

Weigel, Jonathan

Working Paper

The participation dividend of taxation: How citizens in Congo engage more with the state when it tries to tax them

Working Paper Series, No. 20-202

Provided in Cooperation with:

Department of International Development, London School of Economics and Political Science (LSE)

Suggested Citation: Weigel, Jonathan (2020) : The participation dividend of taxation: How citizens in Congo engage more with the state when it tries to tax them, Working Paper Series, No. 20-202, London School of Economics and Political Science (LSE), Department of International Development, London

This Version is available at:

<https://hdl.handle.net/10419/224828>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

Working Paper Series 2020

No.20-202

The Participation Dividend Of Taxation:
How citizens in Congo engage more with the
state when it tries to tax them

Jonathan L. Weigel

Published: May 2020

Department of International Development

London School of Economics and Political Science

Houghton Street

London

WC2A 2AE UK

Tel: +44 (020) 7955 7425/6252

Fax: +44 (020) 7955-6844

Email: j.weigel@lse.ac.uk

Website: www.lse.ac.uk/InternationalDevelopment

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

JONATHAN L. WEIGEL[†]

May 23, 2020

Abstract

This paper provides evidence from a fragile state that citizens demand more of a voice in the government when it tries to tax them. I examine a field experiment randomizing property tax collection across 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 11.5% in treatment. It also increased political participation by about 5 percentage points (31%): citizens in taxed neighborhoods were more likely to attend townhall meetings hosted by the government or to submit evaluations of its performance. To participate in these ways, the average citizen incurred costs 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: H20, P48, D73.*

*I thank the editor, Andrei Shleifer, and four referees for comments that substantially improved the paper. My sincere thanks also to my advisors, Horacio Larreguy, Nathan Nunn, Rohini Pande, and James Robinson, as well as Oriana Bandiera, Robert Bates, Tim Besley, Deborah Boucoyannis, Esther Duflo, Jean-Paul Faguet, Rema Hanna, Xavier Jaravel, Seema Jayachandran, Anders Jensen, Asim Khwaja, Joana Naritomi, Oyebola Okunogbe, Otis Reid, Dina Pomeranz, Wilson Prichard, Vincent Pons, Raul Sanchez de la Sierra, Sandra Sequeira, Kai Thaler, Noam Yuchtman as well as seminar participants at Berkeley (WGAPe), Columbia (NEWEPS), Northwestern (Rookiefest), TSE, TCD (CEPR), UBC, and Zurich for invaluable suggestions. For outstanding research assistance, I thank Anissa Chabib, Morgan Yucel, Gabrielle Corrigan, Elie Kabue Ngindu, and a superb team of enumerators. I am grateful for the collaboration with the Provincial Government of Kasai Central, for the support of Jim Mukenge, James Diderich, and Woody Collins, and for funding from the Fulbright Scholar Program and Weiss Family Program Fund. A prior working paper, from 2017, was titled “Building State and Citizen: How Tax Collection in Congo Engenders Citizen Engagement with the State.” AEA pre-registration ID: AEARCTR-0001316.

[†]LSE, Houghton Street, London, WC2A 2AE (email: j.weigel@lse.ac.uk)

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 paper tests if taxation increases participation in fragile states by examining the randomized rollout of the first large-scale citizen tax campaign in Kananga, D.R. 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). The slogan “no taxation without representation” captures the intuition.

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

²On theory, cf. Besley and Persson (2009), Fukuyama (2011), Acemoglu and Robinson (2019); on empirics, cf. Sanchez de la Sierra (2019), Dittmar and Meisenzahl (2019), Cantoni et al. (2019).

³Faced with rising costs of war, rulers could no longer rely on ‘own revenues’ (land rents, sales of venal offices, seignorage), so most began broadening the tax base and systematizing collection in this period (Brewer, 1990; Gennaioli and Voth, 2015; Cantoni et al., 2019).

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

Given this potential for taxation to promote inclusive governance, donors and policymakers increasingly support domestic revenue mobilization in poor countries with weak and unaccountable states, which collect around 10% of GDP in tax compared to 40% in developed countries.⁵ However, we lack rigorous 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 Kasai 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 over 27,000 properties). In treated neighborhoods, tax collectors went door to door (1) registering property owners and assigning unique tax ID numbers (written on a house wall), and (2) 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

⁵For instance, the OECD noted "Tax reforms can ... enhance accountability between citizens and the state" (OECD, 2010). Similarly, the IMF wrote "Bringing small businesses into the tax net can help secure their participation in the political process and improve government accountability" (IMF, 2011). DFID (2016) shows expanding donor support in this area. Pomeranz and Vila-Belda (2019) review key lessons from recent work in domestic revenue mobilization.

⁶Before 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

I estimate the reduced-form impact of this campaign — i.e. of being registered by the state *and* asked to pay the property tax. Given that states invariably need to collect information about potential taxpayers before they can try to tax them, the campaign is a treatment bundle of theoretical and policy interest.⁷

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 100-fold increase in citizen tax compliance made property tax receipts just under 5% of the provincial government’s total revenue, on par with local governments in more prosperous African countries.⁸ The provincial government evidently viewed the campaign as a success, choosing to continue field-based property tax collection after 2016.

I then use the random assignment of this 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

help isolate the impact of being registered and solicited for taxes by the state for the first time.

⁷The 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).

⁸Property taxes make up 14% of local government revenues in Ghana, 10% in the Gambia, 6% in Sierra Leone, and less than 1% in Liberia and Cameroon (Fjeldstad et al., 2017). Moreover, property tax receipts are typically much lower outside of national capitals.

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

government hosted a series of townhall 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 whose contents were shared with the governor and other top officials. Attending a townhall or submitting an evaluation exhibits willingness to incur costs to have a voice in the 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 townhall meeting or to submit an evaluation — a 31% increase relative to control. I estimate that the average citizen in treatment incurred 43% higher participation costs compared to 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, townhall 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 8 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

¹⁰Individuals who both attended a townhall and submitted an evaluation were 77% more common in treatment than control. See p. 20 for details on estimating the costs of participation.

participation in control — which could arise if, say, households in control anticipated, but never received, visits from tax collectors¹¹ — 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 townhalls 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, NGOs, and other potential providers). In fact, mirroring these heightened perceived obligations of the provincial government, 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 et al., 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

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

with the state.

Although the main contribution of the paper 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, taxpayers should participate more than non-payers. However, there is no correlation between payment and participation in treatment, according to OLS as well as IV estimates leveraging 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 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 that they can use to demand better

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

governance.¹³ The fact that being registered by the state as a taxpayer, but not paying, correlates with participation evokes a bargaining process in which citizens demand better governance before complying with taxes. Text analysis reveals that most of citizens' comments at townhalls 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 individual, "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 response to survey questions reinforces the sense that they participated to bargain for a better fiscal deal. However, that any *tax-payers* 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 paper 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 et al. (2015) and Sanchez de la Sierra (2019) in collecting experimental (or quasi-experimental) data

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

¹⁴Seminal 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.

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

in developing countries to study hypotheses based on histories of now-rich countries' development. While 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, per se, 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 — impact government spending and accountability.¹⁹

The paper 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 more functional states, 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; Naritomi, 2019; Carrillo et al., 2017; Jensen, 2018), tax collector incentives (Khan et al., 2015), providing information about

¹⁶See, e.g., Martin (2014); Sjurgen (2018); Kao et al. (2019); Sjoberg et al. (2019).

¹⁷See, e.g., Paler (2013); De la Cuesta et al. (2020a,b).

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

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

²⁰Exceptions include recent and ongoing work in Ethiopia (Mascagni et al., 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).

