Building State and Citizen: How Tax Collection in Congo Engenders Citizen Engagement with the State

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Abstract

I examine a field experiment randomizing door-to-door tax collection across 431 neighborhoods of a Congolese city. I test the hypothesis that citizens will demand more inclusive governance when they are taxed. As predicted, the campaign increases political participation by 5 percentage points (28%): citizens in taxed neighborhoods are more likely to attend townhall meetings hosted by the government or to submit evaluations of 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' beliefs about government capacity supports this mechanism.

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1 Introduction

There is growing consensus that state capacity — and especially tax capacity — is critical for development (Besley and Persson, 2009; Acemoglu and Robinson, 2013). A growing empirical literature explores why developing countries collect so much less tax than developed countries, 15% of GDP versus 40% of GDP (Besley and Persson, 2014), and what sensible tax policy entails given the different frictions and constraints in these settings (Pomeranz, 2015; Khan et al., 2015). However, the political economy implications of taxation in developing countries have received less attention, despite well-known theories about how taxation ushers in more inclusive and accountable governance. According to seminal accounts, when European rulers in the early modern period began systematically taxing their subjects, the people resisted, demanding public goods and representation in return for tax compliance (Schumpeter, 1918; Tilly, 1985; Bates and Lien, 1985; North and Weingast, 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 central to the birth of inclusive politics. This paper examines a key proposition of this theory: that tax collection increases citizen demand for political participation.

This 'tax-participation hypothesis' is difficult to test in the real world because most governments do not randomly tax their citizens. On the contrary, governments may strategically target certain sectors to maximize revenues while minimizing distortions (Ramsey, 1927), but individuals in these sectors may be better able to participate for other reasons (Bates and Lien, 1985). Moreover, the causal arrow could go the other way if states first expand political participation to justify raising taxes (Lizzeri and Persico, 2004). The positive relationship between tax receipts and political participation in observational data is thus difficult to interpret (Prichard, 2015). Theoretically, it is also not obvious that citizens would choose to engage *more* with a state seeking to tax them. Political action is individually costly but publicly beneficial; citizens might prefer to free ride, or to move where the state cannot tax them

(Tiebout, 1956; Oates, 1969; Scott, 2009).

I test the theorized link between taxation and participation by conducting a field experiment randomizing property tax collection across 431 neighborhoods — covering roughly 33,000 properties — of Kananga, D.R. Congo (DRC), in 2016. In collaboration with the Provincial Government of Kasaï Central, I randomly selected 253 neighborhoods to receive the initial phase of the first-ever property tax collection campaign in the city. In treated neighborhoods, tax collectors went door to door making in-person appeals for the roughly $2 property tax, which they collected on the spot, issuing printed receipts to payers. Control neighborhoods remained in the old declarative system: citizens were supposed to pay at the bank themselves, but in practice less than 1% did.

Before considering the effects of the campaign on political participation, I examine the 'first stage' — whether the campaign achieved the government's goal of raising tax compliance, and thus whether it constitutes a valid test of the "tax-participation hypothesis." Despite the informational frictions and state-capacity constraints that inhibit tax collection in developing countries (Gordon and Li, 2009), the campaign raised property tax compliance from 0.1% in control to 10.3% in treatment neighborhoods. Although low by developed-country standards, this 100-fold increase made property tax receipts roughly 4% of the provincial government's total revenue, on par with local governments in more prosperous African countries.[1] The provincial government evidently viewed the campaign as a success, choosing to continue field-based property tax collection after 2016.

Given that the property tax campaign caused a substantial increase in tax enforcement and in the presence of the formal state in Kananga, I use its random assignment to test the tax-participation hypothesis: namely, that tax collection will increase demand for citizen engagement in the provincial government. Measuring political engagement in nondemocracies is a challenge,

[1]Property taxes make up 14% of local government revenues in Ghana, 10% in the Gambia, 6% in Sierra Leone, and less than 1% in Liberia and Cameroon (Fjeldstad et al., 2017). Moreover, property tax receipts in second cities are typically considerably lower than those in capitals (Jibao and Prichard, 2015).

as survey-based measures are often unreliable.[2] I therefore worked with the government to embed measurement strategies in two real-world channels of citizen engagement with the state.[3] First, the government hosted a series of townhall meetings, in which officials and citizens had a dialog about taxation and public spending in Kananga. Second, citizens could submit anonymous evaluations of the provincial government to a suggestion box whose contents would be shared with the governor and other top officials. Attending a townhall or submitting an evaluation exhibits willingness to exert costly effort to have a voice in the provincial government.

Tax collection increased participation according to both measures: residents of treated neighborhoods were nearly 5 percentage points more likely to attend a townhall meeting or to submit an evaluation card. In percent terms, this is a substantial increase — up 28% from control neighborhoods. Consistent with historical accounts of taxation stimulating citizen-state bargaining, townhall participants publicly demanded better public infrastructure and a more responsive government in exchange for taxes.[4] "Erosion threatens our neighborhoods, and the government does nothing," asked one individual, "so why should we pay?" Submitted evaluation forms were also highly critical — over 90% disapproved of the government — with demands for greater transparency, inclusiveness, and public goods spending.

I rule out several alternative explanations of the observed increase in participation. First, I show that exposure to and familiarity with the research team is balanced across treatment and control, making it unlikely that the results are explained by an artifact of survey enumeration. Second, I demonstrate that the treatment effect is not caused by a decline in participation in the control group, rather than an increase in participation the treatment group. Third, I present evidence that this result does not reflect a sense of unfairness

[2]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).

[3]This approach is similar to that of Olken (2007), Casey et al. (2012), and Paler (2013).

[4]Indeed, 71% of citizen comments at townhalls were complaints about taxation or demands for public goods provision, transparency, and less corruption.

stemming from awareness of the control group, which had not received tax collectors when outcomes were measured. Finally, I provide suggestive evidence that the tax campaign and the accompanying increase in participation with the provincial government may crowd out engagement with local city chiefs, consistent with the idea that the formal state and local forms of governance are substitutes.

I next turn to the mechanism linking tax collection and participation. It is often thought that tax-*payers* participate more because they expect a quid pro quo or exhibit an endowment effect (Fjeldstad and Semboja, 2001; Martin, 2014). However, in this case, the increase in participation is not driven by the 10% of treated individuals who paid. Rather, *everyone* in treatment neighborhoods — payers and those who evade payment — participates 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 its effect on tax payment. The campaign also does not appear to have lower coordination costs associated with participation, another possible mechanism.

Instead, I argue that the tax campaign sends a signal of state capacity that raises the expected benefits of participation. In a weak state setting, the fact that the government was able to administer a citywide tax campaign demonstrates to citizens that it is capable of performing basic state functions.[5] Thinking 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. I outline a simple decision-theoretical framework in Section 6.2 to make this argument concrete.

To test this theory, I show that the treatment effect is more pronounced in areas with less past exposure to the formal state — areas in which the signal sent by the campaign would have been larger. I also examine the effects of the tax campaign on individuals' stated beliefs about the government's extractive and productive capacity. Citizens in treated neighborhoods think that the

[5]This is similar to models emphasizing the signaling role of public projects (Coate and Morris, 1995).

government has more revenue due primarily to enhanced voluntary compliance. Treated citizens who choose to participate are less certain that these new revenues will be spent well in the absence of citizen monitoring. Thus, treated citizens appear more likely to attend townhalls and submit evaluations because after observing the tax campaign they believe the government will be capable of higher spending, and they seek to monitor and influence that spending toward their preferred policy through participation.[6]

This paper provides the first field-experimental test of the tax-participation hypothesis, which is central to historical accounts of the emergence of inclusive and accountable governance in early modern Europe (Schumpeter, 1918; Tilly, 1985; Bates and Lien, 1985; North and Weingast, 1989) as well as the political resource curse literature (Ross, 2001; Van der Ploeg, 2011; Gadenne, 2017). The closest past studies are lab experiments that simulate taxation and participation (Martin, 2014) and survey experiments that prime citizens about the share of taxes in government revenues (Paler, 2013; de la Cuesta et al., 2015). A large observational literature in political science also explores the relationship between taxation, participation, and representation (Ross, 2004; Moore, 2008; Prichard, 2015; Boucoyannis, 2015). In addition to providing evidence of the link between taxation and participation, the paper sketches and tests a simple decision-theoretical mechanism whereby tax collection signals state capacity and raises expected benefits of citizen participation. That changes in tax enforcement convey such information to citizens is relevant to accounts of historical states' efforts to raise revenues (Levi, 1989; Brewer, 1990; Ertman, 1997) and of present-day states investing in extractive capacity (Besley and Persson, 2011).

The paper thus contributes to the literature on state capacity, tax, and development,[7] which has focused less on estimating the political economy effects of increasing tax enforcement. Past work examines how governments can

[6]That awareness of new tax revenues would stimulate participation is consistent with evidence from Brazil showing that citizens are more successful in holding the government accountable to spending tax revenue compared to unobserved transfer revenue (Brollo et al., 2013; Gadenne, 2017).

[7]For an overview of this research agenda, see Besley and Persson (2013).

raise compliance through third-party reporting (Kopczuk and Slemrod, 2006; Kleven et al., 2011; Naritomi, 2015; Kleven, 2014; Pomeranz, 2015; Kleven et al., 2016; Slemrod et al., 2017; Carrillo et al., 2017), tax collector incentives (Khan et al., 2015), relaxing tax burdens (Waseem, 2013), providing information about enforcement or peer behavior (Blumenthal et al., 2001; Del Carpio, 2013; Pomeranz, 2015; Dwenger et al., 2016; Hallsworth et al., 2017), and reducing bureaucratic barriers to compliance (Kleven and Waseem, 2013; Best et al., 2015). In addition to the central finding that tax enforcement stimulates political engagement, this paper shows that low-capacity governments in settings of near-zero citizen compliance can raise property tax receipts through in-person tax appeals that reduce the transaction costs of compliance.

To my knowledge, the paper examines 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.

The paper reviews the setting (Section 2), experimental design (Section 3), and data, estimation, and balance (Section 4), before turning to the main results (Section 5) and mechanisms (Section 6).

