

THE PARTICIPATION DIVIDEND OF TAXATION:  
HOW CITIZENS IN CONGO ENGAGE MORE WITH THE STATE  
WHEN IT TRIES TO TAX THEM

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## Abstract

This paper provides evidence from a fragile state that citizens demand more of a voice in the government when it tries to tax them. I examine a field experiment randomizing property tax collection across 431 neighborhoods of a large Congolese city. The tax campaign was the first time most citizens had been registered by the state or asked to pay formal taxes. It raised property tax compliance from 0.1% in control to 10.4% in treatment. It also increased political participation by nearly 5 percentage points (26%): citizens in taxed neighborhoods were more likely to attend townhall meetings hosted by the government or to submit evaluations of its performance. To participate in these ways, the average citizen incurred costs greater than their daily household income, and treated citizens spent 40% more than control. The results suggest that broadening the tax base has a ‘participation dividend,’ a key idea in historical accounts of the emergence of inclusive governance in early modern Europe and a common justification for donor support of tax programs in weak and fragile states. *JEL: H20, P48, D73.*

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# 1 Introduction

With nearly half of the world’s extreme poor, fragile states pose many of the greatest challenges in development (World Bank, 2011). The origins of state capacity have thus become a focus of recent models (Besley and Persson, 2009; Acemoglu and Robinson, 2017) and empirical work (Sanchez de la Sierra, 2019; Dittmar and Meisenzahl, 2019; Cantoni et al., 2019). The ability to tax, the “hallmark of the state” (Scott, 2017), is deemed especially important for development (Kaldor, 1963; Besley and Persson, 2013). Taxation enables the state to provide public goods, and it is thought to create a *participation dividend* by stimulating political engagement among citizens in states with a broken social compact. This paper tests if tax collection increases participation in fragile states by examining the randomized rollout of the first large-scale citizen tax campaign in Kananga, D.R. Congo.

The supposedly catalytic role of tax collection on political participation is a centerpiece in many accounts of state development in early modern Europe. When rulers began systematically soliciting their subjects for taxes in this period,<sup>1</sup> it triggered new demands for public goods and representation (Schumpeter, 1918; Tilly, 1985). Citizens resisted paying taxes until rulers made concessions. This process of “tax bargaining” between citizens and the state is thought to underlie the joint emergence of participation in politics, tax compliance, and accountable governance.<sup>2</sup> “In exchange for the greater say in government... [citizens] agreed to provide sufficient tax revenue” (North and Weingast, 1989). The slogan “no taxation without

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<sup>1</sup>Faced with rising costs of war, rulers could no longer rely on ‘own revenues’ (land rents, sales of venal offices, seignorage), so most began broadening the tax base and systematizing collection in this period (Brewer, 1990; Gennaioli and Voth, 2015; Cantoni et al., 2019).

<sup>2</sup>Bates and Lien (1985) and Hoffman and Rosenthal (1997) model aspects of tax bargaining. Brautigam et al. (2008) discuss a more macro-level ‘governance dividend’ of tax collection of which citizen participation and tax bargaining is the first step.

representation” captures the intuition.

Given this potential for tax collection to promote inclusive governance, donors and policymakers increasingly support broadening the tax base in fragile states,<sup>3</sup> which collect only 12% of GDP in tax compared to 40% in developed countries (IMF, 2017). However, we lack rigorous evidence on the causal link between tax collection and participation because states typically tax non-randomly, targeting certain firms or individuals based on perceptions of willingness to pay or of taxable income elasticities.<sup>4</sup> Moreover, although a growing literature examines taxation in developing countries (Pomeranz, 2015; Khan et al., 2015; Naritomi, 2019), most research comes from middle-income countries like Chile, Pakistan, and Brazil, which have higher rates of tax compliance and more functional states.

I investigate the supposed participation dividend of tax collection in a weak state seeking to register and tax its citizens for the first time. In 2016, the Provincial Government of Kasai Central, in the D.R. Congo (DRC), launched the first-ever, door-to-door citizen tax collection campaign in the capital city of Kananga. Previously, as in many low-capacity states (Gordon and Li, 2009), the tax base was tiny: a handful of firms downtown as well as vehicles passing through the city’s tolls. Receipts totaled \$2 million in a province of 6 million. Most citizens had never paid, or been solicited for, formal taxes by the modern Congolese state. When a 2015 national decentralization lowered provincial revenues by 40%, the governor turned to the property tax.

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<sup>3</sup>For instance, the OECD noted “Tax reforms can ... enhance accountability between citizens and the state” (OECD, 2010). Similarly, the IMF wrote “Bringing small businesses into the tax net can help secure their participation in the political process and improve government accountability” (IMF, 2011). DFID (2016) shows expanding donor support in this area. Pomeranz and Vila-Belda (2019) review research outputs of these programs.

<sup>4</sup>Theoretically, it is also not obvious that citizens would choose to engage *more* with a state seeking to tax them. Citizens might prefer to evade or move elsewhere (Tiebout, 1956).

In partnership with the government, I randomly selected 253 of the city’s 431 neighborhoods (covering roughly 33,000 properties) to receive the initial phase of the tax campaign. In treated neighborhoods, tax collectors went door to door (1) recording the names of all property owners and assigning them unique tax ID numbers, and (2) 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 tax ministry themselves, but in practice less than 1% did.

I estimate the reduced-form impact of this campaign — i.e. of being registered by the state *and* asked to pay the property tax. Given that states invariably need to collect information about potential taxpayers before they can try to tax them (Scott, 1998; Brewer, 1990; Pomeranz, 2015), the campaign is a treatment bundle of theoretical and policy interest.

I first examine if the campaign achieved the government’s goal of building a property register and raising tax compliance. One might expect it to fail. Large informal sectors in developing countries mean that third-party information is scarce (Jensen, 2018), and high monitoring costs exacerbate principal-agent problems between governments and tax collectors (Khan et al., 2015). These problems are particularly severe in fragile states like the DRC. Nonetheless, the campaign increased the probability of visits from tax collectors by 81 percentage points (from 0.05 in control) and increased property tax compliance by 10 percentage points (from 0.001 in control). In addition to a large expansion of the presence of the formal state in Kananga, this 100-fold increase in citizen tax compliance made property tax receipts just under 5% of the provincial government’s total revenue, on par with local governments in

more prosperous African countries.<sup>5</sup> The provincial government evidently viewed the campaign as a success, choosing to continue field-based property tax collection after 2016.

I then use the random assignment of this campaign to test the hypothesis that when states start to solicit taxes, citizens will respond by demanding more of a voice in the government. To measure such demands, I use two real-world channels of participation that I observed by collaborating with the government.<sup>6</sup> First, the government hosted a series of townhall meetings, in which officials and citizens discussed tax and public spending in Kananga. Second, citizens could submit anonymous evaluations of the provincial government to a drop box downtown whose contents were shared with the governor and other top officials. Attending a townhall or submitting an evaluation exhibits willingness to incur costs to have a voice in the provincial government. Indeed, the average participating individual spent an estimated 105% percent of daily household income on transport to reach the venues for participation.<sup>7</sup>

The tax campaign 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, a 26% increase relative to control. In order to participate in these ways, the average individual in treatment incurred 40.1% higher costs compared to control.<sup>8</sup> Consistent with historical accounts of

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<sup>5</sup>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 are typically much lower outside of national capitals.

<sup>6</sup>This approach is similar to that of Olken (2007), Casey et al. (2012), and Paler (2013).

<sup>7</sup>Factoring in the opportunity cost of spending 3 hours in a townhall meeting, this figure increases to 131% of daily income.

<sup>8</sup>The percent increase in costs to participate (transport costs and opportunity costs) is larger than the percent increase in attending a townhall or submitting an evaluation because a non-trivial share of citizens (especially in treatment) participated in *both* ways. Indeed, the percent increase in *both* participating in a townhall and submitting an evaluation is 77%.

tax bargaining, townhall participants demanded better public infrastructure and a more responsive government in exchange for taxes.<sup>9</sup> Submitted evaluation forms were also highly critical — over 90% expressed disapproval 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 familiarity with and trust in 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 stemming from awareness of the control group, which had not received tax collectors when outcomes were measured. Finally, given the 6-8 month time gap between tax collection and participation, it is unlikely that the main result could be explained by a salience effect of taxation in treatment.

I consider several extensions of the analysis. I first provide suggestive evidence that the tax campaign and the increase in participation with the provincial government crowded out engagement with local city chiefs, consistent with the idea that the formal state and local forms of governance are substitutes (Cheema et al., 2006). Then, I find little evidence of spillovers on compliance or participation, or of decay of the treatment effect over time.

Although the main contribution of the paper is the reduced-form estimates of a participation dividend of tax collection in a setting of low state capacity, I also provide more suggestive evidence about the mechanism. Some have posited that tax-

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<sup>9</sup>“Erosion threatens our neighborhoods, and the government does nothing,” asked one individual, “so why should we pay?” 71% of citizen comments were similar demands for better governance in return for taxes, or general demands for transparency and less corruption.

