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

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This article 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 356 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 11.6% in treatment. It also increased political participation by about 5 percentage points (31%): citizens in taxed neighborhoods were more likely to attend town hall meetings hosted by the government or submit evaluations of its performance. To participate in these ways, the average citizen incurred costs equal to their daily household income, and treated citizens spent 43% more than control. Treated citizens also positively updated about the provincial government, perceiving more revenue, less leakage, and a greater responsibility to provide public goods. 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 states. *JEL* Codes: H20, P48, D73.

I. INTRODUCTION

With nearly half of the world's extreme poor, fragile states pose many of the greatest development challenges.[1] The origins of state capacity have thus become a focus of recent theoretical and empirical work.[2] The ability to tax, the "hallmark of the state," 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 article tests if taxation increases participation in fragile states by examining the randomized rollout of the first large-scale citizen tax campaign in Kananga, Democratic Republic of Congo (DRC).

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, it triggered new demands for public goods and representation (Schumpeter 1918; Tilly 1985).[3] 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.[4] "In exchange for the greater say in government... [citizens] agreed to provide sufficient tax revenue" (North and Weingast 1989, 817). The slogan "no taxation without representation" captures the intuition.

Given this potential for taxation to promote inclusive governance, donors and policy makers increasingly support domestic revenue mobilization in poor countries with weak and unaccountable states, which collect around 10% of GDP in tax compared with 40% in developed countries.[5] However, we lack rigorous

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1. See, for example, the 2011 World Development Report (World Bank 2011).

2. On theory, see Besley and Persson (2009), Fukuyama (2011), Acemoglu and Robinson (2019); on empirics, see Sanchez de la Sierra (2020), Dittmar and Meisenzahl (2020), Cantoni, Mohr, and Weigand (2019).

3. Faced with rising costs of war, rulers could no longer rely on traditional revenue sources (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, Mohr, and Weigand 2019).

4. Bates and Lien (1985), Hoffman and Rosenthal (1997), Moore (2004), and Prichard (2015) theorize tax bargaining, and Brautigam, Fjeldstad, and Moore (2008) discuss a broader governance dividend of taxation.

5. For instance, the Organisation for Economic Co-operation and Development noted "Tax reforms can ... enhance accountability between citizens and the

evidence on the causal link between tax collection and participation because it is seldom random who is inside and outside the tax net. It is also not obvious that citizens would choose to engage more with a state seeking to tax them. Citizens might prefer to evade quietly, or move elsewhere (Tiebout 1956; Scott 2017).

I investigate the supposed participation dividend of tax collection in a weak state seeking to register and tax the majority of its citizens for the first time. In 2016, the Provincial Government of Kasaï Central launched the first citywide citizen tax campaign in the city of Kananga in an effort to raise property tax revenues. To facilitate evaluation, the government randomized the rollout of the campaign across the city's 356 neighborhoods (covering more than 27,000 properties). In treated neighborhoods, tax collectors went door to door (i) registering property owners and assigning unique tax ID numbers (written on a house wall); and (ii) making in-person appeals for the property tax, which they collected on the spot, issuing printed receipts to payers. Control neighborhoods remained in the old "declarative" system: property owners were in theory meant to go to the tax ministry to pay, but compliance was near zero because the government had never before tried to enforce the tax beyond a handful of commercial properties downtown.[6]

I estimate the reduced-form impact of this campaign—that is, 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, the campaign is a treatment bundle of theoretical and policy interest.[7]

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state" (OECD 2010, 3). Similarly, the International Monetary Fund wrote "Bringing small businesses into the tax net can help secure their participation in the political process and improve government accountability" (IMF 2011, 40). The UK Department for International Development (DFID) (2016) shows expanding donor support in this area. Pomeranz and Vila-Belda (2019) review key lessons from recent work on domestic revenue mobilization.

6. Before the campaign, every fifth property in control (and treatment) received fliers providing information about property tax collection. The control group is thus not a "pure" control but the relevant control because the fliers hold constant information about the campaign and thus help isolate the effect of being registered and solicited for taxes by the state for the first time.

7. The importance of information as a precondition of tax enforcement is emphasized in accounts of historical state building (Brewer 1990; Ertman 1997; Scott 1998) as well as recent literature in public finance and development (Kleven et al. 2011; Pomeranz 2015).

I first examine if the campaign achieved the government's goal of registering taxpayers and raising compliance. Despite the state's low capacity, the campaign increased reported visits from tax collectors by 81.5 percentage points and increased taxpayer registration by 78.8 percentage points. It raised property tax compliance by more than 11 percentage points, from 0.1% in control to 11.6% in treatment. There was no corresponding increase in reported bribes, and the overall level of bribes was very low according to multiple measures. The campaign thus represents a major broadening of the tax base and a large expansion of the presence of the formal state in Kananga. The hundredfold 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.[8] The provincial government evidently viewed the campaign as a success, choosing to continue field-based property tax collection after 2016.

I use the random assignment of this tax 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.[9] First, the government hosted a series of town hall meetings, in which officials and citizens discussed taxation and public spending in Kananga. Second, citizens could submit anonymous evaluations of the provincial government to a drop box downtown, and the contents were shared with the governor and other top officials. Attending a town hall or submitting an evaluation exhibits willingness to incur costs to have a voice in the government. Indeed, according to estimated transport costs and opportunity costs of time, the average participating individual spent roughly their daily household income to participate in these ways.

The tax campaign increased participation according to both measures: residents of treated neighborhoods were about 5 percentage points more likely to attend a town hall meeting or to submit an evaluation, a 31% increase relative to control. I

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8. 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, Ali, and Goodfellow 2017). Moreover, property tax receipts are typically much lower outside of national capitals.

9. This approach is similar to that of Olken (2007), Casey, Glennerster, and Miguel (2012), and Paler (2013).

estimate that the average citizen in treatment incurred 43% higher participation costs compared with control. The percent increase in costs is greater than the extensive-margin increase in participation because treated citizens also participated more on the intensive margin.[10] Consistent with historical accounts, town hall participants demanded better public infrastructure and a more responsive government in exchange for taxes. Submitted evaluations similarly demanded more transparency, inclusiveness, and public goods spending. The treatment effects on participation reflect an average time gap between tax collection and outcome measurement of eight months and show no sign of decaying over time.

I provide evidence inconsistent with several alternative explanations of the increase in participation. First, familiarity with and trust in the research team are balanced across treatment and control, mitigating concerns of experimenter demand. Second, the treatment effect does not appear to have been caused by a decrease in participation in control—which could arise if, say, households in control anticipated but never received visits from tax collectors[11]—rather than an increase in treatment. Third, the increase in participation does not appear to reflect a sense of unfairness stemming from awareness of the control group, which had not received tax collectors when outcomes were measured. Finally, the substance of citizens' comments at town halls as well as the treatment effects on evaluation submission make it unlikely that treated citizens simply had more factual questions about taxation or participated more due to salience effects.

In addition to the effects on participation, treated citizens also positively updated their self-reported beliefs about the responsibilities, capacity, and integrity of the provincial government. Consistent with higher demand for good governance, treated citizens viewed the provincial government as having more of a responsibility to provide public goods across a range of sectors (relative to the national government, nongovernmental organizations [NGOs], and other potential providers). In fact, mirroring these heightened perceived obligations of the provincial government,

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10. People who both attended a town hall and submitted an evaluation were 77% more common in treatment than control. See Section IV.B for details on estimating the costs of participation.

11. To test for this type of disappointment effect, I examine participation among flier recipients in control (Section V.C).

treated citizens also reported less engagement with city chiefs (local public good providers), implying the formal state and local forms of governance may be substitutes (Cheema, Khwaja, and Qadir 2006). Alongside the increase in perceived responsibility to provide public goods, treated citizens updated positively about the extractive capacity of the provincial government. They viewed the government as having more information about potential taxpayers, a higher-performing tax department, and overall higher tax revenues. At the same time, they perceived less leakage in tax collection and in government spending. These effects on beliefs about the government further illustrate how expanding the tax net can instill in citizens the sense of an incipient social compact with the state.

Although the main contribution of the article is the reduced-form estimates of a participation dividend of tax collection, I also provide more suggestive evidence on three potential mechanisms. First, according to an entitlement mechanism, taxation increases participation because taxpayers expect reciprocal benefits or exhibit an endowment effect that leads them to participate more (Martin 2014). According to this mechanism, payers should participate more than nonpayers. However, there is no correlation between payment and participation in treatment, according to OLS and IV estimates leveraging the random assignment of tax collectors to neighborhoods and randomly assigned collector bonuses. Rather, what correlates with participation in treatment is being registered as a taxpayer, which is more consistent with the next two mechanisms.

An updating mechanism suggests that tax collection sends a signal of state capacity, which causes citizens to update their beliefs about the government and expect higher returns to participation.[12] The treatment effects on beliefs about the government's fiscal capacity support this mechanism. Also, the increase in participation caused by the campaign is more pronounced in neighborhoods with less past exposure to the formal state—where the signal sent by the campaign would have been stronger. However, there is little evidence of spillovers on participation in control, which are implied by this mechanism. Moreover, households who received more visits from collectors—who presumably received a

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12. This mechanism is similar in spirit to Coate and Morris (1995), who model the informational aspects of public projects.

stronger signal of state capacity—did not participate more than those visited only once.

Finally, according to a bargaining mechanism, when the state starts to solicit citizens for taxes, it gives them a bargaining chip they can use to demand better governance.[13] In Kananga, being registered by the state as a taxpayer but not paying correlates with participation, a fact that evokes a bargaining process in which citizens demand better governance before complying with taxes. Text analysis reveals that most of citizens' comments at town halls were demands for more public goods or more inclusive politics in exchange for future tax compliance. "Erosion threatens our neighborhoods, and the government does nothing," said one person, "so why should we pay?" Written-in comments on evaluations follow a similar pattern. The fact that the tax campaign also increased what citizens deemed the government's responsibilities in surveys reinforces the sense that they participated to bargain for a better fiscal deal. However, that any taxpayers participated is at odds with a pure bargaining channel, since they could not credibly threaten noncompliance. Ultimately, the evidence is most consistent with bargaining; but there is also evidence of updating, and the two need not be mutually exclusive.

This article tests classic theories positing a catalytic role of tax collection on citizen political engagement.[14] 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.[15] This approach is most similar to Khan, Khwaja, and Olken (2015) and Sanchez de la Sierra (2020) in collecting experimental (or quasi-experimental) data in

13. Bates and Lien (1985) and Hoffman and Rosenthal (1997) model tax bargaining.

14. Seminal accounts include Schumpeter (1918), Tilly (1985), Bates and Lien (1985), and North and Weingast (1989); more recent theoretical contributions include Moore (2004), Ross (2004), Paler (2013), Martin (2014), and Prichard (2015). Boucoyannis (2015) challenges the contractarianism implied by many accounts of tax bargaining, stressing the coercion immanent in taxation.