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

enforcement or peer 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 paper 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. Median monthly household income in the study site is roughly \$106 (PPP \$168). The country is often termed a ‘kleptocracy,’ due to the neo-patrimonial regime of long-time president Mobutu Sese Seko (Young and Turner, 2013), or a ‘failed state,’ due to its history of civil conflict (Stearns, 2012). Its fiscal capacity, as proxied by 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 Kasai Central province. In 2015, total tax revenues amounted to a paltry \$0.23 per person in the province. As in many developing countries, the pre-campaign 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 paid, or been 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 (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

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

²³The 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

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 international experts to champion property taxation as “the single greatest opportunity for strengthening local revenue systems” (Moore et al., 2018, p.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 Kasai Central has followed suit. Roughly 90% of property owners in Kananga face a fixed annual liability of 2,000 Congolese Francs (CF), about \$2, which is 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. But 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 enforcing 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

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, p.82).

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

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

²⁶Although 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

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). Nonetheless, individuals in Kananga voice 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* (RIAC), also often organize meetings with citizens and government officials to discuss such issues.²⁸ Second, individuals, or groups of individuals, author formal letters of complaint to the provincial government. The measures of participation used in this study are versions of these forms of political engagement.

In sum, 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.

justify postponing the 2016 elections, which he did (Englebert et al., 2018).

²⁷Although 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).

²⁸For 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.

²⁹Even 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 et al., 2008, p. 12).

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 polygons that approximate *localités*, the lowest administrative unit in the city.³⁰ Neighborhood borders are typically natural boundaries like roads, ravines, or other features easily identifiable from the ground. Among the 431 polygons, 253 were selected randomly to receive the tax campaign in its first phase.³¹ The 178 control polygons 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 the neighborhood (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

³⁰Figures A1 and A7 show examples of neighborhoods.

³¹More neighborhoods were assigned to treatment (1) because the government was eager to have revenues in 2016, and (2) to accommodate cross-randomized anticorruption interventions, as further discussed in Section A2.1.

³²The government ultimately decided to suspend all tax collection in 2017 after violence broke out in the province early that year. It recommenced property tax collection in 2018. For information about the conflict, see Section A1.3. For discussion of how the conflict impacted the study sample, see p. 16.

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

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 twelve work days.³⁴ Teams were then randomly assigned to neighborhoods — in a random order. The campaign had two components carried out by collectors in each neighborhood.

1. **Property register.** 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.
2. **Tax solicitation.** After asking information for the register and assigning the tax ID, 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 (Figure A2). Collectors left the receipt with the taxpayer, with an

³⁴The collectors were 78% male with an average age of 33 years. All of them were from Kananga and fluent in Tshiluba, the local language. Roughly half were full-time employees of the tax ministry, and half were interns 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 et al. (2015). Additionally, 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 will be used in Section VI to instrument for tax payment. The average weekly bonus was about \$4, though more productive collectors earned more than \$10.

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, though the ultimate number of follow-up visits was determined by 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, non-compliant 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 non-compliant: 62% of endline respondents thought that a 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. Pre-campaign property tax compliers thus consisted of a small set of commercial properties in

³⁵The 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 non-compliance in this setting (Weigel, 2018).

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 impact of being registered and solicited for 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 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: (1) administrative data on property tax payment, (2) a baseline survey before the campaign, (3) a midline survey during the campaign, and (4) an endline survey after the campaign.

Administrative data come from the government’s 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: e.g. visit every X^{th} compound, where X is determined by the estimated number of compounds and a target of five surveys

³⁶In 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.

per neighborhood. Enumerators then conducted midline surveys in all compounds on average 2-4 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 then randomly selected a subsample of screening survey participants for the full interview, choosing higher-quality houses with slightly higher probability to focus on the population most affected by the campaign.³⁸ As such, the baseline and endline surveys were administered to independent random samples.³⁹

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 (1) who were at work, (2) who were traveling, and (3) 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.

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

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

³⁹I 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.

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

⁴¹If 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).

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 paper 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.

1. *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.
2. *Registered as taxpayer*: an indicator that the household was registered by collectors and assigned a unique tax ID. This is measured by the presence of a tax ID on the door or wall of a house.⁴²
3. *Property tax compliance*: an indicator for verified payment of the property tax in 2016, on the household or neighborhood level.

Household level: the variable takes a value of 1 if any of three conditions are met: (i) there is a match on tax ID number between household surveys and administrative compliance data; (ii) there is a match on name *within neighborhood* between surveys and administrative data; or

⁴²The 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.

(iii) the household presents a valid printed receipt with the name of the property owner. Conditions (ii) and (iii) 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 non-exempt 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.

4. *Tax revenue per person*: total tax revenues (in Congolese Francs) divided by the estimated number of non-exempt 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 forms of political engagement

⁴³Because collectors assigned tax IDs in treatment only, condition (i) 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 the online appendix, I also show household-level results using only conditions (ii) and (iii).

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

⁴⁵Moreover, 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 exert voice in politics *in the absence of democratic institutions*.

that come at a cost to individuals: attendance at townhall meetings, and submission of government evaluations.

Specifically, in early 2017, the provincial government held five townhall meetings. Chaired by the finance minister and the director general of the tax ministry, the meetings sought to promote dialog between officials and citizens about taxation and public spending in Kananga. Endline participants in treatment and control received official invitations to one of these meetings (Figure A6).⁴⁶ The proceedings were formal and at times heated (see Section A2.3). Townhall 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 13km from endline respondents' homes (Figure A7). Nonetheless, roughly 20% of those who received invitations attended a townhall meeting, indicated by the variable *Townhall meeting attendance*.

The second measure of participation is the submission of anonymous evaluations of the provincial government to a locked drop box in downtown Kananga.⁴⁷ Endline participants received evaluation forms and then chose whether to fill out and drop them off. The form asked one question about the respondent's overall satisfaction with the government, followed by four agree-disagree statements concerning (i) opportunities for participation, (ii) access to information, (iii) spending on public goods, and (iv) citizen reporting of problems.⁴⁸ Citizens could also free write demands or complaints in a section at the end. They were informed that the gov-

⁴⁶After 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.

⁴⁷This 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.

⁴⁸See Section A2.3 for further details.

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

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

To examine the cost of participation incurred by individuals, I construct two additional outcome variables based on estimates of transport and opportunity costs of attending a townhall 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*

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

⁵⁰Townhall meetings lasted 3 hours, and I estimate 1 hour to complete and submit an evaluation.

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

(*transport & opp.*) are thus the estimated transport costs, or transport plus opportunity costs, incurred to attend a townhall 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 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 both 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 punishment 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

⁵²I 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 error.

⁵³The 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 Section A5 for the exact text of the underlying survey questions (and details on all variables).

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 two 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. Finally, *Transparency of government* 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:

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

where i indexes individuals, j neighborhoods, and k the 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}), i.e. 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: (1) neighborhood-level economic, political, and tax-related characteristics from the baseline survey and geographic data; (2) individual demographic and economic characteristics (that are unlikely to have been affected by treatment) from the endline survey; and (3) 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 ($p = 0.66$) and endline variables ($p = 0.33$). To be conservative, imbalanced covariates are included in \mathbf{X}_{ijk} and \mathbf{X}_{jk} , respectively, while Section A4 shows other specifications (including no covariates) for robustness.⁵⁵

⁵⁴Other 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.

⁵⁵The 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.

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 et al., 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 both 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 (Table A3). The campaign increased property tax revenue by about 384 Congolese Francs per household (Column 5), raising property tax receipts to nearly 5% of provincial revenues. The government was apparently pleased by the outcome, which is comparable to property tax rev-

⁵⁶In control neighborhoods, 5% of individuals reported visits from tax collectors. This likely reflects noncompliance among collectors, who at times crossed into to the wrong (control) neighborhoods. Such noncompliance was expected given that the borders between neighborhoods are not always clearly delimited and must be checked using GPS. This noncompliance would, if anything, bias treatment effects toward zero.