*payers* will participate more because they expect a quid pro quo (Prichard, 2015) or exhibit an endowment effect (Martin, 2014). However, in this case, the increase in participation was not driven by the 10% of treated individuals who paid. Rather, *everyone* in treatment neighborhoods — payers and nonpayers — participated at higher levels compared to people in control neighborhoods. According to jackknife IV estimates leveraging the random assignment of tax collectors to neighborhoods, the tax campaign does not appear to have increased participation through its effect on tax payment.<sup>10</sup>

In fact, this empirical pattern is consistent with historical accounts of tax bargaining. Citizens are thought to have demanded better governance *before* they agreed to pay taxes (Bates and Lien, 1985). Otherwise, they would have forfeited their bargaining power. For instance, before England’s 1688 Revolution increased the power of Parliament over the Crown, taxpayers refused to pay the taxes the King demanded, fueling the fiscal crises of the mid 17<sup>th</sup> century (North and Weingast, 1989). Over time, the standoff led rulers to accord citizens greater voice in government: “When ordinary people resisted [taxation] vigorously, authorities made concessions” (Tilly, 1985). In the present context, it would have been a strategic error for citizens to pay before making demands of the government.

Thus, rather than a mechanism operating through payment, I argue that state efforts to broaden the tax base send a signal of future state capacity that raise the expected benefits of participation.<sup>11</sup> In Kananga, the state was effectively absent *ex ante*. Observing a systematic door-to-door tax campaign leads citizens to believe that the government is now more capable of impacting their future wellbeing. Cit-

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<sup>10</sup>The campaign also does not appear to have lowered coordination costs associated with participation, another possible mechanism.

<sup>11</sup>Coate and Morris (1995) similarly model the informational aspects of public projects.

izens might expect the state to continue trying to tax them, or they might expect public goods funded by tax revenues. They thus anticipate greater returns to participation to try to influence future tax policy and government spending, leveraging their bargaining power as potential taxpayers in light of the state’s revealed need for new revenues. I outline this logic in a simple decision-theoretical framework in Section 6.2. This mechanism is relevant in the historical cases examined in the tax-bargaining literature given that, before the emergence of “tax states” (Schumpeter, 1918), the formal state was similarly absent from most citizens’ lives (Cantoni et al., 2019).

Consistent with this mechanism, 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 stronger. I also examine the effects of the tax campaign on individuals’ stated beliefs about the provincial government’s ‘extractive’ and ‘productive’ capacity (Besley and Persson, 2009). Citizens in treated neighborhoods updated their beliefs in two main ways: (i) they realized the government had *more revenue* due to greater extractive capacity; and (ii) they believed the government would *spend more productively*. Interestingly, however, treated citizens who chose to participate were less certain that revenue would be spent well without citizen monitoring. Thus, treated citizens appear to have participated more because they believed the government capable of higher spending, but they also perceived a need to monitor that spending and sought to influence it toward their preferred policy. That awareness of new tax revenues would stimulate participation is consistent with evidence from Brazil showing that municipalities are less corrupt and spend more on public goods when taxes make up a greater share of total revenues (Brollo et al., 2013; Gadenne, 2017).



This paper provides the first field-experimental test of classic theories positing a catalytic role of tax collection on citizen engagement in politics in weak states (Schumpeter, 1918; Tilly, 1985; North and Weingast, 1989). The unique setting, a low-capacity state attempting to systematically register and tax its citizens for the first time, offers a chance to study in real time if there is a ‘participation dividend’ to the establishment of fiscal authority, as suggested by social compact theories of government based on early modern Europe. Most past empirical work on the emergence of fiscal capacity exploits European historical data on interstate military competition (Gennaioli and Voth, 2015), fiscal centralization (Cantoni et al., 2019), the granting of city rights by territorial rulers (Cantoni and Yuchtman, 2014), and the adoption of laws requiring public goods provision (Dittmar and Meisenzahl, 2019). My approach is most similar to Khan et al. (2015) and Sanchez de la Sierra (2019) in collecting experimental (or quasi-experimental) data in developing countries to test hypotheses based on histories of now-rich countries’ development. In studying the participation dividend, per se, past studies have used lab experiments simulating taxation and participation (Martin, 2014) and survey experiments priming citizens about the share of taxes in government revenues (Paler, 2013; de la Cuesta et al., 2015).<sup>12</sup>

The paper also contributes to the empirical literature on tax and development, which has chiefly examined middle-income countries and has focused less on the political economy effects of increasing tax enforcement (Besley and Persson, 2013). Past work examines how governments can raise compliance through third-party reporting (Kleven et al., 2011; Pomeranz, 2015; Naritomi, 2019; Carrillo et al., 2017; Jensen, 2018), tax collector incentives (Khan et al., 2015), providing information

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<sup>12</sup>There is also evidence from developed countries of electoral payoffs from technologies that reduce tax evasion (Casaburi and Troiano, 2015).

about enforcement or peer behavior (Del Carpio, 2013; Pomeranz, 2015), and reducing bureaucratic barriers to compliance (Kleven and Waseem, 2013; Best et al., 2015). The paper contributes to this literature by demonstrating that a rudimentary intervention (in-person tax appeals that reduce the transaction costs of compliance) substantially increased tax receipts in one of the world’s poorest countries.

Finally, to my knowledge, the paper examines the first instance of randomized tax collection. Closest in this regard are Khan et al. (2015), who study randomized tax collector incentives, and Dunning et al. (2015), who study randomized tax holidays in Uruguay.

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 in the world.<sup>13</sup> Median monthly household income in the study site is roughly \$70, 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, or a ‘failed state,’ due to its history of civil conflict (Sanchez de la Sierra, 2019). It has low state capacity across all dimensions, and especially in terms of tax capacity. In tax revenue as a percent of GDP, the DRC ranks 188 out of 200 countries for the period 2000 to 2017.<sup>14</sup>

Kananga is a city of roughly 1 million and the capital of the Provincial Gov-

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<sup>13</sup>See, e.g., <http://wdi.worldbank.org/table/1.2>.

<sup>14</sup>Data available: <https://data.worldbank.org/indicator/gc.tax.totl.gd.zs>.

ernment of Kasai Central. Its tax revenues are extremely low: roughly \$2 million per year in a province of 6 million people. 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. For instance, although the property tax is the only tax to which all residents are subject, only 5.6% of individuals knew of it at baseline. Less than 10% of citizens reported paying any taxes in the previous year.<sup>15</sup> 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 urbanization in Africa is fueling rapid growth in real estate values, making a strong case for property taxation (Fjeldstad et al., 2017). Because valuations can be difficult for low-capacity governments, many African municipalities have simplified property valuations to size-based assessments or fixed-amount levies on properties under a certain threshold (Fjeldstad et al., 2017). The Provincial Government of Kasai Central has followed suit. Roughly 90% of property owners in Kananga must pay a fixed annual property tax of 2,000 Congolese Francs (CF), about \$2, which is the median household’s total daily income.<sup>16</sup> Larger ‘midrange’ houses built of modern materials (i.e. not mudbricks), about 9% of total property owners, pay 6,600 CF. Finally, ‘villas’, Belgian-built compounds with a garage (1% of property owners), are measured; their owners face a rate increasing in size. Prior to the 2016 tax campaign, property owners were

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<sup>15</sup>The most common taxes paid included market fees and a vehicle tax for the small percentage of households that own cars or motorcycles. 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.

<sup>16</sup>Properties owned by state employees, churches, and the elderly are exempted.

supposed to visit the tax ministry themselves to pay. But except for a handful of firms, compliance remained near zero.<sup>17</sup>

Why did the provincial government begin enforcing the property tax in 2016? According to the former finance minister, an unanticipated national policy triggered a 40% reduction in provincial tax receipts, leading the provincial government to increase property tax enforcement to recoup these losses. Specifically, the 2015 *découpage* (administrative splitting) of the 11 old provinces into 26 new provinces meant that the diamond-rich region around Tshikapa, a large source of revenue for the Kananga-based tax ministry, was no longer part of the province.<sup>18</sup> Facing shortfalls, the governor turned to property taxes.

The government, though on paper a democracy, is autocratic, 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 are versions of these forms of political engagement.

In sum, Kananga is a good setting in which to investigate the participation dividend of tax collection because it shares key features with the states in early modern Europe examined in seminal accounts of the taxation-based social compact (Schumpeter, 1918; Tilly, 1985). These works discuss low-capacity autocratic states, strug-

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<sup>17</sup>Of the <300 property tax payments recorded in 2015, 86% were made by firms.

<sup>18</sup>Although decentralization was noted in the 2007 constitution, its sudden implementation in 2015 was a surprise, as evidenced by the chaos it engendered in provincial-level politics (Wille, 2015). The *découpage* is widely thought to be a tactic of incumbent Joseph Kabila to sow bureaucratic confusion and justify postponing the 2016 elections, which he did.

gling to cope with fiscal crises by building a tax bureaucracy and soliciting payment from a broader base in society. In early 2016, the Provincial Government of Kasai Central similarly sought to expand and systematize property tax collection in response to a sudden drop in revenues brought on by an external shock.

### 3 Experimental design

The treatment, randomly assigned on the neighborhood level, is the door-to-door property tax collection campaign, which ran from April to December in 2016. I defined the unit of randomization, the neighborhood, by partitioning a satellite map of the city into 431 polygons that approximate *localités*, the lowest administrative unit in the city.<sup>19</sup> 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 were scheduled to receive the tax campaign in mid 2017.<sup>20</sup>

For the randomization, I constructed 33 strata defined by (i) satellite grid cells of Kananga, and (ii) the estimated population of the neighborhood.<sup>21</sup> These strata help ensure that more populous and downtown parts of the city, which the provincial government had in the past differentially targeted in the little tax collection it conducted, are equally likely to appear in treatment and control.