15. Past empirical work on the emergence of state capacity exploits European historical data on interstate military competition (Gennaioli and Voth 2015), fiscal centralization (Cantoni, Mohr, and Weigand 2019), access to external finance (Queralt 2019), legal training (Cantoni and Yuchtman 2014), and city laws about public good provision (Dittmar and Meisenzahl 2020).

developing countries to study hypotheses based on histories of now-rich countries' development. Although I do not claim that the results of this experiment should inform interpretations of historical European state building, these hypotheses about the role of taxation in the emergence of inclusive governance are relevant in many developing countries today that have low tax take, low citizen participation, and unaccountable governments (of which the DRC is a prominent case). In studying the link between taxation and governance, past studies have used lab experiments simulating taxation and participation,[16] survey experiments priming citizens about the share of taxes in government revenues,[17] or observational comparisons across or within countries.[18] A related literature explores how different sources of revenue (taxes, transfers, or natural resources) affect government spending and accountability.[19]

The article also contributes to the empirical literature on tax and development, which has chiefly examined middle-income countries,[20] with higher rates of initial tax compliance and state capacity, and has focused less on the political economy effects of increasing tax enforcement (Besley and Persson 2013).[21] Past work examines how governments can raise compliance through third-party reporting (Kleven et al. 2011; Pomeranz 2015; Carrillo, Pomeranz, and Singhal 2017; Jensen 2018; Naritomi 2019), tax collector incentives (Khan, Khwaja, and Olken 2015), providing information about enforcement or peer

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16. See, for example, Martin (2014); Sjursen (2018); Kao, Lu, and Queralt (2019); and Sjoberg et al. (2019).

17. See, for example, Paler (2013) and De la Cuesta et al. (2019b, forthcoming).

18. Related studies include Lieberman (2003); Ross (2004); Herb (2005); Haber and Menaldo (2011); Prichard (2015, 2018); Scheve and Stasavage (2016); Rodden (2016); Christensen and Garfias (2018); Gottlieb and Hollenbach (2018); and Meagher (2018).

19. Related studies include Ross (2001); Jensen and Wantchekon (2004); Robinson, Torvik, and Verdier (2006); Dunning (2008); Ramsay (2011); Caselli and Michaels (2013); Brollo et al. (2013); McGuirk (2013); Ferraz and Monteiro (2014); Borge, Parmer, and Torvik (2015); Timmons and Garfias (2015); Chen and Kung (2016); Gadenne (2017); and Martinez (2019).

20. Exceptions include recent and ongoing work in Ethiopia (Mascagni, Mengistu, and Boldeyes 2018), Nigeria (Bodea and LeBas 2016), Rwanda (Mascagni et al. 2016; Tourek 2019), Uganda (Almunia et al. 2019), and Sierra Leone (Jibao and Prichard 2016).

21. There is, however, evidence from developed countries of electoral payoffs from technologies that reduce tax evasion (Casaburi and Troiano 2015).

behavior (Del Carpio 2013; Pomeranz 2015), tax holidays (Dunning et al. 2015), and reducing bureaucratic barriers to compliance (Kleven and Waseem 2013; Best et al. 2015). The article contributes to this literature by demonstrating that a rudimentary intervention (in-person tax appeals) substantially increased tax receipts in one of the world's poorest countries.

II. SETTING

The DRC is the fourth most populous country in Africa and one of the five poorest in the world. Average monthly household income in the study site is roughly $106 (PPP $168). The country is often called a kleptocracy, due to the neopatrimonial regime of longtime president Mobutu Sese Seko (Young and Turner 2013), or a failed state because of its history of civil conflict (Stearns 2012). Its fiscal capacity, as proxied by the tax-GDP ratio, ranks 188 out of 200 countries for 2000–2017.[22]

Kananga is a city of roughly 1 million and the capital of Kasaï Central province. In 2015, total provincial tax revenues amounted to a paltry $0.23 per person in the province. As in many developing countries, the precampaign tax base was tiny: a clutch of formal firms as well as traders moving goods across provincial borders. Although there are many taxes on the books, most citizens had never been registered as taxpayers or solicited for formal taxes by the modern Congolese state before the 2016 campaign. At baseline, only 39% of people had even heard of the provincial tax ministry (see Table II). Less than 8% of individuals in control reported making "any informal or formal payments to the state" in 2016.[23] The lack of a broad tax base is a challenge to governments across the developing world (Gordon and Li 2009).

Property taxes are considered efficient, and urbanization in Africa is fueling rapid growth in real estate values, leading

22. Data available at https://data.worldbank.org/indicator/gc.tax.totl.gd.zs.

23. The translation for "state," *mbulamatadi*, signifies national, provincial, and city governments. The most common taxes citizens reported paying were market fees, vehicle authorizations, and nuisance taxes (that are unlikely to reach the government). About 75% of citizens also reported contributing labor at least once in 2016 to an informal tax (Olken and Singhal 2011) called *salongo*, in which citizens help maintain the streets and clean up after storms. Citizens also pay frequent user fees for education, water, and health care (Paler et al. 2016), though these are not, strictly speaking, compulsory levies. Very low rates of formal tax compliance have been recorded elsewhere in the DRC (Radley 2019, 82).

international experts to champion property taxation as "the single greatest opportunity for strengthening local revenue systems" (Moore, Prichard, and Fjelstad 2018, 152). Because constructing property valuation rolls can be difficult for low-capacity governments, many African municipalities use simplified size-based assessments or fixed-amount levies on properties under a certain threshold (Franzsen and McCluskey 2017). The Provincial Government of Kasaï Central has followed suit. Roughly 90% of property owners in Kananga face a fixed annual liability of 2,000 Congolese francs (CF), about $2, and roughly the median household's daily income.[24] The remaining 10% of property owners face a liability of 6,600 CF, if they live in midrange houses built of modern materials (i.e., not mudbricks), or a variable liability increasing in the property's size, if they live in villas, large compounds with a garage (1% of property owners). Prior to 2016, property owners were in theory supposed to visit the tax ministry themselves to pay. Except for a handful of commercial properties, the government had never tried to enforce the tax, and so compliance remained near zero.[25]

Why did the provincial government begin systematic collection of the property tax in 2016? The unanticipated 2015 *découpage* (administrative splitting) of the 11 old provinces into 26 new provinces meant that the government based in Kananga lost the diamond-rich territory of Tshikapa.[26] This led to a 40% drop in revenues, according to the finance minister.[27] Facing shortfalls, the governor turned to the property tax.

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 2017, and the official 2018 election outcome has been widely challenged (Englebert 2019).

24. Properties owned by state employees, churches, and the elderly are exempted.

25. Of the 216 noncampaign property tax payments in 2016, an estimated 90% were made by firms.

26. Although decentralization was noted in the 2006 constitution, its sudden implementation in 2015 was a surprise and a likely tactic of incumbent Joseph Kabila to undermine political rivals and justify postponing the 2016 elections, which he did (Englebert, Calderon, and Jené 2018).

27. Although the province's population shrank, too, the minister insisted that revenues per person in Kananga decreased substantially after the *découpage*, a testament to the urban bias in government spending in much of sub-Saharan Africa (Bates 2014).

Nonetheless, individuals in Kananga express demands to their leaders in two main ways. First, they hold neighborhood meetings about public-good failures and other political demands and then nominate a representative to bring the case before a provincial deputy. Civil society organizations, such as Société Civile du Congo (SOCICO) and Le Réseau Indépendant Anti-Corruption, also often organize meetings with citizens and government officials to discuss such issues.[28] Second, individuals or groups 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, the focus is a low-capacity autocratic government responding to an external shock that increased its need for revenues by broadening the tax base through property tax collection. In broad strokes, these contextual features mirror those in seminal accounts of the origins of the taxation-based social compact in early modern Europe (Schumpeter 1918; Tilly 1985).[29] These parallels further motivate investigating whether taxation has a participation dividend in the DRC.

III. 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. The unit of randomization, the neighborhood, was defined by partitioning a satellite map of the city into 431 neighborhoods that approximate *localités*, the lowest administrative unit in the city.[30] Neighborhood borders are typically preexisting boundaries like roads, ravines, or other features easily identifiable from the ground. Among the 431 neighborhoods 253 were selected randomly to receive the tax campaign in its first phase.[31]

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28. For example, SOCICO held a series of such meetings about the taxation of informal vendors in and around the main market in Kananga in recent years.

29. Even the use of property tax is similar: "In early modern Europe, monarchs seeking new sources of revenue to fight wars increased taxes on trade, on property, and on ordinary citizens (through various head, 'poll,' or hearth taxes). Yet only taxes on trade (customs duties), goods (excise duties) and fixed property had the potential to be increased significantly, and these increases were subject to consent, which in turn had to be negotiated" (Brautigam, Fjeldstad, and Moore 2008, 12).

30. Online Appendix Figures A1 and A7 show examples of neighborhoods.

31. More neighborhoods were assigned to treatment (i) because the government was eager to have revenues in 2016, and (ii) to accommodate

The 178 control neighborhoods were scheduled to receive the tax campaign in mid-2017.[32]

The randomization used 33 strata defined by (i) satellite grid cells of Kananga, and (ii) the estimated population of neighborhoods (based on house counts from satellite images). These strata help ensure that more populous and downtown parts of the city, which had in the past been differentially targeted for tax enforcement, are equally likely to appear in treatment and control.

Before the tax campaign, every fifth property in all neighborhoods received an informational flier in French and Tshiluba, the most widely spoken local language, announcing that (i) provincial tax collectors were beginning door-to-door property tax collection throughout the city, and (ii) money collected would be used to "secure the province, to kickstart economic development, and to protect the wellbeing of the population."[33] The distribution of fliers in treatment and control helps ensure that estimated treatment effects on participation reflect the impact of being solicited by the state to pay taxes rather than simply information about the campaign.

The government tax collectors working on the property tax campaign were randomly assigned to new teams of three every 12 work days.[34] Teams were randomly assigned to neighborhoods

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cross-randomized anticorruption interventions, as further discussed in Online Appendix Section A2.1.

32. 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 Online Appendix Section A1.3. For discussion of how the conflict impacted the study sample, see Section IV.A.

33. Online Appendix Section A2.1 provides further information about these fliers.

34. The collectors were 78% male with an average age of 33 years. All were from Kananga and fluent in Tshiluba. Roughly half were full-time employees of the tax ministry, and half were interns seeking a permanent contract. In keeping with standard policy at the ministry, a performance-based bonus was paid out to collectors equal to 18% of the total they deposited. As a point of comparison, this bonus is analogous in magnitude to the incentive pay for Pakistani property tax collectors studied in Khan, Khwaja, and Olken (2015). In addition, to encourage collectors to work throughout neighborhoods, rather than just on the main street, midway through the campaign 40% of compounds in remaining treatment neighborhoods were randomly sampled to be eligible for a double collector bonus. This randomized double bonus is used in Section VI to instrument for tax payment. The average weekly bonus was about $4, though more productive collectors earned more than $10.

in a random order. The campaign had two components carried out by collectors in each neighborhood.

(i) Property Registration

First, collectors went door to door identifying all property owners in the neighborhood. Collectors assigned a unique taxpayer ID to each house, written on the wall or door. The tax ID, name of the property owner, and other key information was recorded for each property in the register. The 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 complete neighborhood register before returning for subsequent tax visits.

(ii) Tax Solicitation

After asking information for the register, collectors solicited payment of the property tax during their first visit to a property. When an individual paid the tax, collectors used a tablet application to print a receipt showing the taxpayer ID and name of the property owner (Online Appendix Figure A2). Collectors left the receipt with the taxpayer, with an electronic record saved in the tablet's memory. When collectors deposited the money, tablet data were 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: "This compound has a legal obligation to pay the property tax for 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 recorded appointments and were told to revisit households until they paid, although the ultimate number of follow-up visits was at the discretion of the collector.

