected with slightly higher probability to enable analysis of heterogeneity by wealth level.<sup>26</sup> Only household heads or their spouses were eligible to complete the interview, with preference for the household head. The endline survey contained a range of questions, with emphasis on experiences with the tax campaign and views of the government. Enumerators also distributed invitations to townhall meetings and suggestion cards during the endline interview, as discussed in Section 5.

In addition to the endline survey, I collected data on the subset of baseline participants (N=630) who were property-owning household heads (or their spouses) and still in Kananga at endline to enable analysis of changes of beliefs about the government within individuals over time. Because most analyses in this paper do not involve this repeated baseline sample, I discuss its collection in depth in the Online Appendix Section 2.4.

Table 1: Activities of collectors and enumerators							
Activity	Treated	Control	Timing	Ν	J		
Tax collectors							
Census	Yes	No	Apr-Dec 2016	N/A	253		
Tax collection	Yes	No	Apr-Dec 2016	N/A	253		
Enumerators							
Baseline survey	Yes	Yes	Mar-Apr 2016	2,384	431		
Monitoring surve	ey Yes	Yes	Apr-Dec 2016	$2,\!188$	431		
Endline survey <sup>*</sup>	Yes	Yes	Jan-May 2017	2,913	356		

Notes: \* Townhall meeting invitations and suggestion cards were distributed during the endline survey to all participants in treatment and control neighborhoods. N = sample size, J = number of clusters.

Table 1 summarizes the activities of the collectors and the enumerators. In short, all research components of the study — baseline, monitoring, and endline surveys — were held constant across treatment and control; what varied was assignment of the tax program with its two components: census and collection.<sup>27</sup>

#### 4.1 Summary statistics

Table 2 contains summary statistics from the baseline and endline surveys, revealing several noteworthy characteristics of this population.

government to cancel property tax collection in 2017.

<sup>&</sup>lt;sup>26</sup>The rationale for examining heterogeneity by house quality is discussed in Online Appendix Section 3 and in the pre-analysis plan. See Online Appendix Section 2.4 for further details about sampling and how weights are constructed to estimate quantities of interest that will be representative of all property owners in Kananga.

<sup>&</sup>lt;sup>27</sup>Appendix Figure 10 shows that the total number of surveys administered by neighborhood for the baseline, monitoring survey, and endline is balanced across treatment and control.

First, the Provincial Government of Kasaï Central had considerable support at baseline. Roughly 56% of the sample said they have "a great deal of trust" in the provincial government; in Botswana, a comparatively high-performing state, the corresponding statistic was 53% in 2014.<sup>28</sup> However, they have little past exposure to formal tax collection: only 40% had heard of the provincial tax ministry, and only 5.6% had heard of the property tax.

	Mean	SD	Min	Max	N
Baseline survey data					
Knows tax ministry	0.400	0.490	0	1	2384
Knows property tax	0.0562	0.230	0	1	2384
Reports past visit from tax collector	0.122	0.327	0	1	2384
Trusts provincial government	0.559	0.497	0	1	2383
Voted in 2011 national election	0.738	0.440	0	1	2384
Member of political party	0.263	0.440	0	1	2384
Endline survey data					
Age	48.74	17.09	18	102	2913
Female	0.410	0.492	0	1	2913
Born in Kananga	0.409	0.492	0	1	2913
Years of education	9.609	4.135	0	19	2909
Literate	0.799	0.401	0	1	2913
Unemployed	0.419	0.494	0	1	2913
Household monthly income (USD)	106.4	194.0	0	4800	2903
Lives in non-mudbrick house	0.467	0.499	0	1	2911
Has any source of electricity	0.186	0.389	0	1	2913
Owns motorbike	0.149	0.356	0	1	2913
Owns car or truck	0.0175	0.131	0	1	2913
Works for government	0.234	0.424	0	1	2913

 Table 2: Summary statistics

Notes: summary statistics from baseline survey conducted in March-April 2016 (before the tax campaign) and from endline survey conducted from January-May 2017 (after the tax campaign).

The endline sample has a higher proportion of older men because enumerators deliberately asked to speak with the household head (because this is the individual who typically interacts with tax collectors). Average household income is just over \$100 per month.

#### 4.2 Attrition

Because of the violence affecting the Kasaï region at the time, enumerators were unable to conduct the endline survey in one commune of the city (Nganza) representing 16.4% of the

<sup>&</sup>lt;sup>28</sup>See Afrobarometer, "Trust in institutions, government performance and elections: Findings from Afrobarometer Round 6 Surveys in Botswana."

intended sample. All 71 neighborhoods from this commune were dropped before subjects could be identified and enrolled.<sup>29</sup> Because I stratified the randomization on map grid cells, which approximate communes, the number of neighborhoods dropped is balanced across treatment and control (see Appendix Figure 10).

Within the remaining neighborhoods, 453 of the 3,421 (13.21%) households sampled for endline did not complete the endline survey. Common types of attritors include (1) respondents who are too busy to complete the survey, (2) respondents who leave for a trip, and (3) respondents who say they do not wish to participate without giving a reason. These forms of attrition are balanced across treatment and control (see Appendix Figure 10).

## 5 Hypotheses and measurement of outcomes

This paper tests two principal hypotheses. First, it examines the extent to which to program succeeded at reaching its stated objectives of increasing the presence of the formal state and boosting tax compliance.

**Hypothesis 1**: the tax campaign will increase household visits from tax collectors and payment of the property tax.

Testing Hypothesis 1 determines whether the 2016 tax campaign in Kananga provides a good test of the effects of tax collection on participation. If the campaign failed to meet the objectives of the provincial government, it would not constitute a valid setting in which to test the tax-participation hypothesis. To test Hypothesis 1, I consider two dependent variables.

- 1. Visited by tax collectors. To measure whether assignment to the tax program increases the probability of being visited by provincial tax collectors, I use self-reported survey data. Respondents indicated if their household received a visit from collectors at any time during 2016.
- 2. Paid property tax. To measure if the program increased average tax payment, I use administrative data matching state records to households with the unique codes assigned during the census. At times, household codes were erased (usually due to strong rains), which complicates matching administrative and household data. In such cases, I validate self-reported tax payment in two ways. First, a valid printed receipt with the correct name on it was accepted as proof of payment. Second, I used fuzzy name matching (within neighborhoods) with administrative records among individuals who claimed to have paid the tax but could not produce a receipt. In control neighborhoods, I also used fuzzy name matching restricted to city quartier to match administrative records with household surveys. Although there is error in this method, the process is greatly

<sup>&</sup>lt;sup>29</sup>Because individuals in Nganza were never selected, solicited, or enrolled to participate in the endline survey, it makes little sense to speak of "attrition" of the hypothetical individuals who would have been sampled in this commune. Nonetheless, the intended sample size (based on power calculations) was 4,131 versus the 2,913 achieved in the end (28.1% of the goal).

simplified by the fact that there are only 214 records of non-campaign property-tax payments in 2016, over 90% of which were made by firms.

After testing Hypothesis 1, I turn to the tax-participation hypothesis, which is the core theoretical issue animating this study:

**Hypothesis 2**: the tax campaign will increase citizens' efforts to demand better public services and greater accountability from the provincial government.

Measuring citizens' efforts to make demands of the government in a nondemocracy is an empirical challenge. All types of survey data may be subject to social desirability bias and other forms of measurement error (Mullainathan and Bertrand, 2001; Krosnick, 1999). Such concerns are particularly worrisome when asking individuals about sensitive topics, such as participation in politics in a country without much protection of civil and political rights (Silver et al., 1986). Data on voting or turnout at political protests are unavailable because elections were canceled, and the state forcefully represses protests. Given these constraints, the next best measure is another form of real-world engagement with the provincial government that, like turning up to vote or to protest, comes at a cost to individuals. The analysis therefore considers two channels of participation that I was able to measure by collaborating with the provincial government: (1) attendance at townhall meetings about taxation and public spending in Kananga hosted by the provincial government, and (2) submission of anonymous government suggestion cards at a drop box in the city center.

Concerning the first experimental measure, the provincial government held a series of townhall meetings, chaired by the provincial finance minister and the director general of the tax ministry. We designed the meetings to provide a venue in which citizens and the government could engage in a constructive dialogue about tax collection and public goods provision in Kananga. Enumerators informed endline participants that these meetings were an opportunity to obtain information about the tax system in Kananga and to interact with representatives of the provincial government.<sup>30</sup> Enumerators also distributed invitations (see Figure 1) containing unique codes to verify the identity of the participant and to match turnout data to survey data. Meetings were held in the provincial assembly building and were highly formal (see Figure 2). After initial presentations by the government officials, participants made comments and asked questions; the officials responded in turn. Roughly half of the meeting was dedicated to this type of dialogue between citizens and representatives of the provincial government. Meetings ran for 2-3 hours, so attendance represented a considerable time cost, not to mention the transport cost of reaching the provincial assembly building, which is located up to 13 kilometers, and on average about 5 kilometers, from certain neighborhoods in Kananga (see Appendix Figure 12).<sup>31</sup> In total, 483 individuals (24.9% of endline participants who received invitations) participated in one of the five townhall meetings held from January to April 2017.<sup>32</sup>

 $<sup>^{30}\</sup>mathrm{See}$  Appendix Section 10.2 for the exact script read to respondents and other details about the townhall meetings.

<sup>&</sup>lt;sup>31</sup>Motorcycle taxis ask up to 2 — the daily wage of the median household in the sample — for trips from the outskirts of Kananga to the city center.

<sup>&</sup>lt;sup>32</sup>Unfortunately, the instability in Kasaï reached a level in early April that the government decided to discontinue these meetings. Thus, endline participants after April 1, 2017, did not have the chance to



## Figure 1: Invitation to townhall meeting held at Kananga's Provincial Assembly building on February 20, 2017.

The second experimental outcome is the submission of anonymous suggestions cards in which citizens evaluate the performance, transparency, and inclusiveness of the provincial government.<sup>33</sup> With the government's approval, I created a locked drop box in downtown Kananga (Appendix Figure 14), in which individuals could drop suggestion cards they received during the endline survey (Appendix Figure 13). Following Paler (2013), the cards contained one question concerning the respondent's overall level of satisfaction with the government, followed by four statements concerning (i) opportunities for participation, (ii) access to information, (iii) spending on public goods, and (iv) citizen reporting of problems.<sup>34</sup> Citizens respond if they agreed strongly, agreed, disagreed, or disagreed strongly with these statements. Enumerators first read each of the questions and offered to help respondents fill out the cards. Enumerators also informed respondents that the government would receive all of the cards plus a summary of their contents.

Dropping off the suggestion card in a locked box in downtown Kananga was left to participants. Taking the time and paying the transport to do so exhibits willingness to exert costly effort to have a voice in the provincial government. From January to May 2017, 396 individuals (13.6% of total endline participants) submitted their suggestion cards downtown. I

attend.

<sup>&</sup>lt;sup>33</sup>This suggestion box is similar in spirit to the postcard campaign in Paler (2013) and the anonymous comment forms in Olken (2007).

 $<sup>^{34}</sup>$ See Appendix Section 10.2 for the exact questions and other details about suggestion card submission.



## Figure 2: Photograph of townhall meeting in Kananga's Provincial Assembly building on January 30, 2017.

provided a summary of the results from submitted suggestion cards, as well as the individual cards themselves, to the governor, finance minister, and the directors of the tax ministry.

Turnout at townhall meetings and submission of suggestion cards are the key experimental measures of interest. For completeness, I consider five dependent variables.