⁵⁷As discussed on p. 20 in the online appendix, 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.

enue shares in more prosperous African countries (Fjeldstad et al., 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 *evaded* paying the tax, despite visits from collectors. Why did the campaign cause some, but far from all, individuals to pay the tax? A companion paper 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 (Section A1.2).⁵⁸ There are two likely explanations. First, because this was the first-ever citizen tax campaign, collectors faced high uncertainty about the government’s plans to audit their work and sanction bribe takers. Second, collusive bribery is more likely when collectors and citizens have repeated interactions (Khan et al., 2015). In contrast, this first year of tax enforcement involved, in most cases, a single-shot interaction between collector and citizen. One might expect an increase in bribes in future rounds of property tax collection. The negligible impact on bribe payment means that the campaign could only affect political engagement through collector visits and tax payment.

⁵⁸Reporting 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).

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 townhall attendance (Column 1) and a 2.4 percentage-point (24%) increase in evaluation form submission (Column 2). To capture the intensive margin, Columns 3 and 4 show that the tax campaign stimulated participation 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 townhall 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, Section A4 contains a series of robustness checks, including specifications with (1) no covariates, (2) only gender, age, and age squared as covariates, (3) all candidate covariates listed in the pre-analysis plan, (4) enumerator fixed effects, (5) sampling weights, and (6) heterogeneous treatment effects by house quality. In addition, for the main participation outcomes, I control for the distances to participation venues (Table A4) and for imbalanced covariates and their interactions with treatment (Table A5). I also construct p -values using randomization inference and Bonferroni adjustments.⁵⁹ For index outcomes, I report average effect size (AES) coefficients in Table A24.

⁵⁹See the bottom two rows of Table IV. The Bonferroni-adjusted p -value is calculated following Sankoh et al. (1997) to adjust for correlation between *Townhall 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 $g = m^{(1-\rho)}$.

How costly were these forms of participation? The principal costs were paying a mototaxi to reach the city center, where the townhalls 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 (p. 20), 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 (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 collection and participation. Although splitting the sample makes the estimates predictably noisy, the treatment effect shows no sign of shrinking over time.

⁶⁰If 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 (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.

⁶¹Although 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.

The main hypothesis concerns participation with the level of government that seeks to tax citizens and that 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? While 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 about the importance of citizen monitoring of the government in general (Table A6, Column 1), they did not report higher levels of national political engagement or overall interest in politics (Columns 2-3). 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 (1) asymmetric experimenter demand effects, (2) a decline in participation in control rather than an increase in treatment, (3) a sense of unfairness due to awareness of untaxed control neighborhoods, or (4) more factual questions and greater salience of taxation in treatment? This section explores these possibilities.

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

⁶³Consistent 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.

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 townhall 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 questions asking respondents (1) how much they trust foreign research organizations, (2) whether they remember the enumerator's employer (i.e. the name of the research team), (3) whether they participated in surveys in the past, (4) whether they did not provide a phone number to the enumerator (indicating potential mistrust of the researchers), and (5) whether they provided an incorrect or fake phone number (also indicative of mistrust). No systematic differences appear across treatment and control (Table A9). There is also no heterogeneity in the treatment effect by (self-reported) trust in foreign researchers (Table A7). An indirect demand effect could arise if tax collectors encouraged citizens to participate. However, this is implausible because the townhalls and evaluations had not yet been scheduled or announced at the time of tax collection, and it is unlikely collectors could have anticipated their occurrence.

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 non-recipients (Table A10, Column 2). Re-estimating 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 re-surveyed after the tax campaign.⁶⁵ Although I cannot measure changes in participation, I examine changes in views of the provincial government within individuals over time, 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. Table A11 summarizes fixed-effects regressions with an indicator (*Endline*) for measurement after the tax campaign. If attitudes toward the government deteriorated within in-

⁶⁴Column 1 of Table A10 confirms that control flier recipients were considerably more informed.

⁶⁵I collected these data for a companion paper on the determinants of compliance (Weigel, 2018). This repeated baseline sample is not part of the endline sample for this paper, but it is helpful here to examine changes in beliefs within individuals.

dividuals in control, there would be negative point estimates. For only one of the variables, *Resp. for public goods provision*, is this the case, 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.

Awareness of the untaxed control

Treated individuals might have participated more because they were aware that control neighborhoods had not yet been taxed, and they thought this was unfair. The main result could thus be an experimental artifact, a function of having measured outcomes before the control group received the tax campaign. At first glance, this explanation appears implausible because households were informed that the campaign would eventually reach all neighborhoods. Still, treated individuals could have thought it unfair that they were taxed first.

To explore this possibility, I examine whether treated households near the border with control neighborhoods were more likely to participate compared to households farther from control. If awareness of the untaxed control fueled participation in treatment, then presumably individuals living near a border with control (who are thus more aware of neighborhood-level differences in tax collection) would have been more likely to participate compared to those farther from the border. However, plotting the participation rate in treatment as a function of minimum distance to control reveals no such relationship (Figure A14). Moreover, complaints about the fact that some neighborhoods had been taxed while others had not did not arise

during townhall meetings or on government evaluations. Awareness of the untaxed control, then, does not appear to have been an important stimulus of participation in treatment.

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 behaviorally, does increased attendance of townhall meetings about taxation and spending reflect greater salience of taxation in treatment, with people participating more because taxation was top of mind?

I provide a richer description of the forms of participation considered here in exploring mechanisms in Section VI.C. But, in brief, the majority of citizens' statements during townhalls were demands for public goods or more inclusive governance if citizens were expected to pay taxes (Figure II). Only 12% of townhall questions were clarifications about the tax system, and these appear to have come equally from citizens in treatment and control.⁶⁶ Stronger evidence comes from the treatment effects on submission of government evaluations. The prompts on evaluation forms concerned the overall quality of governance — not taxation — and none of citizens' 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.

⁶⁶This 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 townhall venue and thus rely on the estimations of enumerators sitting in the back.

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 townhall meetings, as noted, the evaluation form prompts did not mention taxation and so could not have ‘primed’ citizens about taxation before they chose to participate. A salience effect thus cannot explain greater evaluation submission, nor could it explain treatment effects on beliefs about the government. Salience effects are also unlikely to persist for 8 months, the average time gap between tax collection and outcome measurement. Finally, the fact that townhalls explicitly 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 constitutional government in England, was born of disputes over King John’s taxes.⁶⁷ The fiscal focus of townhalls 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 each of the variables discussed on p. 21 as the outcome.

The first panel demonstrates that individuals in treated neighborhoods perceived greater responsibilities (by 0.12 standard deviations) for the provincial government in public goods provision relative to other possible providers, such as the national government or NGOs.⁶⁸ Importantly, this result does not appear to reflect changes in

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

⁶⁸Table A8 and Figure A22 show results for sub-indices and underlying survey questions, respec-

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 (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 (Column 4 of Table A6), 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 II 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 SDs). 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 (Figure A13). Finally, citizens in treatment reported more positive views of the overall performance of the tax ministry. In sum, the tax campaign does

tively.

⁶⁹Figure 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 non-trivial levels of public goods provision from the provincial government across each of these sectors.

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

appear to have increased the perceived extractive capacity of the government.

Panel III 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 (Panel IV of Table V). Compared to control, they estimated 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 SD increase). They also estimated that a greater share of total tax revenues would be spent on public goods or other 'good uses', rather than being wasted or stolen (a 0.11 SD 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 II). 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 if not absent state, in which government agents are seldom observed doing meaningful work. In such a setting, receiving home visits from tax collectors facile

⁷¹It 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, re-estimating Table V with only nonpayers returns similar results (Table A15), albeit with slightly smaller coefficients. Nonpayers drew similar inferences as a result of the tax campaign, making a motivated reasoning interpretation unlikely.