The official penalty for noncompliance was a fine of 1.5 times the original tax liability, on top of arrears, due within 30 days. After this, noncompliant households could be summoned to court and face further penalties. In reality, such sanctions were rare among residential property owners. Nonetheless, a majority of citizens believed the government had the will and capacity to pursue the noncompliant: 62% of endline respondents thought that a

TABLE I

COMPONENTS OF THE 2016 PROPERTY TAX CAMPAIGN AND ITS EVALUATION

Activity	Treatment	Control	Actor	Timing	N	J
Tax campaign						
Property register	Yes	No	Collectors	Apr–Dec 2016	20,902	253
Tax solicitation	Yes	No	Collectors	Apr–Dec 2016	20,902	253
Evaluation						
Baseline survey	Yes	Yes	Enumerators	Mar–Apr 2016	2,363	427
Midline survey	Yes	Yes	Enumerators	Apr–Dec 2016	32,866	427
Endline survey	Yes	Yes	Enumerators	Jan–May 2017	2,913	356

Notes. N = number of observations, J = number of clusters (neighborhoods). More neighborhoods were assigned to treatment (i) because the government was eager to have revenues in 2016, and (ii) to accommodate cross-randomized anticorruption interventions, as further discussed in the Online Appendix Section A2.1. Household surveys could not be administered in four neighborhoods that consisted of entirely nonresidential properties, as discussed in Section IV.A. The endline was only administered in 356 neighborhoods due to insecurity in the commune of Nganza, discussed in Section IV.A. All 71 neighborhoods from this commune (balanced across treatment and control) were dropped before respondents could be sampled and invited to participate.

neighbor who refused to pay the property tax would "definitely" or "very likely" be sanctioned.[35]

    In sum, the treatment is the combination of being registered as a taxpayer by the state and being asked to pay the property tax (Table I). Control neighborhoods experienced neither component. Citizens were expected to pay at the tax ministry themselves, as in the old declarative system. Yet the government had never before tried to enforce the tax among the great majority of the population. Precampaign property tax compliers thus consisted of a small set of commercial properties in downtown Kananga. As noted, a selection of households in control neighborhoods also received fliers about property tax collection. The control group is thus not a "pure" control. It is, however, the relevant control for identifying the effect of being registered and solicited for formal taxes by the state for the first time, separate from informational effects due to learning about the tax campaign.[36]

    The main analysis considers the reduced-form impact of the tax campaign as a whole. This is a theory- and policy-relevant

    35. The likely explanation for this apparent contradiction between perceived and actual enforcement is that households distinguished between refusing payment and lacking cash on hand when collectors visited. This interpretation is reinforced by evidence that liquidity constraints are first-order determinants of noncompliance in this setting (Weigel 2018).

    36. In Section V.C, I consider whether receiving information but no visits from the tax campaign could have sent a negative signal about the government and reduced participation.

estimand given that states invariably need information about citizens before they can collect taxes from them.

IV. Data, Estimation, and Balance

IV.A. Data

Data come from four sources: (i) administrative data on property tax payment, (ii) a baseline survey before the campaign, (iii) a midline survey during the campaign, and (iv) an endline survey after the campaign.

Administrative data come from the government's tax database. This database was managed by a private company, Hologram Identification Systems, which integrated raw data from collectors' tablets with data from the bank. Official tax records contain both property tax IDs and owner names, which makes these data linkable with household surveys.

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: for example, visit every Xth compound, where X is determined by the estimated number of compounds and a target of five surveys per neighborhood. Enumerators then conducted midline surveys in all compounds on average two to four weeks after collectors had finished working in a neighborhood.[37] Finally, enumerators administered the endline survey in 2017, after the tax campaign. In each neighborhood, enumerators first conducted a screening survey of roughly 20 property owners, randomly sampling again with a skip pattern. I 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.[38] As such, the baseline and endline surveys were administered to independent random samples.[39]

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37. In control neighborhoods, enumerators similarly waited at least two weeks after an adjacent neighborhood had received tax collectors.

38. Online Appendix Section A2.2 describes this sampling strategy. I also construct weights and reestimate all results to be representative of the population (Online Appendix Section A4).

39. I did not simply track all baseline respondents because (i) I needed a considerably larger endline sample, and (ii) many baseline respondents were renters rather than property owners. I did track a set of baseline respondents for a companion paper (Weigel 2018), as discussed in Section V.C.

Because of insecurity in Kananga in early 2017, enumerators were unable to conduct the endline survey in the commune of Nganza. All 71 neighborhoods from this commune (16% of total neighborhoods) were dropped before respondents could be sampled and invited to participate.[40] Because of the spatial stratification used for randomization, the number of neighborhoods ineligible for endline enumeration is balanced (Table II). During endline, 9.8% of sampled households could not be surveyed.[41] The most common cases included respondents (i) who were at work, (ii) who were traveling, and (iii) who declined participation without a reason. Refusals and overall attrition are balanced across treatment and control (Table II).

Table I summarizes the components of the tax campaign and its evaluation. All research activities—baseline, midline, and endline surveys—were constant across treatment and control. Sampling and enumeration procedures of surveys were identical. What varied across treatment groups was assignment to the tax campaign.

IV.B.   Outcome Measurement

The article examines three sets of outcomes. First, in estimating the effect of the campaign on visits from collectors, the registration of taxpayers, and property tax payment, I consider the following variables.

   (i) *Visited by collector*: an indicator that the household received visits from provincial tax collectors in 2016, self-reported at midline. This and the next variable measure to what extent tax collectors respected their randomized assignments and how thoroughly they worked in assigned neighborhoods.

   (ii) *Registered as taxpayer*: an indicator that the household was registered by collectors and assigned a unique tax

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40. In addition, survey enumeration could not occur in four downtown neighborhoods because they contained only nonresidential properties (shops, government buildings, churches, etc.). Thus, instead of 431 neighborhoods, the final analysis consists of 356 neighborhoods.

41. If no one was present on the first visit, enumerators made at least one more visit. If respondents scheduled appointments, they at times made three or more total visits (9% of attritors).

TABLE II

SUMMARY STATISTICS AND BALANCE CHECKS

	Summary statistics					Balance test	
	Mean	Std. dev.	Min	Max	N	β_1	Std. err.
Panel A: Neighborhood characteristics (baseline)							
Neighborhood size (square km)	0.14	0.08	0	1	356	0.008	0.008
Population density (pop./square km)	1,195.08	585.32	71	3,042	356	−41.534	55.339
Quality of roads (normalized)	0.39	0.23	0	1	356	−0.034	0.024
Quality of public lighting (normalized)	0.14	0.24	0	1	356	−0.049*	0.026
Household size (number of adults)	3.70	1.18	1	8	356	−0.144	0.128
House quality index (normalized)	0.38	0.15	0	1	356	−0.009	0.014
Weekly expenditure (normalized)	0.02	0.05	0	1	356	0.001	0.003
Knows provincial tax ministry (dummy)	0.41	0.28	0	1	356	0.027	0.030
Reports any prior collector visit (dummy)	0.26	0.26	0	1	356	0.006	0.026
Reports ever paying property tax (dummy)	0.06	0.13	0	1	356	0.021	0.014
Political participation index (normalized)	0.39	0.16	0	1	356	−0.003	0.017
Trust in government (normalized)	0.60	0.19	0	1	356	0.017	0.020
Performance of government (normalized)	0.48	0.12	0	1	355	0.022	0.013
Resp. for public goods provision (normalized)	0.34	0.24	0	1	356	−0.026	0.022
Integrity of government spending (normalized)	0.50	0.19	0	1	356	−0.013	0.020
Trust in researchers (normalized)	0.75	0.18	0	1	352	−0.009	0.020
Panel B: Individual characteristics (endline)							
Age (years)	48.59	17.02	18	102	2,913	0.852	0.733
Female (dummy)	0.41	0.49	0	1	2,913	0.028	0.019
Education (years)	9.61	4.13	0	19	2,909	−0.031	0.207
No schooling (dummy)	0.05	0.21	0	1	2,909	−0.005	0.009
Unemployed (dummy)	0.42	0.49	0	1	2,913	0.016	0.020
Government worker (dummy)	0.11	0.31	0	1	2,913	−0.003	0.012
Monthly income (US$)	106.41	193.99	0	4,800	2,903	−0.333	8.217
Household wealth index (normalized)	0.47	0.13	0	1	2,913	−0.017**	0.008
House with non–mud brick walls (dummy)	0.47	0.50	0	1	2,912	−0.034	0.032

TABLE II

(CONTINUED)

	Summary statistics					Balance test	
	Mean	Std. dev.	Min	Max	N	β_1	Std. err.
Any source of electricity (dummy)	0.19	0.39	0	1	2,913	−0.015	0.018
Owns vehicle (dummy)	0.16	0.36	0	1	2,913	−0.006	0.015
Owns business of any size (dummy)	0.22	0.42	0	1	2,913	0.034*	0.018
Has renters in compound (dummy)	0.23	0.42	0	1	2,913	−0.014	0.018
Multiple plot owner (dummy)	0.23	0.42	0	1	2,913	0.005	0.016
Born in Kananga (dummy)	0.41	0.49	0	1	2,913	−0.012	0.020
Majority ethnicity (dummy)	0.73	0.45	0	1	2,913	0.014	0.020
Native Tshiluba speaker (dummy)	0.85	0.36	0	1	2,913	0.012	0.017
Panel C: Survey enumeration characteristics							
Neighborhoods without endline (dummy)	0.17	0.38	0	1	431	−0.028	0.022
Refusals in baseline survey (dummy)	0.04	0.19	0	1	2,363	−0.004	0.009
Refusals midline survey (dummy)	0.07	0.26	0	1	27,443	0.003	0.009
Refusals in endline survey (dummy)	0.04	0.20	0	1	3,232	0.003	0.007
Any attrition in endline survey (dummy)	0.10	0.30	0	1	3,232	0.013	0.012

Notes. The first five columns provide summary statistics about the variables indicated (whose unit/type is indicated in parentheses). Neighborhood-level variables are shown for the 356 neighborhoods in the final analysis, after dropping the commune of Nganza because of the conflict described in Section IV.A. Other variables have a lower N because of nonresponses to certain survey questions. The last two columns summarize results from OLS estimations of equation (1) (without covariates) with each variable as the outcome. See Section IV.D for further details on these comparisons. Data: geographic data and baseline, midline, and endline surveys.

> ID. This is measured by the presence of a tax ID on the door or wall of a house.[42]
>
> (iii) *Property tax compliance*: an indicator for verified payment of the property tax in 2016, on the household or neighborhood level.

42. The advantage of this measure is that it is objective. One drawback is that some IDs were washed away in heavy rains, introducing measurement error. A more worrying form of measurement error would be if households purposefully erased their tax IDs, which might correlate with their participation propensity. According to enumerators, erasing tax IDs, an observable act of defiance to the state, was rare. But I cannot rule out that it sometimes occurred. Thus, I also examine self-reported visits from tax collectors, and in Section VI.A, I explore instruments for registration.

- Household level: the variable takes a value of 1 if any of three conditions are met: (a) there is a match on tax ID number between household surveys and administrative compliance data; (b) there is a match on name within neighborhood between surveys and administrative data; or (c) the household presents a valid printed receipt with the name of the property owner. Conditions (b) and (c) are important both to measure tax compliance accurately because tax IDs were subject to collector typing errors, or erasure in the rain, and to ensure that outcomes are measured symmetrically across treatment groups.[43]
- Neighborhood level: the variable is constructed by collapsing instances of property tax payment in the government database to the neighborhood level and dividing by the estimated number of nonexempt property owners per neighborhood. This estimate for the denominator comes from the midline survey.[44] This measure avoids the measurement error associated with merging administrative data and household surveys.

(iv) *Tax revenue per person*: total tax revenues (in CF) divided by the estimated number of nonexempt property owners per neighborhood. As with the previous variable, the numerator comes from the government's property tax database, and the denominator from the midline survey.