- 1. *Townhall attendance*: an indicator that equals 1 if the individual attended one of the townhall meetings.
- 2. Suggestion card submission: an indicator that equals 1 if the individual submitted a suggestion card.
- 3. *Townhall or suggestion*: an indicator that equals 1 if a respondent participated in either the townhall meeting or submitted a suggestion card.
- 4. *Townhall and suggestion*: an indicator variable that equals 1 if a respondent engaged in both forms of costly participation.
- 5. Costly participation index: a standardized index composed of indicator variables for attendance at a townhall meeting and for submission of a suggestion card. I use standardized indices throughout the paper to facilitate interpretation of coefficient magnitude (in terms of standard deviations) and to reduce risks of type 1 or type 2 error. I construct these indices by first standardizing each component variable, then summing over all questions (ignoring missingness), and finally standardizing the new synthetic variable again. A similar indexing procedure is used whenever there are multiple measures of the same underlying variable.

In addition, to characterize the precise forms of political engagement measured by the experimental outcomes — and to verify that they approximate a process of tax bargaining — I also examine the content of participants' comments, questions, and suggestions at the townhall meetings and on the suggestion cards. Such analysis is confounded by the underlying selection process associated with the observed treatment effects on participation. But it nonetheless helps confirm that these experimental measures are picking up efforts to lobby the government for better public services and greater accountability.<sup>35</sup> Enumerators in the back of the townhall meeting recorded for each citizen comment or question (i) whether the speaker hailed from a treatment or control neighborhood, and (ii) the topics covered in their remarks.<sup>36</sup>

Concerning suggestion cards, examining whether individuals checked the box "I want the Provincial Government of Kasaï Central to do a better job" helps rule out the possibility that individuals are submitting suggestion cards out of idle curiosity, or in fact to voice support for the provincial government. In the analysis that follows, I first consider submission of any suggestion card and then of cards that are *critical* of the government, indicated by checking the aforementioned box. Moreover, an optional comment section at the bottom of the suggestion card gave respondents an opportunity to write in additional messages to the provincial government. Writing in a comment that is either critical of the government or makes an overt demand of the government indicates a stronger desire to have a voice in the government. I therefore first consider submission of any suggestion card and then of cards with critical written suggestions in particular.

Finally, we consider a series of survey questions asking respondents whom they think should be the main provider of public goods across a range of different sectors (education, health care, infrastructure, etc). Across these sectors, the extent to which the respondents chose the provincial government relative to other possible providers — such as the national government, NGOs, or churches — offers one measure of citizens' expectations for public goods provision from the provincial government. The variable *Govt responsibility: Sector-based questions*, an index of sector-specific indicators for respondents choosing the provincial government, is thus increasing in amount of public goods provision expected by citizens from the provincial government specifically. Second, enumerators posed three sets of opposing viewpoints concerning the optimal level of public good provision by the provincial government relative to provision by others, such as foreign aid organizations or individuals themselves. These questions are combined into an index, *Govt responsibility: Hypothetical questions*, increasing in the extent to which participants envision a large role for the provincial government in public goods provision. Both indices are constructed in an identical manner to that described on page  $16.^{37}$ 

<sup>&</sup>lt;sup>35</sup>I did not preregister examination of the content of participants' questions and comments at townhalls or on suggestion cards. Analysis of these data is suggestive and only intended to be descriptive of the nature of citizens' participation in these activities.

<sup>&</sup>lt;sup>36</sup>Participants received a section seating assignment when their invitations were checked outside the provincial assembly building. Treated and control individuals sat in separate sections. Which section was treatment and control was unknown to participants and enumerators.

<sup>&</sup>lt;sup>37</sup>See the notes to Online Appendix Table 19 and 20 for the exact text of the underlying questions.

### 6 Estimation strategy

I primarily consider reduced-form effects of the 2016 property tax collection campaign:

$$y_{ijk} = \beta_1 I_{jk}^{Program} + \alpha_k + \mathbf{X}_{ijk} \mathbf{\Gamma} + \mathbf{X}_{jk} \mathbf{\Phi} + \varepsilon_{ijk}$$
(1)

where *i* indexes individuals, *j* indexes neighborhoods, and *k* indexes the strata used during randomization. There are 33 strata defined by (i) the location of a given neighborhood in Kananga, and (ii) the estimated population of the neighborhood.<sup>38</sup> Geographically based stratification addresses the chief inference problem that this experiment was designed to solve: the selection of certain individuals and firms when states extend the tax net. Specifically, governments typically target wealthier individuals, especially those with easily taxable (immobile) assets, who may be more likely to participate for other reasons independent of taxation. Because wealth and other characteristics that may enter into the government's selection function cluster spatially in urban areas, stratifying on geographically determined strata ensures balance along these characteristics.

 $I_{jk}^{Program}$  is an indicator for neighborhoods that receive the door-to-door tax program, meaning that  $\beta_1$  estimates the average causal effect of the tax program on the outcome of interest  $(y_{ijk})$ , engagement with the provincial government. Standard errors are clustered at the neighborhood level; there are 356 clusters in the main analysis. In addition,  $\alpha_k$  are strata fixed effects, and  $\mathbf{X}_{ijk}$  and  $\mathbf{X}_{jk}$  are individual- and neighborhood-level covariates included to increase precision. As noted in the analysis plan, all regressions include gender, age, and age squared as covariates. I discuss covariate selection in depth in the next section when checking balance.

Estimating Equation 1 with ordinary least squares recovers the reduced-form average causal effect of the tax program on engagement with the provincial government.<sup>39</sup> If we take seriously the signaling model in Section 8.2, then  $\beta_1$  is the theory-relevant quantity of interest. The signal is received by all individuals in neighborhoods assigned to treatment, thus the theory predicts a neighborhood-level average increase in engagement with the level of government that is collecting taxes. If instead we have in mind a mechanism whereby the increase in political engagement operates through tax payment specifically, we would interpret  $\beta_1$  as an intent-to-treat (ITT) effect, with treatment defined as tax payment.<sup>40</sup>

#### 6.1 Balance checks

I check for balance by estimating equation 1 with stratum fixed effects and the three basic covariates, using a range of other variables that might partially explain variation in engage-

<sup>&</sup>lt;sup>38</sup>Location was determined by 11 satellite map grid cells that fully partition the city. Population in each neighborhood was estimated by counting houses visible from satellite images. There is error in this method, but the error will be constant across neighborhoods.

<sup>&</sup>lt;sup>39</sup>Unless stated otherwise, all regressions reported are OLS estimations.

<sup>&</sup>lt;sup>40</sup>For instance, Martin (2014)'s model predicts effects on taxpayers not on individuals living in taxes neighborhoods who do not pay taxes.

ment with the provincial government in place of  $y_{ijk}$ . On the individual level, I check for balance on: years of education, monthly income, wealth, business owner status, property owner status, multiple plot owner status, government worker status, migrant status, majority ethnicity status, literacy, existence of renters in the compound, access to electricity, house wall quality, and ownership of a vehicle.<sup>41</sup> On the neighborhood level, I check for balance on: road quality, quality of public lighting, household wealth, unemployment, access to electricity, past collector visits (reported at baseline), past payment of the property tax (reported at baseline), past political participation and political beliefs (reported at baseline).<sup>42</sup>

In total, one covariate — business owner status — is imbalanced at the 5% level out of the 26 tested for (3.7% of all covariates tested), and three additional covariates — estimated wealth, availability of public lighting, and reported past visits from tax collectors — are imbalanced at the 10% level (15.4% of covariates tested). These results are summarized in Appendix Figure 8. Because this is higher than the expected 10%, I consider an omnibus test of joint orthogonality for the 17 covariates listed in the pre-analysis plan. This test fails to reject the null (F(17, 345) = 1.31, p = 0.18). Running the same test with the full set of possible covariates, included additional variables not mentioned in the pre-analysis plan, the test also fails to reject the null (F(26, 345) = 1.17, p = 0.26). Additionally, following Imbens and Rubin (2015), if I examine not the statistical significance of balance tests but the normalized size of differences across treatment and control, there are also no normalized differences greater than 0.25, the benchmark they suggest in testing for balance (Appendix Table 14).<sup>43</sup> Nonetheless, to be conservative, the four imbalanced covariates are included in  $\mathbf{X}_{ijk}$  and  $\mathbf{X}_{jk}$  in the main specification.<sup>44</sup>

### 7 Results

This section examines first whether the program succeeded at its stated goals of increasing visits from tax collectors and overall tax compliance (Hypothesis 1), before considering if the tax program increased citizen engagement with the provincial government (Hypothesis 2). I find that the program caused an 11.1 percentage-point increase in property tax payment and a 4.9 percentage-point increase in the probability that individuals attend a townhall meeting or submit a suggestion card.

<sup>&</sup>lt;sup>41</sup>Data on these variables comes from the endline survey.

<sup>&</sup>lt;sup>42</sup>To measure past political participation, the baseline survey includes information on (1) knowledge of the governor's name, and (2) participation in elections, parties, and protests (three separate variables combined in an index). All neighborhood-level covariates are constructed from baseline data, except for road quality and public lighting, which were measured at endline but unlikely to change because of treatment.

<sup>&</sup>lt;sup>43</sup>Specifically, Imbens and Rubin (2015) suggest the normalized difference as a scale-invariant measure of the magnitude of the difference across treatment and control:  $\delta = \frac{\mu_1 - \mu_0}{\sqrt{\frac{\sigma_1^2 + \sigma_0^2}{2}}}$ .

<sup>&</sup>lt;sup>44</sup>Bruhn and McKenzie (2009) argue that it is not necessary to control for covariates found to be imbalanced in randomized experiments. Because this is still the most widely used practice in economics, I still do so in the main specifications and then show versions of all regressions without these covariates in robustness tables.

#### 7.1 Did the tax campaign increase tax payment?

As noted, it is not obvious that a tax campaign in Kananga would succeed in achieving its basic goals of increasing visits by tax collectors and payment of the property tax. Congo has been besieged by war, poverty, and institutional decay. Public employees are underpaid and often absent from their posts. In such a setting, will tax collectors recruited to this campaign exert the effort it takes to make door-to-door tax appeals in a large city with few paved roads? Beyond the challenge of motivating public employees, there is the challenge of convincing citizens to pay in a setting with little past exposure to formal taxation. If a collector suddenly appears asking for the property tax, will citizens pay? Given the sorry record of many top-down state building initiatives (Fukuyama, 2017), one might well predict the campaign in question would fail to raise tax compliance.

Nonetheless, the evidence supports Hypothesis 1. Table 3 summarizes OLS estimations of equation 1 using as dependent variables indicators for (i) being visited by tax collectors, and (ii) for paying property taxes (verified in administrative data). Column 1 reveals that the program caused a 64.2 percentage-point increase in the probability that households were visited by tax collectors in 2016.<sup>45</sup> Column 2 demonstrates that the program caused on average a 11.1 percentage-point increase in the probability that households paid the property tax. Given that citizens are unaccustomed to paying official taxes in this setting, this is a large effect: an 18-fold rise relative to control areas.<sup>46</sup>

	Visited by tax collector	Paid property tax
	(1)	(2)
Program	$0.642^{***}$	0.111***
	(0.018)	(0.010)
Covariates	Yes	Yes
Stratum FE	Yes	Yes
$R^2$	0.417	0.085
Observations	2913	2913
Clusters	356	356
Control Mean	0.164	0.006

Table 3: Visits by collectors and payment of the property tax

Standard errors clustered by polygon.  $^{\ast}p < 0.1,^{\ast\ast}p < 0.05,^{\ast\ast\ast}p < 0.01.$ 

Notes: *Visited by tax collectors* is an indicator for households reporting at least one visit by tax collectors in 2016. *Paid property tax* is an indicator for individuals' who paid the property tax in 2016 according to the administrative data.

In addition to main specifications shown in Table 3, a battery of robustness checks is reported

<sup>46</sup>Online Appendix Table 3 contains the raw data on tax compliance in program and control neighborhoods.