Before the tax campaign, households in *all* neighborhoods received informational fliers in French or Tshiluba, the most widely spoken local language, announcing

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<sup>19</sup>The government did not have maps of *localité* borders, hence the need to define these on a satellite map. See Figures A1 and A8 for examples.

<sup>20</sup>The government ultimately decided to suspend all tax collection in 2017 after violence broke out in the province early that year. It recommenced property tax collection in 2018. For information about the conflict, see Section A1.3.

<sup>21</sup>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.

that (i) the provincial 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.”<sup>22</sup> Distributing fliers in treatment and control helps ensure that estimated treatment effects reflect the impact of tax collection rather than simply information about the tax or the campaign.

The 51 government tax collectors working on the property tax campaign were randomly assigned to new teams of three every twelve work days.<sup>23</sup> Teams were then randomly assigned to treated neighborhoods. The order of neighborhoods was also random. Collectors completed two tasks in each neighborhood.

1. **Property register.** First, collectors completed a brief property register to identify all liable property owners in the neighborhood. Collectors assigned a unique code to each house, written in chalk on the wall or door. These codes appear on tax receipts to identify compliant households in the administrative data. The property register was verified by members of the research team with GPS devices to ensure the collectors respected neighborhood boundaries. Collectors received a printed copy of the register before tax collection.
2. **Tax solicitation.** After completing the register, collectors returned to households to solicit payment of the property tax. When an individual paid the tax, collectors used a tablet application to print a receipt (Figure A2). Col-

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<sup>22</sup>See Section A2.1 for more information about these fliers.

<sup>23</sup>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 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 for Pakistani property tax collectors in Khan et al. (2015). Additionally, 40% of property owners in each treated neighborhood were randomly sampled before tax collection 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.

lectors left the receipt with the taxpayer, with an electronic record saved in the tablet’s memory. When collectors deposited the money, tablet data were automatically downloaded, enabling program supervisors to check that the amount deposited equaled the amounts on all receipts issued.

Collectors memorized the following message during training to solicit the tax from households: “This compound has a legal obligation to pay the property tax for the year of 2016. The provincial government will use the money to promote the economic development of the province. If you do not pay today, please indicate a date and time when you will pay and I will return then.” Collectors kept track of appointments and were told to revisit households until they paid. According to household surveys, the modal number of collector visits in treatment neighborhoods was 2, though 21% of the sample report 3 or more visits.

The treatment is the combination of the property register and tax solicitation (Table I). Control neighborhoods experienced neither component. As in the past, citizens in these neighborhoods were expected to pay at the tax ministry themselves. The main analysis considers the reduced-form impact of the tax campaign as a whole. This is a theory- and policy-relevant estimand given that states invariably need information about citizens before they can collect taxes from them (Brewer, 1990; Ertman, 1997; Scott, 2017).

## 4 Data, estimation, and balance

### 4.1 Data

Data come from four sources: (1) administrative data on property tax payment, (2) a baseline survey before the campaign, (3) a midline survey during the campaign, and (4) an endline survey after the campaign.

Administrative data come from the government’s official tax database. This database was 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 property registration.

Baseline survey enumeration occurred just before the property tax campaign. Independent enumerators randomly sampled compounds following skip patterns 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 per neighborhood. Enumerators then conducted midline surveys in all compounds on average 2-4 weeks after collectors had completed a neighborhood.<sup>24</sup> Finally, enumerators administered the endline survey in 2017, after the tax campaign. In each neighborhood, enumerators first conducted a screening survey of roughly 20 households, randomly sampling again with a skip pattern. I then randomly selected a subsample of screening survey participants for the full interview, choosing higher-quality houses with slightly higher probability to focus on the population most affected by the campaign.<sup>25</sup> As such, random sampling for baseline and endline surveys was

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<sup>24</sup>For control neighborhoods, enumerators similarly waited at least two weeks after an adjacent neighborhood had received tax collectors.

<sup>25</sup>Section A2.2 describes this sampling strategy. I also construct weights and re-estimate all results to be representative of the population (Section A4).



independent.<sup>26</sup>

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’s population. All 71 neighborhoods from this commune were dropped before respondents could be sampled and invited to participate. Because of the spatial stratification used for randomization, the number of neighborhoods ineligible for endline enumeration is balanced (Table A6). During endline, 453 of the 3,421 (13.21%) sampled households could not be surveyed. Common causes included (1) being too busy, (2) being on a trip, and (3) declining participation without a reason. Refusal rates and overall attrition are balanced across treatment and control (Table A6).

Table I summarizes the activities of the collectors and the enumerators. All research components of the study — baseline, midline, and endline surveys — were constant across treatment and control. Sampling and enumeration procedures of surveys were identical, as indicated by the balanced length of surveys (Table A6). What varied across treatment groups was assignment to the tax campaign.

## 4.2 Outcome measurement

The paper examines two sets of outcomes. First, in estimating the effect of the campaign on visits from collectors and property tax compliance, I consider the following variables.

1. *Visited by tax collector*: an indicator that the household received visits from provincial tax collectors in 2016, self-reported at midline.

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<sup>26</sup>I did not simply track all baseline respondents because (i) I needed a considerably larger endline sample, and (ii) many baseline respondents were not property owners, but rather members of their families or renters. I did track a set of baseline respondents for a companion paper (Weigel, 2018), as discussed in Section 5.3.2.

2. *Paid property tax*: an indicator that the household paid the property tax in 2016, measured by linking administrative compliance data to household surveys by unique tax ID numbers. In control, I use fuzzy name matching within neighborhoods to match administrative records with household surveys.<sup>27</sup>

Second, testing the supposed participation dividend of tax collection requires measures of political engagement. I cannot use voting data because the DRC is not a liberal democracy: elections were canceled in 2016 and 2017.<sup>28</sup> Self-reported political participation is often subject to measurement error caused by social desirability, time inconsistency, and/or anonymity concerns in repressive settings. To overcome these issues, I worked with the provincial government to embed measurement strategies in two forms of political engagement that come at a cost to individuals: attendance at townhall meetings, and submission of government evaluations.

Specifically, in early 2017, the provincial government held five townhall meetings. Chaired by the finance minister and the director general of the tax ministry, the meetings sought to promote dialog between officials and citizens about tax and public spending in Kananga. All endline participants received official invitations to one of these meetings (Figure A7).<sup>29</sup> The proceedings were formal, if at times heated (see Section A2.4). Townhall attendance indicates a willingness to exert costly effort to have a voice in the government. Participants needed to remember the time of the

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<sup>27</sup>This method is reliable because there are <30 records of non-campaign property-tax payments in 2016 made by individuals.

<sup>28</sup>Moreover, it is more in line with the underlying theory to test this hypothesis in a nondemocracy with a broken social contract. The hypothesis is that tax collection stimulates political participation and ultimately more inclusive governance (which may include democratic elections). To test this hypothesis, one needs to measure how citizens exert voice in politics *in the absence of such institutions*.

<sup>29</sup>After an increase in militia-related insecurity in Kananga in April 2017, the government issued a shelter-in-place edict. This included canceling future meetings. Thus, after April 1, sampled participants did not receive invitations, and I have this outcome for 1,934 of 2,913 total participants.

meeting and pay their own transport to the provincial assembly building, located up to 13km — a minimum \$2 motorcycle taxi ride — from endline respondents’ homes (Figure A8). Nonetheless, 483 individuals (24.9% of those who received invitations) attended a townhall meeting, indicated by the variable *Townhall meeting attendance*.

The second measure of participation is the submission of anonymous evaluations of the provincial government to a locked drop box in downtown Kananga.<sup>30</sup> All endline participants received evaluation forms after the endline survey and then chose whether or not to fill it out and drop it off. The form had one question about the respondent’s overall satisfaction with the government, followed by four agree-disagree statements concerning (i) opportunities for participation, (ii) access to information, (iii) spending on public goods, and (iv) citizen reporting of problems.<sup>31</sup> Citizens could also write additional suggestions in a text box at the bottom. They were informed that the governor and other top officials would receive the evaluations plus a summary of their contents. Filling out the form and paying the transport to the drop box downtown again demonstrates willingness to engage in costly participation with the provincial government. In total, 396 individuals (13.6% of total respondents) submitted their evaluations, indicated by the variable *Evaluation form submission*.

To characterize the intensive margin, I examine two additional outcome variables: *Townhall or evaluation* and *Townhall and evaluation*, indicators for individuals who participated in either or both activities, respectively. Finally, to facilitate interpretation of the magnitude, I examine a standardized index of both underlying participation variables.<sup>32</sup>

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<sup>30</sup>This is similar to the comment forms in Olken (2007) and the postcards in Paler (2013). Forms contained a unique code unknown to the government.

<sup>31</sup>See Section A2.4 for further details.

<sup>32</sup>I construct this and other standardized indices by first standardizing each component variable, summing over all questions, and standardizing the new synthetic variable again. I use this indexing procedure whenever there are multiple measures of the same underlying variable to

I also consider survey evidence about citizen demand for the provincial government to provide public goods. 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). From these data, I use the standardized sum of sector-specific indicators for choosing the provincial government. This variable is thus increasing in the amount of public goods provision demanded from the provincial government relative to other providers. Second, enumerators posed three sets of opposing viewpoints concerning the optimal level of public service provision by the provincial government. These hypothetical questions are combined into an index that is also 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.<sup>33</sup>

### 4.3 Estimation

I primarily use OLS to estimate the following equation:

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

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  $\beta_1$  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,  $\alpha_k$  are strata

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minimize risk of type I and II error.