2 Setting

The DRC is the fourth most populous country in Africa, and one of the five poorest countries in the world.[8] Median monthly household income in the study site is roughly $70, or PPP $111 (Lowes et al., 2017). The country is often termed a "kleptocracy," due to the corrupt rule of long-time president Mobutu Sese Seko (Young and Turner, 2013), or a "failed state," due to the decades of civil conflict (Stearns, 2012; Sánchez de la Sierra, 2014). It has low state capacity across all dimensions,[9] but especially in terms of extractive (tax) capacity. In tax revenue as a percentage of GDP, the DRC ranks 188

[8] See, e.g., `http://wdi.worldbank.org/table/1.2`.

[9] I refer to both 'extractive capacity,' the ability to raise revenues, and 'productive capacity,' the ability to enforce contracts and provide public goods Besley and Persson (2011).

out of 200 countries for the period 2000 to 2017.[10]

Kananga, a city of roughly 1 million, is the seat of the Provincial Government of Kasaï Central, which similarly struggles to build rudimentary state capacity. With nearly 6 million people in the province, total provincial tax receipts from 2010-2015 were around $2 million per year. These receipts chiefly came from trade and rental taxes levied on a handful of firms in downtown Kananga, such as mining and mobile-phone companies. Although there are many taxes on the books, few are enforced among private citizens in Kananga. Before the 2016 property tax campaign, only 40% of individuals knew the name of the provincial tax ministry, and 5.6% of individuals in the sample knew of the property tax (see Table 2). The most common taxes that residents of Kananga paid were market fees and a vehicle tax for owners of cars and motorcycles. But less than 10% of individuals reported paying any taxes in 2015.[11] The lack of a broad tax base is a challenge to governments across the developing world (Gordon and Li, 2009).

Property taxes are thought to be efficient and progressive, and urbanization in Africa is fueling rapid growth in real estate values (Fjeldstad et al., 2017). There is thus a strong case for property taxation in urban Africa (Moore and Wilson, 2017).[12] Because valuations can be difficult for low-capacity governments, many African governments have simplified property valuations to size-based assessments or simply fixed-amounts levied on properties under a certain threshold (Zebong et al., 2017). The Provincial Government of Kasai Central has followed suit. Roughly 90% of property owners in Kananga face a fixed annual property tax rate of 2,000 Congolese Francs (CF), about $2.[13] While this might seem low, $2 is the median household's daily income. Larger

[10]Data available: `https://data.worldbank.org/indicator/gc.tax.totl.gd.zs`.

[11]This low figure is partially offset by contributions in informal taxes (Olken and Singhal, 2011), the most notable of which is *salongo*, an activity organized by local notables (avenue chiefs) in which citizens sweep the streets and clean up after storms. About 30% of respondents reported that *salongo* occurs at least once per month in their neighborhood, though only 16% of households reported regularly contributing.

[12]In studying the political economy effects of taxation, the property tax is interesting because it is highly visible to households, unlike indirect or inflation taxes.

[13]Some properties are exempted, including those owned by state employees, churches, the disabled, and the elderly.

houses in the city center, about 9% of the property owners in Kananga, face a 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 face a rate above 50,000 CF. Prior to the 2016 tax campaign, property owners were supposed to visit the tax ministry themselves to declare the value of their property and pay the tax. But except for a handful of firms, compliance remained near zero.[14]

Why did the provincial government begin enforcing the property tax in 2016? The government's own explanation is that an unanticipated national policy severely undermined provincial tax receipts, leading the government to experiment with enforcing the property tax. Specifically, the 2015 *découpage* (administrative splitting) of the 11 old provinces into 26 new provinces led to the reduction of the revenue base of the Kananga-based provincial government due to the loss of the diamond-rich region around Tshikapa.[15] This external shock makes an overarching reverse causality story — that the government first increased participation to justify expanding taxation (Lizzeri and Persico, 2004) — unlikely in this setting.

The government, though on paper a democracy, is authoritarian, and citizens have few formal avenues of participation in politics. Elections were canceled in 2016 and again in 2017. Nonetheless, individuals in Kananga voice grievances to their political leaders in two main ways. First, they hold local meetings about public-good failures and other political demands and then nominate a representative to bring the case before a provincial deputy. Second, individuals, or groups of individuals, author formal letters of complaint to the provincial government. The measures of participation used in this study approximate these forms of political engagement.

In sum, Kananga is a good setting in which to test the tax-participation hy-

[14] There are fewer than 300 records of property tax payments from 2015, 86% of which were made by firms.

[15] Although this decentralization policy 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). The implementation of the *découpage* in 2015 is widely thought to be a tactic of incumbent president Joseph Kabila to sew bureaucratic confusion and justify postponing the 2016 elections, which he did.

pothesis because it shares several key features with the states in early modern Europe examined in historical accounts of the emergence of inclusive institutions (Schumpeter, 1918; Tilly, 1985). These theories discuss low-capacity autocratic states, struggling to cope with fiscal crises by building a tax bureaucracy. In early 2016, the Provincial Government of Kasaï Central was at precisely this moment in its development: it sought to systematize property tax collection to counteract a sudden drop in revenues brought on by an external shock.

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.[16] Neighborhood borders are typically natural boundaries like roads, ravines, or other features easily identifiable from the ground. Among the 431 polygons, 253 were selected randomly to receive the tax campaign in its first phase. The 178 control polygons would receive the program in 2017, after outcome measurement.[17]

For the randomization, I constructed 33 strata defined by (i) satellite grid cells of Kananga, and (ii) the estimated population of the neighborhood.[18] Stratifying in this way addresses a potential inference problem that this experiment was designed to solve: the targeting of certain households or neighborhoods when states extend the tax net. For instance, the state might differen-

[16]The government did not have maps of localité borders, hence the need to define these on a satellite map. See Appendix Figure 2 and Online Appendix Figure 1 for examples.

[17]The government ultimately deviated from this plan because violence broke out in the province in early 2017, leading it to suspend all tax collection that year. It recommenced property tax collection in 2018. For information about the conflict, see Online Appendix Section 1.3.

[18]I used 11 satellite map grid cells that fully partition the city. Population in each neighborhood was estimated by counting houses visible from satellite images.

tially tax wealthier areas, whose inhabitants may be more likely to participate for other reasons independent of taxation. Because wealth and other characteristics in the tax collector's selection function cluster spatially in downtown urban areas, stratifying on geographic location and population helps improve balance along these key dimensions that may be particularly vulnerable to selection.

Before the tax campaign, households in *all* neighborhoods received informational fliers in French or Tshiluba, the most widely spoken African language in Kananga, announcing that (i) the government would be collecting property taxes in the months ahead, and (ii) money collected would be used to "promote the economic development of the province." Distributing the fliers in treatment and control neighborhoods, which would also eventually receive tax collectors, ensures that estimates of the campaign's impact reflect the increase in tax collection rather than simply information about the campaign.

The 54 government tax collectors working on the property tax campaign were randomly assigned to new teams of three every twelve work days.[19] Teams were then randomly assigned to treated neighborhoods. The order of neighborhoods was also random. Collector teams completed two tasks in each neighborhood.

1. **Census**: First, collectors completed a brief census to identify all liable property owners in the neighborhood. Collectors also assigned a unique code to each house in the neighborhood, written in chalk on the wall or door. These codes were subsequently used on tax receipts to identify compliant households in the administrative data. The census was

[19]The collectors were 78% male with an average age of 33 years. All of them were from Kananga and fluent in Tshiluba, the local language. Roughly half were full-time employees of the tax ministry, and half were part-time interns. In keeping with standard policy at the tax ministry, a small performance-based bonus was paid out to those working on the campaign: 18% of the total 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. This randomized double bonus is examined in a separate project on the effects of collector characteristics on tax compliance. The average weekly bonus was about $4, though more productive collectors earned more than $10.

verified by members of the research team with GPS devices to ensure the collectors respected neighborhood boundaries. Collectors received a printed copy of the census before tax collection.

2. **Tax collection**: Upon completion of the census, 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.[20] 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 the bundle of these two components of the tax campaign (Table 1). Control neighborhoods experienced neither component; as in the past, citizens in these neighborhoods were expected to pay at the tax ministry themselves. When examining mechanisms (Section 6), I will provide suggestive evidence about the effects of tax payment per se, separate from effects of tax collector visits. However, the main analysis considers the reduced-form impact of the campaign as a whole, which is the theory- and policy-relevant quantity of interest given that states invariably conduct censuses before they can systematize tax collection (Brewer, 1990; Scott, 2009).

4 Data, estimation, and balance

4.1 Data

Data come from four sources: (1) administrative data on property tax payment, (2) a baseline survey conducted before the campaign, (3) a midline 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

[20] See Online Appendix Figure 2 for receipt examples.

Table 1: Activities of collectors and enumerators

Activity	Treated	Control	Timing	N	J
Tax collectors					
Census	Yes	No	Apr-Dec 2016	20,902	253
Tax collection	Yes	No	Apr-Dec 2016	20,902*	253
Enumerators					
Baseline survey	Yes	Yes	Mar-Apr 2016	2,384	431
Midline survey	Yes	Yes	Apr-Dec 2016	33,019	431
Endline survey	Yes	Yes	Jan-May 2017	2,913	356

Notes: *20,902 is the number of households to which collectors were supposed to visit after the census, an upper bound on the number that received actual tax visits. N = sample size, J = number of clusters.

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 bank data. I link official tax records to survey data using the unique household tax identification numbers assigned during the census.

Baseline survey enumeration occurred in all neighborhoods in 2016, just before the property tax campaign. 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} compound, 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.

During the campaign, enumerators conducted midline 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 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 longer survey asking more details about interactions with tax collectors and a handful of questions concerning views of the government.

Finally, enumerators administered an endline survey from January to May in 2017, after the conclusion of the tax campaign. Enumerators first conducted a short screening survey of roughly 20 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 higher-quality houses selected with slightly higher probability to focus on the population most affected by the campaign.[21] Only household heads or their spouses were eligible to complete the interview. 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 evaluation cards during the endline interview, as discussed in Section 4.2.[22]

Because of insecurity in Kananga in early 2017, enumerators were unable to conduct the endline survey in the commune of Nganza, representing about 15% of the city population. All 71 neighborhoods from this commune were dropped before respondents could be sampled and invited to participate. Because I stratified the randomization on a spatial unit approximating communes, the number of neighborhoods ineligible for endline survey enumeration is balanced

[21] Wealthier households, as measured by the quality of house walls and roof, were more likely to receive multiple visits from collectors and to pay the tax. But because these households are rare in peripheral areas of Kananga, fully random sampling would mean few of them would end up in the sample. A higher selection probability for these wealthier households implies that my estimates capture the impact of the campaign on those most affected by it. It also improves power for analysis of heterogeneity by wealth (Online Appendix Section 5). I also construct weights to make estimates fully representative of the population and report all results in the Online Appendix. Online Appendix Section 2.2 provides details on the construction of weights.