The second set of outcomes concerns political engagement. I cannot use voting data because the DRC is not a democracy.[45] Self-reported political participation is often subject to measurement error caused by social desirability, time inconsistency, and/or anonymity concerns in repressive settings. I thus worked with the provincial government to embed measurement strategies in two

43. Because collectors assigned tax IDs in treatment only, condition (a) is never met in control. For robustness, I show results on the neighborhood level, which measures compliance symmetrically across treatments, and obviates the need to merge administrative and household data. In Online Appendix Section A3.1, I also show household-level results using only conditions (b) and (c).

44. The government had no census or property register for the whole city.

45. Moreover, it is more in line with the underlying theory to test for a participation dividend in a nondemocracy with a broken social contract. The hypothesis is that tax collection stimulates political participation and, in the longer run, more inclusive governance (which may include democratic elections). To test this hypothesis, one needs to measure how citizens make political demands in the absence of democratic institutions.

forms of political engagement that come at a cost to individuals: attendance at town hall meetings and submission of government evaluations.

Specifically, in early 2017, the provincial government held five town hall 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 taxation and public spending in Kananga. Endline participants in treatment and control received official invitations to one of these meetings (Online Appendix Figure A6).[46] The proceedings were formal and at times heated (see Online Appendix Section A2.3). Town hall attendance indicates a willingness to exert costly effort to have a voice in the government. Participants needed to remember the time of the meeting and pay their transport to the provincial assembly building, located up to 13 km from endline respondents' homes (Online Appendix Figure A7). Nonetheless, roughly 20% of those who received invitations attended a town hall meeting, indicated by the variable *Town hall 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.[47] Endline participants received evaluation forms and then chose whether to fill out and drop them off. The form asked one question about citizens' 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.[48] Citizens could also free write demands or complaints in a section at the end. They were informed that the governor and other top officials would receive the evaluations plus a summary of their contents, but their identities would remain confidential. Filling out the form and paying for transport to the drop box downtown again demonstrates willingness to engage in costly participation with the provincial government. In total, 11% of those who received evaluations filled them out and deposited

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46. 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.

47. This measure is similar to the comment forms in Olken (2007) and modeled on the postcards in Paler (2013). Forms contained unique codes unknown to the government but linkable to surveys.

48. See Online Appendix Section A2.3 for further details.

them downtown, indicated by the variable *Evaluation form submission*.

To characterize the extensive and intensive margins, I examine two additional outcome variables: *Town hall or evaluation* and *Town hall and evaluation*, indicators for people who participated in either or both activities, respectively. Finally, to facilitate interpretation of magnitudes, I examine the standardized sum of both underlying participation variables, *Index (town hall & evaluation)*.

To examine the cost of participation incurred by individuals, I construct two more outcome variables based on estimates of transport and opportunity costs of attending a town hall or submitting an evaluation. To participate in these ways, individuals needed to reach the city center, and the predominant mode of transport in Kananga is mototaxis.[49] To estimate the trip cost for participating individuals, I use the average price paid by enumerators on trips from the city center to Kananga's 431 neighborhoods. To measure individuals' opportunity cost of time, I estimate their hourly wage using endline survey data.[50] To make cost estimates economically meaningful, I transform them into shares of average household daily income at endline.[51] The variables *Cost of participation (transport)* and *Cost of participation (transport & opp.)* are thus the estimated transport costs, or transport plus opportunity costs, incurred to attend a town hall and/or submit an evaluation as a share of average daily income.

Another important set of outcomes are citizens' beliefs about the responsibility, capacity, and integrity of the government. These outcomes draw on endline survey data, which in most cases I examine as standardized indices.[52] To investigate the

49. For people who both attended a meeting and submitted an evaluation, I assume separate trips. The provincial assembly building (where town halls occurred) and the drop box were 1 km apart (Online Appendix Figure A7), but there was no detectable increase in form submission on town hall days. Most double participants appear to have made independent trips.

50. Town hall meetings lasted three hours, and I estimate one hour to complete and submit an evaluation.

51. Specifically, I estimate average household daily income for each neighborhood using endline self-reports of weekly and monthly income.

52. I construct these indices by first standardizing each component variable, summing over all questions, and standardizing the new synthetic variable again. I use this procedure whenever the text refers to standardized indices to minimize risk of type I and II errors.

taxes-for-services social compact, I examine the index *Resp. for public goods provision*, which is increasing in the perceived responsibility of the provincial government in public goods provision relative to other possible providers.[53]

Following Besley and Persson (2009), I examine extractive capacity, the government's ability to raise tax revenue, and productive capacity, its ability to provide public goods. For extractive capacity, I examine four survey-based outcomes. *Information about taxpayers* is an index increasing in how much information the government is perceived to possess about potential taxpayers (household location, compliance status, occupation, and income). *Ability to punish evaders* is an index increasing in the perceived likelihood of sanctions against households that refuse to pay the property tax or pay a bribe instead. *Perceived citizen compliance* is a variable increasing in the share of other households whom respondents think paid the property tax in 2016. *Performance of tax ministry* is an index increasing in citizens' overall trust in and approval of the provincial tax ministry. For productive capacity, I consider two survey-based outcomes. *Ability to provide public goods* is an index increasing in the perceived ability of the provincial government to provide electricity, paved roads, and security efficiently and effectively, assuming it has the will to do so. *Performance of government* is an index increasing in citizens' trust in and approval of the provincial government in general.

Finally, to examine citizens' perceptions of the integrity of the government and its agents, I examine three survey-based outcomes. *Integrity of tax collectors* is a variable increasing in the perceived amount of money collected in property taxes that will reach state coffers, and *Integrity of government spending* is a variable increasing in the perceived share of tax revenues that will be spent on public services or other good uses and thus not lost to high-level corruption or misallocation. *Transparency of government*

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53. The index is composed of two sets of survey questions, which I also examine individually in the Online Appendix. 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. Second, enumerators posed three sets of opposing viewpoints concerning the optimal level of public service provision by the provincial government. See Online Appendix Section A5 for the exact text of the underlying survey questions (and details on all variables).

is increasing in perceptions of the government's efforts to inform citizens about its policies and programs.

IV.C. Estimation

I primarily use OLS to estimate the following equation:

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

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

IV.D. Balance

To check the randomization, I estimate equation (1) (without covariates) using as the outcome: (i) neighborhood-level economic, political, and tax-related characteristics from the baseline survey and geographic data; (ii) individual demographic and economic characteristics (that are unlikely to have been affected by treatment) from the endline survey; and (iii) several overall survey enumeration characteristics, such as attrition and survey refusals (Table II). In total, one neighborhood-level covariate (quality of public lighting) is imbalanced at the 10% level; one individual-level covariate (an index of estimated household wealth) is imbalanced at the 5% level,[54] and another (business owner status) is imbalanced at the 10% level. Thus, 3% (9%) of survey variables are imbalanced at the 5% (10%) level. An omnibus test of joint orthogonality fails to reject the null for the baseline variables ($p = .66$) and endline variables ($p = .33$). To be conservative, imbalanced covariates are included in $\mathbf{X}_{ijk}$ and $\mathbf{X}_{jk}$, respectively, while

54. Other proxies of socioeconomic status, such as education, income, employment, and specific plot characteristics (e.g., wall quality and access to electricity) are not statistically different across treatment groups.

Online Appendix Section A4 shows other specifications (including no covariates) for robustness.[55]

V. Results

V.A. *Collector Visits, Taxpayer Registration, and Payment*

This section considers whether the campaign achieved the government's goals of registering taxpayers and raising tax compliance. One might have expected it to fail. Large informal sectors in developing countries make third-party information scarce (Jensen 2018), and high monitoring costs exacerbate principal-agent problems between governments and tax collectors (Khan, Khwaja, and Olken 2015). These problems are particularly severe in fragile states like the DRC. Would collectors undertake this work as planned, and would citizens pay when collectors arrived at their doorstep for the first time?

Table III summarizes OLS estimations of equation (1). The campaign caused an 81.5 percentage point increase in self-reported visits from tax collectors (column (1)) and a 78.8 percentage point increase in taxpayer registration (column (2)).[56] It also caused on average a 10–11 percentage point increase in property tax payment according to household-level (column (3)) and neighborhood-level (column (4)) estimations.[57] The magnitude of the estimated increase in compliance is also analogous when examining only the endline sample (Online Appendix Table A3). The campaign increased property tax revenue by about 384 CF per household (column (5)), raising property tax receipts to nearly 5% of provincial revenues. The government was

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55. The exception is when analyzing tax compliance outcomes in the administrative data because I lack individual-level covariates for the universe of potential taxpayers. Including the one imbalanced neighborhood-level covariate (quality of public lighting) in estimations with administrative outcomes does not noticeably change the results.

56. In control neighborhoods, 5% of individuals reported visits from tax collectors. This likely reflects noncompliance among collectors, who at times crossed into 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.

57. As discussed in the Online Appendix Section A3.1, that the neighborhood-level estimate slightly exceeds the household-level estimate is consistent with measurement error from matching administrative tax records to household surveys biasing estimated compliance toward zero.

TABLE III

Effects of the Campaign on Collector Visits, Taxpayer Registration, Property Tax Compliance, and Revenues

Dependent variable:	Visited by collector	Registered as taxpayer	Property tax compliance		Tax revenue per person
Unit:	Household (1)	Household (2)	Household (3)	Neighborhood (4)	Neighborhood (5)
Campaign	0.815***	0.788***	0.103***	0.115***	367.295***
	(0.013)	(0.009)	(0.007)	(0.009)	(62.518)
Stratum FE	Yes	Yes	Yes	Yes	Yes
R^2	0.640	0.577	0.054	0.396	0.173
Observations	27,443	27,443	27,443	356	356
Clusters	356	356	356	N/A	N/A
Control mean	0.0499	0.0000	0.0006	0.0005	1.5683

Notes. Visited by collector is an indicator for households reporting at least one visit by tax collectors in 2016. *Registered as taxpayer* is an indicator for households that were registered by collectors and assigned a unique tax ID. *Property tax compliance* is an indicator for households that paid the property tax in 2016. *Tax revenue per person* is the total property tax receipts per neighborhood divided by the estimated number of nonexempt property owners. See Section IV.B for details on these variables. The unit of analysis in the first three columns is the individual household, and the data include the universe of potential taxpayers (excluding the commune of Nganza). The unit in the last two columns is the neighborhood, which reduces potential for measurement error in merging administrative data with household surveys to estimate tax compliance and revenues. Tax revenue is measured in Congolese francs. Data: midline survey merged with government tax database.

apparently pleased by the outcome, which is comparable to property tax revenue shares in more prosperous African countries (Fjeldstad, Ali, and Goodfellow 2017), and chose to continue door-to-door collection in subsequent years.

Although a 10–11 percentage point increase in tax compliance is substantial, the majority of individuals still avoided 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 investigates this question (Weigel 2018). Briefly, tax compliers tended to have more education, income, wealth, and formal employment. In addition, individuals who at baseline perceived a higher probability of punishment for evasion were marginally more likely to pay, as were individuals who professed more positive baseline attitudes toward the provincial government. These results are consistent with models of tax compliance focused on pecuniary factors (Allingham and Sandmo 1972) as well as models emphasizing "tax morale" (Luttmer and Singhal 2014).

Importantly, the tax campaign does not appear to have increased bribes according to multiple measures (Online Appendix

Section A1.2).[58] This likely reflects two factors. 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, Khwaja, and Olken 2015). In contrast, this first year of tax collection 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 effect on bribe payment means that the campaign could only affect political engagement through tax solicitation and payment.