<sup>33</sup>See Section A6 for the exact text of the underlying questions.

fixed effects, and  $\mathbf{X}_{ijk}$  and  $\mathbf{X}_{jk}$  are individual- and neighborhood-level covariates. Regressions control for gender, age, and age squared, with additional covariates included when specified.

## 4.4 Balance

To check the randomization, I estimate Equation 1 (without covariates) using as the outcome: thirteen neighborhood characteristics from the baseline survey, thirteen individual characteristics from the endline survey, and six characteristics of survey enumeration (Table A6). In total, of individual-level covariates, one (an index of estimated household wealth) is imbalanced at the 5% level,<sup>34</sup> and one (business owner) status, is imbalanced at the 10% level. Of neighborhood-level covariates, one (quality of public lighting) is imbalanced at the 10% level. Thus, 3.1% (9.3%) of variables are significantly imbalanced at the 5% (10%) level. An omnibus test of joint orthogonality fails to reject the null for the individual variables ( $F = 0.90, p = 0.55$ ) and the neighborhood-level variables ( $F = 0.71, p = 0.75$ ). To be conservative, the three imbalanced covariates are included in  $\mathbf{X}_{ijk}$  and  $\mathbf{X}_{jk}$ , respectively, in analysis of endline data, with robustness checks showing other covariate regimes in Section A4.

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<sup>34</sup>Reassuringly, other proxies of socioeconomic status, such as education, income, employment, and compound characteristics (electricity, walls, vehicle ownership, presence of renters), are balanced.

## 5 Results

### 5.1 Effects on tax compliance

This section considers whether the campaign raised tax compliance through household visits by collectors. It is not obvious that a tax campaign in the DRC would succeed. Bureaucrats are underpaid and have low morale, while citizens have little exposure to formal tax collection. Will collectors undertake this work as planned, and will citizens pay when collectors arrive at their doorstep for the first time?

Table II summarizes OLS estimations of Equation 1. The campaign caused an 81.5 percentage-point increase in reported visits from tax collectors.<sup>35</sup> It also caused on average an 10.3 percentage-point increase in property tax payment, a 100-fold rise relative to control. For this and subsequent estimations, Section A4 contains a series of robustness checks, including specifications with (1) no covariates, (2) only gender, age, and age squared as covariates, (3) all candidate covariates listed in the pre-analysis plan, (4) enumerator fixed effects, (5) sampling weights, and (6) heterogeneous treatment effects by wealth.

Although a 10 percentage-point increase in tax compliance is substantial, the majority of individuals still *evaded* paying the tax, despite visits from collectors. Why did the campaign cause some, but far from all, individuals to pay the tax? A companion paper provides a full treatment of this question (Weigel, 2018). Briefly, tax compliers tended to have more education, income, wealth, and formal employment (Table A3). In addition, individuals who at baseline perceived a higher probability of

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<sup>35</sup>In control neighborhoods, 5% of individuals reported visits from tax collectors. This likely reflects noncompliance among collectors, who at times crossed into to the wrong (control) neighborhoods. Such noncompliance was expected given that the borders between neighborhoods are not always clearly delimited and must be checked using GPS. This noncompliance would, if anything, bias treatment effects toward zero.

punishment for evasion were marginally more likely to pay — as were individuals who *ex ante* professed more positive attitudes toward the provincial government. These results are consistent with classic cost-benefit models of tax compliance (Allingham and Sandmo, 1972) as well as models emphasizing “tax morale” (Luttmer and Singhal, 2014).

It is also worth noting that the tax campaign does not appear to have increased bribes according to multiple measures (Section A1.2).<sup>36</sup> This result is unsurprising for two reasons. First, because this was the first-ever citizen tax campaign, collectors faced high uncertainty about the government’s plans to audit their work and sanction bribe takers. Second, collusive bribery is more likely when collectors and citizens have repeated interactions (Khan et al., 2015). In contrast, this first year of tax enforcement involved, in most cases, a single-shot interaction between collector and citizen. One might expect an increase in bribes in future rounds of property tax collection. The negligible impact on bribe payment means that the campaign could only affect political engagement through collector visits and tax payment.

## 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 citizens will demand more of a voice in politics when the state tries to tax them. Estimations of Equation 1, summarized in Table III, support this hypothesis. The campaign triggered a 4.2 percentage-point (23%) increase in townhall attendance (Column 1) and a 2.4 percentage-point (24%) increase in evaluation form submission (Column 2). To capture the intensive margin, Columns 3 and 4 show that the tax campaign stimulated participation

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<sup>36</sup>Reporting bribes is not taboo in Kananga: in a study of tolls in Kananga, nearly half of motorcycle taxi drivers openly admitted to paying bribes (Reid and Weigel, 2017).

in either outcome by 4.7 percentage points (26%) and in both outcomes by 2.7 percentage points (77%).<sup>37</sup> These treatment effects amount to a 0.14 standard-deviation increase in participation (Column 5). The results are robust to the checks described above (Section A4), estimating average effect size (AES) coefficients (Table A9), and controlling for the distances to participation venues (Table A8). Table III also reports  $p$ -values constructed using randomization inference.

How costly were these forms of participation? The principal financial cost was paying a mototaxi to reach the city center, where the townhalls took place and the drop box was located.<sup>38</sup> To measure mototaxi prices, I use receipts for trips taken by enumerators from the center to the city’s 431 neighborhoods. I then use endline survey data to estimate household daily income.<sup>39</sup> The average participating individual spent more than their daily household income (105%) on transport to participate in these ways.<sup>40</sup> Comparing the average individual in treatment and control, I find that the tax campaign caused a 40.1% increase in such expenditures.

A natural concern is whether individuals participated to make demands of the provincial government, or whether they simply had more factual questions or general curiosity about the 2016 tax campaign? Examining the statements made by participants during townhall meetings suggests that citizens used them as an op-

<sup>37</sup>Of the 550 individuals who participated in a townhall meeting or submitted their evaluation, 144 (26%) did both; 102 of these 144 (71%) hailed from treated neighborhoods.

<sup>38</sup>For individuals who both attended a meeting and submitted an evaluation, I assume separate trips. The provincial assembly building (where townhalls occurred) and the drop box were 1km apart (Figure A8), but there was no detectable increase in form submission on townhall days. Most double participants appear to have made independent trips.

<sup>39</sup>Specifically, I combine citizens’ household income estimates for the past week and the past month to estimate daily income. For the 2.5% of individuals who report 0 income for both, I set their income to the minimum positive income value reported by another household in their neighborhood. Results are similar if instead I estimate income using weekly expenditure on phone credit or transport.

<sup>40</sup>Including the opportunity cost of attending a three-hour townhall meeting increases this estimate to 131% of daily income.



portunity to bargain with government officials over the quality of governance in Kananga. Over 70% of these statements were (i) demands for better governance in exchange for compliance with provincial taxes, or (ii) related questions about public spending, public goods in Kananga, or provincial corruption (Figure A11). “Why should the inhabitants of Lukonga [a commune of Kananga] pay taxes,” one participant asked, “when the roads are in such disastrous condition?”<sup>41</sup> Such complaints evoke a bargaining process in which citizens demand better governance in exchange for tax compliance (Bates and Lien, 1985).

Additional suggestive evidence about the motive behind participation comes from examining whether, conditional on attending a meeting, citizens from treated neighborhoods were more likely to ask about factual details of provincial taxation (21% of total townhall statements). This comparison is difficult to interpret because speaking at the meetings is endogenous to participation. Nonetheless, it reinforces the descriptive evidence above that, according to simple difference-in-means tests, treated individuals were no more likely to ask factual questions about the tax campaign, but they were roughly twice as likely to ask about provincial spending and public goods ( $p=0.050$ ).

Further evidence comes from submitted evaluations, which did not mention taxation but asked about the inclusiveness and transparency of the government. Submitted evaluations were highly critical: over 90% expressed overall disapproval of the provincial government. Similarly, respondents overwhelmingly demanded more avenues of participation, access to information, and public goods spending (Figure A14). In addition, 39% of individuals wrote in additional suggestions, of which the most frequent topics include: general demands for better governance, demands for

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<sup>41</sup>Participant question from January 30 townhall meeting (author’s translation).

specific public goods projects, and demands for greater monitoring of the provincial government and improved transparency.<sup>42</sup> “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.”

Participants in treatment neighborhoods were particularly likely to use the evaluations to demand better governance. If we re-estimate the specification in Column 2 of Table III using an outcome variable indicating submission of (i) evaluations that express *disapproval* of the government, and (ii) evaluations that contain critical *written-in suggestions* or demands,<sup>43</sup> individuals in treatment were still more likely to participate compared to control (Table A7). This evidence reinforces that the treatment effect reflects an increase in demand for inclusive, high-quality governance.

Finally, if the increases in participation reflect greater demand for good governance, individuals in treated neighborhoods should also hold stronger views about the obligation of the provincial government to provide public goods. Regression results examining survey-based indices described on p. 19 confirm this intuition (Table IV). Individuals in treated neighborhoods demand a larger role (by 0.11 standard deviations) for the provincial government in public goods provision relative to other possible providers, such as the national government or NGOs.<sup>44</sup> Importantly, this result does not just reflect changes in beliefs about the *current* levels of public

<sup>42</sup>Section A2.4 provides more details about submitted evaluation card contents.