[22] 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. I collected these data for a companion paper on the determinants of compliance. This repeated baseline sample is not part of the endline sample examined in this paper, but I use it in Section 5.3.2 to examine changes in beliefs within individuals. Online Appendix Section 2.2 contains more details on this sample.

across treatment and control (Table 3).

For the endline survey, 453 of the 3,421 (13.21%) sampled households could not be surveyed. Common reasons included (1) being too busy, (2) begin on a trip, and (3) declining participation without a reason. These forms of attrition are balanced across treatment and control (Table 3).

Table 1 summarizes the activities of the collectors and the enumerators. In short, all research components of the study — baseline, midline, and endline surveys — were held constant across treatment and control. The sampling and enumeration procedures of these surveys were identical in all neighborhoods, as indicated by the balanced length of surveys across treatment and control (Table 3). What varied across treatment and control was assignment of the tax campaign with its two components: census and collection.

4.2 Outcome measurement

The paper examines two sets of outcomes. First, it examines the 'first stage': whether the 2016 property tax campaign increased visits from tax collectors and raised tax compliance. I consider two variables:

1. *Visited by tax collectors.* To measure if the campaign increased the probability of being visited by tax collectors, I use self-reported midline 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 campaign increased tax payment, I use administrative data linked to midline survey data by unique tax ID numbers.[23] In control neighborhoods, I used fuzzy name matching within neighborhoods to match administrative records with household surveys. This method is reliable because there are less than 30 records of non-campaign property-tax payments in 2016 made by individuals.

Second, testing the tax-participation hypothesis requires measures of cit-

[23] At times, household codes were erased (usually due to rain). In such cases, I validate self-reported tax payment in two ways. First, a valid printed receipt bearing the property owner's correct name 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.

izens' efforts to participate in politics and demand better governance. Such efforts are hard to observe, especially in a nondemocracy. Survey data may be subject to social desirability bias and other forms of measurement error (Mullainathan and Bertrand, 2001), especially when asking about sensitive issues in a country few political rights. Given these challenges, I worked with the provincial government to embed measurement strategies in two forms of political engagement that, like turning up to vote or to protest, come at a cost to individuals: attendance at townhall meetings, and submission of government evaluations.

Specifically, from January to April 2017, the provincial government held five townhall meetings, chaired by the finance minister and the director general of the tax ministry, to provide a venue for dialog with citizens. The government agreed to let our research distribute the official invitations (see Online Appendix Figure 11) to the meetings to all participants of the endline survey. The enumerators described the meetings as a chance to obtain information about taxation and public spending in Kananga and to ask questions of government officials. The invitations contained unique codes to verify participants' identities and to match turnout data to survey data. The meetings, held in the provincial assembly building, were formal (see Online Appendix Figure 12). After initial presentations by the government officials, participants made comments and asked questions; the officials responded in turn.[24]

Townhall meeting attendance indicates a willingness to exert costly effort to have a voice in the government. Citizens had to remember the date and time of the meeting and pay for their transport to the provincial assembly building, located up to 13 kilometers, and on average about 5 kilometers, from participants' households (see Appendix Figure 2). Motorcycle taxi drivers ask up to $2 — the median household daily income in the sample — for a oneway trip from the outskirts of Kananga to the city center. Nonetheless, 483 individuals (24.9% of endline respondents who received invitations) participated in a townhall meeting.[25]

[24]See Online Appendix Section 2.3 for other details about the townhall meetings.
[25]The militia-related insecurity in Kananga increased in early April, and the government

The second measure of participation is the submission of anonymous evaluations of the performance, transparency, and inclusiveness of the provincial government.[26]. Enumerators distributed evaluation forms to all endline participants at the conclusion of the survey. Participants then chose whether or not to submit their form in a locked drop box in downtown Kananga. The evaluation form 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.[27] Finally, citizens could add additional suggestions in a text box at the bottom. Enumerators informed respondents that the governor and other top officials would receive the evaluation forms plus a summary of their contents. Filling out the form and paying the transport to find the drop box in downtown Kananga is a form of costly participation with the provincial government.[28] From January to May 2017, 396 individuals (13.6% of total endline participants) submitted their evaluation forms.

Turnout at townhall meetings and submission of evaluations are the outcomes of interest. For completeness, I consider five dependent variables:

1. *Townhall attendance*: an indicator for individuals who attended a townhall meeting.
2. *Evaluation card submission*: an indicator for individuals who submitted an evaluation.
3. *Townhall or evaluation*: an indicator for individuals who either attended a townhall or submitted an evaluation.
4. *Townhall and evaluation*: an indicator for individuals who both attended a townhall and submitted an evaluation

discontinued these meetings, urging all citizens to stay in their homes. Thus, endline participants after April 1, 2017, never received invitations.

[26]This measure is similar in spirit to the comment forms in Olken (2007) and the postcard campaign in Paler (2013).

[27]See Online Appendix Figure 14) for an example evaluation followed by a translation of the exact questions. The evaluation card, in French and Tshiluba, contained a unique code, unknown to the government, which I use to link cards to surveys.

[28]For illiterate participants, enumerators read the forms and offered to help fill them out.

5. *Costly participation index*: a standardized index composed of indicator variables for townhall attendance and evaluation card submission.[29]

In addition, to validate that these measures capture a desire to have a voice in politics and demand better governance, I summarize the content of participants' comments, questions, and suggestions at the townhall meetings and on the evaluation cards. These outcomes are endogenous to participation, but nonetheless helpful in characterizing precisely what is picked up by these measures.

Finally, to further examine demands of the provincial government, I consider survey evidence. First, respondents answered questions about whose responsibility it is to provide public goods across six different sectors, such as education and infrastructure, choosing for each among the provincial government and other possible providers (the national government, NGOs, churches, etc). The variable *Responsibility of provincial government in public goods provision (sector-based)*, a standardized sum of sector-specific indicators for choosing the provincial government, is thus increasing in the amount of public goods provision demanded from the provincial government relative to other possible providers. Second, enumerators posed three sets of opposing viewpoints concerning the optimal level of public good provision by the provincial government relative to other providers. These hypothetical questions are combined into an index that is increasing in the extent to which participants envision a large role for the provincial government in public goods provision. Both indices are examined individually and in an aggregate index.[30]

4.3 Estimation

I primarily use OLS to estimate the following equation:

[29] I use standardized indices throughout the paper to facilitate interpretation of coefficient magnitude (in terms of standard deviations). I construct these indices by first standardizing each component variable, summing over all questions, and standardizing the new synthetic variable again. A similar indexing procedure is used whenever there are multiple measures of the same underlying variable.

[30] See the notes to Online Appendix Table **??** and **??** for the exact text of the underlying questions.

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

where i indexes individuals, j neighborhoods, and k the strata used during randomization. $I_{jk}^{Campaign}$ is an indicator for neighborhoods that receive the door-to-door tax campaign, meaning that β_1 estimates the average causal effect of the tax campaign on the outcome of interest (y_{ijk}), i.e. political participation. Standard errors are clustered at the neighborhood level (356 in total). In addition, α_k are strata fixed effects, and $\mathbf{X}_{ijk}$ and $\mathbf{X}_{jk}$ are individual- and neighborhood-level covariates. All regressions include gender, age, and age squared as covariates. Additional covariates are at times also included, as noted below.

4.4 Balance

To check the randomization, I estimate Equation 1 with thirteen individual-level variables from the endline survey, thirteen neighborhood-level variables from the baseline survey,[31] and six variables about survey enumeration itself (Table 3). In total, two individual-level covariates, household wealth index and business owner status, are imbalanced at the 10% level, and one neighborhood-level covariate, quality of public lighting, is imbalanced at the 10% level. Thus, as expected, 9.3% of variables are found to be significant at the 10% level. An omnibus test of joint orthogonality fails to reject the null for the individual variables ($F(17, 345) = 1.32, p = 0.20$) and the neighborhood-level variables ($F(13, 346) = 0.71, p = 0.75$). To be conservative, the three imbalanced covariates are included in $\mathbf{X}_{ijk}$ and $\mathbf{X}_{jk}$, respectively for the main specifications, with robustness checks showing invariance to the specific covariates used in the online appendix.

[31] Two exceptions are road quality and public lighting, which were measured at endline.

5 Results

5.1 Effects on tax compliance

This section first considers the 'first stage' — whether the campaign raised tax compliance through household visits by collectors — before turning to effects on participation. It is not obvious that a tax campaign in the DRC would succeed in achieving the government's goals of increasing collector visits and citizen compliance. Public employees are underpaid and often absent from their posts. Will collectors respond to incentives and go door to door making tax appeals in a city with few paved roads? Beyond motivating public employees, there is the challenge of convincing citizens to pay in a setting with little past formal taxation. If a collector suddenly appears at their doorstep, will citizens pay?

Despite the difficult circumstances, the government deemed the tax campaign a success. Table 4 summarizes OLS estimations of Equation 1 using as dependent variables indicators for (i) being visited by tax collectors, and (ii) paying property taxes. The campaign caused a 81.5 percentage-point increase in reported visits from tax collectors.[32] It also caused on average an 10.3 percentage-point increase in property tax payment. Given that citizens are unaccustomed to paying formal taxes in this setting, this is a large effect: a 100-fold rise relative to control areas.

For this and all subsequent estimations, I conduct a series of robustness checks in Online Appendix Section 5, including specifications with (1) only gender, age, and age squared as covariates, (2) all possible 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 effects by wealth.

Although a 10 percentage-point increase in tax compliance is substantial,

[32] In control neighborhoods, 5% of individuals report being visited by tax collectors. This likely reflects noncompliance among tax collectors, who at times crossed into to the wrong (control) neighborhoods. Such noncompliance is to be expected given that the borders between neighborhoods are not clearly delimited in Kananga and so must be checked using GPS. Such noncompliance would, if anything, bias average effects toward zero.

the majority of individuals still *evade* paying the tax — despite visits from collectors. 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, 2018) and in Online Appendix Section 1.2. Briefly, tax compliers are more likely to be male, educated, wealthy, and employed, implying that liquidity constraints shape compliance. Individuals who at baseline perceived a higher probability of punishment for evasion are more likely to pay — as are individuals who ex ante professed more positive attitudes toward the provincial government. There is thus support for classic cost-benefit models of tax compliance (Allingham and Sandmo, 1972) as well as evidence that "tax morale" plays a role (Luttmer and Singhal, 2014).