<sup>&</sup>lt;sup>45</sup>In control neighborhoods, 16.4% of individuals report being visited by tax collectors, yet the rate of payment in these areas is less than 1%. Most likely, these households are confusing visits from enumerators and tax collectors, as enumerators conducted an audit survey in all neighborhoods while the tax campaign was ongoing. Thus, estimate in the first column is likely a lower bound on the true increase in visits from tax collectors.

in Online Appendix Section 3. Robustness checks include specifications with (1) only the three basic covariates (gender, age, age squared), (2) all covariates listed in the pre-analysis plan, (3) enumerator fixed effects, (4) sampling weights, and (5) interactions with a house quality variable to examine heterogeneous treatment effects by wealth. These same robustness checks are repeated in the Online Appendix for all subsequent estimations reported in the paper.

Although an 11 percentage-point increase in tax compliance is substantial in this context, the great majority of individuals still *evade* paying the tax — despite visits from collectors at their households. Why does the program move some, but far from all, individuals to pay the tax? A complete treatment of this question is provided in a companion paper (Weigel, 2017). Briefly, tax compliers are more likely to be male, educated, wealthy, employed, and to work for the government in some capacity. Individuals who perceive at baseline a higher probability of punishment for evaders are more likely to pay — as are individuals who profess more positive attitudes toward the provincial government. There is thus support for seminal cost-benefit models of tax compliance (Allingham and Sandmo, 1972) as well as models of "tax morale" (Luttmer and Singhal, 2014). Online Appendix Section 2.2 contains more information about compliance and complier characteristics.

Before turning to the main test of the tax-participation hypothesis, it is worth noting that the program does not appear to have increased bribes, as discussed in Section 2 in the Online Appendix. In interpreting the reduced-form impacts of the program, we can therefore be confident that any effects detected result from increases in visits from tax collectors and in tax payment, as described in this section.

## 7.2 Testing the tax-participation hypothesis

Does the tax program boost citizens' efforts to lobby the provincial government for better public services? Estimations of Equation 1, summarized in Table 4, support the taxparticipation hypothesis. Columns 1 and 2 show that the program triggered a 4.4 percentagepoint increase in townhall attendance and a 2.6 percentage-point increase in suggestion card submission, respectively. To shed light on the intensive margin, Columns 3 and 4 show that the program stimulated participation in either outcome by 4.9 percentage points and in both outcomes by 2.8 percentage points. Column 5 reveals that these treatment effects amount to a 0.14 standard deviation increase in costly participation.

Of the 600 participants who participated in either the townhall meeting or the suggestion box, 179 (30%) participated in both; 128 of the 179 (72%) hailed from program neighborhoods. The location of the provincial assembly building, where the townhall meetings occurred, and the suggestion box were about 1 kilometer apart in downtown Kananga (see Figure 12). However, suggestion card submission did not spike on the days of townhall meetings. Most double participants appear to have made independent trips to attend the townhall and to submit their suggestion card.

The results are robust to a series of robustness checks as well as estimating average effect size (AES) coefficients in place of *Costly participation index*, following Clingingsmith et al.

(2009).<sup>47</sup> Controlling for the distance between participants' houses and the location of the townhall meeting and the suggestion box (shown in Appendix Figure 12) does not affect the estimates.<sup>48</sup> Aggregating the data to the cluster (neighborhood) level and reestimating the treatment effects of the program also returns very similar results (with somewhat larger standard errors).<sup>49</sup> Constructing *p*-values using randomization inference techniques — reported as *RI p-value* in Table 4 — or multiple comparison adjustments (Online Appendix Table 22) does not meaningfully affect the statistical significance of the estimates.

	Townhall	Suggestion	Townhall	Townhall	Costly
	meeting	card	or	and	participation
	attendance	submission	suggestion	suggestion	index
	(1)	(2)	(3)	(4)	(5)
Program	0.044**	0.026**	0.049***	0.028***	0.144***
	(0.020)	(0.012)	(0.016)	(0.009)	(0.043)
Covariates	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes
$R^2$	0.062	0.055	0.067	0.038	0.070
Observations	1934	2912	2913	2913	2913
Clusters	252	356	356	356	356
Dep var	0-1	0-1	0-1	0-1	Standardized
Control Mean	.18	.1	.18	.035	057
RI $p$ -value	0.0344	0.0446	0.0050	0.0040	0.0012

Table 4: Attendance at townhall meetings and submission of suggestion cards

Standard errors clustered by polygon. \*p < 0.1,\*\* p < 0.05,\*\*\* p < 0.01

Notes: Townhall attendance is an indicator variable that equals 1 if a participant attended a townhall meeting. Suggestion card submission is an indicator variable that equals 1 if a participant submitted his or her suggestion card. Townhall or suggestion indicates that a participant attended either a townhall meeting or submitted a suggestion card. Townhall and suggestion indicates that a participant attended a townhall meeting and submitted a suggestion card. Costly participation index is a standardized index of two indicator variables that equal 1 if a participant (1) turned up at a townhall meeting, or (2) submitted a suggestion card, respectively.

To demonstrate the effect visually, Figure 3 shows the predicted probability that the median individual in the treatment and control group will attend a townhall meeting or submit a

 $<sup>^{47}\</sup>mathrm{See}$  Online Appendix Section 3 for robustness checks and Table 21 for AES estimation.

<sup>&</sup>lt;sup>48</sup>Although including stratum fixed effects obviates the need for distance controls — strata were defined in part based on location — estimation results controlling for distance are in Online Appendix Table 9.

<sup>&</sup>lt;sup>49</sup>See Online Appendix Table 10. This exercise is motivated by concerns about within-cluster spillovers causing bias when randomization is on the cluster level, outcome data on the individual level, and there is imperfect receipt of treatment. In this case, the ITT effect returns the effect of the treatment plus the spillover effects on untreated individuals in treated clusters. If we think of treatment as payment of the property tax — rather than the tax program as a whole — then this concern may be justified because the tax program likely causes other effects that do not operate through payment. Indeed, this is the essence if the idea of a signaling mechanism linking tax collection and participation. However, because the quantity of interest in this analysis is the average effect of the tax campaign, not just the effect through payment, such within-cluster spillovers are simply part of the program effect I seek to estimate. Nonetheless, results are shown in the Online Appendix.

suggestion card. These distributions are simulated using parameters from a logistic estimation of Equation 1 with *Townhall or suggestion* as the dependent variable, following King (1998).<sup>50</sup>

Do higher rates of townhall attendance and suggestion card submission necessarily indicate that citizens are participating in order to demand better public services and greater account-ability? Or, given that these avenues of participation were a relatively new phenomenon in this context, are individuals taking part out of idle curiosity, or some other reason? I examine the questions and comments participants raised during townhall meetings and on suggestion cards to argue that citizens took seriously these channels of participation as a means through which to communicate their demands of the provincial government.



### Predicted probability of attending townhall or submitting suggestion card For median individual in treatment and control group

## Figure 3: Predicted probabilities of attending townhall meeting or submitting a suggestion card for the median individual in treatment and control group.

As shown in Figure 4, the primary topics raised during the townhall meetings were the provincial tax system in general and the 2016 property tax campaign specifically.<sup>51</sup> The

<sup>&</sup>lt;sup>50</sup>Specifically, I estimated Equation 1 using a logit model. Then, exploiting the asymptotic normality of the maximum likelihood estimator, I drew 1,000 sets of coefficients from a multivariate normal with center and dispersion parameters given by empirical estimates. I obtain a predicted probability for the median individual in the treatment and control group using each set of parameters, and then take 10,000 draws from a Bernoulli distribution using this predicted probability to account for what King (1998) calls "fundamental uncertainty." The average of these draws is one observation in the distributions drawn in Figure 3.

<sup>&</sup>lt;sup>51</sup>Among the most common questions within these topics were (i) what happens to the money collected on the ground, (ii) what happens to the money once it reaches the tax ministry, (iii) why the government began to enforce the property tax when it did, (iv) what taxes the government will be enforcing in the future.



#### Figure 4: Topics of participants' questions and suggestions during townhall meetings in Kananga.

next largest topics were provincial revenues and spending — in particular spending of tax receipts — the provision of public goods (or lack thereof) in Kananga, and corruption in the provincial government. This distribution of topics reflects that citizens used the townhall meetings and suggestion cards to threaten noncompliance with future taxation as a means of generating government commitments to greater public spending or representation. Many participants raised the poor quality of roads and the lack of electricity in the city, demanding the government address these problems if it plans to tax the population more vigorously. "Why should the inhabitants of Lukonga [a commune of Kananga] pay taxes," one participant asked, "when the roads are in such disastrous condition?"<sup>52</sup> Citizens were eager to stand up and participate in the dialogue; at most meetings, there were more hands in the air than time allowed for. Citizens raising specific problems in specific neighborhoods is demonstrative of a bargaining process in which citizens demand better governance in exchange for tax compliance.

One alternative explanation of these results is that more individuals from treated neighborhoods participated in the townhall not to try to hold the government accountable but because they simply had questions about the 2016 property tax campaign. However, a simple difference-in-means test rules out this possibility: on average individuals in treated neighborhoods were less likely to ask a question about the 2016 tax campaign or provincial taxes, and slightly more likely to ask about provincial spending and public goods in the city, though these differences are not statistically significant.

 $<sup>\</sup>overline{}^{52}$ Participant question from January 30 townhall meeting. Translation from French by the author.

	Critical suggestion card	Written-in suggestion
	(1)	(2)
Program	0.022**	0.020**
	(0.010)	(0.008)
Covariates	Yes	Yes
Stratum FE	Yes	Yes
$R^2$	0.048	0.037
Observations	2913	2913
Clusters	356	356
Control Mean	0.078	0.033

 Table 5: Submission of critical and commented suggestion cards

Standard errors clustered by polygon. \*<br/>  $p < 0.1, ^{\ast\ast} p < 0.05, ^{\ast\ast\ast} p < 0.01.$ 

Notes: *Critical suggestion card* is an indicator variable for individuals who submitted a suggestion card that was critical of the government (determined by the respondent's choice to check the box "I want the Provincial Government of Kasa" Central to do a better job" on the card). *Written-in suggestion* is an indicator for respondents who dropped off cards with critical written-in suggestions for the government in addition to the five multiple-choice questions filled out by everyone (see Appendix Figure 13).

Analysis of suggestion cards reveals a similar pattern. Participants who submitted their cards were critical of the government: over 90% demanded that the government do a better job. Similarly, Appendix Figure 15 shows that respondents overwhelmingly demand more inclusiveness and accountability from their government in the other four multiple-choice questions on the card. In addition, 39% of individuals filled out the optional suggestion section — further indication of costly effort. Appendix Figure 16 shows the most common comments in this section: general demands for better provincial governance, demands for more public goods, and demands for greater monitoring of the government and improved transparency. Participants often cited specific examples of poor quality roads, lack of electricity, or advancing erosion in certain neighborhoods, demanding that the government remedy the situation. "We ask our government to draw its attention especially to Quartier Kapanda, Avenue Lubanza," wrote one participant, "where we are threatened by erosion, and we note that our government has never built anything to counter erosion in this quarter. The provincial government could also examine the Katumba canal, which is cut off on the Katoka side.<sup>53</sup> Appendix Table 15 shows the most common substantive words used in suggestion card comments, reinforcing that participants submitted suggestion cards as a means of communicating their demands to the provincial government.

Consistent with Hypothesis 2, individuals in program neighborhoods were more likely to submit (i) suggestion cards that are *critical* of the government, and (ii) cards that contain optional written-in suggestions with a critical message or demand of the government.<sup>54</sup> Ta-

<sup>&</sup>lt;sup>53</sup>Another respondent wrote the following: "The provincial government should stabilize the erosions in our neighborhood: Tukombe, especially on avenue Mpokolo as well as Nkumbikumbi, for the nuns of Tukombe."