<sup>43</sup>This latter variable equals 1 only if the written-in comment was critical or made a demand of the provincial government. Comments that were 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.

<sup>44</sup>The standard errors are larger when considering sector-based questions rather than hypothetical questions, but the magnitude of the coefficients is nearly identical. Figure A24 shows results for the constituent survey questions of these indices.

goods provision. An analogous set of questions asked how much citizens perceive the provincial government to be currently providing in the same sectors. No systematic differences appear across treatment and control (Figure A28). The evidence in Table IV therefore suggests that the tax campaign expanded the extent of public goods provision that citizens *demand* of the provincial government.

### 5.3 Alternative explanations

Rather than demand for better governance, do higher rates of participation in treatment reflect (1) experimenter demand effects, (2) a decline in participation in control rather than an increase in treatment, (3) a sense of unfairness due to awareness of untaxed control neighborhoods, or (4) the short-run salience of taxation in treated neighborhoods? This section explores these possibilities.

#### 5.3.1 Experimenter demand effects

One concern is whether the observed increase in participation is an artifact of the research components of the experiment. Treated citizens might have been more likely to participate if they had more contact with or were treated differently by enumerators, became more trusting of the research team, and thus felt more emboldened to participate as a result.

To preclude such issues, all research procedures were held constant across treatment and control, as evidenced by the balance in measurable characteristics of survey enumeration (Table A6). Moreover, enumerators administered surveys in a random order, frequently alternating between control and treatment neighborhoods. Individuals in treatment and control received the same information about the townhall meetings and government evaluations, and participation always occurred *after* end-

line survey enumeration to minimize potential demand effects.

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 (indicating potential mistrust of the researchers), and (5) whether they provided an incorrect or fake phone number to the enumerator (also indicative of mistrust). No systematic differences appear across treatment and control (Table A10).

An indirect demand effect could arise if tax collectors encouraged citizens to participate. However, this is implausible because the townhalls and evaluations had not yet been scheduled or announced at the time of tax collection.

### 5.3.2 Declining participation in control

A second alternative explanation is that the treatment effects result not from higher participation in treatment but from lower participation in control. It is possible that control individuals expected visits from tax collectors, and when they never received them, they concluded that the government was less capable than they previously thought — and hence decided to participate less.<sup>45</sup>

I investigate this hypothesis using a sample of 630 baseline participants whom enumerators re-surveyed after the tax campaign.<sup>46</sup> Although I cannot measure

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<sup>45</sup>Alternatively, control individuals might have participated less because they thought it would increase the probability of visits from tax collectors in the future. If this were the case, control households that received fliers before the campaign would have been the least likely to participate because they had the most information about the government’s plans for property tax collection. However, flier recipients in control are no less likely to participate in the townhall or evaluation submission ( $t = -0.66$ ).

<sup>46</sup>I collected these data for a companion paper on the determinants of compliance (Weigel, 2018). This repeated baseline sample is not part of the endline sample for this paper, but it is helpful

changes in participation, I examine changes in beliefs about the provincial government within individuals over time, specifically: (1) the responsibility of the provincial government in public goods provision (the same sector-based question examined in Table IV); (2) trust in the provincial government and tax ministry; and (3) the share of taxes that respondents perceive to be spent well and not wasted or stolen. Table A11 summarizes fixed-effects regressions with an indicator (*Post*) for measurement after the tax campaign, interacted with the treatment indicator. If attitudes towards the government deteriorated in control, there would be negative point estimates on *Post*. For none of these measures is the coefficient negative and statistically different from zero.<sup>47</sup> At least for this set of individuals tracked from baseline to endline, those in control do not seem to have updated negatively about the government.

### 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. At first glance, 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 they were taxed first.

To explore this possibility, I examine whether treated households near the border with control neighborhoods were more likely to participate compared to households

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here to examine changes in beliefs within individuals.

<sup>47</sup>The increase in the perceived responsibility of the government to provide public goods in the treatment group (Column 1) corroborates the results in Table IV.

farther from control. If awareness of the untaxed control fueled participation in treatment, then presumably individuals living near a border with control (and thus more aware of neighborhood-level differences in tax collection) would have been more likely to participate compared to those further from the border. However, plotting the participation rate in treatment as a function of minimum distance to control reveals no such relationship (Figure A16). Moreover, complaints about the fact that some neighborhoods had been taxed while others had not did not arise during townhall meetings or on government evaluations. Awareness of the randomized rollout of the campaign appears to have been low. This is not surprising because the unit of randomization, the neighborhood, was quite fine, averaging only 131 plots. If larger regions of the city had been taxed before others, citizens might have been more likely to notice the phased rollout.

#### **5.3.4 Short-term salience of taxation**

Another interpretation of the results is that the increase in participation is driven by the short-term salience of taxation in treated neighborhoods. In this interpretation, the treatment is akin to a prime, and individuals are thought to participate more in townhall meetings simply because taxation is top of mind, not because they have higher demand for public goods or good governance.

Although many public programs may function in part through salience effects, this interpretation is difficult to sustain in the present context. First, there was on average a 6-8 month gap between tax collection and the forms of participation measured as outcomes. Salience and priming effects are unlikely to persist this long. Second, although townhall invitations did mention taxation as a subject of the meetings, the evaluation forms made no mention of taxation. The treatment

effects on this outcome — and the survey outcomes in Table IV — thus could not plausibly stem from the salience of taxation. Finally, the idea that citizens respond to taxation by demanding better governance *in exchange for taxation* is in fact the theory this paper seeks to test. It is this process of “tax bargaining” that is thought to trigger the evolution of more inclusive and responsive governance (Bates and Lien, 1985). The exchanges between citizens and government during townhall meetings are thus consistent with the tax-bargaining hypothesis.

## 5.4 Further analyses

### 5.4.1 Crowd out of local participation

The tax-bargaining literature hypothesizes that citizens will demand more of a voice *in the government that seeks to tax them*, i.e. the provincial government in this case. But could an increase in participation with the provincial government crowd out participation at other levels of government?

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 in the index *Engagement with national politics*. A separate index, *Interest in politics*, combines questions about political news consumption and knowledge, a chance to learn information about the national government, and hypothetical questions about the role of citizens in politics. To measure local engagement, the survey asked about views of and engagement with city chiefs, local notables with two main responsibilities: (1) organizing weekly *salongo*, an informal tax in which citizens contribute labor toward local public goods, such as maintaining neighborhood roads (Olken

and Singhal, 2011); (2) mediating local disputes to avoid escalation to the formal court system. I combine all questions about city chiefs in the index *Engagement with local city chiefs*.

There are no detectable differences in national political engagement or interest in politics across treatment and control (Table A12). Column 3, however, suggests that the tax campaign crowded out participation at the local level. Treated individuals report fewer consultations with city chiefs as well as diminished views of their quality (Figure A27). The effect is more pronounced among relatively poorer individuals. Although only suggestive, this result has an intuitive interpretation. 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. Formal taxation may crowd out informal labor taxation like *salongo*. This result supports the view that building the state can undermine local, informal forms of governance (Cheema et al., 2006).

#### 5.4.2 Spillovers

Because of the cluster-level randomization, I can estimate spillover effects of the tax campaign following Miguel and Kremer (2004). Specifically, I exploit random variation in (a) the number of treated neighborhoods adjacent to control neighborhoods (controlling for the number of total adjacent neighborhoods), and (b) the length of control neighborhoods' borders shared with treatment neighborhoods (controlling for the total length of each control neighborhood's borders).

There is evidence of spillovers in reported visits from tax collectors in control (Table A13). This is not surprising due to the lack of clear on-the-ground markers



between some neighborhoods. However, there is not a detectable in tax compliance in control. There is also no statistically significant spillover on participation. However, the estimates are consistently positive and of non-trivial magnitude, so it is possible I am underpowered in this analysis. In fact, the presence of spillovers on participation but not payment would be consistent with the mechanism proposed in Section 6.2, through which the tax campaign sent a signal of state capacity that raised the expected benefits of participation.<sup>48</sup>

### 5.4.3 Persistence of the treatment effect

Will the increase in participation persist over time? As noted, the average time gap between tax collection and participation is 6-8 months, so the main estimates already demonstrate persistence. Moreover, I exploit the variation in this time gap to estimate decay formally. This variation is random because the order in which both collectors and enumerators worked in neighborhoods were random.

Figure A17 shows the estimated treatment effect after taking quartiles of the data according to the lag between tax collection and participation. The treatment effect becomes smaller over time, but this decay is only marginally significant. Specifically, the difference in 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 ( $t = 1.79$ ). Thus, although there appears to be a slight decline in the impact of the tax campaign over time, the degree of persistence is perhaps the more surprising implication given that a tumultuous period of political uncertainty (including the cancellation of the 2016 national election) and civil conflict occurred between tax collection and participation.

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<sup>48</sup>Such spillovers would bias the treatment effect toward zero.

## 6 Mechanisms

This section examines possible mechanisms behind the increase in participation caused by the tax campaign: (1) individual *payers* in treated neighborhoods participated at higher rates because they expected reciprocal benefits or derived greater expressive utility from voicing their grievances; (2) the tax campaign sent a signal of state capacity that raised the expected benefits of participation.<sup>49</sup> Although the evidence in this section is more suggestive, it supports the second mechanism.