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.

VI Mechanisms

This section examines possible mechanisms behind the increase in participation caused by the tax campaign: (1) taxpayers in treatment participated more because they felt ownership over public revenues and expected public goods in return (*entitlement*); (2) the tax campaign sent a signal of state capacity that raised the expected benefits of participation (*updating*); and (3) 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 *taxpayers* 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

⁷²A 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 Section A3.3.4.

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

from comparing different complier subgroups. Specifically, I exploit variation in (i) whether households in treated neighborhood 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 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 as well as 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.

1. 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 .

2. Take a linear combination of the collector-specific coefficients to construct a neighborhood-level instrument, i.e.

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

where δ_i weights the collector-specific coefficients.⁷⁴

I construct an instrument analogously for property registration. These 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

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 Table A13 shows that this incentive did indeed increase the probability that households paid the tax. Using both 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

⁷⁴For 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.

⁷⁵Appendix 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 non-response in the midline survey in treatment, which should capture aspects of underlying collector traits that impact participation through channels other than registration or payment (Table A14).

⁷⁶The government introduced these bonuses halfway through the campaign after realizing that some collectors were not comprehensively revisiting the city's remote reaches. See p. 13 for more details on collector compensation.

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 townhall or submit an evaluation. This positive relationship holds when including enumerator (Column 2) and collector fixed effects (Column 3). IV also estimates 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 5-6). 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 (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 we now turn.

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 impacting their

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

future wellbeing 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 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 et al., 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? Table A16 shows that indeed participators in treatment neighborhoods on the whole updated analogously about the government’s extractive capacity. In fact, they were also more likely to think that non-compliance would be punished by the government — even though there was no (detectable) average

⁷⁸Indeed, 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” (p. 66).

⁷⁹That 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).

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 (1) any past visits from government tax collectors, and (2) any past engagement in political protests. The former measure captures state activity in the neighborhood, while the latter captures respondents' exposure to the state outside of the neighborhood.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 (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, Figure A18 plots participation levels in control as a function 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 neigh-

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

borhoods, controlling for the number of total adjacent neighborhoods.⁸¹ Although there are small-in-magnitude spillovers in reported visits from tax collectors (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 righthand 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 7-8 of Table VI are at odds with an updating mechanism.

⁸¹Alternatively, 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).

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

⁸³This 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.

⁸⁴Specifically, 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.

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 the estates demanding better governance *before* they agreed to pay taxes (Bates and Lien, 1985). Otherwise, they would have forfeited their bargaining power. For instance, before England’s 1688 Revolution increased the power of Parliament over the Crown, taxpayers refused to pay the taxes the King demanded, fueling the fiscal crises of the mid 17th century (North and Weingast, 1989). Over time, the standoff led rulers to accord citizens greater voice in government: “When ordinary people resisted [taxation] vigorously, authorities made concessions” (Tilly, 1985). 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 tax payer, but not further interactions with tax collectors, is what predicts participation (Columns 7-8) 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 townhalls, 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 top-

ics. 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 governance (more public goods, more participation). That said, in the context of townhall meetings with government officials, such complaints registered as threats of future non-compliance — a useful bargaining tactic if the government was to take seriously citizens’ demands.⁸⁶ The fact that citizens had chosen to accept the government’s invitation to attend a townhall meeting in the first place — and to make formal complaints in the presence of heads of the finance ministry

⁸⁵An 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.”

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

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 townhalls.⁸⁸ I use Latent Dirichlet Allocation (LDA) (Blei et al., 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.’

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 (Figure A19). In addition, 39% of individuals wrote in 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 com-

⁸⁷Even 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 evaluation form nor these survey questions prompted citizens to think about taxation, respectively.

⁸⁸In addition to selecting from a list of pre-specified topics, analyzed in Figure II, enumerators were encouraged to summarize each comment in a text box.

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.”

ments 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,’ ‘Kasai,’ and ‘country’ juxtaposed with ‘development,’ ‘people,’ ‘population,’ ‘ask,’ and ‘better.’

Moreover, citizens in treatment were more likely to submit evaluations with such demands. Re-estimating the treatment effect on evaluation submission (Column 2 of Table IV) 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 to control (Table A19). In sum, descriptive evidence about citizens’ comments at townhalls and on submitted evaluation forms suggests citizens participated in order 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 (Panel I) is evidence of tax collection stimulating demand for better governance. Additionally, 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 (Table A16). Although drawing on correlations that are suggestive at best, 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

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 paper analyzed the first door-to-door property tax collection campaign in the city of Kananga, D.R. Congo, which increased tax compliance by 11 percentage points. It used the random assignment of the campaign to investigate the supposed participation dividend of tax collection, finding that citizens in taxed neighborhoods were 5 percentage points more likely to attend a townhall meeting or to 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

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

⁹¹How 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.

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 paid, the political response is, from the government's perspective, 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 under-exploit property and income taxes, preferring less efficient and more regressive tax structures than is typically deemed optimal (Gordon and Li, 2009).

LONDON SCHOOL OF ECONOMICS AND CEPR

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

References

- Acemoglu, Daron and James A Robinson**, *The narrow corridor: States, societies, and the fate of liberty*, Penguin Press, 2019.
- Allingham, Michael G and Agnar Sandmo**, “Income tax evasion: A theoretical analysis,” *Journal of Public Economics*, 1972, 1 (3-4), 323–338.
- Almunia, Miguel, Jonas Hjort, Justine Knebelmann, and Lin Tian**, “Strategic or confused firms? Evidence from ‘missing’ transactions in Uganda,” *Working Paper*, 2019.
- Bates, Robert H**, *Markets and states in tropical Africa: The political basis of agricultural policies*, University of California Press, 2014.
- **and Da-Hsiang Donald Lien**, “A note on taxation, development, and representative government,” *Politics & Society*, 1985, 14 (1), 53–70.
- Besley, Timothy and Torsten Persson**, “The origins of state capacity: Property rights, taxation and policy,” *The American Economic Review*, 2009, 99 (4), 1218–1244.
- **and –**, *Taxation and development*, Vol. 5, Elsevier, 2013.
- Best, Michael Carlos, Anne Brockmeyer, Henrik Jacobsen Kleven, Johannes Spinnewijn, and Mazhar Waseem**, “Production versus revenue efficiency with limited tax capacity: Theory and evidence from Pakistan,” *Journal of Political Economy*, 2015, 123 (6), 1311–1355.
- Blei, David M, Andrew Y Ng, and Michael I Jordan**, “Latent Dirichlet Allocation,” *Journal of Machine Learning Research*, 2003, 3 (Jan), 993–1022.
- Bodea, Cristina and Adrienne LeBas**, “The origins of voluntary compliance: Attitudes toward taxation in urban Nigeria,” *British Journal of Political Science*, 2016, 46 (1), 215–238.
- Borge, Lars-Erik, Pernille Parmer, and Ragnar Torvik**, “Local natural resource curse?,” *Journal of Public Economics*, 2015, 131, 101–114.
- Boucoyannis, Deborah**, “No taxation of elites, no representation: State capacity and the origins of representation,” *Politics & Society*, 2015, 43 (3), 303–332.
- Brautigam, Deborah, Odd-Helge Fjeldstad, and Mick Moore**, *Taxation and state-building in developing countries: Capacity and consent*, Cambridge University Press, 2008.
- Brewer, John**, *The sinews of power: War, money, and the English state, 1688-1783*, Harvard University Press, 1990.
- Brollo, Fernanda, Tommaso Nannicini, Roberto Perotti, and Guido Tabellini**, “The political resource curse,” *The American Economic Review*, 2013, 103 (5), 1759–1796.
- Cantoni, Davide and Noam Yuchtman**, “Medieval universities, legal institutions, and the commercial revolution,” *The Quarterly Journal of Economics*, 2014, 129 (2), 823–887.