<sup>&</sup>lt;sup>54</sup>This latter variable equals 1 only if the written-in comment is critical or makes a demand of the provincial government. Comments that are complimentary of the government (5.5% of total comments) and comments about the Harvard research team (3.3%), militia-related violence (3.9%), or some other topic not relevant

	Govt responsibility:	Govt responsibility:	Govt responsibility:
	Index of all questions	Sector-based questions	Hypothetical questions
	(1)	(2)	(3)
Program	$0.112^{**}$	0.088	0.088**
	(0.052)	(0.053)	(0.041)
Covariates	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes
$R^2$	0.041	0.043	0.030
Observations	2913	2813	2900
Clusters	356	356	356
Control Mean	-0.066	-0.051	-0.053

Table 6: Perceived role of the provincial government in public goods provision

Standard errors clustered by polygon. \*<br/>  $p < 0.1,^{\ast\ast} p < 0.05,^{\ast\ast\ast} p < 0.01.$ 

Notes: The dependent variable in Column 1 is increasing in the perception that the provincial government should be a primary provider of public goods in Kananga. It is a standardized index based on the variables that makeup the next two indices. The dependent variable in Column 2 is an index increasing in the degree to which participants chose the provincial government as the main provider of public goods across various sectors, such as schools and health care. The dependent variable in Column 3 is an index increasing in the degree to which participants deem the provincial government the principal provider of public goods in three hypothetical survey questions. See the notes to Online Appendix Table 19 and 20 for the exact text of the underlying questions.

ble 5 shows that if we consider only cards that expressed disapproval of the government (Column 1) or cards that included a written-in comment making a demand or critique of the government (Column 2), the estimated causal effect of the tax program is still statistically significant. These results rule out the possibility that the reduced-form effect on submission of suggestion cards is driven by citizens who are content with the government or participating out of curiosity.

Finally, if the increases in participation reflect a desire to engage in tax bargaining, we might expect individuals in treated neighborhoods to express stronger views about the obligation of the provincial government to provide public goods. Table 6 shows regression results examining survey-based indices increasing in the perceived responsibility of the provincial government in public goods provision confirms this supposition. Individuals in treated neighborhoods perceive a larger role (by 0.112 standard deviations) for the provincial government in public goods provision. Examining the sub-indices, the standard errors are larger when considering the viewpoint questions rather than the sector-based questions, for which the point estimate is not statistically significant.<sup>55</sup> Consistent with Hypothesis 2, individuals in program areas expect more public services from the provincial government relative to individuals in control areas.<sup>56</sup>

to a process of tax bargaining (4.4%) are excluded.

<sup>&</sup>lt;sup>55</sup>Online Appendix Figure 13 shows results for the individual survey questions that make up the indices considered in Table 6.

<sup>&</sup>lt;sup>56</sup>Are citizens in program neighborhoods really demanding more public service delivery in the future — something they could potentially influence through participation and monitoring — or do the results in Table 6 simply reflect changed beliefs about the current level of services provided by the provincial

In sum, consistent with the tax-participation hypothesis, individuals in program neighborhoods appear more willing to exert costly effort in order to have a voice in the provincial government and to demand more accountability and more public goods provision.

### 7.3 Alternative explanations

This section considers three alternative explanations of the observed effects of the tax campaign on costly participation. Rather than a process of tax bargaining with the provincial government, do higher rates of participation in program neighborhoods reflect (1) an artifact of the research design, such as experimenter demand effects, (2) 'disappointment effects,' in which the control group participates less rather than the treatment group participating more, or (3) a broader expansion of civic engagement rather than an increase in costly participation with the provincial government?

#### 7.3.1 Experimentally induced bias

A natural concern is whether the observed increase in participation is an artifact of the research design, induced not by tax collection but by a difference in how data were collected in program neighborhoods relative to control neighborhoods. Are citizens participating not to demand better governance but because they are more trusting of or familiar with the research team?

As noted in Table 1, all research procedures were held constant across treatment and control neighborhoods. The baseline, monitoring, and endline survey forms and procedures were identical. Enumerators were assigned to conduct surveys in a randomly generated order, frequently alternating between working in control and treatment areas. Moreover, participants in treatment and control neighborhoods received the same information about the townhall meetings and about the suggestion cards. Participation in townhall meetings and submission of suggestion cards always occurred *after* endline survey enumeration to minimize experimenter demand effects on respondents' decisions to participate. Even if demand effects remain, they should be constant across treatment and control because all survey and experimental procedures were identical.

Nonetheless, to test formally for different levels of trust or familiarity with the research team, we consider survey questions asking respondents (1) how much they trust foreign research organizations, (2) whether they can identify correctly the employer of the enumerator at the conclusion of the endline survey, and (3) whether they report having participated in surveys or other activities with the research team prior to the current project. Table 7 shows the results of regressions using each of these variables as the outcome. No systematic differences appear across treatment and control.

government? A series of questions asked respondents how much they believe the provincial government currently provides in the same sectors noted above.<sup>57</sup> No systematic differences appear between people in treated and control neighborhoods (see Table 23 in the Online Appendix). The results in Table 6 therefore indicate that the program increased individuals' expectations for *future* service delivery.

	Trusts researchers	Remembers researchers	Participated in past
	(1)	(2)	(3)
Program	0.059	-0.031	0.024
	(0.056)	(0.047)	(0.037)
Covariates	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes
$R^2$	0.038	0.122	0.016
Observations	2733	2913	2913
Clusters	356	356	356
Control Mean	3.092	0.034	-0.014

Table 7: Knowledge of and trust in the research team

Standard errors clustered by polygon. \*p < 0.1,\*\* p < 0.05,\*\*\* p < 0.01.

Notes: *Trusts researchers* is a standardized measure of respondents' self-reported trust levels in foreign research organizations. Enumerators read the following prompt: "I am going to name a number of organizations. For each one, could you tell me how much confidence you have in them: is it a great deal of confidence, quite a lot of confidence, not very much confidence or none at all?" *Remembers researchers* is a standardized measure of respondents' ability to correctly identify the employer of the enumerator at the conclusion of the endline survey. *Participated in past* is a standardized measure of respondents' self-reported participation in past research activities conducted by our team in Kananga.

#### 7.3.2 Disappointment effects

A second alternative explanation is that the reduced-form results are driven not by increasing participation in the treatment group, but by decreasing participation in the control group. Because informational fliers announcing the campaign were distributed in all neighborhoods, it is possible that individuals in control neighborhoods choose to participate less often because they were informed about the tax collection campaign but never received visits from tax collectors before outcomes were measured during the endline survey.<sup>58</sup> They might have concluded that the government was less capable than previously thought, and chosen to participate less often as a result.

I investigate this hypothesis using the subsample of 630 individuals whom the enumerators successfully tracked from baseline to endline. Although I do not have repeated measures of costly participation, I examine changes in beliefs about the provincial government within individuals over time. Presumably if the main results were driven by a 'disappointment effect,' we could expect to observe a decline in reported trust in the government, approval of the government, and the perceived honesty of the government. Table 8 summarizes fixed-effects regressions with an indicator (*Endline*) for measurement at endline and an interaction with the program indicator. If attitudes towards the government deteriorated in the control as a result of not receiving the tax program, the point estimates on the *Endline* variable would be negative. For none of these variables is the coefficient negative and different from zero. If anything, individuals in control (and treatment) neighborhoods appear more likely

<sup>&</sup>lt;sup>58</sup>As noted, the campaign was designed to be phased into the control group after outcomes had been measured in the treatment group. However, the militia violence in Kasaï led the government to cancel property tax collection in 2017.

to approve of the provincial government and tax ministry at endline. It is therefore unlikely that a negative response in control neighborhoods explains the estimated treatment effects of the tax program.

Table 8: Changes in beliefs from the tax program								
	Govt responsibility:	Trust in	Evaluation of	Honesty of				
	sector-based questions	government	government	government				
	(1)	(2)	(3)	(4)				
ProgramXEndline	$0.674^{**}$	0.183	-0.199	-9.355				
	(0.278)	(0.255)	(0.309)	(38.888)				
Endline	0.004	0.022	$0.495^{**}$	38.539				
	(0.211)	(0.206)	(0.240)	(31.458)				
Observations	1237	1233	1183	1229				
$R^2$	0.032	0.004	0.014	0.006				

Standard errors clustered by polygon. \*p < 0.1, \*\*p < 0.05, \*\*\*p < 0.01.

Notes: Govt responsibility: sector-based questions is increasing in the perception that the provincial government should be a primary provider of public goods in Kananga. It is constructed as described in the notes to Table 6. Trust in government is an index increasing in trust of the government, constructed using the self-reported trust levels of the provincial government and tax ministry. Approve of government is an index increasing in views of the government. It is constructed using the self-reported evaluation of the "performance" of the provincial government and tax ministry. Honesty of government is increasing in perceived honesty of the provincial government, constructed as described in footnote 72. These outcomes are observed twice for the subsample of 630 individuals tracked from baseline to endline.

These regressions also replicate the results in Table 6: the tax program increases individuals' beliefs about the responsibilities of the provincial government to provide public goods (Column 1).

#### 7.3.3Civic-mindedness in all domains

The theories of tax bargaining animating this paper predict increases in engagement with the government that receives the revenues of the taxation in consideration — in this case, the Provincial Government of Kasaï Central — with the goal of obtaining concessions in the form of public goods provision of accountability. Another alternative explanation of the observed increases in costly participation is that taxation causes people to become more politically and civically active in all domains. Increased willingness to monitor and have a voice in the provincial government could in fact be a byproduct of this deeper process of the development of civically minded citizens.

To test this alternative explanation, this section considers survey evidence about respondents' self-reported participation in national politics, engagement with local government brokers known as city chiefs, and interest in politics in general. The outcome variables are constructed as follows.

1. Engagement with national politics. Respondents reported their current and future

participation in national elections, parties, marches, protests, and rallies.<sup>59</sup> These five questions are combined into an index of expected future participation in national politics, using the same procedure described on page 16.

2. Engagement with local city chiefs. To measure local political engagement, I consider survey evidence on individuals reported interactions with and beliefs about city chiefs. Local city chiefs act as intermediaries — brokers — between citizens and the government. The chef de commune, chef de quartier, and the chef d'avenue are the bottom link in the chain of the city government bureaucracy. Dating back to the Mobutu era, city chiefs tend to the following tasks: (1) organizing weekly salongo, an informal tax in which citizens contribute labor toward local public goods provision, such as cleaning and repairing roads<sup>60</sup>; (2) communicating grievances to the government and advocating on behalf of the community; and (3) mediating local disputes to avoid escalation to the courts. Although they hold office for life and have elevated social status, city chiefs are appointed.

To measure views of and engagement with city chiefs, survey questions asked respondents about their familiarity of the avenue chief's name, the frequency with which they consult the commune, quartier, and avenue chiefs (respectively) to discuss community problems, the frequency with which they participate in *salongo*, their trust levels in the avenue and quartier chief, and the perceived responsiveness of the avenue chief. As an outcome variable, we consider a standardized index of these survey variables.<sup>61</sup>

3. General interest in politics. Concerning general interest in politics, survey questions ask respondents how frequently they listen to the radio (Column 2) or read articles about politics (Column 3). Three conjoint-choice questions offered participants a chance to learn a fact about the government or a fact about other private enterprises and organizations in Kananga. These five variables are combined into an index.<sup>62</sup>

Estimating Equation 1 with OLS, there is no detectable difference in anticipated future national political participation or general interest in politics (Table 9).<sup>63</sup> Column 2, however, indicates that individuals in treatment report lower levels of engagement with city chiefs. Examining the point estimates on the individual survey questions suggests that this decrease reflects both fewer consultations to city chiefs as well as diminished views of their quality (see Online Appendix Figure 15). The effect is more pronounced among relatively poorer individuals (see Online Appendix Table 28).

This erosion of the perceived legitimacy of city chiefs has an intuitive interpretation in the context. City chiefs have more responsibilities in poorer, less modern neighborhoods. In

<sup>&</sup>lt;sup>59</sup>The exact formulation of these questions is in the notes to Online Appendix Table 27.