### 6.1 Tax payment as the driver of participation

Some scholars assume that *taxpayers* are those who participate more in response to government efforts to collect taxes. Tax payment could stimulate a sense of ownership over public revenues, leading taxpayers to expect public goods and better governance as a *quid pro quo* (Prichard, 2015). Alternatively, tax payment could trigger participation through a behavioral response akin to an endowment effect or a version of the sunk cost fallacy (Martin, 2014).

A naive test of these mechanisms is to examine whether payers participated more than nonpayers in treated neighborhoods.<sup>50</sup> Although payment is an 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

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<sup>49</sup>A third possible mechanism is that the tax campaign lowered the coordination costs of participation by stimulating common grievances and communication. I discuss (and find meager evidence for) this mechanism in Section A5.

<sup>50</sup>Comparing payers to non-payers 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 in control is effectively zero.) A less complicated comparison is compliers to never-takers in treatment neighborhoods only, shown in Table A14.

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 were no more likely to participate compared to nonpayers in treatment neighborhoods (Table A14).

Similarly, one can 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 likely due to idiosyncratic human error.<sup>51</sup> A mechanism operating through payment would imply no difference in participation between nonpayers who were and were not visited. On the other hand, a signaling mechanism (discussed in the next section) would predict differences. As shown in Table A14, there is a significant positive association between participation and tax collector visits. These correlations suggest that the mechanism operates through the experience of collector visits rather than tax payment per se.

A more rigorous test of payment-based mechanisms requires an instrument for endogenous tax payment. Assignment to treatment is an obvious candidate, but the exclusion restriction would be violated given that 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 instruments for two endogenous regressors

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<sup>51</sup>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 documenting each house in the property register, so they had little incentive to skip houses. To reinforce this interpretation, I compare the coordinates taken during the collectors' property registration survey to coordinates from the midline survey, conducted by enumerators with greater experience using GPS devices, to identify unvisited properties. This analysis does not reveal a pattern consistent with collectors deliberately skipping certain houses, i.e. those that are larger and might pay bigger bribes (Figure A20). Instead, unvisited houses appear idiosyncratically distributed, indicative of human error. To be conservative, I also instrument for visits.

— 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 campaign captured by  $I^{Visited\ only}$ .

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

A common pitfall of IV analysis with multiple endogenous variables is reliance on instruments that identify the same endogenous regressor, leaving the other regressor unidentified (even if the joint first-stage  $F$ -stat is large). Fortunately, one can construct  $F$ -stats for each endogenous variable independently, thereby verifying that both regressors are separately identified by the instruments (Angrist and Pischke, 2008, pp. 217-218). In the tables that follow, these  $F$ -stats will be reported as “AP  $F$ -stat.”

I thus construct leave-one-out jackknife IV (JIVE) 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.

1. 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  $j$ .
2. Take a linear combination of the collector-specific fixed effects to construct a

neighborhood-level instrument, i.e.

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

where  $\delta_i$  weights the collector-specific fixed effects.<sup>52</sup>

I construct JIVE instruments for both endogenous variables: *Visit propensity* for  $I^{Visited\ only}$ , and *Payment propensity* for  $I^{Visited\ and\ paid}$ . The logic of this strategy 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. Figure A18 plots, for each tax collector who worked on the campaign, the correlation between visits and payment, conditional on household covariates and stratum fixed effects. Despite the fact that collecting tax payments was impossible without visiting households (and thus the correlation cannot be negative), for only 38% of collectors is the correlation coefficient statistically different from zero. This considerable variation in the observed effort and effectiveness of tax collectors is reassuring for this estimation approach.

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 vary between 0 and 1 for treated neighborhoods depending on the predicted effort (or effectiveness) of the assigned collectors. Some neighborhoods are randomly assigned to a set of collectors likely to exert high effort; others are assigned to collectors likely to demonstrate high effectiveness. If these qualities are sufficiently uncorrelated, there should be a first stage for  $I^{Visited\ only}$

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<sup>52</sup>For simplicity, collectors are weighted evenly, though due to sick days and other factors some worked for more days than others.

and for  $I^{Visited\ and\ paid}$ .

Table V 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  $I^{Visited\ and\ paid}$  suggests that there are indeed valid instruments in both of the first stage equations.<sup>53</sup> Including enumerator fixed effects, as in all robustness checks (Section A4), further strengthens the first stage.

This 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 statistically indistinguishable from zero (and in fact negative). On the other hand, the coefficient on  $I^{Visited\ only}$  is consistently positive and often statistically significant. Its magnitude is 2-3 times the reduced-form effect of the campaign on participation. Ultimately, the large standard errors make this analysis only suggestive. But the most natural interpretation is that tax payment does not appear to have an effect on participation separate from the effect of being visited by tax collectors. Indeed, we can never reject equivalence of the coefficients on  $I^{Visited\ and\ paid}$  and  $I^{Visited\ only}$ . The available evidence is more consistent with a mechanism in which the campaign increased participation through collector visits rather than through tax payment.

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<sup>53</sup>The  $F$ -stat reported here is the standard joint test of the exogenous instruments; the individual Angrist-Pischke (AP)  $F$ -stats for 2SLS with multiple endogenous variables are reported in Table VI showing the second-stage results.

## 6.2 Tax collection as a signal of state capacity

One such mechanism is as follows: a first-ever tax campaign stimulates participation by sending a signal of state capacity that raises the expected benefits of participation. Citizens who observe the campaign update that the government has greater means and is more capable than previously thought. They therefore anticipate greater returns to engaging with members of the government, for example by advocating for more public spending in their neighborhood. This mechanism predicts higher participation among everyone in treatment neighborhoods — payers and nonpayers.

### 6.2.1 Decision-theoretical framework

Imagine there is a government and one citizen who is uncertain about the capacity of the government. The government sets a policy  $g(\theta, \lambda)$ , 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 monitor the government. The citizen incurs a cost  $c$  to participate, and receives utility  $u(g(\theta, \lambda))$  from the policy.

Government capacity ( $\theta$ ) is meant generally. It could be 'extractive capacity,' i.e. ability to collect taxes, or 'productive capacity,' i.e. ability to provide public goods and enforce contracts (Besley and Persson, 2009). A signal of either type of capacity triggers participation because the citizen believes the government will be more likely to affect his future well being — through tax collection or public goods provision — and thus he has an incentive to try to influence public policy to be as favorable as possible.

Concretely, the government can provide public goods, which increase the citizen's utility, and extract taxes, which decrease the citizen's utility. The citizen's preferred policy (high public goods, low taxes) results when the government is high capacity

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

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

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

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

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

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

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

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

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



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

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

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

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

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

This framework suits weak-state settings, such as the DRC, in which the government is effectively absent ex ante and thus a citywide tax campaign plausibly shocks citizens' beliefs about its capacity. The framework does not suit settings in

which citizens are habituated to being taxed and have high priors about the state's capacity. That said, a low-tax, low-capacity equilibrium characterizes the cases in early modern Europe that scholars draw on in theorizing the participation dividend of tax collection.<sup>54</sup>

### 6.2.2 Evidence

One implication of this mechanism is that the treatment effect should be larger in neighborhoods with less past exposure to the state. Where the state has been effectively absent, receiving a visit from government agents conducting a property register and collecting taxes should send a stronger signal of capacity compared to neighborhoods habituated to the state. Thus, in neighborhoods unaccustomed to the state, more individuals should update their beliefs beyond the threshold and choose to participate.

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. The former measure captures state activity in the neighborhood, while the latter captures respondents' exposure to the state outside of the neighborhood. I use neighborhood estimates of these variables and split the sample at the median. 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

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<sup>54</sup>Indeed, state capacity is thought to have emerged only when rulers were forced to systematize tax collection, often when facing foreign threats (Tilly, 1985). In contrast to prior modes of revenue mobilization (land rents, tax farming, sales of venal offices), broad-based tax collection required a professional bureaucracy run by full-time skilled workers as well as information about taxable actors in the economy. Brewer (1990) describes how meritocracy emerged first in the British state's tax department, which came closer to "Max Weber's idea of bureaucracy than any other government agency in eighteenth-century Europe" (p. 66)

neighborhoods. These results are consistent with the idea that citizens update their beliefs more — and thus participate more — when they are less accustomed to the presence of the state.

As a second form of evidence, I show that citizens’ self-reported beliefs about the capacity of the government shift in response to the tax campaign. I estimate  $q$  using survey data. Following Besley and Persson (2009), 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 (2019) 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 information about taxpayers and a credible threat of punishment for evasion. As measures, I thus use two survey-based indices.<sup>55</sup>

- (a) *Information about citizens*: increasing in how much information the government is perceived to possess about citizens (e.g. household location, compliance status, occupation, income).
- (b) *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 support of the tax ministry and confidence in its collectors. I measure this with the following indices.

- (a) *Performance of tax ministry*: increasing in citizens’ overall trust in and approval of the provincial tax ministry.

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<sup>55</sup>See Section A6 for details on all variables.

- (b) *Integrity of tax collectors*: increasing in the perceived amount of money collected in property taxes that will reach state coffers.
- (c) *Perceived citizen compliance*: increasing in the share of other households whom respondents think paid the property tax in 2016. This approximates the state’s de facto extractive capacity.

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 ability to control high-level corruption and spend prudently. I examine the following survey-based measures.