It is also worth noting that the program does not appear to have increased bribes (Online Appendix Section 1), which is unsurprising given that collusion between collectors and citizens requires repeated interactions but this was the first such tax campaign in Kananga. The impact of the campaign on political engagement therefore likely reflect the increases it caused in collector visits and tax compliance.

5.2 Effects on political participation

Given that the campaign increased collector visits and tax compliance, I use its random assignment to test the hypothesis that taxation raises demand for political participation. Estimations of Equation 1, summarized in Table 5, support this hypothesis. The campaign triggered a 4.4 percentage-point increase in townhall attendance (Column 1) and a 2.6 percentage-point increase in evaluation card submission (Column 2). To capture 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. These treatment effects amount to a 0.14 standard-deviation increase in participation (Column 5).

Of the 600 participants who participated in a townhall meeting or submitted their evaluation, 179 (30%) participated in both; 128 of the 179 (72%)

hailed from treated neighborhoods. The provincial assembly building, where the townhall meetings occurred, and the suggestion box were about 1 kilometer apart in downtown Kananga (see Figure 2). However, evaluation card submission did not increase on the days of townhall meetings. Most double participants appear to have made independent trips to attend the townhall and to submit their evaluations.

The results are robust to the checks described above, as well as estimating average effect size (AES) coefficients, following Clingingsmith et al. (2009).[33] Controlling for the distance between participants' houses and the location of the townhall meeting and the suggestion box does not affect the results (Online Appendix Table 14). As noted in Table 5, constructing p-values using randomization inference or Bonferroni adjustments does not meaningfully affect the statistical significance of the estimates.[34]

Did treated individuals attend townhall meetings to demand more inclusive and higher-performing governance, or did they simply have more factual questions about the rules and procedures of the 2016 property tax campaign? Examining the questions and comments participants raised during townhall meetings demonstrates that citizens took them seriously as an opportunity to make demands of the provincial government. The most frequent topics raised during the meetings included: (1) complaints about provincial taxes, (2) details about provincial taxes, (3) provincial government spending, (4) public goods in Kananga (or lack thereof), and (5) provincial corruption (Online Appendix Figure 16). As this distribution of topics attests, citizens used the townhall meetings to demand better governance if they are to pay taxes. "Why should the inhabitants of Lukonga [a commune of Kananga] pay taxes," one participant asked, "when the roads are in such disastrous condition?"[35] Such complaints, which were frequent and often quite impassioned, evoke a bargain-

[33]See Online Appendix Section 5 for robustness checks and Table 15 for AES estimation.

[34]The Bonferroni-adjusted p-value is calculated following Aker et al. (2011) to adjust for correlation between *Townhall meeting attendance* and *Evaluation card submission*. If m is the number of correlated outcome variables and ρ is the average correlation coefficient among the other outcome variables, the Bonferroni p-value with a correlation adjustment equals $1-(1-p)^g$, where $g = m^{(1-\rho)}$.

[35]Participant question from January 30 townhall meeting (author's translation).

ing process in which citizens demand more public goods in exchange for tax compliance (Bates and Lien, 1985).

A moral formal test of this alternative explanation is whether, conditional on attending a meeting, treated citizens are more likely to ask about details of provincial taxation (the second category above). A simple difference-in-means test suggests otherwise: treated individuals are no more likely to ask about such details. But they are roughly twice as likely to ask about provincial spending and public goods (p=0.050). This comparison is difficult to interpret, as speaking at the meetings is endogenous to participation. Nonetheless, it offers suggestive evidence that treated citizens are not simply showing up to ask clarifying questions about the tax campaign, but rather they are trying to demand better governance.

Further evidence comes from the evaluation forms, which contained no mention of the campaign or taxes in general but focused on the inclusiveness and transparency of the government. Analysis of submitted evaluations reveals a similar pattern. Citizens who dropped off their evaluations were highly critical: over 90% expressed overall disapproval of the provincial government. Similarly, respondents overwhelmingly demand more avenues of participation, access to information, and public goods spending (Online Appendix Figure 17). In addition, 39% of individuals wrote in additional suggestions at the bottom of the form. The three most frequent topics include: general demands for better provincial governance, demands for specific public goods projects, and demands for greater monitoring of the government and improved transparency.[36] "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." As in the townhall meetings, participants cited examples of poor quality roads, lack of electricity, or advancing erosion to demand more public goods provision from the

[36]Online Appendix Figure 18 shows the full distribution of topics, and Online Appendix Table 6 shows the most common substantive words used in citizens' suggestions.

government.

Moreover, if we examine only the submission of (i) evaluations that express disapproval of the government, and (ii) evaluations that contain written-in suggestions with a critical message or demand of the government,[37] treated individuals are still more likely to participate than control individuals (Online Appendix Table 16). This evidence reinforces that individuals are not simply submitting evaluations out of idle curiosity or in support of the government.

Finally, if the increases in participation reflect greater demand for good governance, we might expect individuals in treated neighborhoods to express stronger views about the obligation of the provincial government to provide public goods. Regression results examining survey-based indices described on p. 18 confirm this supposition (Table 6). Individuals in treated neighborhoods envision a larger role (by 0.112 standard deviations) for the provincial government in public goods provision relative to other possible providers. Examining the sub-indices, the standard errors are larger when considering the sector-baed questions rather than the hypothetical questions, but the magnitude of the coefficient is identical.[38] Importantly, this result does not appear to reflect changed beliefs about the *current* levels of public goods provision by the provincial government. An analogous set of survey questions asked respondents how much they believe the provincial government currently provides in the same sectors. No systematic differences appear in beliefs about current provision between people in treated and control neighborhoods (Online Appendix Table 24). Table 6 therefore indicates that the tax campaign expands the perceived obligation of the provincial government to provide public goods in Kananga in the future.

In sum, consistent with the tax-participation hypothesis, individuals in program neighborhoods appear more willing to exert costly effort to have a voice

[37]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 (4.4%) are coded as 0.

[38]The Online Appendix (Figure 24) shows results for the individual survey questions that make up these indices.

in the provincial government and to demand more accountability and more public goods provision.

5.3 Alternative explanations

Rather than higher demand for inclusive and accountable governance, do higher rates of participation in program neighborhoods reflect (1) experimenter demand effects, (2) a decline in participation in control rather than an increase in participation in treatment, and (3) a sense of unfairness due to awareness of untaxed control neighborhoods? This section explores these possibilities. It also examines how the campaign affects political engagement at the national and local level.

5.3.1 Experimenter demand effects

A natural concern is whether the observed increase in participation is an artifact of how survey data were collected in treated and control neighborhoods. Treated citizens could be participating more because they had more contact with enumerators or became more trusting of the research team.

To preclude such issues, all research procedures were held constant across treatment and control neighborhoods (Table 1). Baseline, midline, and endline survey sampling strategies, survey forms, and other enumeration procedures were identical, as evidenced by the balanced duration of surveys across treatment and control (Table 3). Enumerators were also assigned to neighborhoods in a random order, frequently alternating between control and treatment. Participants in treatment and control received the same information about the townhall meetings and the evaluation cards. Participation in townhall meetings and submission of evaluations always occurred *after* endline survey enumeration to minimize experimenter demand effects on respondents' decisions to participate.

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 know the employer of the

enumerator, (3) whether they participated in surveys in the past, (4) whether they did not provide a phone number to the enumerator, indicative of either genuinely not having one or mistrusting the researchers, and (5) whether they provided an incorrect or fake phone number to the enumerator, also indicative of mistrust. Table 19 shows the results of regressions using each of these variables as the outcome. No systematic differences appear across treatment and control.

5.3.2 Declining participation in control neighborhoods

A second alternative explanation is that the results are driven not by increasing participation in the treatment group, but by decreasing participation in the control group. Because the tax campaign was announced throughout the city, it is possible that individuals in control neighborhoods chose to participate less often because they were informed about the campaign but never received visits from collectors. 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 cannot measure changes in participation, I examine changes in beliefs about the provincial government within individuals over time. I consider three survey questions collected at baseline and endline: (1) the responsibility of the provincial government in public goods provision (the same sector-based question examined in Table 6), (2) trust in the provincial government and tax ministry, and (3) the perceived honesty of the provincial government. Table 20 summarizes fixed-effects regressions with an indicator (*Endline*) for measurement at endline and an interaction with neighborhood treatment status. If attitudes towards the government deteriorated in the control as a result of not receiving the tax campaign, we would expect the point estimates on the *Endline* variable to be negative. For none of these variables is the coefficient negative and different from zero. At least for this group of individuals tracked from baseline to endline, control individuals do not seem to update negatively

about the government or to expect less public goods provision from it.

The observed increase within individuals in the perceived responsibility of the provincial government to provide public goods in the treatment group corroborates (Column 1) reinforces the endline-only treatment effect observed in Table 6.

5.3.3 Awareness of the untaxed control

Treated individuals might have participated more because they were aware that control neighborhoods had not yet been taxed, and they thought this was unfair. The main result could thus be an experimental artifact, a function of having measured outcomes before the control group received the tax campaign.

This explanation appears implausible because households were informed that the campaign would eventually reach all neighborhoods. Still, treated individuals could have thought it unfair that their neighborhood was taxed earlier than others.

To explore this possibility, I examine whether treated households near control neighborhoods (i.e. near the border) are more likely to participate compared to households farther from control. If awareness of control drives participation in treatment, then we would expect individuals who live closer to control to be more likely to participate. However, plotting the participation rate in treatment as a function of minimum distance to control reveals no such relationship (Online Appendix Figure 28). Moreover, complaints about the fact that some neighborhoods had been taxed while others had not did not come up during townhall meetings or in the government evaluation forms. Awareness of the randomized rollout of the campaign appears to have been low. This is not surprising because of the fine unit of randomization: the neighborhood, averaging about 131 plots. If large regions of the city were taxed before others, citizens might have been more likely to notice the phased rollout.

5.3.4 Crowd out of national or local participation

The theory motivating this paper holds that citizens seek to hold accountable the government that taxes them and receives their money, i.e. the provincial government in this case. Does the increase in participation with the provincial government crowd out participation at other levels of government? Although not strictly speaking an alternative explanation, the possibility is important to consider if we are to understand the political economy effects of tax collection.