 $<sup>^{60}</sup>Salongo$  is an example of "informal taxation," such as that discussed in Olken and Singhal (2011)

<sup>&</sup>lt;sup>61</sup>Respondents who themselves report being local city chiefs are excluded from analysis of these variables.

<sup>&</sup>lt;sup>62</sup>See the notes to Online Appendix Table 29 and 30 for details on these survey questions.

<sup>&</sup>lt;sup>63</sup>Nor does it appear to cause citizens to envision a more active role for citizens in politics in general. The survey also asked participants three questions concerning the purported tradeoff between democratic inclusion and government efficiency. These questions make up the index *Preference for democracy*, which also returns a null result when used as the dependent variable in Equation 1 (see Online Appendix Section 30).

	Engagement with	Engagement with	General interest					
	national politics	local city chiefs	in politics					
	(1)	(2)	(3)					
Program	0.037	-0.103**	-0.009					
	(0.044)	(0.045)	(0.040)					
Covariates	Yes	Yes	Yes					
Stratum FE	Yes	Yes	Yes					
$R^2$	0.068	0.087	0.167					
Observations	2912	2825	2913					
Clusters	356	356	356					
Control Mean	-0.003	0.041	0.039					

Table 9: Effects on broader civic-mindedness

Standard errors clustered by polygon. \*<br/>  $p < 0.1, ^{\ast\ast} p < 0.05, ^{\ast\ast\ast} p < 0.01.$ 

Notes: Engagement with national politics is a standardized index increasing in future plans to participate in national politics in the context of: elections, political parties, protests, marches, or rallies. Notes: Engagement with local city chiefs is a standardized index increasing in the revealed legitimacy of local city chiefs. It is composed of measures of the familiarity of the avenue chief's name, indicators of visits to the commune, quartier, and avenue chiefs (respectively) to discuss community problems, indicators of having contributed in salongo (local public good projects organized by city chiefs), trust levels of the avenue and quartier chief, and perceived responsiveness of avenue chief. Respondents who themselves report being local city chiefs are excluded from these regressions. General interest in politics is a standardized index increasing in revealed and self-reported interest in learning about the government and politics. See Online Appendix Tables 27, 29, and 28 for details about these indices.

downtown Kananga, many people interact directly with the provincial government rather than relying on city chiefs as intermediaries. The program therefore represents a larger increase in exposure to the formal state among relatively less wealthy individuals. As they come in direct contact with the provincial government because of the tax program, citizens appear to seek out city chiefs less often and to find them less legitimate as government intermediaries. This result supports the view that building the central state can undermine local intermediaries institutions (Cheema et al., 2006), implying that central and local governance structures can be substitutes.

Overall, these results do not suggest that a broader expansion of civic tendencies explains the increase in costly participation with the provincial government. Rather they reinforce the interpretation that the observed increases in costly participation reflect citizens' specific demands of the level of government that sought to extract more taxes.

## 8 Mechanisms: Why did the tax program cause citizens to engage more with the provincial government?

Having established that the program caused a reduced-form increase in costly participation that resembles tax bargaining with the provincial government, and having ruled out alternative explanations, this section investigates the mechanisms behind this effect. I examine two families of mechanisms: those that operate through tax payment specifically, which expect only taxpayers to be participating more, and signaling mechanisms, which posit an effect on taxpayers and non-payers alike. I first demonstrate that a payment-based mechanism is unlikely to be driving the results in this setting. I then present a decision-theoretical model showing how the tax campaign sends a signal of state capacity that raises the expected benefits of participation. Survey evidence of citizens' stated beliefs about the government supports this mechanism.

#### 8.1 Payment-based mechanisms

Payment-based mechanisms, in which tax collection and participation are linked via tax payment *per se*, have received the most attention in the literature and are often implicit in studies that do not speak to microfoundations directly. For instance, tax payment is thought to stimulate a sense of ownership over public revenues, leading taxpayers to expect something in return as a *quid pro quo* (Prichard, 2015). Such expectations might be rooted in norms of reciprocity: after paying taxes, citizens feel entitled to better governance. Reciprocal expectations may be particularly strong in settings with clientelistic governments and in cultures with strong gift-giving norms.<sup>64</sup> Another payment-based mechanism expects taxation to activate an endowment effect by taking away earned income and thus increasing the expressive utility associated with participation (Sandbu, 2006; Martin, 2014).<sup>65</sup>

A first test of this mechanism is to examine rates of participation among payers and nonpayers in treated neighborhoods. Payment-based mechanisms would predict that payers are more likely than non-payers to attend townhall meetings and to submit suggestion cards. Conversely, a signaling mechanism would not predict differential participation by tax compliance status. If what stimulates participation is the informational signal conveyed to citizens by the fact that the government administered a tax campaign, then *all* individuals in treated neighborhoods — payers as well as individuals who manage to evade the tax — who receive this information should participate at similar, higher rates relative to control individuals.

A second test is to compare participation among individuals who were present at their household when tax collectors came and individuals who not present. Payment-based mechanisms

<sup>&</sup>lt;sup>64</sup>Reciprocal exchanges of private goods are a common way in which individuals interact with the state in developing countries (Stokes et al., 2013). Anthropologists have written extensively on gift-giving norms (Mauss, 2000), and political scientists have noted in particular their role in sustaining "big man"-style politics in Africa (Schatzberg, 1997).

 $<sup>^{65}</sup>$ For instance, in Martin's (2014) model, taxation heightens the "expressive benefit from taking punitive action against bad leaders" because an unexpected drop in earned income due to tax payment shifts individuals below their reference point (regarding expected earned income) into the realm of losses. Loss aversion implies that taxed individuals care more about waste and corruption in government than do untaxed individuals, thereby magnifying the utility achieved from communicating their grievances and sanctioning venal politician. This expressive benefit is similar to the D term used to capture non-instrumental utility from voting (Aldrich, 1993). A testable implication of this mechanism is that the tax program should magnify citizens' grievances regarding the provincial government, particularly among taxpayers. I examine the evidence on views of the provincial government and the tax ministry in Online Appendix Section 3.5. There is little evidence to support an expressive-utility mechanism in this context.

expect no difference in participation across these two groups. On the other hand, a signaling mechanism expects those who were present for tax collector visits to receive the signal, while those who were absent may not. I exploit this natural (though non-random) variation in exposure to the tax collection program to provide suggestive evidence about the plausibility of payment-based or signaling mechanisms in this context.

In summary, if the increase in participation reflects a payment-based mechanism, then in a sample restricted to treated neighborhoods, we would predict (i) higher participation among payers compared to non-payers, and (ii) no difference among individuals who were present when tax collectors visited compared to those who were not present.

Columns 1 and 4 in Table 10 compare the participation of payers and non-payers within treated neighborhoods.<sup>66</sup> Payers are not systematically different in this respect. Columns 2 and 5 show that individuals reporting visits from tax collectors are more likely to engage in costly participation compared to individuals in treated neighborhoods who do not report visits. A horserace regression with both (endogenous) variables demonstrates that participation is positively correlated with being visited by tax collectors, but not with tax payment. A signaling mechanism accounts for these results, while a payment-based mechanism does not.

	Townh	all or sug	gestion	Costly participation index			
	(1)	(2)	(3)	(4)	(5)	(6)	
Paid	0.016		0.007	-0.045		-0.069	
	(0.034)		(0.034)	(0.082)		(0.082)	
Visited		$0.050^{**}$	$0.049^{**}$		$0.125^{**}$	$0.133^{**}$	
		(0.024)	(0.024)		(0.063)	(0.063)	
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	
Observations	1703	1703	1703	1703	1703	1703	
$R^2$	0.074	0.076	0.076	0.078	0.080	0.081	
Control Mean	0.178	0.178	0.178	-0.057	-0.057	-0.057	
Clusters	211	211	211	211	211	211	

 Table 10: Heterogeneity in participation within treated neighborhoods

Standard errors clustered by polygon. \*<br/>  $p < 0.1,^{\ast\ast} p < 0.05,^{\ast\ast\ast} p < 0.01.$ 

Notes: *Costly participation index* is a standardized index of two indicator variables that equal 1 if a participant (1) turned up at a townhall meeting, or (2) submitted a suggestion card, respectively. The sample is restricted to treated neighborhoods.

However, the correlations in Table 10 could belie causal relationships consistent with this mechanism. Testing whether payment causes citizens to engage in the government requires

<sup>&</sup>lt;sup>66</sup>Results are analogous using the full sample, but the interpretation is clearer by restricting the sample to treatment neighborhoods. Comparing payers to non-papers in the full sample effectively compares a set of compliers in program neighborhoods to a mix of never-takers in program neighborhoods plus compliers and never-takers in control neighborhoods. (We assume away the existence of always-takers in this population since payment absent the program is effectively zero.) This is a complicated comparison. The cleaner comparison is compliers to never-takers in program neighborhoods only, shown in Table 10.

an instrumental variables approach to estimate effects of tax payment separately from informational effects of the program.

Random assignment to the tax program is an obvious instrument for endogenous tax payment. However, there is an exclusion restriction violation because the tax campaign likely has other direct effects, such as the signal sent about the capacity of the government when tax collectors arrive at one's doorstep for the first time. To solve this problem, I instrument for two endogenous regressors — tax collector visits only  $(I^{Visited only})$  and collector visits plus payment  $(I^{Visited and paid})$  — to identify the independent causal effect of paying taxes on participation separate from other direct effects of the program that are captured by  $I^{Visited only}$ . The estimating equation (with two endogenous regressors) is therefore:

$$y_{ijk} = \beta_1 I_{ijk}^{Visited \ only} + \beta_2 I_{ijk}^{Visited \ and \ paid} + \alpha_k + \mathbf{X}_{ijk} \mathbf{\Gamma} + \mathbf{X}_{jk} \mathbf{\Phi} + \varepsilon_{ijk}$$
(2)

The challenge is finding a set of instruments capable of *independently* identifying these two endogenous parameters. That is, I need at least one instrument to appear in the firststage equation for each endogenous regressor. A common pitfall of estimations with multiple endogenous variables is using multiple instruments that identify the same endogenous regressor, leaving the other regressor unidentified (even if the joint first-stage F-statistic is large). Fortunately, Angrist and Pischke (2000) demonstrate how to construct F-statistics for each endogenous variable independently, thereby offering a means of verifying that both regressors are separately identified by the instruments (Angrist and Pischke, 2008, pp. 217-218). In the tables that follow, these F-statistics will be reported as AP F-stat.

As prespecified, I construct jackknife IV (JIVE) or leave-one-out instruments for  $I^{Visited only}$ and  $I^{Visited and paid}$ , respectively. These JIVE instruments exploit the random assignment of tax collectors to neighborhoods. They are constructed as follows.

- Predict a fixed effect,  $\hat{\lambda}_{i,-j}$ , for the  $i^{th}$  collector in neighborhood j by running a regression of the endogenous variable on tax collector dummies, the familiar set of covariates, and stratum fixed effects in all assigned neighborhoods other than j.
- Take a linear combination of the collector-specific fixed effects to construct a neighborhood-

level instrument, i.e. Payment propensity =  $\sum_{i=1}^{3} \delta_i * \hat{\lambda}_{i,-j}$ , where and  $\delta_i$  weights the collector-specific fixed effects.

This same procedure is used to construct JIVE instruments for both endogenous variables, called *Visit propensity* for  $I^{Visited only}$  and *Payment propensity* for  $I^{Visited and paid}$ . In essence the JIVE estimator can be thought of as a continuous version of  $I^{Program}$ : it equals 0 for control neighborhoods and then varies between 0 and 1 for neighborhoods assigned to the tax program. It is increasing in the average predicted ability of the tax collectors randomly assigned to the neighborhood. For simplicity, collectors are weighted evenly.