V.B. Political Participation

Given that the campaign increased taxpayer registration and 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 IV, support this hypothesis. The campaign triggered a 4.5 percentage point (26%) increase in town hall attendance (column (1)) and a 2.4 percentage point (24%) increase in evaluation form submission (column (2)). To capture the extensive and intensive margins, columns (3) and (4) show that the tax campaign stimulated participation in either outcome by 5.0 percentage points (31%) and in both outcomes by 2.7 percentage points (77%). Of the 554 individuals who participated in a town hall meeting or submitted an evaluation, 145 did both; 103 of these 145 (71%) hailed from treated neighborhoods. These treatment effects amount to a 0.15 standard deviation increase in participation (column (5)).

For this and subsequent estimations, Online Appendix Section A4 contains a series of robustness checks, including specifications with (i) no covariates; (ii) only gender, age, and age squared as covariates; (iii) all candidate covariates listed in the preanalysis plan; (iv) enumerator fixed effects; (v) sampling weights; and (vi) heterogeneous treatment effects by house quality. In addition, for the main participation outcomes, I control for the distances to participation venues (Online Appendix Table A4) and for imbalanced covariates and their interactions with treatment (Online Appendix Table A5). I also construct p-values using

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58. Reporting bribes is not taboo in Kananga: in a study of the city's tolls, nearly half of motorcycle taxi drivers openly admitted to paying bribes (Reid and Weigel 2020).

TABLE IV
EFFECTS OF THE CAMPAIGN ON PARTICIPATION

	Town hall meeting attendance (1)	Evaluation form submission (2)	Town hall or evaluation (3)	Town hall and evaluation (4)	Index (town hall & evaluation) (5)	Cost of participation (transport) (6)	Cost of participation (transport & opp.) (7)
Campaign	0.045**	0.024**	0.050***	0.027***	0.145***	0.050***	0.071***
	(0.020)	(0.012)	(0.016)	(0.009)	(0.043)	(0.017)	(0.021)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.068	0.055	0.071	0.039	0.073	0.054	0.058
Observations	1,934	2,913	2,913	2,913	2,913	2,913	2,913
Clusters	252	356	356	356	356	356	356
Control mean	0.17	0.099	0.16	0.035	−0.077	0.11	0.16
Dep. var.	Binary	Binary	Binary	Binary	Std. index	% Daily inc.	% Daily inc.
Rand. inf. p	.023	.058	.0048	.0048	.0022	.0072	.0022
Bonferroni p	.033	.067	N/A	N/A	N/A	N/A	N/A

Notes. Town hall meeting attendance is an indicator variable that equals 1 if an individual attended a town hall meeting. *Evaluation form submission* is an indicator variable that equals 1 if an individual submitted an evaluation. *Town hall or evaluation* indicates that an individual either attended a town hall meeting or submitted an evaluation. *Town hall and evaluation* indicates that an individual attended a town hall meeting and submitted an evaluation. *Index (town hall & evaluation)* is the standardized sum of *Town hall meeting attendance* and *Evaluation form submission. Cost of participation (transport)* and *Cost of participation (transport & opp.)* are the estimated transport costs, or transport plus opportunity costs (respectively), incurred by individuals to attend a town hall and/or submit an evaluation as a share of average daily household income. See Section IV.B for details on all variables. Covariates include gender, age, age squared, wealth, a business owner dummy, and the quality of public lighting in the neighborhood, as discussed in Section IV.D. Online Appendix Section A4 shows other covariate regimes. The last two rows show *p*-values from randomization inference (with 5,000 iterations) and with Bonferroni adjustments, respectively. Data: endline survey merged with town hall attendance and submitted evaluation records (with estimates from enumerator motorcycle taxi receipts. The sample size is smaller in column (1) because the government discontinued town halls after April 1 due to insecurity in Kananga. Endline respondents sampled after this date never had a chance to attend a meeting.

randomization inference and Bonferroni adjustments.[59] For index outcomes, I report average effect size coefficients in Online Appendix Table A24.

How costly were these forms of participation? The principal costs were paying a mototaxi to reach the city center, where the town halls took place and the drop box was located, as well as the opportunity cost of time spent participating. According to estimates of transport costs only, the average participating individual spent 73% of their daily household income on transport to participate in these ways. Factoring in the estimated opportunity cost of time increases this figure to 103%.[60] Comparing the average individual in treatment and control, the tax campaign caused a roughly 43% increase in such expenditures.[61] The percent increase in estimated costs exceeds the percent increase in extensive-margin participation because treated citizens also participated more on the intensive margin.

A natural question is whether these treatment effects persisted over time. The average gap in time between tax collection and outcome measurement is 8 months (Online Appendix Figure A11), with a minimum and maximum gap of roughly 4 months and 13 months, respectively. I exploit variation in this time gap to examine persistence more formally. This variation is random because the order in which the tax campaign progressed neighborhood to neighborhood was random, as was the order of outcome measurement. Figure I shows the estimated treatment effect in 5, 10, 15, and 20 quantiles of this time gap between tax

59. See the bottom two rows of Table IV. The Bonferroni-adjusted p-value is calculated following Sankoh, Huque, and Dubey (1997) to adjust for correlation between *Town hall meeting attendance* and *Evaluation form submission*. If m is the number of correlated outcome variables and ρ is the average correlation coefficient among the other outcome variables, the Bonferroni p-value with a correlation adjustment equals $1 - (1 - p)^g$, where $g = m^{(1-\rho)}$.

60. If participants shared motorcycle taxis, this approach would overestimate transport costs. I find evidence of motorcycle taxi sharing among 8% of respondents in control and 12% in treatment (Online Appendix Table A20). Scaling the estimates for these rates of taxi sharing suggests that average spending of participators is 69% of daily income according to the transport cost measure, and 99% according to the transport plus opportunity cost measure.

61. Although these costs may seem high, it is worth noting that the campaign represents a discrete shift in the relationship between citizen and state that augurs higher future tax liability and greater potential future benefits from public goods. The chance to influence these longer-term costs and benefits of government may help explain citizens' decision to incur high participation costs today.
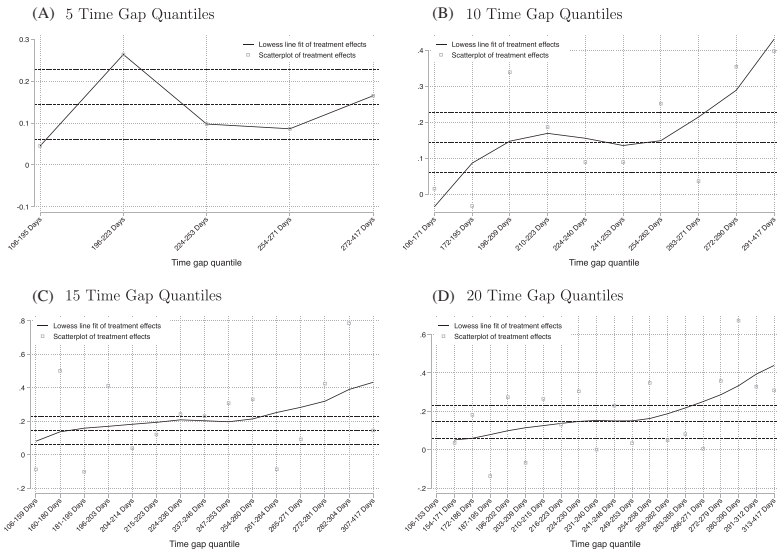
FIGURE I

Effects of the Campaign on Costly Participation in 5, 10, 15, and 20 Quantiles of the (Random) Time Gap between the Tax Campaign and Outcome Measurement

The measure of participation (dependent variable) is *Index (town hall & evaluation)*, and thus the magnitudes can be interpreted in standard deviation units. The dotted lines show the average treatment effect (in standard deviation units) and 95% confidence interval for the whole sample. The gray squares indicate the point estimate of the treatment effect in each quantile, the days of which are indicated on the *x*-axis. The time gap is random because both the timing of the campaign and the timing of outcome measurement were random. In the panel with 20 quantiles, to make the figure more legible, I omit the treatment effect in the first quantile, which has a magnitude of 1.95.

collection and participation. Although splitting the sample makes the estimates predictably noisy, the treatment effect shows no sign of shrinking over time.

The main hypothesis concerns participation with the level of government that seeks to tax citizens and will receive tax revenue, which in this case is the provincial government. But could the tax campaign and the higher provincial engagement it triggered have knock-on effects on national or local participation? Although I lack measures of costly participation at other levels of government, I provide suggestive evidence from survey questions.[62] Although treated respondents reported stronger beliefs

62. These questions and variables are described in depth in Online Appendix Section A3.1.

about the importance of citizen monitoring of the government in general (Online Appendix Table A6, column (1)), they did not report higher levels of national political engagement or overall interest in politics (columns (2) and (3)). However, treated individuals did report less engagement with local city chiefs (column (4)). Although only suggestive, this last result is consistent with citizens substituting engagement from the local to the provincial level as the state expands its presence through formal taxation.[63]

V.C. Alternative Explanations

Rather than demand for better governance, do higher rates of participation in treatment reflect (i) asymmetric experimenter demand effects, (ii) a decline in participation in control rather than an increase in treatment, (iii) a sense of unfairness due to awareness of untaxed control neighborhoods, or (iv) more factual questions and greater salience of taxation in treatment? This section explores these possibilities.

1. Experimenter Demand Effects. One possible 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 II). All participants received the same information about the town hall meetings and government evaluations, and participation always occurred after endline survey enumeration to minimize potential demand effects.

To test formally for different levels of trust or familiarity with the research team, I consider survey-based variables measuring (i) how much respondents trust foreign research organizations, (ii) whether they remember the enumerator's employer (i.e., the name of the research team), (iii) whether they participated in surveys in the past, (iv) whether they did not provide a phone number to

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63. Consistent with this interpretation, this crowd-out effect is more pronounced in poorer, peripheral neighborhoods, where city chiefs were ex ante more active and the state more absent.

the enumerator (indicating potential mistrust of the researchers), and (v) whether they provided an incorrect or fake phone number (also indicative of mistrust). No systematic differences appear across treatment and control (Online Appendix Table A9). There is also no heterogeneity in the treatment effect by (self-reported) trust in foreign researchers (Online Appendix Table A7). An indirect demand effect could arise if tax collectors encouraged citizens to participate. However, this is implausible because the town halls and evaluations had not yet been scheduled or announced at the time of tax collection, and it is unlikely collectors could have anticipated their occurrence.

2. Declining Participation in Control. Another 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.

I investigate this "disappointment effect" hypothesis by examining if control households that received informational fliers before the campaign exhibited lower participation. If the treatment effect were explained by a disappointment effect, then we would expect decreases in participation to be concentrated among those who received informational fliers in control and were thus most informed about the campaign.[64] However, flier recipients in control did not participate less than nonrecipients (Online Appendix Table A10, column (2)). Reestimating the main results while excluding control flier recipients also does not affect the estimated effect of the tax campaign (column (3)), as this alternative hypothesis would predict.

A second test exploits a small separate sample of baseline participants in control whom enumerators resurveyed after the tax campaign.[65] Although I cannot measure changes in participation, I examine changes in views of the provincial government within

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64. Online Appendix Table A10, column (1) confirms that control flier recipients were considerably more informed.