Although I lack measures of costly participation at other levels of government, I provide suggestive evidence from survey questions about engagement at the national and local level. Specifically, respondents indicated their current and future participation in national elections, parties, marches, protests, and rallies, which I combine into the index *Engagement with national politics*. A separate set of questions, combined in the index *Interest in national politics*, gauges interest in politics by asking about the frequency of news consumption and whether respondents choose to receive political information rather than other types of information. To measure local engagement, the survey asked about city chiefs, who are local notables and intermediaries with the government. These chiefs have two main responsibilities: (1) organizing weekly *salongo*, an informal tax in which citizens contribute labor toward local public goods, such as cleaning and repairing roads (Olken and Singhal, 2011); (2) mediating local disputes to avoid escalation to the courts. Survey questions, combined into the index *Engagement with local city chiefs*, asked respondents about their familiarity with and trust in these chiefs as well as the frequency of their interactions with them.

There are no detectable differences in national political engagement or interest in politics across treatment and control (Table 21). Column 3, however, indicates that the tax campaign appears to have crowded out participation at the local level. Treated individuals report fewer consultations with city chiefs as well as diminished views of their quality (Online Appendix Figure 27). The effect is more pronounced among relatively poorer individuals. Although only suggestive, this result has an intuitive interpretation in the context. City chiefs are more active in poorer, peripheral neighborhoods, where the formal state

is essentially absent. As the state expands its presence by collecting taxes, citizens may substitute engagement with the provincial government for engagement with local chiefs. This result supports the view that building the state can undermine local intermediaries (Cheema et al., 2006).

6 Mechanisms

This section examine three possible mechanisms behind the increase in participation caused by the tax campaign: (1) individual *payers* in treated neighborhoods participate at higher rates because they expect reciprocal benefits or derive greater expressive utility from voicing their grievances; (2) the tax campaign sends a signal of state capacity that raises the expected benefits of participation; (3) the tax campaign lowers the coordination costs of participation by stimulating common grievances and communication. Although the evidence in this section is more suggestive, it supports the second mechanism.

6.1 Tax payment as the cause of participation

Most accounts of the political economy effects of tax collection assume that *payers* are the ones participating more and trying to hold the government accountable. Tax payment could stimulate a sense of ownership over public revenues, leading taxpayers to expect public goods and better governance in return as a *quid pro quo* (Prichard, 2015). Such reciprocal expectations may be particularly strong in clientelistic settings in which political support is often exchanged for material goods (Stokes et al., 2013). Alternatively, tax payment could activate an endowment effect by taking away earned income and thus increasing the expressive utility associated with participation (Sandbu, 2006; Martin, 2014).

A simple test of this mechanism is to examine whether payers participated more than non-payers in treated neighborhoods.[39] Although payment is an

[39]Comparing payers to non-papers in the full sample would be harder to interpret because it would compare compliers in treatment to a mix of never-takers in treatment plus compliers and never-takers in control. (I assume away the existence of always-takers since payment

endogenous outcome of treatment, this correlation can still be informative, especially in the case that payment and participation are uncorrelated. Given that the likely unobserved sources of bias (income, education, views of the government, etc) in a regression of participation on payment would bias the coefficient on payment away from zero, estimating a zero correlation coefficient would be difficult to reconcile with a payment-based mechanism. Interestingly, payers appear no more likely to participate compared to non-payers in treatment neighborhoods (Table 7 Columns 1 and 4). This is true using different measures of costly participation as the outcome.

A second test is to compare participation among individuals in treated neighborhoods who did and did not receive visits from tax collectors. Although collectors were supposed to visit all households in a neighborhood, they sometimes skipped households. I suspect that collectors typically skipped households by accident, due to the fact that neighborhoods bear little resemblance to a grid, and it is easy to lose track of one's position in the neighborhood, even when guided by a GPS device. Moreover, collectors received a piece-rate wage for completing each house in the census, so they had little incentive to skip houses.[40] A mechanism operating through payment would expect no difference in participation between non-payers who were and were not visited. However, there is a positive association between participation and tax collector visits, independent of payment (Columns 2 and 5). Moreover, including indicators for payment and collector visits in the same regression finds that visits, not payment, is positively correlated with participation (Columns 3 and 6). Although only speculative, these correlations suggest that the mechanism operates through the experience of tax collector visits rather than tax payment per se.

A more rigorous test uses instrumental variables to estimate effects of tax payment separately from informational effects of the tax campaign conveyed

in control is effectively zero.) A less complicated comparison is compliers to never-takers in treatment neighborhoods only, shown in Table 7.

[40]That said, it is still possible that there is an endogenous component to tax collector visits, particularly because my measure is self-reported, and households could underreport visits. I thus instrument for visits as described in the next paragraph.

through collector visits. Random assignment to the tax campaign is an obvious instrument for endogenous tax payment. But a first-ever door-to-door tax campaign likely has other direct effects on participation since it conveys information about the government. I therefore need instrument for two endogenous regressors — collector visits only ($I^{Visited\ only}$) and collector visits plus payment ($I^{Visited\ and\ paid}$) — to identify the causal effect of paying taxes on participation separate from other informational effects of the program captured by $I^{Visited\ only}$:

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

A common pitfall of IV analysis with multiple endogenous variables is reliance on multiple instruments that identify the same endogenous regressor, leaving the other regressor unidentified (even if the joint first-stage F-statistic is large). Angrist and Pischke (2000) describe 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.

I thus 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. The intuition behind these instruments is that a collector's effort in a given neighborhood can be predicted by his or her observed effort in all other assigned neighborhoods. The instruments are constructed as follows.

- Predict a fixed effect, $\hat{\lambda}_{i,-j}$, for collector i in neighborhood j by estimating Equation 1 with tax collector dummies and the endogenous variable as the outcome in all assigned neighborhoods other than j.
- Take a linear combination of the collector-specific fixed effects to con-

struct a neighborhood-level instrument, i.e.

$$Payment\ propensity = \sum_{i=1}^{3} \delta_i * \hat{\lambda}_{i,-j}$$

where and δ_i weights the collector-specific fixed effects.[41]

This procedure is used to construct JIVE instruments for both endogenous variables: *Visit propensity* for $I^{Visited\ only}$, and *Payment propensity* for $I^{Visited\ and\ paid}$. The logic of these instruments is that collectors vary in their effort and effectiveness, and the two traits are not perfectly correlated. Some collectors make many visits (high effort) but collect few taxes (low effectiveness). Others make fewer visits but are more skilled at convincing citizens to pay taxes. Online Appendix Table 23 shows the variation in visits and payment within and across collectors. For about half of collectors, the correlation between visits and payment is not statistically different from zero (conditional on household covariates and stratum fixed effects).

The JIVE instruments can be thought of as a continuous predictor of treatment intensity along these two dimensions (effort and effectiveness): they equal 0 for control neighborhoods, and then varies between 0 and 1 for treated neighborhoods depending on the predicted effort (or effectiveness) of the assigned collectors. Intuitively, some neighborhoods are randomly assigned to a set of collectors who are likely to exert high effort; others are assigned to collectors likely to demonstrate high effectiveness. If these qualities are sufficiently uncorrelated, then there should be a first stage for $I^{Visited\ only}$ and for $I^{Visited\ and\ paid}$.

Table 8 reveals that 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 *Payment propensity* predicts $I^{Visited\ and\ paid}$ is reassuring that there are indeed valid instruments in both of the first stage

[41] For simplicity, collectors are weighted evenly, though due to sick days and other factors some worked for more days than others.

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 9 showing the second-stage results. Including enumerator fixed effects, as I do in robustness checks (Online Appendix), further strengthens the first stage.

Second-stage estimation generates little evidence that the increase in participation goes through tax payment. Although standard errors are large, the estimated coefficient on $I^{Visited\ and\ paid}$ is always negative and statistically indistinguishable from zero. On the other hand, the coefficient on $I^{Visited\ only}$ is consistently positive and often marginally significant. Its magnitude is about two or three times as large as that of the main effect of the campaign on participation. Ultimately, the large standard errors make this analysis suggestive at best. But the most natural interpretation of these results is that tax payment does not appear to have a strong causal effect on participation separate from the effect of being visited by tax collectors. The fact that we can never reject equivalence of the coefficients on $I^{Visited\ and\ paid}$ and $I^{Visited\ only}$ reinforces this interpretation.

In summary, the available evidence is consistent with a mechanism in which tax collector visits, not tax payment, drives the increase in participation in treatment neighborhoods.

6.2 Tax collection as a signal of state capacity

One mechanism consistent with the analysis in the previous section holds that a first-ever citizen tax campaign will stimulate participation by sending a signal of state capacity that raises the expected benefits to participation. The intuition is that citizens who observe the campaign will update that the government has greater means and is more capable than previously thought. They will therefore anticipate greater returns to engaging with members of the government, advocating for more public spending in their neighborhood, for example. This mechanism predicts higher participation among everyone in treatment neighborhoods — taxpayers as well as evaders.

6.2.1 Decision-theoretical framework

To make this mechanism clear, consider a setup with the government and one citizen who is uncertain about the capacity of the government. 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 (θ) is meant generally. It could be 'extraction capacity,' i.e. ability to collect taxes, or 'productive capacity,' i.e. ability to provide public goods and enforce contracts (Besley and Persson, 2011). A signal of either type of capacity triggers participation because citizens believe the government will be more likely to affect their future well being — through tax collection or public goods provision — and thus they have an incentive to try to influence public policy to be as favorable as possible.

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

$$u(g^+) \geq u(g^0) \geq u(g^-) \tag{3}$$

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

If the citizen participates, his expected utility is

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

If he doesn't participate, his expected utility is

$$EU_0 = p(u(g^-)) + (1-p)(u(g^0)) \tag{5}$$

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:

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

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:

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

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

Now assume that before citizens chooses whether to participate, the government launches a tax campaign, which sends a signal about its capacity (θ). Citizens know that a high-capacity government administers a tax campaign with probability α, and a low-capacity government administers a tax campaign with probability β. Then as long as $\alpha \geq \beta$, we have by Bayes' Theorem that the posterior probability (q) that the government is high capacity conditional on having administered a tax campaign is given by:

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

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 distribution of q, as shown in Figure 1. 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 campaign.