- , **Cathrin Mohr**, and **Matthias Weigand**, “The rise of fiscal capacity,” *Working Paper*, 2019.
- Carpio, Lucia Del**, “Are the neighbors cheating? Evidence from a social norm experiment on property taxes in Peru,” *Working Paper*, 2013.
- Carrillo, Paul, Dina Pomeranz**, and **Monica Singhal**, “Dodging the taxman: Firm misreporting and limits to tax enforcement,” *American Economic Journal: Applied Economics*, 2017, 9 (2), 144–164.
- Casaburi, Lorenzo and Ugo Troiano**, “Ghost-house busters: The electoral response to a large anti-tax evasion program,” *The Quarterly Journal of Economics*, 2015, 131 (1), 273–314.
- Caselli, Francesco and Guy Michaels**, “Do oil windfalls improve living standards? Evidence from Brazil,” *American Economic Journal: Applied Economics*, 2013, 5 (1), 208–38.
- Casey, Katherine, Rachel Glennerster**, and **Edward Miguel**, “Reshaping institutions: Evidence on aid impacts using a preanalysis plan,” *The Quarterly Journal of Economics*, 2012, 127 (4), 1755–1812.
- Cheema, Ali, Asim Ijaz Khwaja**, and **Adnan Qadir**, “Local government reforms in Pakistan: Context, content and causes,” *Decentralization and local governance in developing countries*, 2006, pp. 257–84.
- Chen, Ting and James Kung**, “Do land revenue windfalls create a political resource curse? Evidence from China,” *Journal of Development Economics*, 2016, 123, 86–106.
- Christensen, Darin and Francisco Garfias**, “The politics of property taxation: Fiscal infrastructure and electoral incentives in Brazil,” *Working Paper*, 2018.
- Coate, Stephen and Stephen Morris**, “On the form of transfers to special interests,” *Journal of Political Economy*, 1995, 103 (6), 1210–1235.
- De la Cuesta, Brandon, Helen Milner, Lucy Martin**, and **Daniel Nielson**, “Do indirect taxes promote accountability? Testing the effects of revenue modality on citizen behavior,” *Working Paper*, 2019.
- , **Helen V Milner, Daniel Nielson**, and **Steve Knack**, “Does source matter to citizens? Experimental evidence on aid, oil and taxes from Ghana and Uganda,” *Proceedings of the National Academy of Sciences*, 2020, pp. 23–24.
- , **Lucy Martin, Helen Milner**, and **Daniel Nielson**, “Owning it: Accountability and citizens’ ownership over oil, aid, and taxes,” *Journal of Politics*, 2020.
- DFID**, “DFID spend on improving tax systems,” *Policy Report*, 2016.
- Dittmar, Jeremiah E and Ralf R Meisenzahl**, “Public goods institutions, human capital, and growth: Evidence from German history,” *The Review of Economic Studies*, 2019.
- Dunning, Thad**, *Crude democracy: Natural resource wealth and political regimes*, Vol. 7, Cambridge University Press Cambridge, 2008.

- , **Felipe Monestier, Rafael Piñeiro, Fernando Rosenblatt, and Guadalupe Tuñón**, “Is paying taxes habit forming? Theory and evidence from Uruguay,” *Working Paper*, 2015.
- Englebert, Pierre**, “Aspirations and realities in Africa: The DRC’s electoral sideshow,” *Journal of Democracy*, 2019, 30 (3), 124–138.
- , **Alma Bezares Calderon, and Lisa Jené**, “Provincial tribalisation: The transformation of ethnic representativeness under decentralisation in the DR Congo,” *SLRC Working Paper*, 2018.
- Ertman, Thomas**, *Birth of the Leviathan: Building states and regimes in medieval and early modern Europe*, Cambridge University Press, 1997.
- Ferraz, Claudio and Joana Monteiro**, “Learning to punish: Resource windfalls and political accountability in Brazil,” *Working Paper*, 2014.
- Fjeldstad, Odd-Helge, Merima Ali, and Tom Goodfellow**, “Taxing the urban boom: Property taxation in Africa,” *CMI Policy Paper*, 2017.
- Franzsen, Riel and William McCluskey**, *Property tax in Africa*, Cambridge, MA: Lincoln Institute of Land Policy, 2017.
- Fukuyama, Francis**, *The origins of political order: From prehuman times to the French Revolution*, Farrar, Straus and Giroux, 2011.
- Gadenne, Lucie**, “Tax me, but spend wisely? Sources of public finance and government accountability,” *American Economic Journal: Applied Economics*, 2017, 9 (1), 274–314.
- Gennaioli, Nicola and Hans-Joachim Voth**, “State capacity and military conflict,” *The Review of Economic Studies*, 2015, 82 (4), 1409–1448.
- Gordon, Roger and Wei Li**, “Tax structures in developing countries: Many puzzles and a possible explanation,” *Journal of Public Economics*, 2009, 93 (7), 855–866.
- Gottlieb, Jessica and Florian M Hollenbach**, “Fiscal capacity as a moderator of the taxation-accountability hypothesis,” *Working Paper*, 2018.
- Haber, Stephen and Victor Menaldo**, “Do natural resources fuel authoritarianism? A reappraisal of the resource curse,” *American Political Science Review*, 2011, 105 (1), 1–26.
- Herb, Michael**, “No representation without taxation? Rents, development, and democracy,” *Comparative Politics*, 2005, pp. 297–316.
- Hoffman, Philip T and Jean-Laurent Rosenthal**, “The political economy of warfare and taxation in early modern Europe: Historical lessons for economic development,” in “Frontiers of Institutional Economics,” Academic Press, 1997, pp. 31–55.
- IMF**, “Revenue mobilization in developing countries,” *Policy Report*, 2011.
- Jensen, Anders**, “Employment structure and the rise of the modern tax system,” *Working Paper*, 2018.

- Jensen, Nathan and Leonard Wantchekon**, “Resource wealth and political regimes in Africa,” *Comparative Political Studies*, 2004, 37 (7), 816–841.
- Jibao, Samuel and Wilson Prichard**, “Rebuilding local government finances after conflict: Lessons from a property tax reform programme in post-conflict Sierra Leone,” *The Journal of Development Studies*, 2016, 52 (12), 1759–1775.
- Kaldor, Nicholas**, “Will underdeveloped countries learn to tax?,” *Foreign Affairs*, 1963, 41 (2), 410–419.
- Kao, Jay, Xiaobo Lu, and Didac Queralt**, “Expected return for taxation: Do elites differ from nonelites?,” *Working Paper*, 2019.
- Khan, Adnan Q, Asim I Khwaja, and Benjamin A Olken**, “Tax farming redux: Experimental evidence on performance pay for tax collectors,” *The Quarterly Journal of Economics*, 2015, 131 (1), 219–271.
- Kleven, Henrik J and Mazhar Waseem**, “Using notches to uncover optimization frictions and structural elasticities: Theory and evidence from Pakistan,” *The Quarterly Journal of Economics*, 2013, 128 (2), 669–723.
- Kleven, Henrik Jacobsen, Martin B Knudsen, Claus Thustrup Kreiner, Søren Pedersen, and Emmanuel Saez**, “Unwilling or unable to cheat? Evidence from a tax audit experiment in Denmark,” *Econometrica*, 2011, 79 (3), 651–692.
- Lieberman, Evan S**, *Race and regionalism in the politics of taxation in Brazil and South Africa*, Cambridge University Press, 2003.
- Luttmer, Erzo F P and Monica Singhal**, “Tax Morale,” *Journal of Economic Perspectives*, 2014, 28 (4), 149–168.
- Martin, Lucy**, “Taxation, loss aversion, and accountability: Theory and experimental evidence for taxation’s effect on citizen behavior,” *Working Paper*, 2014.
- Martinez, Luis R**, “Sources of revenue and government performance: Evidence from Colombia,” *Working Paper*, 2019.
- Mascagni, Giulia, Andualem Mengistu, and Firew B Boldeyes**, “Can ICTs increase tax? Experimental evidence from Ethiopia,” *Working Paper*, 2018.
- , **Christopher Nell, Nara Monkam, and Denis Mukama**, “The carrot and the stick: Evidence on voluntary tax compliance from a pilot field experiment in Rwanda,” *Working Paper*, 2016.
- McGuirk, Eoin F**, “The illusory leader: Natural resources, taxation and accountability,” *Public Choice*, 2013, 154 (3-4), 285–313.
- Meagher, Kate**, “Taxing times: Taxation, divided societies and the informal economy in Northern Nigeria,” *The Journal of Development Studies*, 2018, 54 (1), 1–17.
- Miguel, Edward and Michael Kremer**, “Worms: Identifying impacts on education and health in the presence of treatment externalities,” *Econometrica*, 2004, 72 (1), 159–217.