65. 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 article, but it is helpful here to examine changes in beliefs within individuals.

individuals over time in the control group, specifically: the performance of the provincial government, trust in the government, the integrity of government spending, and the responsibility of the provincial government in public goods provision. Online Appendix Table A11 summarizes fixed-effects regressions with an indicator (*Endline*) for measurement after the tax campaign. If attitudes toward the government deteriorated within individuals in control, there would be negative point estimates. This is the case for only one of the variables, *Resp. for public goods provision*, and the coefficient is not statistically significant; the other three variables have positive but insignificant point estimates. 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. This combination of results makes it unlikely that the main treatment effect reflects declining participation in control.

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 with households 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 with those farther from the border. However, plotting the participation rate in treatment as a function of minimum distance to control reveals no such relationship (Online Appendix Figure A14). Moreover, complaints about the fact that some neighborhoods had been taxed while others had not did not arise during town hall meetings or on government evaluations. Awareness of the untaxed control, then, does not appear to have been an important stimulus of participation in treatment.

4. Factual Questions and Salience of Taxation. Might treated individuals have participated more because they simply had

factual questions about tax laws and procedures? Or, more be-haviorally, does increased attendance of town hall meetings about taxation and spending reflect greater salience of taxation in treat-ment, with people participating more because taxation was top of mind?

I provide a richer description of the forms of participation con-sidered here in exploring mechanisms in Section VI.C. In brief, the majority of citizens' statements during town halls were demands for public goods or more inclusive governance if citizens were ex-pected to pay taxes. Only 12% of town hall questions were clar-ifications about the tax system, and these appear to have come equally from citizens in treatment and control.[66] Stronger evi-dence comes from the treatment effects on submission of govern-ment evaluations. The prompts on evaluation forms concerned the overall quality of governance—not taxation—and none of citi-zens' written-in comments at the bottom of submitted evaluations were clarification questions about taxes. It is thus implausible that the increase in evaluation submission could reflect factual questions about taxation. Treatment effects on beliefs about the government, considered in Section V.D, are also inconsistent with this alternative explanation.

A similar logic makes it unlikely that the treatment effects stem from greater salience of taxation in treatment. Although taxation was a stated topic of town hall meetings, as noted, the evaluation form prompts did not mention taxation and so could not have primed citizens about taxation before they chose to par-ticipate. A salience effect thus cannot explain greater evaluation submission, nor can it explain treatment effects on beliefs about the government. Salience effects are also unlikely to persist for eight months, the average time gap between tax collection and outcome measurement. Finally, the fact that town halls explic-itly focused on taxation and spending is consistent with historical accounts of "tax bargaining," in which citizens demanded better governance in exchange for tax compliance (Bates and Lien 1985). Indeed, the Magna Carta, the foundation of inclusive and consti-tutional government in England, was born of disputes over King

66. This comparison must be interpreted cautiously because speaking at the meetings is endogenous to participation, and I cannot perfectly observe citizens' treatment status after they enter the town hall venue and thus rely on the esti-mations of enumerators sitting in the back.

TABLE V

EFFECTS OF THE CAMPAIGN ON CITIZENS' BELIEFS ABOUT THE GOVERNMENT

Dependent variable	$\hat{\beta}$	Std. err.	R^2	N	μ_c
Panel A: Responsibility of government					
Resp. for public goods provision	0.117**	0.051	0.041	2,913	−0.063
Panel B: Extractive capacity of government					
Information about taxpayers	0.147***	0.044	0.086	2,910	−0.075
Ability to punish tax evaders	0.046	0.047	0.044	2,883	−0.027
Perceived citizen tax compliance	0.343***	0.053	0.100	1,954	−0.170
Performance of tax ministry	0.122***	0.047	0.065	2,791	−0.104
Panel C: Productive capacity of government					
Ability to provide public goods	−0.013	0.053	0.038	2,484	0.007
Performance of government	0.043	0.049	0.042	2,795	−0.048
Panel D: Integrity of government					
Integrity of tax collectors	0.187***	0.044	0.043	2,732	−0.136
Integrity of government spending	0.109**	0.050	0.054	2,766	−0.100
Transparency of government	0.031	0.045	0.078	2,890	−0.042

Notes. Each row summarizes an OLS estimation of equation (1), with the dependent variable noted in the first column. $\hat{\beta}$ 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 in Section IV.B and in detail in Online Appendix Section A5, is standardized to facilitate interpretation of coefficient magnitude. Data: endline survey. The number of observations varies across regressions due to nonresponse for specific survey questions.

John's taxes.[67] The fiscal focus of town halls is a feature, not a bug, of the measurement strategy.

V.D. Beliefs about the Government

This section examines effects of the campaign on citizens' beliefs about the responsibilities of the provincial government, the capacity of the government, and the integrity of its agents. Table V summarizes estimations of equation (1) using these variables, discussed in Section IV.B, as the outcome.

Panel A demonstrates that individuals in treated neighborhoods perceived greater responsibilities (by 0.12 standard deviations) of the provincial government in public goods provision relative to other possible providers, such as the national govern-

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67. "No 'scutage' or 'aid' may be levied in our kingdom without its general consent," the Magna Carta reads (para. 12), enshrining the principle that citizens deserve a voice in deciding matters of taxation (Acemoglu and Robinson 2019, 175).

ment or NGOs.[68] Importantly, this result does not appear to reflect changes in beliefs about current levels of public 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 (Online Appendix Table A8, column (4)). The tax campaign thus appears to have increased the perceived obligation of the provincial government to provide public goods in Kananga.[69] This finding mirrors the lower levels of reported engagement with city chiefs (Online Appendix Table A6, column (4)), who organize local public good provision. The tax campaign appears to have caused a shift in the perceived locus of responsibility for providing services from local chiefs to the provincial government.

Panel B considers beliefs about the extractive capacity of the provincial government. The tax campaign increased citizens' perceptions about how much information the government possesses about potential taxpayers, especially the locations of their properties and their tax compliance status.[70] It did not detectably increase beliefs about the credibility of punishment for evasion, though the coefficient is positive. Nonetheless, citizens in treated neighborhoods perceived considerably higher levels of citizen compliance with the property tax (by 0.34 standard deviations). In other words, they updated about the de facto extractive capacity of the government. The modal citizen in treatment in fact guessed the level of tax compliance in the neighborhood correctly (Online Appendix Figure A13). Finally, citizens in treatment reported more positive views of the overall performance of the tax ministry. In sum, the tax campaign does appear to have increased the perceived extractive capacity of the government.

68. Online Appendix Table A8 and Figure A22 show results for subindices and underlying survey questions, respectively.

69. Online Appendix Figure A12 compares citizens' beliefs about the responsibility of the government to provide public goods across different sectors to their expectations for how the tax revenues would actually be spent. Most expected spending on infrastructure, consistent with government messaging that campaign revenues would be used to "promote economic development." Although infrastructure was among citizens' spending priorities, they also demanded nontrivial levels of public goods provision from the provincial government across other sectors, such as water, security, and health.

70. This inference is correct: the government did in fact build a database with detailed information about potential taxpayers that it plans to use for future tax collection.

Panel C explores beliefs about government capacity to provide public goods. Treated citizens do not appear to have updated about the technology of public goods provision: they did not report thinking the government could build roads or electricity infrastructure more efficiently. Treated citizens also did not evaluate the government as a whole more positively, as they did the tax ministry (though it is possible this test is underpowered to detect a smaller effect).

However, treated citizens did update about the integrity of the government and its tax collectors (Table V, Panel D). Compared to control, they estimated that a greater share of taxpayer money collected by state agents would be deposited in the state account rather than staying in collectors' pockets (a 0.19 standard deviation increase). They also estimated that a greater share of total tax revenue would be spent on public goods or other "good uses," rather than being wasted or stolen (a 0.11 standard deviation increase). Updating positively about the integrity of tax collection and spending is consistent with the more positive evaluation of the performance of the tax ministry (Panel B). That said, citizens did not update positively about the overall transparency of the government.

It might appear counterintuitive that citizens updated positively about the government tax apparatus after it started taxing them—especially among households that evaded the tax.[71] But these results must be interpreted in the context of a weak state that is largely absent from most citizens' daily lives. In such a setting, receiving home visits from tax collectors facile with mobile technologies, being registered as a taxpayer and assigned a taxpayer ID, 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.

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71. 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 treatment effect. However, reestimating Table V with only nonpayers returns similar results (Online Appendix Table A15), albeit with slightly smaller coefficients. Nonpayers drew similar inferences as a result of the tax campaign, making a motivated reasoning interpretation unlikely.

VI. Mechanisms

This section examines possible mechanisms behind the increase in participation caused by the tax campaign: (i) taxpayers in treatment participated more because they felt ownership over public revenues and expected public goods in return (*entitlement*); (ii) the tax campaign sent a signal of state capacity that raised the expected benefits of participation (*updating*); and (iii) tax solicitation provided citizens with a bargaining chip that they used to demand public goods and inclusive governance in exchange for future compliance (*bargaining*).[72] Although the evidence in this section is more suggestive, it is most consistent with tax bargaining and to a lesser extent with updating.

VI.A. Tax Payment as Entitlement

Some scholars assume that tax-*payers* will participate more in response to government efforts to collect taxes. Payment could stimulate a sense of ownership over public revenues, leading taxpayers to expect reciprocity in the form of public goods and better governance (Prichard 2015).[73]

A first form of evidence relevant for distinguishing between mechanisms comes from comparing different complier subgroups. Specifically, I exploit variation in (i) whether households in treated neighborhoods were registered by collectors, and (ii) whether registered households ultimately paid the property tax. If the uptick in participation is concentrated among households who were registered by collectors but did not pay, this would be consistent with tax bargaining (or updating). Conversely, if registered households who paid participated more, this would suggest an entitlement mechanism.

Although payment is clearly endogenous, which households were registered is more idiosyncratic. When registering and assigning tax IDs to households, collectors were supposed to visit all households in a neighborhood, but they missed some. I suspect such omissions stem from human error because collectors received a piece-rate wage for documenting each house in the property

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72. A fourth 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 Online Appendix Section A3.3.4.

73. Payment could also raise participation through a behavioral channel akin to an endowment effect (Martin 2014).

register. Their incentive was to be as comprehensive as possible. Yet neighborhoods in Kananga bear little resemblance to a grid, and it is easy to lose track of one's position, even when guided by a GPS device. It thus seems unlikely that the reasons for skipping households are related to their underlying participation propensities.

Nonetheless, to be conservative, I also instrument for being registered and for paying taxes. To do this, I use a leave-one-out jackknife IV (JIVE) strategy leveraging the random assignment of tax collectors to neighborhoods and randomly assigned double collector bonuses. The intuition behind the JIVE instruments is that a collector's performance in a given neighborhood can be predicted by his or her performance in all other assigned neighborhoods. The instrument for property tax payment is constructed as follows.

(i) Predict a coefficient, $\hat{\lambda}_{i,-j}$, for collector i in neighborhood j by calculating the average share of households who paid the tax in all other neighborhoods assigned to collector i.

(ii) Take a linear combination of the collector-specific coefficients to construct a neighborhood-level instrument, that is,

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

where δ_i weights the collector-specific coefficients.[74]

I construct an instrument analogously for property registration. These two JIVE instruments are different because collectors vary in their effort (predictive of registering a large share of households) and effectiveness (predictive of collecting a high level of tax), and the two traits are not perfectly correlated.[75]

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74. For simplicity, collectors are weighted evenly, though due to sick days some worked for more days than others. Recall that collectors were assigned to neighborhoods in groups of three.