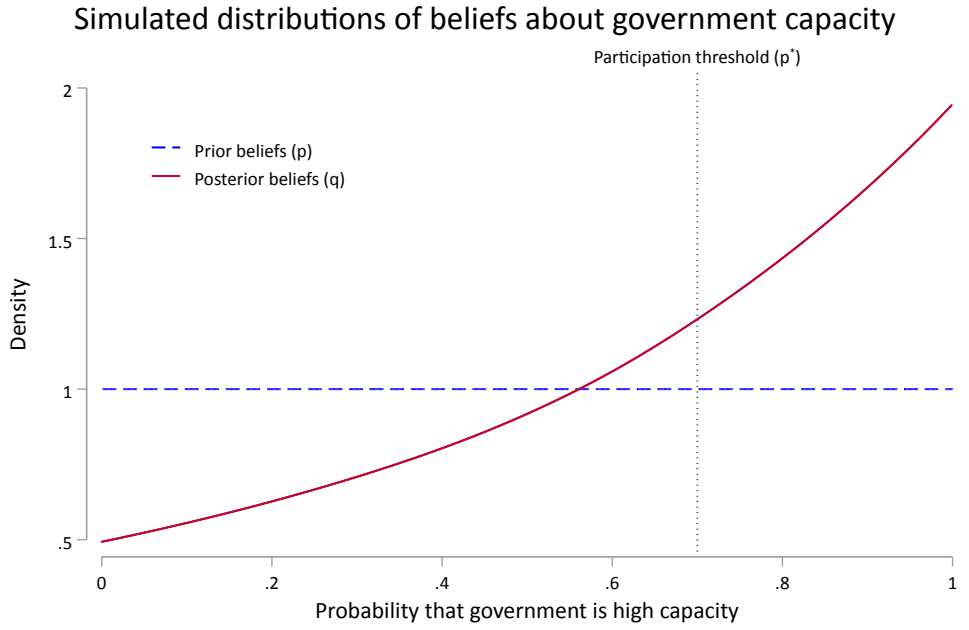

Figure 1: Simulated distributions of prior and posterior beliefs about government capacity.

Thus, more citizens choose to participate after the tax campaign because the information it conveys about the capacity of the state raises the expected benefits of participation. This framework is compelling in a weak-state setting, such as Congo, in which the government is effectively absent in the pre-period. That the provincial government successfully implemented a citywide, door-to-door tax collection campaign demonstrates an ability to achieve its goals and a new level of engagement in society. Receiving this signal of capacity, treated 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 logic this framework is intended to capture only holds in low-capacity

settings. In high capacity states in which citizens are habituated to taxation, an increase in tax enforcement likely has an ambiguous effect on participation. Some citizens might choose to protest new taxes, while others might invest in strategies for evasion (Hirschman, 1970; Alstadsæter et al., 2017).

That said, a low-tax, low-capacity equilibrium characterizes the settings in early modern Europe discussed in the theories animating this paper (Schumpeter, 1918; Tilly, 1985; Bates and Lien, 1985; North and Weingast, 1989). Indeed, Tilly (1985) argues that European monarchies only built modern bureaucracies with high state capacity in their quest to raise tax revenue.

6.2.2 Evidence

One observable implication of this mechanism is that the treatment effect should be larger in neighborhoods with less past exposure to the state. In neighborhoods in which the state has been effectively absent, receiving a visit from tax collectors using tablets and receipt printers should send a relatively stronger signal about the capacity of the state compared to neighborhoods in which people are habituated to the presence of the state. Thus, in neighborhoods unaccustomed to the state, more individuals should update their beliefs beyond the threshold and choose to participate.

To test this hypothesis, I measure past state exposure on the neighborhood level in two ways: (1) the number of past visits to the neighborhood from state agents reported at baseline; (2) the number of individuals who report ever having participated in a political protest at baseline. I use neighborhood-levels of these two variables and split the sample at the median, interacting an indicator for high state exposure with the campaign indicator. The treatment effect is indeed larger in neighborhoods with *less* past state exposure. An F-test rejects the equivalence between the effects in low- and high-exposure neighborhoods. These results are consistent with the idea that citizens unaccustomed to state presence receive a stronger signal of state capacity when they observe the tax campaign relative to citizens habituated to the state; this stronger signal leads them to update their beliefs in a more pronounced fashion and thus attend townhalls and submit evaluations in greater numbers.

As a second test of the mechanism, I show that citizens' self-reported beliefs about the capacity of the government shift in response to the tax campaign. I estimate p and q using respondents' self-reported beliefs about government capacity. Following Besley and Persson (2011), I examine both extractive capacity, the government's ability to raise tax revenue, and productive capacity, its ability to enforce contracts and provide public goods. As Besley (2018) notes, a government can raise revenues through coercion or by fostering voluntary compliance. I thus split extractive capacity into a coercive and a voluntary component.

1. **Extractive capacity - coercive compliance**. Coercion requires (i) information about taxpayers,[42] and (ii) a credible threat of punishment for evasion. I therefore use the following variables.[43]
    - *Information about citizens*: increasing in how much information the government is perceived to possess about citizens (i.e. household location, compliance status, occupation, income).
    - *Ability to punish evaders*: increasing in the perceived likelihood of punishment against households that refuse to pay the property tax or pay a bribe instead.
2. **Extractive capacity - voluntary compliance**. Voluntary compliance requires citizen approval of the tax ministry and confidence that its collectors will not simply pocket taxpayer money. As measures, I use the following variables.
    - *Performance of tax ministry*: increasing in citizens' trust in and approval of the provincial tax ministry.
    - *Taxes not pocketed*: increasing in the perceived amount of money collected in property taxes that will reach state coffers. This variable is similar to the previous one but phrased more explicitly to capture beliefs about collector corruption.
    - *Neighbors paid taxes*: increasing in the percentage of households on

[42] A large literature notes the importance of information, especially third party information, in achieving high levels of tax compliance (Pomeranz, 2015; Kleven et al., 2011).
[43] See Online Appendix Section 6 for details on all variables.

the same street whom respondents think paid the property tax in 2016.[44] This question offers an estimate of the revealed extractive capacity of the state.

3. **Productive capacity**. Once the state has resources, it needs capacity to deploy those resources productively rather than wasting or stealing them. Productive capacity is thus a function of the technology of public goods provision as well as the administrative ability to spend prudently and control high-level corruption. I examine the following variables.
    - *Ability to provide public goods*: increasing in the perceived ability (i.e. technology) of the provincial government to provide public goods (electricity, paved roads, security) efficiently and effectively, assuming it has the will to do so.
    - *Performance of government*: increasing in citizens' trust in and approval of the provincial government. This variable proxies citizens' beliefs of whether the government will deploy resources in a favorable manner.
    - *Taxes well spent*: increasing in the perceived amount of tax revenues that will be spent on public services or other 'good uses' and not wasted or stolen. This variable captures beliefs about high-level corruption, specifically how much taxpayer money will actually be spent well.

Table 11 summarizes estimations of Equation 1 using each of these variables as the outcome. Concerning the coercive component to extractive capacity (Panel I), the tax campaign increased citizens' perception about how much information the government possesses about citizens, especially the locations of their properties and their tax compliance status. But it did not substantially impact beliefs about the credibility of punishment for tax evaders (though I may be underpowered to detect a smaller effect). These inferences drawn by treated citizens are essentially correct. Thanks to the campaign, the gov-

[44] Because of the fine unit of randomization, streets in Kananga are typically partitioned into treatment and control at different points. It is unlikely, then, that citizens' would answer this question thinking only of households within one treatment group.

ernment does have a new database with detailed information about potential taxpayers that it can use to collect more tax in the future. Moreover, to the best of my knowledge, the extent to which the government pursued sanctions against noncompliant households was limited. It chose rather to use its modest coercive capacity to pursue arrears and fines among a handful of firms accused of evasion of various other taxes and fees.

Regarding the voluntary component of extractive capacity, treated citizens appear to have updated their beliefs considerably. They view the tax ministry more positively and have more confidence in its collectors, thinking a greater share of taxpayer money will be deposited in the state account (rather than staying in collectors' pockets). It might be surprising that citizens update *positively* about the government tax apparatus when it starts to tax them systematically. However, in the context of a 'failed state' in which government employees are seldom seen doing much of anything (aside from stopping vehicles to solicit bribes), these results make sense in response to a citywide door-to-door tax campaign involving the use of tablets and receipt printers.

Importantly, citizens in treated neighborhoods also believe that many more people in Kananga are paying taxes. Again, this might appear puzzling given that treated individuals don't think punishment of evasion is much more likely compared to control individuals. But in conjunction with the results on voluntary compliance, it seems that citizens (correctly) expect their neighbors pay taxes due to quasi-voluntary motives, such as the belief that a larger share of that money will actually reach state coffers.

It is possible that this pattern of belief changes simply reflects the fact that payers convince themselves that tax collectors are trustworthy after they have paid, an example of ex post motivated reasoning. Because there are more payers in treatment neighborhoods, such motivated reasoning could explain the average treatment effect. However, re-estimating the results in Table 11 with only non-payers returns similar results (Online Appendix Table 17). The magnitudes of the coefficients are somewhat smaller, suggesting that payers update particularly strongly on these dimensions. But non-payers also appear to have drawn the same inferences as a result of the tax campaign, making

a motivated reasoning interpretation of the average effects unlikely. It might seem counterintuitive that households that managed to evade the tax would update positively — not negatively — about the state's capacity. As noted, this likely reflects the very low level of state presence in Kananga before the campaign. Visits from collectors led citizens to update positively about the state regardless of whether the collectors succeeded in convincing them to pay the tax.

The results from Panel II of Table 11 suggest that treated citizens realize that the provincial government has more revenues as a result of its new investments in property tax collection. But do they also update about its productive capacity? Panel III shows little evidence that the tax campaign increases perceptions of the government's technology of public goods provision. It is unsurprising that observing tax collectors in the field would not lead citizens to think the government could now build a road more efficiently. Treated citizens also do not evaluate the government as a whole more positively, as they did the tax ministry. However, they do update about the share of tax revenues that will go to public goods spending or other good uses. This result mirrors the higher confidence among treated citizens in provincial tax collectors and the tax ministry. Updating about the share of revenues that are well spent implies that, conditional on the same public goods provision technology, treated citizens do perceive the government to have greater productive capacity.