- Moore, Mick**, “Revenues, state formation, and the quality of governance in developing countries,” *International Political Science Review*, 2004, 25 (3), 297–319.
- , **Wilson Prichard, Odd-Helge Fjeldstad et al.**, “Taxing Africa,” *University of Chicago Press Economics Books*, 2018.
- Naritomi, Joana**, “Consumers as Tax Auditors,” *American Economic Review*, 2019.
- North, Douglass C and Barry R Weingast**, “Constitutions and commitment: The evolution of institutions governing public choice in seventeenth-century England,” *The Journal of Economic History*, 1989, 49 (4), 803–832.
- OECD**, “Citizen-state relations: Improving governance through tax reform,” *Policy Report*, 2010.
- Olken, Benjamin A**, “Monitoring corruption: Evidence from a field experiment in Indonesia,” *Journal of Political Economy*, 2007, 115 (2), 200–249.
- **and Monica Singhal**, “Informal taxation,” *American Economic Journal: Applied Economics*, 2011, 3 (4), 1–28.
- Oster, Emily**, “Unobservable selection and coefficient stability: Theory and evidence,” *Journal of Business & Economic Statistics*, 2019, 37 (2), 187–204.
- Paler, Laura**, “Keeping the public purse: An experiment in windfalls, taxes, and the incentives to restrain government,” *American Political Science Review*, 2013, 107 (4), 706–725.
- , **Wilson Prichard, Raul Sanchez de la Sierra, and Cyrus Samii**, “Survey on total tax burden in the DRC,” *DFID Report*, 2016.
- Pomeranz, Dina**, “No taxation without information: Deterrence and self-enforcement in the value-added tax,” *American Economic Review*, 2015, 105 (8), 2539–2569.
- **and José Vila-Belda**, “Taking state-capacity research to the field: Insights from collaborations with tax authorities,” *Annual Review of Economics*, 2019, 11.
- Prichard, Wilson**, *Taxation, responsiveness and accountability in Sub-Saharan Africa: The dynamics of tax bargaining*, Cambridge University Press, 2015.
- , “Electoral competitiveness, tax bargaining and political incentives in developing countries: Evidence from political budget cycles affecting taxation,” *British Journal of Political Science*, 2018, 48 (2), 427–457.
- Queralt, Didac**, “War, international finance, and fiscal capacity in the long run,” *International Organization*, 2019, 73 (4), 713–753.
- Radley, Ben**, *Mining industrialisation in the African periphery: Disruption and dependency in South Kivu, DRC*, PhD Dissertation, 2019.
- Ramsay, Kristopher W**, “Revisiting the resource curse: Natural disasters, the price of oil, and democracy,” *International Organization*, 2011, 65 (3), 507–529.

- Reid, Otis and Jonathan L Weigel**, “The supply of bribes: Evidence from roadway tolls in the DRC,” *Working Paper*, 2020.
- Robinson, James A, Ragnar Torvik, and Thierry Verdier**, “Political foundations of the resource curse,” *Journal of Development Economics*, 2006, 79 (2), 447–468.
- Rodden, Jonathan**, “Decentralized rule and revenue,” *Working Paper*, 2016.
- Ross, Michael L**, “Does oil hinder democracy?,” *World Politics*, 2001, 53 (3), 325–361.
- , “Does taxation lead to representation?,” *British Journal of Political Science*, 2004, 34 (2), 229–249.
- Sanchez de la Sierra, Raul**, “On the origin of states: Stationary bandits and taxation in Eastern Congo,” *Journal of Political Economy*, 2019.
- Sankoh, Abdul J, Mohammad F Huque, and Satya D Dubey**, “Some comments on frequently used multiple endpoint adjustment methods in clinical trials,” *Statistics in Medicine*, 1997, 16 (22), 2529–2542.
- Scheve, Kenneth and David Stasavage**, *Taxing the rich: A history of fiscal fairness in the United States and Europe*, Princeton University Press, 2016.
- Schumpeter, Joseph A**, “The crisis of the tax state,” *International Economic Papers*, 1918, 4, 5–38.
- Scott, James C**, *Seeing like a state: How certain schemes to improve the human condition have failed*, Yale University Press, 1998.
- , *Against the grain: A deep history of the earliest states*, Yale University Press, 2017.
- Sjoberg, Fredrik Matias, Jonathan Mellon, Tiago Carneiro Peixoto, Johannes Zacharias Hemker, and Lily Lee Tsai**, “Voice and punishment: A global survey experiment on tax morale,” *Working Paper*, 2019.
- Sjursen, Ingrid**, “Accountability and taxation: Experimental evidence,” *Working Paper*, 2018.
- Stearns, Jason**, *Dancing in the glory of monsters: The collapse of the Congo and the great war of Africa*, Public Affairs, 2012.
- Tiebout, Charles M**, “A pure theory of local expenditures,” *Journal of Political Economy*, 1956, 64 (5), 416–424.
- Tilly, Charles**, “War making and state making as organized crime,” *Violence: A reader*, 1985, pp. 35–60.
- Timmons, Jeffrey F and Francisco Garfias**, “Revealed corruption, taxation, and fiscal accountability: Evidence from Brazil,” *World Development*, 2015, 70, 13–27.
- Tourek, Gabriel**, “Targeting in tax behavior: Evidence from Rwandan firms,” *Working Paper*, 2019.

Weigel, Jonathan L, “The taxman cometh: A virtuous cycle of compliance and state legitimacy in the D.R. Congo,” *Working Paper*, 2018.

World Bank, “World Development Report 2011: Conflict, security, and development,” *Policy Report*, 2011.

Young, Crawford and Thomas Edwin Turner, *The rise and decline of the Zairian state*, University of Wisconsin Press, 2013.

VIII Tables and figures

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

N = number of observations, J = number of clusters (neighborhoods). More neighborhoods were assigned to treatment (1) because the government was eager to have revenues in 2016, and (2) to accommodate cross-randomized anticorruption interventions, as further discussed in Section A2.1. The endline was only administered in 356 neighborhoods due to insecurity in the commune of Nganza, discussed on p. 16. All 71 neighborhoods from this commune (balanced across treatment and control) were dropped before respondents could be sampled and invited to participate.