- (a) *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.
- (b) *Performance of government*: increasing in citizens’ trust in and approval of the provincial government in general.
- (c) *Integrity of government*: increasing in the perceived share of tax revenues that will be spent on public services or other ‘good uses’ and not lost to high-level corruption or misallocation.

Table VIII summarizes estimations of Equation 1 using each of these variables as the outcome. Concerning extractive capacity (Panel I), the tax campaign increased citizens’ perceptions about how much information the government possesses about citizens, especially the locations of their properties and their tax compliance status.<sup>56</sup> But it did not substantially impact beliefs about the credibility of punishment for

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<sup>56</sup>This inference is correct: the government did build a database with detailed information about potential taxpayers that it can use to collect more tax in the future.

evasion (though I may be underpowered to detect a small effect). Panel II shows that treated citizens updated their beliefs substantially about the government’s ability to generate voluntary compliance. At endline, they viewed the tax ministry more positively and had more confidence in its collectors, thinking a greater share of taxpayer money would be deposited in the state account (rather than staying in collectors’ pockets). Importantly, citizens in treated neighborhoods also perceived much higher levels of citizen compliance with the property tax. In other words, they updated about the de facto extractive capacity of the government.<sup>57</sup>

It might at first appear counterintuitive that citizens updated *positively* about the government tax apparatus after it started taxing them — especially among households that *evaded* the tax.<sup>58</sup> But these results must be interpreted in the context of a weak and essentially absent state, in which government agents are seldom observed doing meaningful work. In such a setting, receiving home visits from tax collectors facile with mobile technologies, being assigned a tax ID number and entered into a government database, and being solicited to pay a formal tax is likely to send a stronger signal about the government than is the fact that this year they managed not to pay.

Panel III explores whether treated citizens think the government of capable of more public goods provision. They do not appear to update about the *technology* of

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<sup>57</sup>The fact that treated citizens perceive higher tax compliance but not a greater probability of punishment suggests that they expected others to pay taxes due to non-pecuniary motivations (Luttmer and Singhal, 2014; Besley, 2019), such as the enhanced legitimacy of the tax apparatus. As a companion paper shows (Weigel, 2018), citizens were in fact correct in believing that non-pecuniary factors motivated payment in this setting.

<sup>58</sup>It is possible that this pattern of belief changes simply reflects the fact that payers convinced themselves that tax collectors were trustworthy after they paid, an example of ex post motivated reasoning. Because there were more payers in treatment, such motivated reasoning could explain the average effect. However, re-estimating Table VIII with only nonpayers returns similar results (Table A15), albeit with slightly smaller coefficients. Nonpayers clearly also drew the same inferences as a result of the tax campaign, making a motivated reasoning interpretation unlikely.

public goods provision: the treatment did not lead citizens to think the government could now build a road more efficiently. Treated citizens also did not evaluate the government as a whole more positively, as they did the tax ministry. However, they did update about the *share* of tax revenues that would go to public goods spending or other good uses (*Integrity of government*). This result mirrors the higher confidence among treated citizens that collectors would deposit tax money to the state. Updating about the integrity of the government implies that, conditional on the same public goods provision technology, treated citizens did perceive the government to have somewhat greater productive capacity after the tax campaign.

In sum, the tax campaign caused citizens to believe that (i) the government had more revenue due to greater extractive capacity, and (ii) that it would spend more of that revenue productively. These results are consistent with a mechanism by which the tax campaign signals capacity and raises citizens' expectations about the benefits of participation.<sup>59</sup> They are also 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

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<sup>59</sup>A different interpretation is that citizens participated to try to access patronage goods rather than to try to influence public goods provision. Although *g* encompasses any public and private goods that the government distributes, 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 forms 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 (Figure A19). That said, this interpretation of greater *patronage capacity* is consistent with the theoretical framework, and I cannot rule it out entirely.

participators and non-participators in treatment neighborhoods (Table A16). Although these comparisons are not identified, they can nonetheless help to interpret the average effects on beliefs in Table VIII. Participators' beliefs diverge meaningfully from the average in three ways. First, they were more likely to believe the state would punish tax evasion compared to non-participators.<sup>60</sup> Second, participators in treatment were less confident than non-participators that tax collectors would not simply pocket the money they collected. Similarly, they were also less confident that tax revenues would fund public goods and not be wasted or embezzled.

Thus, while participators in treatment also updated about state's extractive capacity and total revenues, they appear to have been more concerned about the *uses* of tax money compared to non-participators. This pattern of correlations has an intuitive interpretation. Observing the tax campaign caused citizens to update about the size of the budget and the potential for public spending, creating an incentive for participation. This incentive was offset for some by confidence that the government would spend the money productively (i.e. provide  $g^+$ ) even without citizen monitoring. Only those who were less confident about government spending chose to participate in order to monitor and influence spending toward their preferred policy. "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."<sup>61</sup>

If updating about state capacity is the right mechanism, then how important is the *tax* component of this campaign? Would we expect the same outcomes if instead citizens received home visits providing information about new government

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<sup>60</sup>Moreover, participators in treatment think the probability of punishment for evasion is nearly 0.3 standard deviations more likely compared to individuals in control.

<sup>61</sup>"I ask that the government show the population what it achieves with this money," wrote another individual.

programs or the amount of tax revenues? The study design provides direct evidence on this question because, before the campaign, every fifth house in Kananga was visited and provided with an informational flier about government taxation, with no tax solicitation (see p. 13). Households in control that received fliers were no more or less likely to participate than other households (Section 5.3.2). This result suggests that, compared to purely informational visits, having an agent of the state solicit taxes sends a stronger and more credible signal about the will and capacity of the government to increase future tax enforcement.<sup>62</sup>

## 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 investigate the supposed participation dividend of tax collection, finding that citizens in taxed neighborhoods were nearly 5 percentage points more likely to attend a townhall meeting or to submit a government evaluation. Participating individuals demanded more public goods and a more inclusive government. The evidence is consistent with a mechanism through which tax collection sent a signal of state capacity that raised the expected

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<sup>62</sup>It is also worth noting that the specific ways in which citizens updated their beliefs and the comments they raised during townhall meetings reflect the experience of being asked by the state to pay formal taxes. Regarding beliefs, treated citizens perceived the government to have more information about citizens, a higher performing tax ministry, higher citizen tax compliance, and less leakage of taxpayer money. In contrast, citizens did not update about the technology of public goods provision or the overall performance of the government. Thus, citizens appear not to have blanket updated their beliefs about the government, but rather they updated in *specific ways that map closely to the mechanics of the tax campaign*. Moreover, at townhall meetings, almost all of citizens' comments were demands for better governance *in exchange for taxes* (Figure A11). Consistent with the logic of tax bargaining, citizens appear to have used the fact that they had been solicited for taxes to demand public goods and a voice in politics. It is unlikely townhalls would have taken this form absent tax collection.



benefits of participation.

One implication of this study is that the political response to tax collection may be disproportionately large relative to the increase in tax revenues. Even if the state only succeeds in marginally raising revenues, its efforts to do so may trigger a large increase in citizen participation due to the signal of state capacity sent by an increase in tax collection/enforcement. However, the political response may also be larger for the property tax than for indirect and less visible taxes, such as consumption taxes that pass through to consumers in the form of higher prices. These observations could thus explain why developing countries collect so little revenue in property taxes, despite the theoretical advantages of property taxation: forward-looking governments may anticipate a large political response from taxing property and choose less visible tax instruments instead.

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## 8 Tables and figures

**Table I: Activities of collectors and enumerators**

| Activity              | Treated | Control | Timing       | N      | J   |
|-----------------------|---------|---------|--------------|--------|-----|
| <b>Tax collectors</b> |         |         |              |        |     |
| Property register     | Yes     | No      | Apr-Dec 2016 | 20,902 | 253 |
| Tax collection        | Yes     | No      | Apr-Dec 2016 | 20,902 | 253 |
| <b>Enumerators</b>    |         |         |              |        |     |
| 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 |

N = number of observations, J = number of clusters. Note that the property register contains information on all compounds, including (exempted) government buildings, churches, and empty lots, while the midline survey is only conducted with privately owned plots liable for the property tax. The endline has fewer clusters due to the insecurity in Kananga, discussed on p. 16.

**Table II: Effects of the campaign on collector visits and compliance**

|              | Visited by tax collector<br>(1) | Paid property tax<br>(2) |
|--------------|---------------------------------|--------------------------|
| Campaign     | 0.815***<br>(0.013)             | 0.103***<br>(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 collector* 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. 17 for details on these variables. Data from Nganza are excluded (see p. 16). Data: midline survey merged with government tax database.