In sum, citizens in treated neighborhoods updated their beliefs in two main ways: (i) they believe the government has more revenue due to enhanced voluntary compliance; and (ii) they believe the government will spend more of that revenue productively. These results are consistent with a mechanism by which the tax campaign sends a signal of government capacity that raises citizens' expectations about the benefits of participating in the townhall meeting and of submitting an evaluation. That awareness of new government revenues would stimulate participation is consistent with evidence from Brazil showing that citizens are more successful in holding the government accountable when revenues come from taxes rather than (unobserved) transfers (Brollo et al.,

2013; Gadenne, 2017).

To provide further suggestive evidence about the importance of beliefs about government capacity in citizens' decisions to participate, I examine the beliefs of participators and non-participators in treatment neighborhoods (Table 18). Although these comparisons are not identified, they can nonetheless help to interpret the average treatment effects on beliefs in Table 11. In particular, participators appear to have updated their beliefs in line with the average with three exceptions. First, they are more likely to believe the state will punish tax evasion compared to non-participators (even though the campaign does not have a detectable average effect on punishment beliefs). Second, participators in treatment are less convinced than non-participators that property tax collectors do not simply pocket the money they collect. Similarly, they are also less confident that the money raised during the campaign will go to public goods and not be wasted or lost to high-level corruption. However, they still perceive citizen compliance to be higher compared to individuals in control neighborhoods.

Thus, while participators in treatment also update about state's extractive capacity and total revenues, they are more concerned about the uses of tax money compared to non-participators. This pattern of correlations has an intuitive interpretation. Observing the tax campaign causes citizens to update about the size of the public budget, creating an incentive for participation. However, this incentive is offset among some individuals by confidence that the government will already spend the money in a productive manner, even in the absence of citizen monitoring. In other words, confident citizens believe the government will provide g^+ even if they don't participate. Only those who are less confident about the government's plans to spend the tax revenues choose to participate. Thus, treated citizens appear more likely to attend townhalls and submit evaluations because after observing the tax campaign they believe the government will be capable of higher spending that they seek to monitor and influence toward their preferred policy through participation.

Participants' demands for the government's aid in repairing roads and countering erosion in their neighborhoods — citing specific streets and canals in

particularly bad condition (p. 23) — in their townhall and evaluation comments reinforce this interpretation of citizens participating in order to monitor and influence public spending. Moreover, a number of citizens explicitly demanded transparency and accountability regarding the new revenues in their comments on submitted evaluations. "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 seek to shape how the government spends the revenues from the property tax campaign, consistent with the proposed mechanism.

A different interpretation is that citizens participate to try to access patronage goods rather than to try to influence public goods provision. The fact that treated citizens do not update about the technology of public goods provision could support this interpretation. Although g encompasses any public and private goods that the government distributes in the theoretical framework, several pieces of evidence make a patronage story less likely. First, although citizens might have expected handouts at townhall meetings (there were none), it is hard to imagine they would have expected patronage goods to result from submitting evaluations because the cards were anonymous and deposited in a drop box that did not involve interaction with government officials. Second, although citizens made neighborhood-specific demands during townhall meetings, they did not make overt requests for fully individualistic benefits. Third, when asked how the money would be spent, most people guessed roads (49%) or education (19%), while only 11% said waste/leakage (Online Appendix Figure 29). That said, I view this interpretation of greater *patronage capacity* as consistent with with the theoretical framework and cannot rule it out entirely.

6.3 Taxation lowers the cost of coordination

It is also 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 have anticipated being more effective in lobbying for public spending in their specific neighborhood if multiple residents attend townhall meetings together, or submit evaluations 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, a coordination mechanism could explain the increase in participation.[45]

At first glance, this explanation appears unlikely because there were no instances of individuals from the same neighborhood standing up together at townhall meetings to make an overtly joint demand. Also, the intracluster correlation of participating in either the townhall or evaluation submission is relatively low (0.073), and there are not obvious patterns in the spatial distribution of participators across or within neighborhoods that would suggest a collective action mechanism, as demonstrated in Online Appendix Figures 19 and 20.[46] However, it is still possible that the tax campaign enabled coordination in more subtle ways.

In the Online Appendix, I consider four tests of a collective-action mechanism (Section 3). 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 marginally significant 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 7).

Second, I use the GPS coordinates of participants' households to measure if individuals who participate in the townhall or the evaluation card exercise

[45] Coordination effects would likely be complements to updating about state capacity in this setting. Citizens will not only anticipate greater individual-level benefits to participation after observing the tax campaign; they will also be better able to coordinate with their neighbors to jointly lobby the government. That said, Arias et al. (2017) argue that, theoretically, updating and coordination can be complementary, substitutes, or independent mechanisms.

[46] 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.

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 the treatment indicator are negative, they are not statistically different from zero (see Online Appendix Table 8). Participators are not more clustered 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 activity, as each of these variables is thought to promote collective action in past research. There are no positive heterogeneous effects in this analysis (Online Appendix Table 9). If anything, the treatment effect is larger in areas with *lower* collective-action potential.

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 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 campaign, making it more likely that they will be tipped past the participation threshold and choose to attend the townhall meeting or submit an evaluation card.[47]

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

[47] That said, an alternative interpretation of these results (e.g. Online Appendix Table 9) 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.

neighborhoods (Online Appendix Table 10). But individuals in treatment neighborhoods with higher circulation of rumors appear no more likely to participate than individuals in treatment neighborhoods with fewer rumors.

In sum, although there is some suggestive evidence that the tax campaign could have stimulated coordination among citizens, it is unlikely that lowering the cost of collective action is the principal mechanism explaining the reduced-form increase in participation.

7 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 10 percentage points. It used the random assignment of the campaign to generate field-experimental support for the tax-participation hypothesis. Citizens in taxed neighborhoods were nearly 5 percentage points more likely to attend townhall meetings or to submit an evaluation of the government. Participating individuals demanded more public goods and more accountability from the government. I argue that the increase in participation reflects a mechanism through which tax collection sends a signal of state capacity that raises the expected benefits of participation. Survey evidence on individuals' beliefs is consistent with this mechanism.

Will the increase in participation persist over time? A partial answer comes from exploiting random variation in the time lag between the tax campaign and the opportunities to participate. Figure 3 shows the estimated treatment effect after taking quartiles of the data according to the lag between tax collection and the distribution of evaluation cards and invitations to townhall meetings. The treatment effect appears to decay over time, though the decay is only marginally significant.[48] Individuals whose neighborhood was taxed 3-6 months before they had a chance to participate were more than twice as

[48]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 ($t = 1.79$).

likely to do so compared to individuals whose lag was 9-14 months.[49]

The level of future participation thus may depend on the government's future tax enforcement strategies. Although the government canceled most tax collection in 2017 due to the conflict in Kasaï, it recommenced property tax collection in 2018. When collectors return for the second time to solicit property taxes, citizens may demand to see the public goods that resulted from the first campaign before paying. Those civic-minded individuals who paid in 2016 because of an implicit taxes-for-public goods social contract may be willing to exert more effort to lobby or protest the government if they do not observe new public goods.[50] 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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[49] By design, this time lag is random because the date a given neighborhood received tax collectors and the date of endline survey enumeration were both random.

[50] For instance, Besley (2018) discusses 'negative reciprocity' among civic-minded individuals who report *less* income and thus expose themselves to potentially higher sanctions risk in protest if the state fails to provide public goods in exchange for taxes. A similar logic might apply to participation, in which civic-minded individuals get utility from protesting a government that does not follow through on its end of the taxes-for-public goods bargain.

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8 Appendix

Table 2: Summary statistics

	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

Summary statistics from baseline survey (conducted in March-April 2016) and endline survey (conducted in January-May 2017).

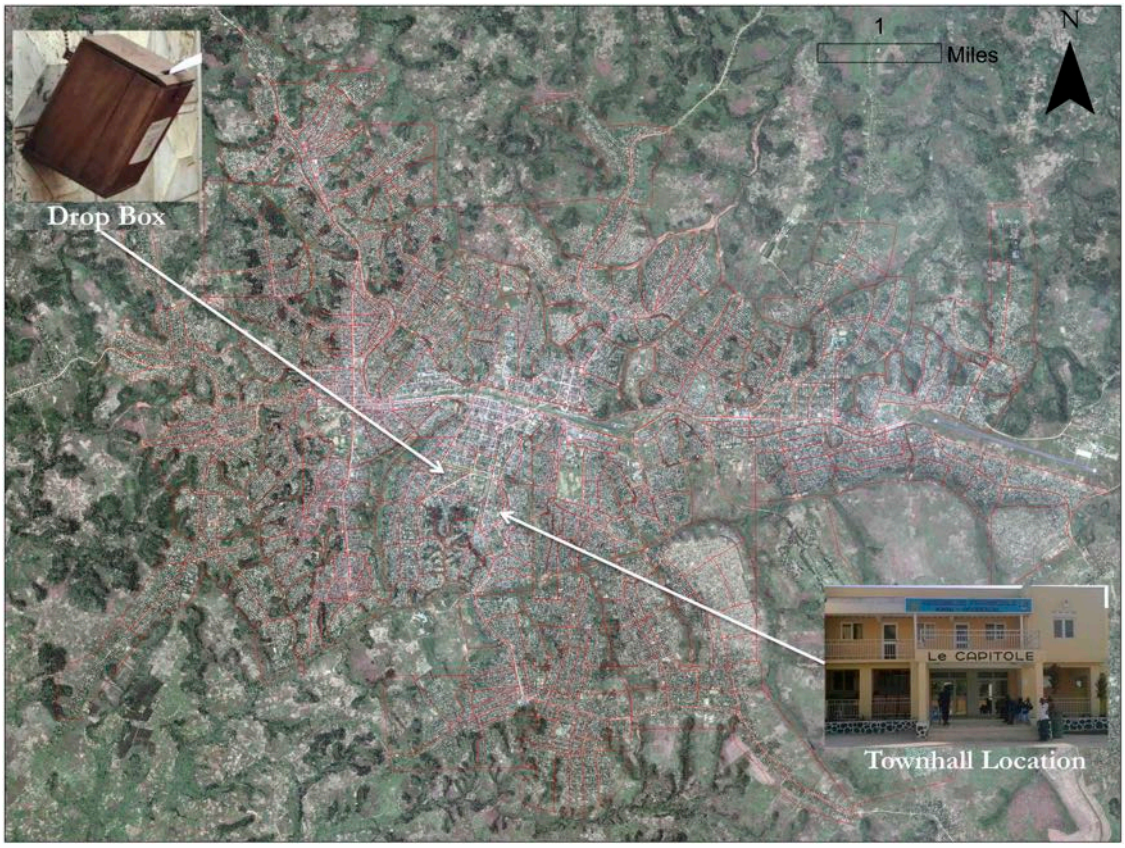

Figure 2: Locations of provincial assembly building (townhall meeting location) and suggestion box in downtown Kananga.