Table 11 shows the first stage. Both instruments predict  $I^{Visited only}$ , and Payment propensity instrument strongly predicts  $I^{Visited and paid}$ . Although the endogenous regressors will be jointly identified by the full set of instruments in two-stage least squares, the fact that only

	8						
	Visite	d only	Visited and pai				
	(1)	(2)	(3)	(4)			
Payment propensity	$0.767^{***}$	$0.709^{***}$	$0.267^{***}$	$0.287^{***}$			
	(0.076)	(0.073)	(0.041)	(0.040)			
Visit propensity	$0.361^{***}$	$0.443^{***}$	-0.116*	-0.155**			
	(0.127)	(0.119)	(0.064)	(0.061)			
Covariates	Yes	Yes	Yes	Yes			
Stratum FE	Yes	Yes	Yes	Yes			
Enum FE	No	Yes	No	Yes			
$R^2$	0.214	0.239	0.083	0.099			
F-stat	147.861	144.754	34.337	41.278			

Table 11: IV - First stage

Standard errors clustered by polygon. \*p < 0.1,\*\* p < 0.05,\*\*\* p < 0.01

Notes: *Visited only* is an indicator that the household reports being visited by tax collectors in 2016. *Visited and paid* is an indicator for individuals' who paid the property tax in 2016 according to the administrative data.

Payment propensity predicts  $I^{Visited and paid}$  is reassuring that there are indeed valid instruments in both of the first stage equations. The *F*-statistic reported here is the standard joint test of the three exogenous instruments; the individual Angrist-Pischke (AP) *F*-statistics for 2SLS with multiple endogenous variables are reported in Table 12 showing the second-stage results. These AP *F*-statistics range from 9.1 to 11.8 in the first stage equation for  $I^{Paid}$ , which is above or just under the Stock and Yogo benchmark of 10 (Stock and Yogo, 2002). Including enumerator fixed effects, which I do in every robustness table in the Online Appendix, strengthens the first stage substantially, with the corresponding *F*-statistics increasing to between 16 and 18. As such, specifications with and without enumerator fixed effects are shown in Table 12.

Following this IV strategy, we can test whether individuals who pay taxes because of the program have are more likely to engage in costly participation, as implied by payment-based mechanisms. There is no evidence to support this possibility: although standard errors are large, the estimated coefficient on  $I^{Visited and paid}$  is never statistically distinguishable from zero. On the other hand, the coefficient on  $I^{Visited}$  is consistently positive and marginally significant in a number of these second-stage regressions. Consistent with the correlations in Table 10, the IV results suggest that the main effect of the tax campaign on participation is driven by visits from tax collectors not by tax payment specifically.

In summary, there is little evidence to support a payment-based mechanism linking taxation and participation in this context. A mechanism that does not operate through tax payment specifically — such as a signaling or collective action mechanism — is more likely to be driving the reduced-form effects on participation documented in the previous sections.

	Townhall attendance		Suggestion card submission		Townhall or suggestion		Townhall and suggestion		Costly participation index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Visited only	$0.171^{*}$	0.173**	0.038	0.055	0.136	0.158**	$0.106^{*}$	0.101**	$0.454^{*}$	0.486**
	(0.094)	(0.078)	(0.075)	(0.057)	(0.099)	(0.076)	(0.060)	(0.045)	(0.262)	(0.191)
Visited and paid	-0.368	-0.264	-0.007	-0.059	-0.209	-0.236	-0.265	-0.189	-0.889	-0.800
	(0.360)	(0.268)	(0.317)	(0.230)	(0.421)	(0.300)	(0.269)	(0.191)	(1.155)	(0.803)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Enum FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	1934	1934	2912	2912	2913	2913	2913	2913	2913	2913
Clusters	252	252	356	356	356	356	356	356	356	356
AP $F$ -stat (Visited only)	40.978	63.355	42.721	73.352	42.743	73.402	42.743	73.402	42.743	73.402
AP $F$ -stat (Visited and paid)	11.824	18.060	9.164	16.479	9.163	16.484	9.163	16.484	9.163	16.484
F-test $p$ (equivalence)	0.225	0.190	0.907	0.686	0.504	0.286	0.254	0.212	0.338	0.187

Table 12: IV - Second Stage: Distinguishing the effects of tax collector visits and tax payment

Standard errors clustered by polygon. \*<br/>  $p < 0.1, ^{\ast\ast} p < 0.05, ^{\ast\ast\ast} p < 0.01$ 

Notes: Townhall attendance is an indicator for attending a townhall meeting. Suggestion card submission is an indicator for submitting a suggestion card. Costly participation index is a standardized index of two indicator variables that equal 1 if a participant (1) turned up at a townhall meeting, or (2) submitted a suggestion card, respectively. Townhall or suggestion indicates that a participant attended either a townhall meeting or submitted a suggestion card. Townhall and suggestion indicates that a participant attended a townhall meeting and submitted a suggestion card.  $I^{Visited only}$  is an indicator for households that received tax collector visits but did not pay the property tax.  $I^{Visited and paid}$  is an indicator for households that received tax collector visits and paid the tax. AP F-stats report the endogenous regressor-specific Angrist-Pischke F-statistic for 2SLS with multiple endogenous regressors. F-test (equivalence) tests for equivalence of the coefficients on Visited only and Visited and paid (p-value reported).

#### 8.2 Taxation as a signal of government capacity

In contrast to payment-based mechanisms, the signaling family of mechanisms expects a novel tax collection campaign to stimulate participation by sending a public signal about the government that raises the expected benefits to participation. The intuition is that citizens who receive this signal will update that the government is more capable than previously thought and hence anticipate greater returns from efforts to influence future public policy. The signal will be observed by all those who receive visits from tax collectors. Thus, signaling mechanisms predict higher levels of participation among all those living in areas affected by tax collection—taxpayers as well as individuals who evade the tax. The next subsection outlines a simple decision-theoretical account of citizens' choice to participate because this class of mechanisms has received less attention in the literature but is well suited to weak-state settings, such as Congo or, for that matter, Europe in the early modern period.<sup>67</sup>

#### 8.2.1 Model

Consider a setup involving the government and a citizen. The government sets a policy  $g(\theta, \lambda)$ , where  $\theta \in \{H, L\}$  indicates whether the government is high or low capacity, and  $\lambda \in \{1, 0\}$  indicates the citizen's decision to participate in government monitoring. The citizen incurs a cost c to participate, and receives utility  $u(g(\theta, \lambda))$  from the government's policy.

Government capacity ( $\theta$ ) is meant very generally. It could be tax extraction capacity, public good provision capacity, spending capacity, or information capacity. The model is agnostic and intended to encompass all aspects of capacity.<sup>68</sup> The key assumption is that a signal of any type of state capacity triggers costly participation because citizens believe the government will be more likely to intervene in society in the future — for good or for ill — and they want to have a voice to steer public policy to be as favorable as possible.

More specifically, the government can provide public goods, which the citizen hopes to maximize, and extract taxes, which the citizen hopes to minimize. The government sets the most favorable policy from the perspective of the citizen (high public goods, low taxes) when it 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, there is no value to the citizen from having a voice in policymaking. Call this policy  $g^0$ . When the government is high capacity and the citizen does not participate, however, the policy could be worse for the citizen than that chosen when  $\theta = 0$  because the government might use its new capacity to collect more taxes without providing more public goods. Call this least-preferred (by the citizen) policy  $g^-$ . To summarize:

$$u(g^+) \ge u(g^0) \ge u(g^-)$$
 (3)

<sup>&</sup>lt;sup>67</sup>Although I did not include this particular model in my pre-analysis plan, I did prespecify testing a mechanism by which individuals update about the capacity of the government as a result of the tax campaign.

<sup>&</sup>lt;sup>68</sup>I explore specific mechanisms in the signaling family when testing the model in Section 8.

In the absence of the tax campaign, the citizen believes that the government is high capacity with probability  $p \sim F(\cdot)$  and low capacity with probability 1 - p.

If the citizen participates, his expected utility is

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

If he doesn't participate, his expected utility is

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

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

$$p^* = \frac{c}{u(g^+) - u(g^-) - 2u(g^0)} \tag{6}$$

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:

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

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

Now assume that before citizens decide to participate or not, the government launches a tax campaign, which sends a signal about its capacity ( $\theta$ ). Citizens know 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  $\alpha \geq \beta$ , we have by Bayes' Theorem that the probability (q) that the government is high capacity conditional on having administered a tax campaign is given by:

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

Let us now assume that  $F(\cdot)$  is a uniform distribution, i.e. that  $p \sim U(0, 1)$ . Letting  $\alpha = 0.8$ and  $\beta = 0.4$ , we can then simulate the posterior distribution q, as shown in Figure 5. An arbitrary threshold  $(p^*)$  is shown in red at a value of 0.7. Individuals with values of p that fall to the right of this line 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, such that they choose to participate only after receiving the signal sent by the tax program.

According to this model, more citizens choose to participate after the tax program because the information it sends about government capacity raises the expected benefits of participa-



#### Simulated distributions of beliefs about government capacity

#### Figure 5: Simulated distributions of prior and posterior beliefs about government capacity.

tion. This model is compelling in a weak-state setting, such as Congo. That the government successfully implemented a large-scale tax collection campaign demonstrates an ability to achieve its goals and a new level of engagement in society. Receiving this signal, citizens anticipate that the government will continue to be more active and capable in the future, and they therefore seek a voice in shaping its future policies and programs.

The model is less applicable in settings of high state capacity or where citizens are habituated to tax collection. In such settings, increased enforcement of a tax may well trigger citizen resistance, but the signal this sends may have a more ambiguous effect on participation. In a strong, predatory state, for example, citizens might respond to such a signal by participating less and generally trying to become invisible to the state (Scott, 2009). Thus, as a scope condition, the predictions of this model should be taken as conditional on low state capacity.

That said, low-tax, low-capacity equilibria are most relevant to the state-building theories animating this paper. Tilly (1985) argues that European monarchies achieved modern state capacity precisely through the hard work of building a citizen tax base. This argument presupposes that state capacity was low before monarchs increased tax extraction.<sup>69</sup> Thus, although less applicable in high-state-capacity settings, a signaling model is well suited to the theories of state building and tax bargaining motivating this paper.

<sup>&</sup>lt;sup>69</sup>This argument — made by Tilly (1985); Levi (1989), and others — has been challenged by Boucoyannis (2015), who presents evidence that in England, state capacity came prior to widespread tax extraction.

#### 8.2.2 Evidence

To test the signaling model in the data, I estimate p and q using respondents' self-reported beliefs about government capacity. The model predicts that individuals in the treatment group will be more likely to view the government as high capacity at endline compared to the control group, i.e. that E(q) > E(p). I test if the program shifted individuals' beliefs about the following dimensions of state capacity:

- 1. Capacity to extract taxes. After witnessing the tax program, citizens might update about the government's capacity to extract taxes from them in the future. I test this possibility by examining survey data on individuals' beliefs about the probability of punishment of tax evaders. *Capacity to extract taxes* is an index increasing in perceived capacity, constructed from respondents' answers to two questions concerning the likelihood that the government will "pursue and enforce" sanctions if a member of the neighborhood refuses to pay the tax or pays a bribe in place of the tax.<sup>70</sup>
- 2. Capacity to provide public goods. Alternatively, citizens might update about the government's implementation capacity to realize its agenda. They might therefore assume that it can provide more public goods in Kananga and choose to participate to try to steer public good spending toward their neighborhood. I consider survey evidence on endline participants' views about service provision capacity to test this mechanism. *Capacity to provide public goods* is an index increasing in perceived capacity, constructed from respondents' answers to three questions concerning the ability of the government to repair Kananga's road system, provide electricity, and apprehend a criminal.
- 3. Spending capacity (size of budget). Similar to the previous mechanism, citizens might simply update about the size of the government's budget. After the campaign, individuals realize the government has a new pot of taxpayer money, and they want to have a voice in determining how it will be spent. This is close to the logic in Brollo et al. (2013).<sup>71</sup>

Estimating government revenues is difficult for anyone, much less individuals with little past contact with the formal state. To test if individuals update about the revenues of the government, I therefore examine (i) perceptions about how many people on one's street paid the property tax, and (ii) perceptions of corruptions in the tax ministry and government. Individuals who believe a larger number of their neighbors paid the property tax mechanically update about the total revenues of the government after the tax campaign — so long as they also believe that money collected by tax collectors will

<sup>&</sup>lt;sup>70</sup>The exact text for these questions and those that make up the subsequent indices can be found in the notes to variable-specific robustness tables in Online Appendix Section 3.