75. Online Appendix Section A3.3.1 shows that collector effort and effectiveness are only weakly correlated; it also contains the first-stage table. As a check of the identifying assumption behind this strategy, there is no statistically significant correlation between these JIVE instruments and nonresponse in the midline survey in treatment, which should capture aspects of underlying collector traits

The other instrument exploits collector double bonuses, randomly assigned on the household level to incentivize collectors to work throughout neighborhoods and not just on the main street.[76] Online Appendix Table A13 shows that this incentive did indeed increase the probability that households paid the tax. Using the JIVE instruments and the double collector bonus indicator generates a strong first stage for registration status (F-stat $= 56$–64, depending on the specification) and a weak first stage for payment (F-stat $= 4$–6). The resulting IV estimates should therefore be interpreted as suggestive at best.

Table VI summarizes the associations between participation and these different complier margins in treatment neighborhoods. There is a consistently positive correlation between participation and registration as a taxpayer. Households with a tax ID are about 4 percentage points more likely to attend a town hall or submit an evaluation. This positive relationship holds when including enumerator (column (2)) and collector fixed effects (column (3)). IV estimates also return positive coefficients, but they are not statistically significant.

In contrast, conditional on being registered, payers appear no more likely to participate than nonpayers (columns (5) and (6)). Both OLS and IV estimates suggest this conclusion. Given that the likely unobserved sources of bias (earnings, opportunity cost of time, views of the government, etc.) in a regression of participation on payment would bias the coefficient on payment away from zero, the fact that we observe no positive relationship between these variables is telling.[77] Moreover, following Oster (2019), the amount of selection on unobservables toward zero that would be necessary to justify even a modest positive causal effect of payment on participation is implausibly large (Online Appendix Figure A15). This empirical pattern makes an entitlement mechanism unlikely in this setting; it is more consistent with tax bargaining or with updating, to which I now turn.

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that impact participation through channels other than registration or payment (Online Appendix Table A14).

76. The government introduced these bonuses halfway through the campaign after realizing that some collectors were not comprehensively revisiting the city's remote reaches. See Section III for more details on collector compensation.

77. Coefficient stability analysis, that is, sequentially introducing observable controls, confirms this intuition that the direction of the bias is away from zero (Online Appendix Table A12).

TABLE VI

CORRELATES OF PARTICIPATION WITHIN TREATMENT NEIGHBORHOODS

Dependent variable	Town hall or evaluation							
Estimator	OLS	OLS	OLS	IV	OLS	IV	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Registered as taxpayer	0.038**	0.050***	0.040**	0.124	0.040**	0.151	0.040**	0.051***
	(0.018)	(0.018)	(0.019)	(0.108)	(0.019)	(0.140)	(0.019)	(0.019)
Property tax compliance					−0.008	−0.111	−0.016	−0.008
					(0.035)	(0.468)	(0.039)	(0.038)
Number of collectors							−0.010	−0.003
							(0.012)	(0.012)
Number of collector visits							0.018	0.017
							(0.013)	(0.013)
Collector time spent (log)							−0.007	−0.010
							(0.013)	(0.014)
Observed tablet/printer							0.006	0.003
							(0.027)	(0.027)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Enumerator FE	No	Yes	No	Yes	No	Yes	No	Yes
Collector FE	No	No	Yes	No	No	No	No	No
R^2	0.081	0.110	0.120	0.102	0.081	0.096	0.082	0.111
Observations	1,703	1,703	1,703	1,703	1,703	1,703	1,691	1,691
Clusters	211	211	211	211	211	211	211	211
Dep. var. mean	0.208	0.208	0.208	0.208	0.208	0.208	0.208	0.208

Notes. The outcome is an indicator for individuals who attended a town hall or submitted an evaluation. *Registered as taxpayer* is an indicator for households with tax IDs. *Property tax compliance* is an indicator for households that verifiably paid the property tax. *Number of collectors* is a count of the number of different tax collectors the respondent remembers visiting the household. *Number of collector visits* is a count of the number of visits by tax collectors the respondent reported receiving in 2016. *Collector time spent (log)* is the logarithm of the estimated time spent at the household by tax collectors in total in 2016. *Observed tablet/printer* is whether the respondent reported observing collectors' tablets and/or receipt printers. The number of observations drops in columns (7) and (8) due to "don't know" responses to *Collector time spent (log)*. IV estimates use the JIVE instruments *Registration propensity* and *Payment propensity*, as well as a dummy variable for households assigned to the double collector bonus. Online Appendix Table A13 shows the first stage. Data: endline survey merged with participation records and government tax database.

VI.B. *Updating about Government Capacity*

According to an updating mechanism, state efforts to broaden the tax base send a signal of future state capacity that raises the expected benefits of participation. In settings where citizens have little contact with the formal state, there is likely to be uncertainty about its capacity. Observing a systematic citywide tax campaign may lead citizens to believe that the government is now more capable of affecting their future well-being by continuing tax collection or by providing more public goods. They thus anticipate higher returns to participation to try to influence future tax policy and government spending. I outline this logic in a simple decision-theoretical framework in Online Appendix Section A3.3.2. This mechanism is relevant in fragile states, such as the DRC, as well as in the historical cases noted above where, before the emergence of "tax states" (Schumpeter 1918), the formal state was similarly absent from most citizens' lives (Cantoni, Mohr, and Weigand 2019).[78]

The strongest evidence for this mechanism is the treatment effects on beliefs about the extractive capacity of the provincial government examined in Table V. Citizens in treated neighborhoods viewed the government as having more information about potential taxpayers and a higher-performing tax department; they updated considerably about citizen tax compliance. These average effects are consistent with the logic of an updating mechanism: believing the government had greater ability to extract resources in the future as well as more tax revenue at its disposal, citizens may have anticipated greater returns to participation.[79] Is such updating correlated with participation? Online Appendix Table A16 shows that indeed participators in treatment neighborhoods on the whole updated analogously about the government's

78. 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 English state's tax department, which came closer to "Max Weber's idea of bureaucracy than any other government agency in eighteenth-century Europe" (66).

79. That awareness of new tax revenues could stimulate participation is consistent with evidence from Latin America showing that local governments are less corrupt and spend more on public goods when taxes comprise a greater share of total revenues (Brollo et al. 2013; Gadenne 2017; Martinez 2019).

extractive capacity. In fact, they were also more likely to think that noncompliance would be punished by the government—even though there was no (detectable) average increase in such beliefs.

Another 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, being registered as a taxpayer by government agents should send a stronger signal of capacity and more individuals should update their beliefs sufficiently to choose to participate. I measure state exposure at baseline as the share of households reporting (i) any past visits from government tax collectors, and (ii) any past engagement in political protests. The former measure captures state activity in the neighborhood, whereas the latter captures respondents' exposure to the state outside of the neighborhood.[80] I split these neighborhood-level measures at the median. Though only suggestive, the treatment effect is indeed larger in neighborhoods with less past state exposure according to both measures (Online Appendix Table A17). These results are consistent with more citizens updating their beliefs about state capacity past a threshold level necessary for participation where they were less accustomed to the state ex ante.

Other empirical tests, however, are inconsistent with an updating mechanism. First, an updating mechanism would predict spillovers: control individuals living across the street from a treated neighborhood would have been more likely to observe tax collectors working on the campaign and thus update their beliefs about state capacity. However, Online Appendix Figure A18 plots participation levels in control as a function of the distance to the nearest treated neighborhood, and the relationship is essentially flat. More formally, following Miguel and Kremer (2004), I estimate spillovers by exploiting random variation in the number of treated neighborhoods adjacent to control neighborhoods, controlling for the number of total adjacent neighborhoods.[81] Although there are small-in-magnitude spillovers in reported visits from tax collectors

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80. These are imperfect measures—administrative data on state presence at this level would be preferable but do not exist—but they are the best pretreatment indicators available.

81. Alternatively, I use variation in the length of control neighborhoods' borders shared with treatment neighborhoods (controlling for the total length of each control neighborhood's borders).

(Online Appendix Table A18),[82] there are no detectable spillovers on participation (or tax compliance).[83] The lack of participation spillovers diverges from the predictions of an updating mechanism.

Second, one might expect that citizens who were visited multiple times by tax collectors during the campaign would have received a stronger signal of capacity and thus participated more. A similar logic applies to households who received visits from more tax collectors (i.e., all three instead of just one), to households at which collectors spent more time, and to households that observed collectors' tablets and receipt printers. The last two columns of Table VI consider whether households that report having had these more intensive interactions with tax collectors also participated more. However, when endline survey measures capturing such interactions are included on the right-hand side,[84] none has a meaningful correlation with participation. Instead, what correlates with participation is simply whether a household was ever visited by collectors and registered as a taxpayer. If indeed these other dimensions of citizens' interactions with tax collectors sent a stronger signal of state capacity, then columns (7) and (8) of Table VI are at odds with an updating mechanism.

VI.C. Bargaining

The logic of tax bargaining is that the solicitation of taxes by the state gives citizens a bargaining chip that they can use to demand more public goods and more inclusive governance (Hoffman and Rosenthal 1997). Indeed, most historical accounts note that citizens 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 seventeeth century (North and

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82. Such spillovers likely reflect the lack of on-the-ground markers between some neighborhoods.

83. This analysis is sufficiently powered to rule out an effect of 2.3 percentage points associated with a one standard deviation increase in the length of borders shared with treatment.

84. Specifically, I include (i) the number of collectors who visited the house, (ii) the number of reported collector visits, (iii) the total amount of time collectors spent at the house, and (iv) whether collectors were seen using the tablet and receipt printer.

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, 183). According to this bargaining logic, it would in fact be a strategic error for citizens to pay before making their demands of the government.

The complier analysis in Table VI evokes a bargaining process. Being registered by the state as a taxpayer but not paying taxes is what correlates with participation. Fully 86% of citizens in treatment who chose to participate had managed to avoid paying taxes in the campaign. Moreover, the fact that being registered as a taxpayer, but not further interactions with tax collectors, is what predicts participation (columns (7) and (8)) is consistent with bargaining: it is the moment the state begins to solicit citizens for taxes that discretely changes the social contract and activates citizens' political engagement.

Another key source of evidence for this mechanism is descriptive: at town halls, did citizens demand more inclusive and responsive government as a precondition to future tax compliance? During the meetings, enumerators sat in the back and recorded the topics of citizens' remarks. Figure II shows the distribution of topics. The most common were indeed demands for better governance in exchange for taxes. Described as "Demands (taxes for better govt)," such comments (i) directly broached how provincial tax revenues would be spent, or (ii) tied provincial taxation with demands about public goods, corruption, or citizen monitoring. "Why should the inhabitants of Lukonga pay taxes," one participant asked, "when the roads are in such disastrous condition?"[85] Such comments exhibit tax bargaining between citizens and the state.