Table II: Summary statistics and balance checks

Neighborhood characteristics (baseline)	<i>Summary statistics</i>					<i>Balance test</i>	
	Mean	SD	Min	Max	N	β_1	SE
Neighborhood size (square km)	0.14	0.08	0	1	356	0.008	0.008
Population density (pop./square km)	1195.08	585.32	71	3042	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
Individual characteristics (endline)							
Age (years)	48.59	17.02	18	102	2913	0.852	0.733
Female (dummy)	0.41	0.49	0	1	2913	0.028	0.019
Education (years)	9.61	4.13	0	19	2909	-0.031	0.207
No schooling (dummy)	0.05	0.21	0	1	2909	-0.005	0.009
Unemployed (dummy)	0.42	0.49	0	1	2913	0.016	0.020
Government worker (dummy)	0.11	0.31	0	1	2913	-0.003	0.012
Monthly income (USD)	106.41	193.99	0	4800	2903	-0.333	8.217
Household wealth index (normalized)	0.47	0.13	0	1	2913	-0.017**	0.008
House with non-mudbrick walls (dummy)	0.47	0.50	0	1	2912	-0.034	0.032
Any source of electricity (dummy)	0.19	0.39	0	1	2913	-0.015	0.018
Owens vehicle (dummy)	0.16	0.36	0	1	2913	-0.006	0.015
Owens business of any size (dummy)	0.22	0.42	0	1	2913	0.034*	0.018
Has renters in compound (dummy)	0.23	0.42	0	1	2913	-0.014	0.018
Multiple plot owner (dummy)	0.23	0.42	0	1	2913	0.005	0.016
Born in Kananga (dummy)	0.41	0.49	0	1	2913	-0.012	0.020
Majority ethnicity (dummy)	0.73	0.45	0	1	2913	0.014	0.020
Native Tshiluba speaker (dummy)	0.85	0.36	0	1	2913	0.012	0.017
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	2363	-0.004	0.009
Refusals midline survey (dummy)	0.07	0.26	0	1	27443	0.003	0.009
Refusals in endline survey (dummy)	0.04	0.20	0	1	3232	0.003	0.007
Any attrition in endline survey (dummy)	0.10	0.30	0	1	3232	0.013	0.012

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 on p. 16. Other variables have a lower N because of non-responses 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 II for further details on these comparisons. Data: geographic data and baseline, midline, and endline surveys.

Table III: Effects of the campaign on collector visits, taxpayer registration, property tax compliance, and revenues

<i>Dependent variable:</i>	Visited by collector	Registered as taxpayer	Property tax compliance		Tax revenue per person
<i>Unit:</i>	<i>Household</i>	<i>Household</i>	<i>Household</i>	<i>Neighborhood</i>	<i>Neighborhood</i>
	(1)	(2)	(3)	(4)	(5)
Campaign	0.815*** (0.013)	0.788*** (0.009)	0.103*** (0.007)	0.115*** (0.009)	367.295*** (62.518)
Stratum FE	Yes	Yes	Yes	Yes	Yes
R^2	0.640	0.577	0.054	0.396	0.173
Observations	27443	27443	27443	356	356
Clusters	356	356	356	<i>N/A</i>	<i>N/A</i>
Control Mean	0.0499	0.0000	0.0006	0.0005	1.5683

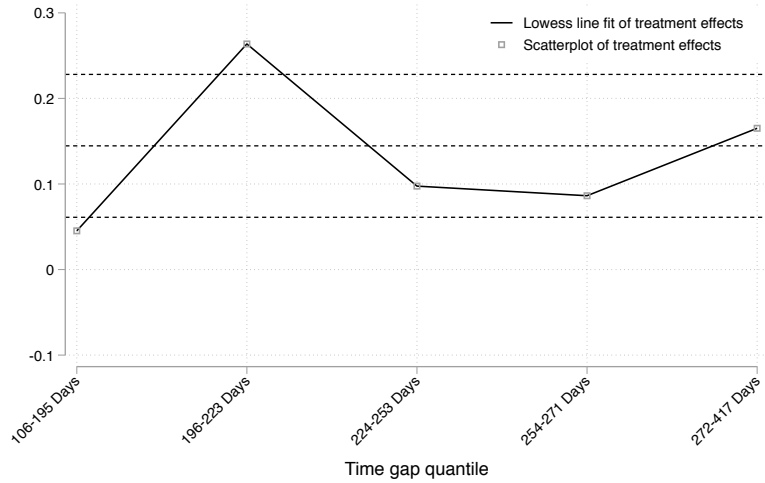
Visited by collector is an indicator for households reporting at least one visit by tax collectors in 2016. *Registered as taxpayer* is an indicator for households that were registered by collectors and assigned a unique tax ID. *Property tax compliance* is an indicator for households that paid the property tax in 2016. *Tax revenue per person* is the total property tax receipts per neighborhood divided by the estimated number of potential taxpayers. See p. 18 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, as discussed on p. 16). 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.

Table IV: Effects of the campaign on participation

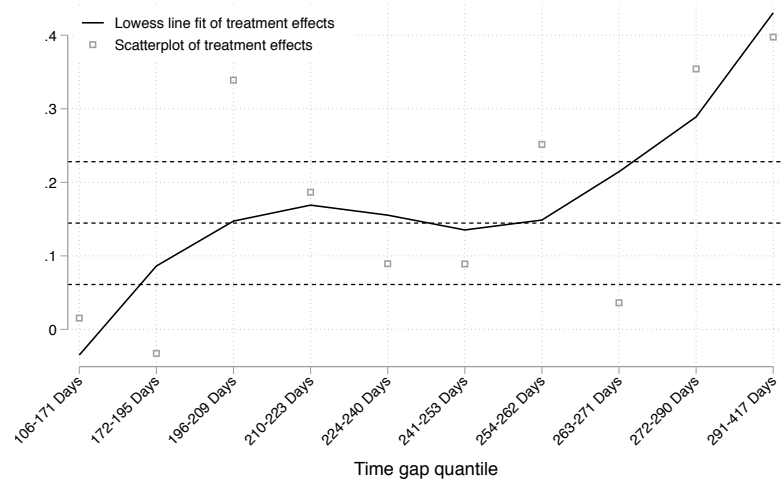
	Townhall meeting attendance (1)	Evaluation form submission (2)	Townhall or evaluation (3)	Townhall and evaluation (4)	Index (townhall & evaluation) (5)	Cost of participation (transport) (6)	Cost of participation (transport & opp.) (7)
Campaign	0.045** (0.020)	0.024** (0.012)	0.050*** (0.016)	0.027*** (0.009)	0.145*** (0.043)	0.050*** (0.017)	0.071*** (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	1934	2913	2913	2913	2913	2913	2913
Clusters	252	356	356	356	356	356	356
Control Mean	0.17	0.099	0.16	0.035	-0.077	0.11	0.16
Dep. Var.	Binary	Binary	Binary	Binary	Std. Index	% Daily Inc.	% Daily Inc.
Rand. Inf. p	0.023	0.058	0.0048	0.0048	0.0022	0.0072	0.0022
Bonferroni p	0.033	0.067	N/A	N/A	N/A	N/A	N/A

Townhall meeting attendance is an indicator variable that equals 1 if an individual attended a townhall meeting. *Evaluation form submission* is an indicator variable that equals 1 if an individual submitted his or her evaluation. *Townhall or evaluation* indicates that an individual either attended a townhall meeting or submitted an evaluation form. *Townhall and evaluation* indicates that an individual attended a townhall meeting and submitted an evaluation form. *Index (townhall & evaluation)* is the standardized sum of *Townhall 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 townhall and/or submit an evaluation as a share of average daily household income. See p. 20 for details on all variables. Covariates include gender, age, age squared, wealth, a business owner dummy, and a dummy for public lighting in the neighborhood, as discussed on p. 23. 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, as described on p. 26. Data: endline survey merged with townhall attendance and submitted evaluation records as well as cost estimates from enumerator motorcycle taxi receipts. The sample size is smaller in Column 1 because the government discontinued townhalls after April 1 due to insecurity in Kananga. Endline respondents sampled after this date never had a chance to attend a meeting (see p. 19).