**Table III: Effects of the campaign on participation**

|                | Townhall<br>meeting<br>attendance<br>(1) | Evaluation<br>form<br>submission<br>(2) | Townhall<br>or<br>evaluation<br>(3) | Townhall<br>and<br>evaluation<br>(4) | Costly<br>participation<br>index<br>(5) |
|----------------|--|---|-------------------------------------|--------------------------------------|---|
| Campaign       | 0.042**<br>(0.020)                       | 0.024**<br>(0.012)                      | 0.047***<br>(0.016)                 | 0.027***<br>(0.009)                  | 0.139***<br>(0.043)                     |
| Covariates     | Yes                                      | Yes                                     | Yes                                 | Yes                                  | Yes                                     |
| Stratum FE     | Yes                                      | Yes                                     | Yes                                 | Yes                                  | Yes                                     |
| $R^2$          | 0.065                                    | 0.055                                   | 0.068                               | 0.039                                | 0.071                                   |
| Observations   | 1934                                     | 2912                                    | 2913                                | 2913                                 | 2913                                    |
| Clusters       | 252                                      | 356                                     | 356                                 | 356                                  | 356                                     |
| Control Mean   | 0.18                                     | 0.1                                     | 0.18                                | 0.035                                | -0.057                                  |
| Dep. Var.      | Binary                                   | Binary                                  | Binary                              | Binary                               | Standardized                            |
| Rand. Inf. $p$ | 0.044                                    | 0.056                                   | 0.0050                              | 0.0040                               | 0.0012                                  |

*Townhall meeting attendance* is an indicator variable that equals 1 if a participant attended a townhall meeting. *Evaluation form 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 form. *Townhall and evaluation* indicates that a participant attended a townhall meeting and submitted an evaluation form. *Costly participation index* is a standardized index of *Townhall meeting attendance* and *Evaluation form submission*. See p. 18 for details on these variables. Covariates include gender, age, age squared, wealth, a business owner dummy, and a dummy for public lighting in the neighborhood, as discussed on p. 20. Section A4 shows other covariate regimes. The last row shows randomization inference  $p$ -values. Data: endline survey merged with townhall attendance and submitted evaluation records. The sample size is smaller in Column 1 because the government discontinued townhalls after April 1 due to insecurity in Kananga. Endline respondents sampled after this date never had a chance to attend a meeting (see p. 17).



**Table IV: Effects of the campaign on the perceived responsibility of the provincial government to provide public goods in Kananga**

|              | Responsibility of the provincial government<br>in public goods provision |                  |                    |
|--------------|--|------------------|--------------------|
|              | (full index)   | (sector-based)   | (hypotheticals)    |
|              | (1)  | (2)              | (3)                |
| Campaign     | 0.114**<br>(0.051)   | 0.087<br>(0.053) | 0.097**<br>(0.041) |
| Covariates   | Yes  | Yes              | Yes                |
| Stratum FE   | Yes  | Yes              | Yes                |
| $R^2$        | 0.042  | 0.043            | 0.028              |
| Observations | 2913   | 2813             | 2900               |
| Clusters     | 356  | 356              | 356                |
| Control Mean | -0.066   | -0.051           | -0.053             |

All outcomes are standardized indices increasing in the perception that the provincial government should be the primary provider of public goods in Kananga. The outcome in Column 1 is an aggregate index. The outcome in Column 2 is based on sector-specific questions about the government's responsibility relative to other possible providers (national government, NGOs, etc). The outcome in Column 3 is based on hypothetical survey questions about the role of the provincial government in service provision. See p. 19 for details on these variables. Data: endline survey.

**Table V: IV - First stage: Predicting visits and tax payment using randomly assigned tax collector effort and effectiveness**

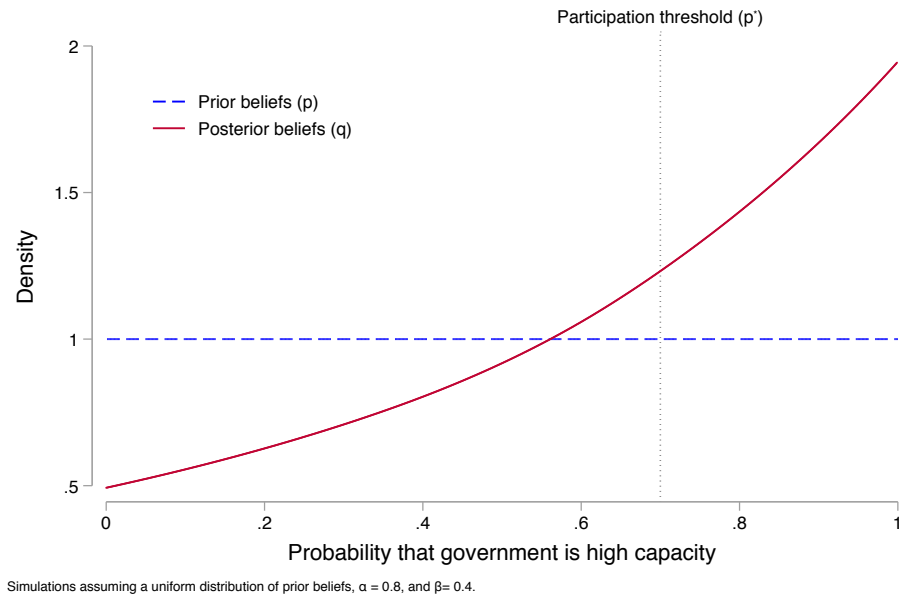
|                           | Visited only        |                     | Visited and paid    |                     |
|---------------------------|---------------------|---------------------|---------------------|---------------------|
|                           | (1)                 | (2)                 | (3)                 | (4)                 |
| Payment propensity (JIVE) | 0.767***<br>(0.076) | 0.709***<br>0.073   | 0.267***<br>(0.041) | 0.287***<br>(0.040) |
| Visit propensity (JIVE)   | 0.361***<br>(0.127) | 0.443***<br>(0.119) | -0.116*<br>(0.064)  | -0.155**<br>(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               |
| Observations              | 2913                | 2913                | 2913                | 2913                |
| Clusters                  | 356                 | 356                 | 356                 | 356                 |
| Dep. Var. Mean            | 0.487               | 0.487               | 0.065               | 0.065               |
| $F$ -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 (JIVE)* 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 (JIVE)* 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. 35 for details about these instruments. Data: endline survey merged with government tax database.

**Table VI: IV - Second stage: Distinguishing the effects of collector visits and tax payment on participation**

|                                 | Townhall meeting attendance |         | Evaluation form submission |         | Townhall or evaluation |         | Townhall and evaluation |         | 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 III. As in Table V, *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 (see p. 35).  $F$ -test (equivalence) reports the  $p$  value for tests for equivalence of the coefficients on *Visited only* and *Visited and paid*. Data: endline survey merged with government tax database as well as townhall attendance and submitted evaluation records. The sample size is smaller in Columns 1-2 for the same reason noted in Table III: endline respondents sampled after April 1 did not have an opportunity to attend a townhall meeting because government discontinued these meetings due to insecurity in Kananga.



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

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

|                                | Townhall or evaluation |                     |                     |
|--------------------------------|------------------------|---------------------|---------------------|
|                                | (1)                    | (2)                 | (3)                 |
| Campaign                       | 0.047***<br>(0.016)    | 0.061***<br>(0.023) | 0.080***<br>(0.021) |
| Campaign X Past visits (high)  |                        | -0.030<br>(0.033)   |                     |
| Past visits (high)             |                        | 0.031<br>(0.024)    |                     |
| Campaign X Past protest (high) |                        |                     | -0.073**<br>(0.034) |
| Past protest (high)            |                        |                     | 0.035<br>(0.022)    |
| Covariates                     | Yes                    | Yes                 | Yes                 |
| Stratum FE                     | Yes                    | Yes                 | Yes                 |
| $R^2$                          | 0.068                  | 0.069               | 0.070               |
| Observations                   | 2913                   | 2913                | 2913                |
| Clusters                       | 356                    | 356                 | 356                 |
| Control Mean                   | 0.18                   | 0.18                | 0.18                |
| $F$ -test $p$ -value           |                        | 0.013               | 0.00074             |

The outcome is the same as Column 3 in Table III. *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. See p. 41 for further details about these variables. Data: endline survey merged with participation records and neighborhood-level measures from baseline survey.

**Table VIII: Effects of the campaign on citizens' beliefs about the extractive and productive capacity of the provincial government**

| <i>Dependent variable</i>                                   | $\beta$  | SE    | $R^2$ | N    | $\mu_c$ |
|---|----------|-------|-------|------|---------|
| <i>Panel I: Extractive capacity - coercive compliance</i>   |          |       |       |      |         |
| Information about citizens                                  | 0.147*** | 0.044 | 0.086 | 2910 | -0.080  |
| Ability to punish evaders                                   | 0.046    | 0.047 | 0.044 | 2883 | -0.017  |
| <i>Panel II: Extractive capacity - voluntary compliance</i> |          |       |       |      |         |
| Performance of tax ministry                                 | 0.123*** | 0.047 | 0.065 | 2791 | -0.076  |
| Integrity of tax collectors                                 | 0.188*** | 0.044 | 0.043 | 2732 | -0.119  |
| Perceived citizen compliance                                | 0.343*** | 0.052 | 0.100 | 1954 | -0.179  |
| <i>Panel III: Productive capacity</i>                       |          |       |       |      |         |
| Ability to provide public goods                             | -0.013   | 0.053 | 0.038 | 2484 | 0.009   |
| Performance of government                                   | 0.043    | 0.049 | 0.042 | 2795 | -0.030  |
| Integrity of government                                     | 0.109**  | 0.050 | 0.054 | 2766 | -0.062  |

Each row summarizes an OLS estimation of Equation 1, with the dependent variable noted in the first column.  $\beta$  is the coefficient on the treatment indicator, followed by the cluster-robust standard error,  $R^2$ , number of observations, and control group mean. There are 356 clusters. Each dependent variable, described briefly on p. 42 and in detail in Section A6, is standardized to facilitate interpretation of coefficient magnitude. Data: endline survey. The number of observations varies across regressions due to non-response for specific survey questions.