<sup>&</sup>lt;sup>71</sup>In their model, citizens observe public goods but not non-tax sources of government revenues; they are therefore unable to sanction politicians who extract rents from grant or national resource income. But when they pay taxes, taxpayers have more information about the public budget and thus governmental waste and corruption. This reduced uncertainty about public revenues enables them to monitor politicians more effectively. In the terms of the previous section's model, new information about public revenues and corruption raises the perceived participation divided d, which means, all else equal, that more individuals turn out.

reach the state coffers and ultimately be spent on public services with positive probability. *Percent of neighbors who paid* is an index increasing in the estimated percentage of neighbors who paid the property tax in 2016. I consider this variable alongside a measure of perceived leakage of revenues between tax collection and public service delivery. Specifically, *Honesty of government* is an index increasing in respondents' perceptions of how much money collected in property taxes will actually be spent on public goods.<sup>72</sup>

4. Information capacity (legibility of population). Finally, citizens may update about their legibility to the state: whether the government could find them if it wanted to (Scott, 1998). Such legibility is a precondition for tax extraction; it also determines targeting of public services or the distribution of patronage goods by the state. I test this mechanism by asking citizens how much information they believe the state knows about their household. *Legibility to the state* is an index increasing in the perceived information capacity of the government, constructed from four survey questions asking individuals if they think the government knows their address, occupation, monthly income, and whether their neighbors paid taxes.

Table 13 summarizes estimations of Equation 1 using each of these variables as the outcome. There is no evidence to support the notion that the program causes individuals to update about the capacity of the government to extract taxes (Column 1) or to provide public goods (Column 2) in the future. However, we find positive treatment effects in the anticipated direction concerning the perceived level of public revenues and the legibility of the population to the state.

Specifically, Column 3 reveals that the tax program substantially raises perceptions about the number of other individuals in Kananga who are paying property taxes. Moreover, treated individuals also view the government as more honest (Column 4) — that is, they believe a larger proportion of the money collected in taxes will actually be spent on public goods, rather than being pocketed or wasted by tax collectors and other bureaucrats.<sup>73</sup> Putting these two findings together, individuals in treated neighborhoods realize the government has a new source of funds due to the tax campaign. In other words, treated individuals update about the spending capacity of the provincial government — and seek a voice in determining how the new tax revenues are spent. The revenues could be spent on public goods or more particularistic benefits, such as patronage. In the model, g encompasses any public and private goods that the government might distribute. The fact that citizens do not appear to update about the capacity of the government to provide public goods (Table 13, Column 2) could imply that individuals are seeking patronage by participating. However, handout seeking is an unlikely cause of suggestion card submission because cards were anonymous.

Analysis of respondents' submitted suggestion cards reinforces this interpretation. In the

<sup>&</sup>lt;sup>72</sup>The index is made of two questions. The first question asked respondents to imagine \$1,000 received by the government from the property tax campaign, and to then divide this amount into two sums: the amount that will be spent on public goods, and the amount that will be stolen or wasted. Another question asked the respondent to do a similar exercise, this time dividing \$1,000 taken by tax collectors into the amount that will actually make it to the state account versus the amount that will go into collectors' pockets.

<sup>&</sup>lt;sup>73</sup>The average response among individuals in treated neighborhoods is that 54.3% of funds collected as taxes will make it to the government and be spent on public goods.

		Spending capacity			
	Capacity	Capacity	Percent of	Honesty	Information
	to extract	to provide	neighbors	of	capacity
	taxes	public goods	who paid	government	(legibility)
	(1)	(2)	(3)	(4)	(5)
Program	0.051	-0.011	$0.341^{***}$	$0.174^{***}$	$0.147^{***}$
	(0.049)	(0.054)	(0.052)	(0.049)	(0.044)
Covariates	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes
$R^2$	0.044	0.038	0.103	0.061	0.085
Observations	2883	2484	1954	2777	2910
Clusters	356	326	349	356	356
Control Mean	-0.017	0.009	-0.179	-0.106	-0.080

 Table 13: Perceived capacity of the provincial government

Standard errors clustered by polygon. \*p < 0.1, \*\*p < 0.05, \*\*\*p < 0.01.

Notes: Capacity to extract taxes is an index increasing in perceived capacity to punish individuals who evade the property tax. Capacity to provide public goods is an index increasing in perceived capacity to provide public goods. Percent of neighbors who paid is an index increasing in the estimated percentage of individuals on one's street who paid the property tax in 2016. Honesty of government is an index increasing in the amount of money collected in property taxes will actually be spent on public goods. Information capacity (legibility) is an index increasing in the perceived information capacity of the government.

optional commentary section, a number of cards referred explicitly or implicitly to the money collected during the tax campaign. "The provincial government should do more," wrote one individual, "and inform us how *this money* will be spent on public infrastructure and not wasted on other things." Another individual wrote the following: "I ask that the government show the population what it achieves with *this money*" (emphasis added). These individuals appear to have in mind the new revenues produced by the tax campaign; their demands concern the uses of these new revenues.

Turning to the last column of Table 13, the program increases citizens' perceptions of what the provincial government knows about them — their legibility to the state — by nearly 0.15 standard deviations on average. This effect is driven by stronger beliefs that the government knows the respondent's address and whether their neighbors paid the property tax, likely a reflection of the census embedded in the tax campaign. Citizens realize that they are now in a government database and thus findable if the government tries to reach them in the future. Knowing their legibility to the state, individuals might choose to engage in costly participation to minimize future tax obligations and to maximize their ability to benefit from public goods or patronage that might be targeted among individuals in the government's database. In the terms of the model, if they are illegible to the state, then participation is useless. However, if they are legible to the state and they participate, they can try to realize the positive public policy outcome  $g^+$  and avoid the negative outcome  $g^-$ . This latter outcome might well result if they do not participate but are legible because the government knows how to extract taxes from them in the future. In sum, the evidence is consistent with a mechanism by which the tax program sends a signal of government capacity that raises citizens' expectations about the benefits of participating in the townhall meeting and of submitting a suggestion card. Specifically, citizens in treated neighborhoods update about the total revenues of the government, which leads them to try to have a voice in determining how the government spends these new revenues. That we see participants demanding the government's aid in repairing roads and countering erosion in their neighborhoods — citing specific streets and canals in particularly bad condition (page 25) — in their townhall and suggestion card comments reinforces this interpretation. Additionally, treated citizens realize they are more legible to the state, which may lead them to try to influence their household's future tax bill and share of public or patronage good spending.

This interpretation is intuitive in a weak state setting. That the government administered a citywide door-to-door property tax campaign indicates a new level of capacity and engagement with the population. Observing this, individuals in taxed neighborhoods are more willing to exert costly effort to lobby for increased public service delivery in their neighborhoods and lower levels of future taxation.

### 8.3 Does the signal promote coordination?

In addition to causing individuals to update their beliefs about the government's capacity, it is possible that the public signal sent by the tax campaign enabled coordination among citizens and thus helped solve the collective action problem associated with participation (Olson, 2009). Individuals might well have anticipated being more effective in lobbying for public spending in their specific neighborhood if multiple residents attend townhall meetings together, or submit suggestion cards making similar demands. If the signal sent by the tax campaign lowered the costs of coordination by stimulating common grievances and communication about the government, taxation, and public services in Kananga, the reduced-form effects observed previously might be more a function of coordination effects than pure updating effects.<sup>74</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 costly participation in either the townhall or suggestion card submission is relatively low (0.073), and there are not obvious patterns in the spatial distribution of participators across or within neighborhoods that would suggest a collective action mechanism, as demonstrated in Online Appendix Figures

<sup>&</sup>lt;sup>74</sup>A collective-action mechanism could theoretically operate independently from updating about state capacity. Arias et al. (2017b) argue that public signals can stimulate coordination even in the absence of updating, or even if the partial effect of updating would go in the opposite direction from the partial effect of coordination. That is, theoretically, updating and coordination can be complementary, substitutes, or completely independent mechanisms. See also (Arias et al., 2017a). In the framework discussed in the previous section, collective action is likely to complement the updating effect of the signal sent by a tax campaign. Citizens will not only anticipate greater individual-level benefits to engagement with the state after observing the state's new efforts to collect taxes; they will also be better able to coordinate with their neighbors about how best to lobby the government.

18 and  $19.^{75}\,$  However, it is still possible that the tax campaign enabled coordination and collective action in more subtle ways.

In the Online Appendix, I consider four tests of a collective-action mechanism (Section 4). First, I examine if treated townhall participants are more likely to show up to the meetings with other members of the neighborhood compared to control participants. There is suggestive evidence to this effect, indicating that treated individuals might have coordinated more with others in their neighborhood when deciding whether to participate and traveled to the townhall meetings together (see Online Appendix Table 40).

Second, I use the GPS coordinates of participants' households to measure if individuals who participate in the townhall or the suggestion card exercise are more clustered geographically within treatment neighborhoods relative to control neighborhoods, as one would expect if lower coordination costs were the key mechanism. To do this, I calculate the average euclidean distance among the households of all participators in each neighborhood. Although the point estimates on a tax program indicator are negative, they are not statistically different from zero (see Online Appendix Table 41). Individuals who attend a townhall or submit a suggestion card are not more likely to live near other participators in treatment neighborhoods relative to control neighborhoods.

Third, I use pre-treatment data to test if the program had larger treatment effects in neighborhoods with higher collective action potential. Specifically, I examine heterogeneous effects based on neighborhoods' baseline level of political activity, ethnic homogeneity, population density, and city chief (government broker) activity, as each of these variables has been linked to collective action potential in past research. There are no positive heterogeneous effects in this analysis (Online Appendix Table 42). In fact, if anything, the treatment effect appears larger in areas with *lower* collective-action potential (measured by the aforementioned proxies). Figure 6 demonstrates this point graphically. It shows stratum-level participation in control neighborhoods within a stratum.<sup>76</sup> The graph highlights that in strata where participation is already high, there is less evidence of a consistent treatment effect. The average effect noted in the main results section derives chiefly from parts of the city with low levels of participation absent the program.

These findings depart from the most natural interpretation of a coordination mechanism because one would expect a larger effect in areas that are more conducive to collective action under this hypothesis. Rather, this pattern of heterogeneous effects is more consistent with a signaling model in which the subgroup of individuals with high prior beliefs about government capacity are already participating more absent the program, and thus we would not hypothesize a large treatment effect on this subgroup. It is individuals with middling beliefs about state capacity who will update the most after receiving the signal sent by the tax campaign and thus shift across the threshold into participation. Put differently, for individuals with less prior exposure to the formal state, the informational signal sent by the tax collection program is stronger than it is for individuals who are habituated to

<sup>&</sup>lt;sup>75</sup>For instance, if participants' households were more densely clustered in treatment neighborhoods, this observation might suggest a collective-action mechanism. I explore this possibility formally below.

<sup>&</sup>lt;sup>76</sup>As noted, strata are defined based on location in the city and total population size.



## Figure 6: Costly participation within strata compared across treatment and control.

interacting with the provincial government directly. Individuals with less past exposure to the state should thus update more about the capacity of the government because of the tax program, making it more likely that they will be tipped past the participation threshold and choose to attend the townhall meeting or submit a suggestion card.<sup>77</sup>

Finally, I examine if the program stimulated the diffusion of rumors about the campaign, and whether neighborhoods with higher rates of rumor transmission exhibit larger treatment effects. There is some (marginally significant) evidence of higher circulation of rumors about the tax campaign in treatment neighborhoods (Online Appendix Table 43). But individuals in treatment neighborhoods with higher circulation of rumors appear no more likely to participate than individuals in treatment neighborhoods with fewer rumors. Although this regression is not identified, it is difficult to explain this pattern of correlations if communication about the campaign were driving the elevated rates of participation in treated neighborhoods.