Table 3: Balance checks

Individual-level variables (endline)	**Coefficient**	**SE**	**t**
Years of education	0.054	0.048	1.121
Monthly income	-0.027	0.028	-0.961
Household wealth index	-0.120	0.066	-1.816
Business owner	0.034	0.018	1.867
Government worker	-0.008	0.015	-0.503
Multiple plot owner	0.011	0.016	0.704
Born in Kananga	0.004	0.019	0.209
Majority ethnicity	0.012	0.019	0.636
Literate	-0.017	0.014	-1.186
Has renters in compound	-0.025	0.038	-0.662
Has electricity	-0.015	0.045	-0.345
Non-mudbrick walls	-0.036	0.032	-1.123
Owns vehicle	0.001	0.015	0.069
Neighborhood-level variables (baseline)			
Quality of roads	0.104	0.077	1.358
Quality of public lighting	-0.048	0.026	-1.869
Unemployment	0.013	0.022	0.587
Household wealth index	-0.010	0.013	-0.818
Access to electricity	-0.007	0.011	-0.686
Past collector visits to neighborhood	0.013	0.024	0.542
Past tax compliance in neighborhood	0.012	0.075	0.162
Knows governor's name	0.016	0.037	0.427
Participation in elections, parties, protests	-0.002	0.015	-0.150
Perception of government performance	0.017	0.011	1.542
Perception of government corruption	0.002	0.017	0.112
Trust in government	0.033	0.023	1.397
Importance of prov. govt. for public goods	-0.174	0.142	-1.227
Survey enumeration characteristics			
Attrition in endline survey	0.010	0.012	0.881
Refused endline survey midway through	-0.002	0.005	-0.442
Polygons dropped because of conflict	-0.015	0.020	-0.735
Length of baseline survey	0.845	1.293	0.654
Length of midline survey	19.079	17.120	1.114
Length of endline survey	-13.836	22.620	-0.612

Results from OLS estimations of Equation 1 with each of the listed variables as the outcome.

Table 4: Effects of the campaign on collector visits and compliance

	Visited by tax collector	Paid property tax
	(1)	(2)
Campaign	0.815***	0.103***
	(0.013)	(0.007)
Stratum FE	Yes	Yes
R^2	0.640	0.054
Observations	27,443	27,443
Clusters	356	356
Control Mean	0.050	0.001

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. See p. 15 for details on these variables.

Table 5: Effects of the campaign on costly participation

	Townhall meeting attendance	Evaluation card submission	Townhall or evaluation	Townhall and evaluation	Costly participation index
	(1)	(2)	(3)	(4)	(5)
Campaign	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
Obs.	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
Rand. Inf. p	0.034	0.045	0.0050	0.0040	0.0012
Bonferroni p	0.042	0.052			

Townhall attendance is an indicator variable that equals 1 if a participant attended a townhall meeting. *Evaluation card submission* is an indicator variable that equals 1 if a participant submitted his or her evaluation. *Townhall or evaluation* indicates that a participant attended either a townhall meeting or submitted an evaluation card. *Townhall and evaluation* indicates that a participant attended a townhall meeting and submitted an evaluation card. *Costly participation index* is a standardized index of *Townhall attendance* and *Evaluation card submission*. See p. 18 for details on these variables.

Table 6: Effects of the campaign on the perceived responsibility of the provincial government to provide public goods

	Responsibility of the provincial government in public goods provision		
	(full index)	(sector-based)	(hypotheticals)
	(1)	(2)	(3)
Campaign	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

The outcome in Column 1 is an index increasing in the perception that the provincial government should be a primary provider of public goods in Kananga. The outcome in Column 2 is an index increasing in the degree to which participants think the provincial government should be the main provider of public goods across various sectors. The outcome in Column 3 is an index increasing in the degree to which participants think the provincial government should be the principal provider of public goods in hypothetical survey questions. See p. 18 for details on these variables.

Table 7: Correlations of participation with payment and presence during tax collector visits within treated neighborhoods

	Townhall or evaluation			Costly participation index		
	All	*Nonpayers*	*All*	*All*	*Nonpayers*	*All*
	(1)	(2)	(3)	(4)	(5)	(6)
Paid	0.015		0.006	-0.048		-0.072
	(0.034)		(0.034)	(0.082)		(0.082)
Visited		0.045*	0.050**		0.124**	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	1505	1703	1703	1505	1703
R^2	0.073	0.075	0.075	0.077	0.080	0.080
Control Mean	0.178	0.178	0.178	-0.057	-0.057	-0.057
Clusters	211	211	211	211	211	211

Townhall or evaluation and *Costly participation index*, the same outcomes as in Table 5, are increasing in costly participation (see p. 18). *Paid property tax* and *Visited by tax collectors* indicate payment and visits from collectors, respectively, as noted on p. 15. The sample is restricted to treated neighborhoods in all columns. In Columns 2 and 5, the sample is further restricted to non-payers in treated neighborhoods.

Table 8: IV - First stage

	Visited only		Visited and paid	
	(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

Visited only is an indicator for household visited by tax collectors that did not pay the property tax. *Visited and paid* is an indicator for households who were visited and paid the property tax. *Payment propensity* is a leave-one-out estimator that uses randomly assigned tax collectors' observed payment rates in other neighborhoods to predict the payment rate in a given neighborhood. *Visit propensity* is a leave-one-out estimator that uses randomly assigned tax collectors' observed visit rates in other neighborhoods to predict the visit rate in a given neighborhood. See p. 31 for details on the construction of these instruments.

Table 9: IV - Second Stage: Distinguishing the effects of tax collector visits and tax payment on participation

	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

The outcomes are identical to those in Table 5. As in Table 8, *Visited only* and *Visited and paid* indicate households that received visits from tax collectors but did not and did pay, respectively. 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).

Table 10: Heterogeneous effects of the campaign on participation by past presence of the state in the neighborhood

	Townhall or suggestion card		
	(1)	(2)	(3)
Campaign	0.049***	0.062***	0.082***
	(0.016)	(0.023)	(0.021)
Campaign X Past visits (high)		-0.030	
		(0.033)	
Past visits (high)		0.038	
		(0.027)	
Campaign X Past protest (high)			-0.073**
			(0.034)
Past protest (high)			0.035
			(0.022)
Covariates	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes
R^2	0.067	0.068	0.069
Observations	2913	2913	2913
Clusters	356	356	356
Control Mean	.18	.18	.18
F-test p-value		.01	.00049

The outcome is the same as Table 5. *Past visits (high)* indicates neighborhoods above the median level of past visits from government agents reported during baseline. *Past protest (high)* indicates neighborhoods above the median level of past citizen participation in protests reported during baseline. Interactions with the tax campaign indicator are also included.

Table 11: Effects of the campaign on extractive and productive capacity

Dependent variable	β	SE	R^2	N	μ_c
Panel I: Extractive Capacity - Coercive Compliance					
Information about citizens	0.152***	0.044	0.085	2910	-0.080
Ability to punish evaders	0.048	0.048	0.044	2883	-0.017
Panel II: Extractive Capacity - Voluntary Compliance					
Performance of tax ministry	0.122***	0.047	0.065	2791	-0.076
Taxes not pocketed	0.188***	0.044	0.043	2732	-0.119
Neighbors paid taxes	0.348***	0.052	0.102	1954	-0.179
Panel III: Productive Capacity					
Ability to provide public goods	-0.012	0.053	0.038	2484	0.009
Performance of government	0.045	0.049	0.042	2795	-0.030
Taxes well spent	0.108**	0.050	0.054	2766	-0.062

This table summarizes OLS estimations of Equation 1. β is the coefficient on the treatment indicator, followed by the cluster-robust standard error, r-squared, number of observations, and control group mean. There are 356 clusters. Each dependent variable, described briefly on p. 38 and in detail in Online Appendix Section 6, is standardized to facilitate interpretation of coefficient magnitude. The number of observations varies across regressions due to non-response for specific survey questions.

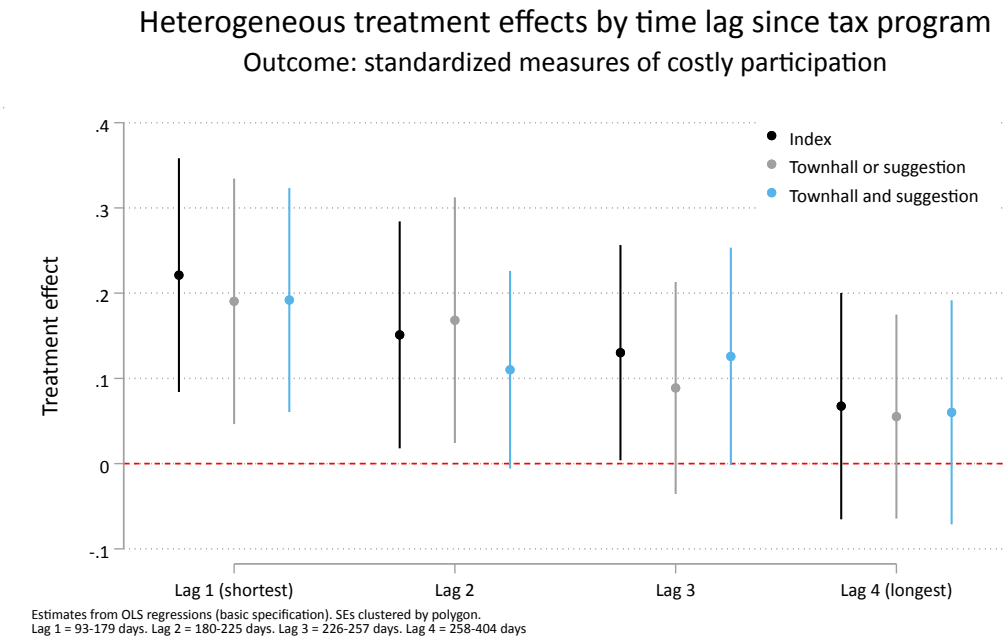

Figure 3: Heterogeneous effects of the campaign by time lag between tax collection and opportunities for participation.