Complaints about property taxes, or taxes in general, were the second most common type of comment, followed by more factual questions about tax details, and finally demands about corruption and public goods without links to taxation. Do tax complaints constitute tax bargaining? Grumbling about taxes, which people have no doubt done since time immemorial, is in general not the same as tax bargaining. What is distinctive about the processes of tax bargaining that began in the early modern period was the emergence of a sense of legitimate exchange of tax compliance for better

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85. An example of the former type of comment in this category is the following: "There must be more transparency about your spending and tax revenues. You must communicate to the population what you are doing."
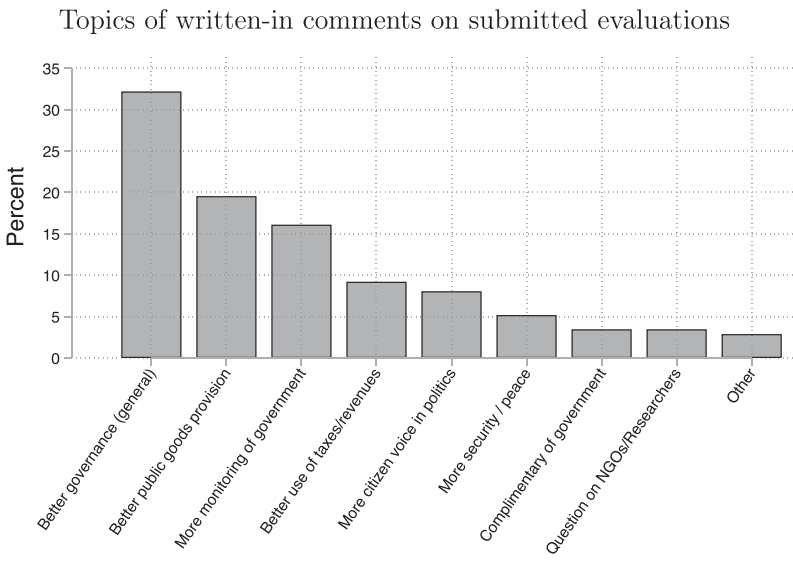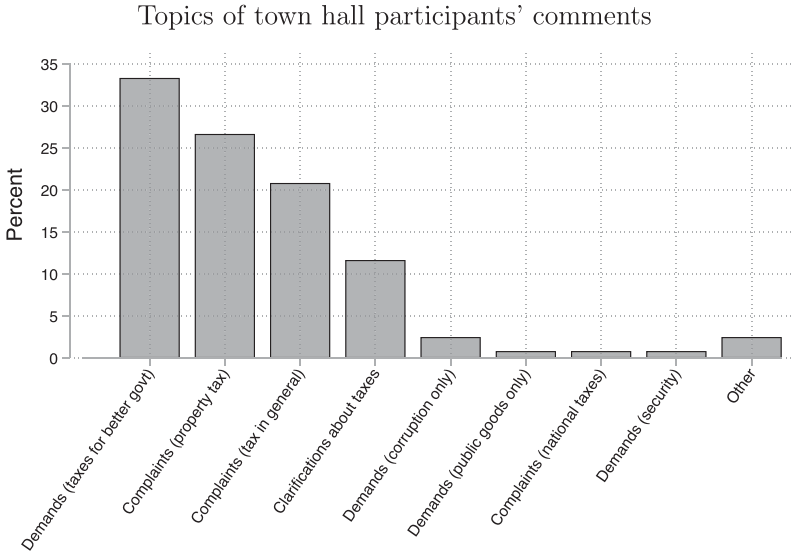
Topics of town hall participants' comments

Topics of written-in comments on submitted evaluations

Topics of Citizens' Comments at Town Hall Meetings and on Submitted
Government Evaluations

Figure II (*Continued*). For details on the classification of town hall comment topics, see Section VI.C, as well as Online Appendix Section A2.3. Briefly, *Demands (taxes for better govt)* include comments (i) directly discussing how provincial tax revenues would be spent, and (ii) explicitly linking provincial taxation with demands about public goods, corruption, or citizen monitoring. This category thus captures the sense of exchange of taxes for more public goods or more avenues of participation at the center of theories of tax bargaining. If citizens brought up other governance failures without mentioning taxation in the same comment, then this is coded in a different category, for example, "Demands (corruption only)" or "Demands (public goods only)." The second panel provides the topics of citizens' written-in comments at the bottom of submitted evaluations.

governance (more public goods, more participation). That said, in the context of town hall meetings with government officials, such complaints registered as threats of future noncompliance—a useful bargaining tactic if the government were to take seriously citizens' demands.[86] The fact that citizens had chosen to accept the government's invitation to attend a town hall meeting in the first place—and to make formal complaints in the presence of heads of the finance ministry and tax department—suggests a larger goal of extracting policy concessions.[87]

Further evidence comes from text analysis of enumerators' written summaries of citizens' comments during town halls.[88] I use latent Dirichlet allocation (LDA) (Blei, Ng, and Jordan 2003) to characterize the five main topics that emerge and report the ten most common words associated with each topic (Table VII). The results further illustrate a process of tax bargaining unfolding at these meetings: almost all topics contain the words "pay" and/or "tax" as well as words associated with better governance such as "manager," "schools," "information campaign," "transparency," and "participation."

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86. Such threats would have been credible because most attendees had not paid 2016 taxes. Put differently, bargaining to get a better fiscal deal could involve attempts (i) to lower the effective price of public goods holding constant their quantity, or (ii) to increase the quantity of public goods received for a given amount of tax. Tax complaints are a possible tactic of the first strategy.

87. Even if one views tax complaints as run-of-the-mill grumbling, rather than as a bargaining tactic, it is worth noting that 34% of citizen comments were explicit quid pro quo demands for better governance in exchange for tax compliance. Moreover, the increase in submissions of government evaluations and in the perceived responsibilities of the government could not be explained by tax grumbling given that neither the evaluation form nor these survey questions prompted citizens to think about taxation, respectively.

88. In addition to selecting from a list of prespecified topics, analyzed in Figure II, enumerators summarized each comment in a text box.

TABLE VII

TOPICS OF CITIZEN COMMENTS AT TOWN HALLS AND WRITTEN-IN COMMENTS ON
SUBMITTED EVALUATIONS

Order	(1)	(2)	(3)	(4)	(5)
Panel A: Topics of citizen comments at town hall meetings					
1	pay	tax	necessary	pay	pay
2	necessary	population	population	take	must
3	population	necessary	collectors	without	population
4	tax	pay	pay	decision	why
5	why	know	know	why	others
6	agents	do	see	necessary	collectors
7	time	collectors	tax	participation	agents
8	collectors	why	without	tax	nothing
9	communes	nothing	information campaign	others	participation
10	manager	schools	transparency	agents	tax
Panel B: Topics of written-in comments on submitted evaluations					
1	government	government	government	government	government
2	water	provincial	provincial	provincial	province
3	ask	should	should	work	country
4	roads	more	population	province	leaders
5	electricity	work	especially	do	population
6	improve	public	erosion	better	good
7	jobs	goods	needs	ask	ask
8	people	concerning	people	would	development
9	more	ask	security	central	love
10	who	because	take	Kasaï	could

Notes. This table reports the first ten words in each of the five main topics identified by latent Dirichlet allocation (Blei, Ng, and Jordan 2003) applied to two sources of text that offer insight into citizens' reasons for choosing to participate. Panel A concerns a transcription of citizens' comments during town hall meetings. Panel B concerns citizens' written-in comments in the optional bottom section of evaluation forms. See text for further details on the method and its interpretation.

The evidence from submitted evaluation forms reinforces the sense of citizens bargaining for a better fiscal deal. Over 90% of submitted evaluations demanded overall improvement from the government; they also overwhelmingly demanded more avenues of participation, access to information, and public goods spending (Online Appendix Figure A19). In addition, 39% of individuals wrote comments in the text box at the bottom of the form, of which the most frequent topics include (i) general demands for better governance, (ii) demands for specific public goods projects, and (iii) demands for greater monitoring of the provincial government and improved transparency over spending in particular (Figure II). "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."[89] To characterize these written comments further, Table VII reports the ten most common words for each of the five top topics identified by LDA. Topics 1–3 focus on public goods failures with words like "government," "water", "roads," "electricity," "erosion," "security", "jobs," "work," "public," "goods," "improve," and "needs." Topics 4–5 focus more on accountability and leadership, suggested by words like "leaders," "government," "Kasaï," and "country" juxtaposed with "development," "population," "ask," and "better."

Moreover, citizens in treatment were more likely to submit evaluations with such demands. Reestimating the treatment effect on evaluation submission (Table IV, column (2)) using an outcome indicating submission of evaluations demanding the government do a better job (rather than expressing satisfaction), or (ii) evaluations containing written-in demands, individuals in treatment were still more likely to participate compared with control (Online Appendix Table A19). In sum, descriptive evidence about citizens' comments at town halls and on submitted evaluation forms suggests citizens participated to bargain for better, more inclusive governance.

Finally, the observed effects on beliefs (Section V.D) provide some additional evidence of bargaining. The fact that treated respondents viewed the provincial government as more responsible for providing a range of public goods (Table V, Panel A) is evidence of tax collection stimulating demand for better governance. In addition, while participators in treatment generally updated their beliefs in line with the average, they had less confidence that tax money would reach the state and ultimately fund public goods rather than be wasted or embezzled (Online Appendix Table A16). Although drawing on correlations that are not straightforward to interpret, such heterogeneity is consistent with participating citizens demanding citizen monitoring of government spending *before* accepting broad-based taxation. "The provincial government should do more," wrote one individual on an evaluation form, "and inform us how this money will be spent on public infrastructure and not wasted on other things."[90]

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89. "The provincial government should stabilize the erosion in Tukombe," wrote another, "especially on Avenue Mpokolo as well as Nkumbikumbi, for the nuns of Tukombe."

90. "I ask that the government show the population what it achieves with this money," wrote another.

One empirical pattern inconsistent with bargaining is the fact that any taxpayers participated at all. In seeking to bargain with the government, threats of future noncompliance by citizens who had already paid their taxes would not be credible. By paying, citizens lost their key bargaining chip. Yet in the data, payers do not appear to have participated significantly less than nonpayers who were also registered by collectors. This observation suggests that other mechanisms, such as updating, may also be at work.

On net, the evidence is most consistent with a bargaining mechanism, but there is also evidence of updating, and these mechanisms need not be mutually exclusive.

VII. Conclusion

This article analyzed the first door-to-door property tax collection campaign in the city of Kananga, DRC, which increased tax compliance by over 11 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 5 percentage points more likely to attend a town hall meeting or submit a government evaluation. The tax campaign also caused citizens to update their beliefs about the provincial government, perceiving higher revenues, greater integrity in spending, and a greater responsibility to provide public goods.[91] The analysis of mechanisms is more suggestive, but the evidence is most consistent with citizens bargaining for more public goods and more inclusive governance in exchange for future tax compliance. There is also some evidence that the campaign sent a signal of state capacity that raised the expected benefits of participation.

One implication of this study is that governments may anticipate large increases in political participation from enforcing direct taxes like the property tax and choose to rely on indirect taxes instead. Because all households who are solicited to pay the tax are more likely to participate, not just those who actually pay, the political response is, from the government's perspective,

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91. How the government responds is beyond the scope of this study. In the context of this experiment, it would be difficult to credibly estimate treatment effects because public goods are (i) typically provided at a more aggregated administrative unit than that used for randomly assigning tax collection (the neighborhood), and (ii) an outcome of longer-run budgetary processes.

disproportionately large relative to the increase in revenues. Anticipating this bargaining response, forward-looking governments may therefore raise revenues through less visible tax instruments like trade, excise, and consumption taxes, or seignorage, which pass through to consumers in the form of higher prices and are less likely to stimulate participation.[92] Minimizing citizen participation may thus offer a political economy explanation for why many nondemocratic governments in poor countries underexploit property and income taxes, preferring less efficient and more regressive tax structures than is typically deemed optimal (Gordon and Li 2009).

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SUPPLEMENTARY MATERIAL

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online.

DATA AVAILABILITY

Data and code replicating tables and figures in this article can be found in Weigel (2020), in the Harvard Dataverse, doi: 10.7910/DVN/FN24Y4.

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92. Cross-country and lab-in-the-field evidence supports this intuition (De la Cuesta et al. 2019a).

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