In sum, although there is some suggestive evidence that the tax campaign could have stimulated coordination among citizens, it is unlikely that lowering the barriers to collective action is the principal mechanism driving the reduced-form treatment effect on costly participation

<sup>&</sup>lt;sup>77</sup>That said, an alternative interpretation of these results (e.g. Online Appendix Table 42) is that the tax campaign has an average effect on participation precisely because it catalyzes coordination in neighborhoods that have a deficit of collective action ex ante. The effect is more muted where collective action is already high because the additional boost to coordination is unnecessary: people are already participating. In this reading, then, the campaign fills a collective action deficit in certain areas.

given (i) the inconsistent evidence in the four tests considered, and (ii) the stronger evidence of the signaling mechanism explored in the previous section. Indeed, the most plausible mechanism is that the tax program sends a signal about the size of the government's budget and the legibility of the population to the state. Receiving this signal, citizens anticipate greater returns to engagement with the state and so are more likely to attend townhall meetings and to submit government suggestion cards.

## 9 Conclusion

This paper analyzed the first door-to-door property tax collection campaign in the city of Kananga, D.R. Congo, which increased tax compliance by 11 percentage points. It used the random assignment of the campaign to generate the first field-experimental support for the tax-participation hypothesis. Citizens in taxed neighborhoods were nearly 5 percentage points more likely to attend government townhall meetings or to submit a suggestion card evaluating the government. Participating individuals demanded more public goods and more accountability from the government, consistent with a process of tax bargaining. Available survey evidence suggests that this increase in engagement with the provincial government reflects a signaling mechanism by which citizens update about the capacity of the government and therefore expect greater returns to participation.



#### Heterogeneous treatment effects by time lag since tax program Outcome: standardized measures of costly participation

## Figure 7: Heterogeneous treatment effects by time lag between tax collection and endline survey enumeration.

Will the increase in engagement with the state persist over time? A partial answer comes

from exploiting random variation in the time lag between the tax program and the endline survey. Figure 7 shows the estimated treatment effect after taking quartiles of the data according to the lag between tax collection and endline survey enumeration.<sup>78</sup> The treatment effect decays over time.<sup>79</sup> Individuals who received tax collectors in their neighborhood about 3-6 months before they completed the endline survey are more than twice as likely to engage in costly engagement with the government than are individuals whose lag was 9-14 months.<sup>80</sup>

Should we conclude, then, that the increase in engagement with the government is a shortterm response to the enforcement of a new tax that fades to zero over time? This interpretation does not necessarily follow because the future of property taxation was uncertain at the time of endline survey enumeration and the measurement of costly participation. In particular, the government discontinued property tax collection in 2017 due to the conflict in Kasaï. Endline participants likely vary in their beliefs about the probability of future enforcement, but unfortunately I did not measure such beliefs.<sup>81</sup> Ultimately, the process of tax bargaining implies a back and forth between the citizens and the government on the road to a new political equilibrium. A true test of the persistence of the observed increase in citizen engagement with the state will be possible after the government's next move regarding taxation and public service delivery. Exploring the dynamics of this process is fertile ground for future research on the relationship between taxation, citizen engagement, and government responsiveness.

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<sup>&</sup>lt;sup>78</sup>Because we are partitioning treated units into quartiles, the standard errors in estimating each coefficient are large.

<sup>&</sup>lt;sup>79</sup>The decay is marginally statistically significant. Specifically, the change in magnitude of the treatment effect between periods 1 and 2, and between periods 1 and 3, is not significant. But the change in magnitude from period 1 to period 4 is marginally significant (t = 1.79).

<sup>&</sup>lt;sup>80</sup>By design, the time lag between collection and endline survey enumeration is random because the date a given neighborhood received tax collectors and the date of endline enumeration were both randomly assigned. That said, due to the violence that occurred in Kananga in the middle of endline survey enumeration, enumerators were unable to follow the random schedule perfectly. They were thus granted discretion in skipping certain unstable neighborhoods on their randomly generated lists. As such, to account for possible time trends (e.g. related to the violence) that could be correlated with the time lags examined in Figure 7, Online Appendix Figure 12 is a coefficient plot summarizing regressions that also control for the month of endline survey enumeration. The magnitude of the estimated coefficients decreases, especially for the longer lags; but the treatment effects remain strong for the first two lags, and the overall pattern is analogous to that in Figure 7.

<sup>&</sup>lt;sup>81</sup>In fact, the provincial government is planning another property tax campaign for 2018.

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## 10 Appendix

### 10.1 Balance checks



### Balance check on individual-level covariates All variables standardized

1 = covariates mentioned in pre-analysis plan. Estimates from OLS regressions (basic specification).





#### Balance check on polygon-level covariates All variables standardized

#### Figure 9: Balance checks on polygon-level covariates.



### Balance check on research design procedures All variables standardized



Variable	Normalized difference $(\delta)$
Individual-level covariates:	
Years of education	008
Monthly income	066
Estimated wealth index	099
Business owner	.083
Government worker	024
Property owner	004
Born in Kananga	064
Majority ethnicity	.014
Literate	068
Has renters in compound	041
Has electricity	038
Non-mudbrick walls	042
Owns vehicle	034
Polygon-level covariates:	
Quality of roads	.135
Quality of public lighting	191
Household wealth index	061
Past collector visits to neighborhood	.165
Past tax compliance in neighborhood	.028
Knows governor's name	.029
Participation in elections, parties, protests	024
Perception of government performance	.161
Perception of government corruption	.017
Trust in government	.148
Importance of govt in public good provision	127
Unemployment	.061
Access to electricity	053
Research design procedures:	
Attrition - all participants	.044
Attrition - repeat baseline participant	.059
Attrition - new endline participant	.018
Stopped survey midway through	007
Polygons dropped because of conflict	04
Repeat baseline participant	.073
Total baseline surveys per polygon	049
Total monitoring surveys per polygon	056
Total endline surveys per polygon	.025

Table 14: Normalized differences in covariates across treatment and control

Notes: Normalized differences in variables across treatment and control, following Imbens and Rubin (2015).

## 10.2 Additional details about townhall meetings and suggestion card submission

This section contains further detail about the townhall meetings hosted by the provincial government and about the suggestion cards.



# Figure 11: Provincial Assembly building (Capitole) in Kananga, the location of the townhall meetings. Photo credit: CAID 2016.

All townhall meetings took place in the provincial assembly building, as shown in Figure 11. The location of the meeting is shown on the map in Figure 12. When inviting endline participants (in all neighborhoods) to townhall meetings, enumerators read the following text to participants: "Also, I would like to invite you to a meeting held by the Provincial Government to increase transparency about property tax collection. At this meeting, members of the Provincial Government will discuss the results of the recent campaign. They will also answer questions from citizens. If you are interested in coming, I can give you an invitation. Are you interested?"

Enumerators confirmed the identity of participants who showed up at the Provincial Assembly building by comparing code-specific headshots on the tablet to the individual at the townhall meeting. In 24 cases (4.9% of participants), the spouse of the respondent showed up instead of the respondent. (In almost all of these cases, the husbands of female respondents came in the place of their wives.) Because the spouse's participation also indicates costly effort on the part of the household, these individuals are coded as turning out in the main analysis. However, the results are robust to coding these individuals as no-shows.

All participants were invited to receive a suggestion card. Enumerators read the following introductory script: "Now, I'd like to show you a suggestion card that you can send to the provincial government. The suggestion card asks for your opinions about the work of the provincial government. I can help you fill it out, and then it is your decision whether or not you want to deliver it in a locked drop box at our office in Biancky [downtown Kananga].



## Figure 12: Locations of provincial assembly building (townhall meeting location) and suggestion box in 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, a summary of the information on the card and the cards themselves will be anonymously sent to the government so they know about your opinions and suggestions." The suggestion box was placed on the research office gate (see Figure 14) rather than a government building to ensure the anonymity of participants who chose to drop off their suggestion cards. At the conclusion of the study, the suggestion cards were given to the governor, finance minister, and head of the tax ministry, along with a short report summarizing their contents and containing a typed list of all written-in suggestions.

The suggestion card, in French on one side and in Tshiluba on the other, contained a unique code, unknown to the government, with which the research team linked cards to household surveys.

The full text of the questions on the suggestion card is as follows: "First, please choose which of the following options you agree with most: (1) I am satisfied with the Provincial Government of Kasaï Central and don't want to change anything about how it works, (2) I want the Provincial Government of Kasaï 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 should provide more opportunities for public participation

in provincial government decisions. (2) The provincial government should provide better and easier access to information about provincial government programs and policies. (3) The Provincial Government of Kasaï Central should spend more money on public goods and development and less money on administration. (4) The provincial government should provide a central location where I can freely report problems with public services."

Carte de	suggestion Code :					
Cochez les réponses qui correspondent à votre choix A. Veuillez choisir le point de vue avec lequel vous êtes d'accord :						
□ 1 : Je suis satisfait avec le Gou Central et je ne veux pas change opérations Cr	uvernement Provincial du Kasaï er quelque chose à propos de ses itical					
2 : Je voudrais que le Gouverner fasse mieux son travail.	ment Provincial du Kasaï Central					
B. <u>Maintenant veuillez m'indiq</u> d'accord, d'accord, pas d'acco propositions suivantes :	uer si vous êtes complètement rd, pas du tout d'accord avec les					
Le Gouvernement Provincial doit faciliter la participation de ses citoyens à ses décisions.	Le Gouvernement Provincial doit dépenser plus d'argent sur les biens publics et moins d'argent sur l'administration.					
□ Complètement d'accord □ D'accord □ Pas d'accord □ Pas du tout d'accord	<ul> <li>Complètement d'accord</li> <li>D'accord</li> <li>Pas d'accord</li> <li>Pas du tout d'accord</li> </ul>					
Le Gouvernement Provincial doit fournir plus d'accès aux informations sur ses programmes	Le Gouvernement Provincial doit créer un endroit en ville où je peux signaler les problèmes en rapport avec les services publics.					
□ Complètement d'accord □ D'accord □ Pas d'accord □ Pas du tout d'accord	<ul> <li>Complètement d'accord</li> <li>D'accord</li> <li>Pas d'accord</li> <li>Pas du tout d'accord</li> </ul>					
Commentaire : Com	mented					

Figure 13: Annotated (in red) suggestion card in French. A Tshiluba version was printed on the other side. 'Critical' indicates the disapproving response to the first question on the suggestion card, and 'Commented' indicates where some respondents wrote additional suggestions to the government. Critical and commented suggestion cards are examined in Table 5.



Figure 14: Suggestion box in downtown Kananga.



Content of submitted suggestion cards

++ 'strongly agree', + 'agree', - 'disagree', -- 'strongly disagree'

Figure 15: Content of suggestion cards delivered downtown.



Figure 16: Written-in comments on suggestion cards delivered downtown.

Oruer	Tiogram	Control
1	Government	Government
2	Provincial	Provincial
3	Should	Province
4	Work	Development
5	Population	Population
6	Province	Should
7	Better	Work
8	Roads	Roads
9	Good	People
10	Kasai	Water
11	Central	Improve
12	People	Good
13	Water	Policies
14	State	Electricity
15	Public	Goods
16	Erosion	Leaders
17	Electricity	Social
18	Country	Citizens
19	Needs	Love
20	Agents	Economic
20	Agents	Economic

## Table 15: Frequent words in suggestion card comments Order Program Control

Notes: The 20 most common substantive words on written-in comments of submitted suggestion cards. Words common to both lists shown in bold.