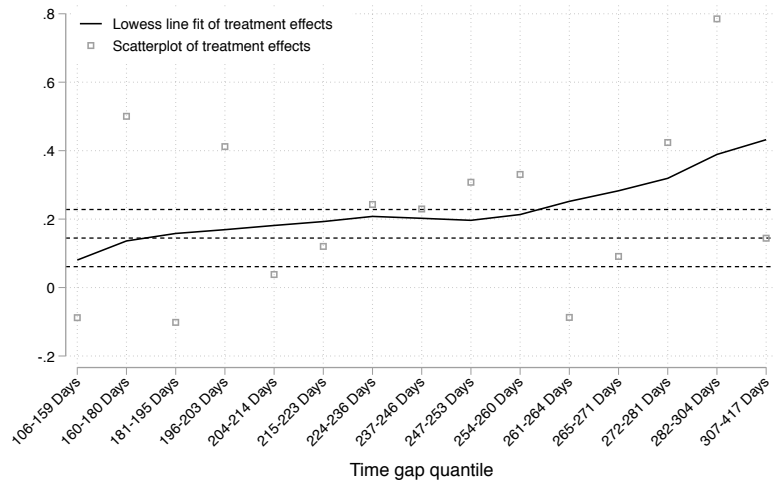
Panel A: 5 Time Gap Quantiles



Panel B: 10 Time Gap Quantiles



Panel C: 15 Time Gap Quantiles



Panel D: 20 Time Gap Quantiles

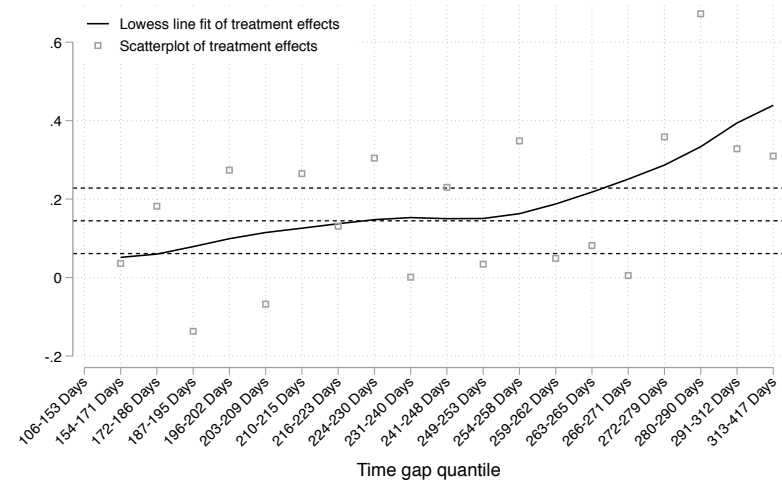


Figure I: Effects of the campaign on costly participation, *Index (townhall & evaluation)*, in 5, 10, 15, and 20 quantiles of the (random) time gap between the tax campaign and the outcome measurement.

Notes: The dotted lines show the average treatment effect (in standard deviation units) and 95% confidence interval for the whole sample. The grey 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.

Table V: Effects of the campaign on citizens' beliefs about the government

<i>Dependent variable</i>	β	SE	R^2	N	μ_c
<i>Panel I: Responsibility of government</i>					
Resp. for public goods provision	0.117**	0.051	0.041	2913	-0.063
<i>Panel II: Extractive capacity of government</i>					
Information about taxpayers	0.147***	0.044	0.086	2910	-0.075
Ability to punish tax evaders	0.046	0.047	0.044	2883	-0.027
Perceived citizen tax compliance	0.343***	0.053	0.100	1954	-0.170
Performance of tax ministry	0.122***	0.047	0.065	2791	-0.104
<i>Panel III: Productive capacity of government</i>					
Ability to provide public goods	-0.013	0.053	0.038	2484	0.007
Performance of government	0.043	0.049	0.042	2795	-0.048
<i>Panel IV: Integrity of government</i>					
Integrity of tax collectors	0.187***	0.044	0.043	2732	-0.136
Integrity of government spending	0.109**	0.050	0.054	2766	-0.100
Transparency of government	0.031	0.045	0.078	2890	-0.042

Each row summarizes an OLS estimation of Equation 1, with the dependent variable noted in the first column. β is the coefficient on the treatment indicator, followed by the cluster-robust standard error, R^2 , number of observations, and control group mean. There are 356 clusters. Each dependent variable, described briefly on p. 21 and in detail in Section A5, is standardized to facilitate interpretation of coefficient magnitude. Data: endline survey. The number of observations varies across regressions due to non-response for specific survey questions.

Table VI: Correlates of participation within treatment neighborhoods

<i>Dependent variable</i>	Townhall or evaluation							
	OLS		IV	OLS		IV	OLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Registered as taxpayer	0.038** (0.018)	0.050*** (0.018)	0.040** (0.019)	0.124 (0.108)	0.040** (0.019)	0.151 (0.140)	0.040** (0.019)	0.051*** (0.019)
Property tax compliance					-0.008 (0.035)	-0.111 (0.468)	-0.016 (0.039)	-0.008 (0.038)
Number of collectors							-0.010 (0.012)	-0.003 (0.012)
Number of collector visits							0.018 (0.013)	0.017 (0.013)
Collector time spent (log)							-0.007 (0.013)	-0.010 (0.014)
Observed tablet/printer							0.006 (0.027)	0.003 (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	1703	1703	1703	1703	1703	1703	1691	1691
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

The outcome is an indicator for individuals who attended a townhall or submitted an evaluation (the same as in Column 3 in Table IV). *Property tax compliance* is an indicator for households that verifiably paid the property tax, as discussed on p. 18. *Registered as taxpayer* is an indicator for households with tax IDs. *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 7-8 due to "don't know" responses to *Collector time spent (log)*. IV estimates use the JIVE instruments *Visit propensity* and *Payment propensity*, discussed on p. 37, as well as a dummy variable for households assigned to the double collector bonus. Table A13 shows the first stage. Data: endline survey merged with participation records and government tax database.

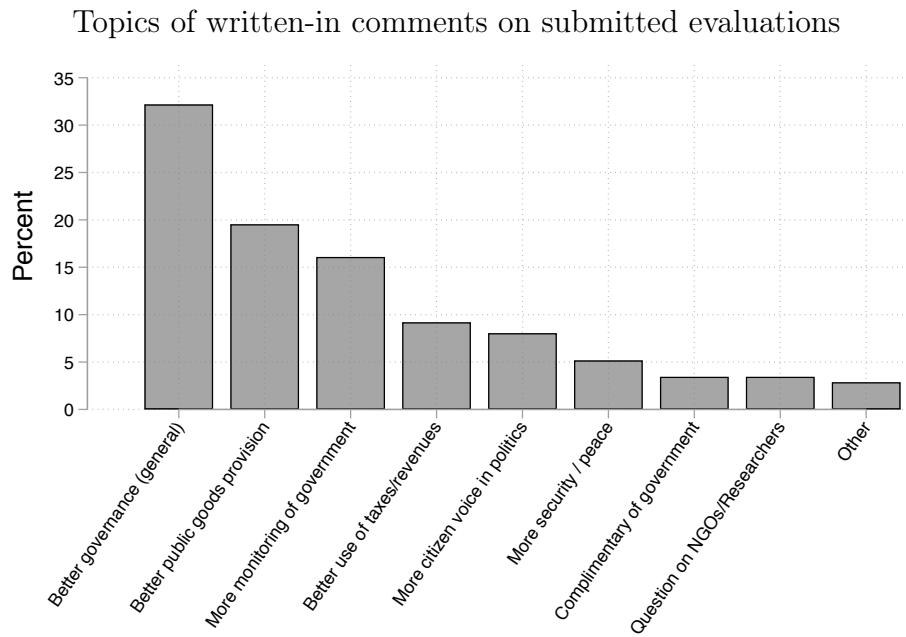
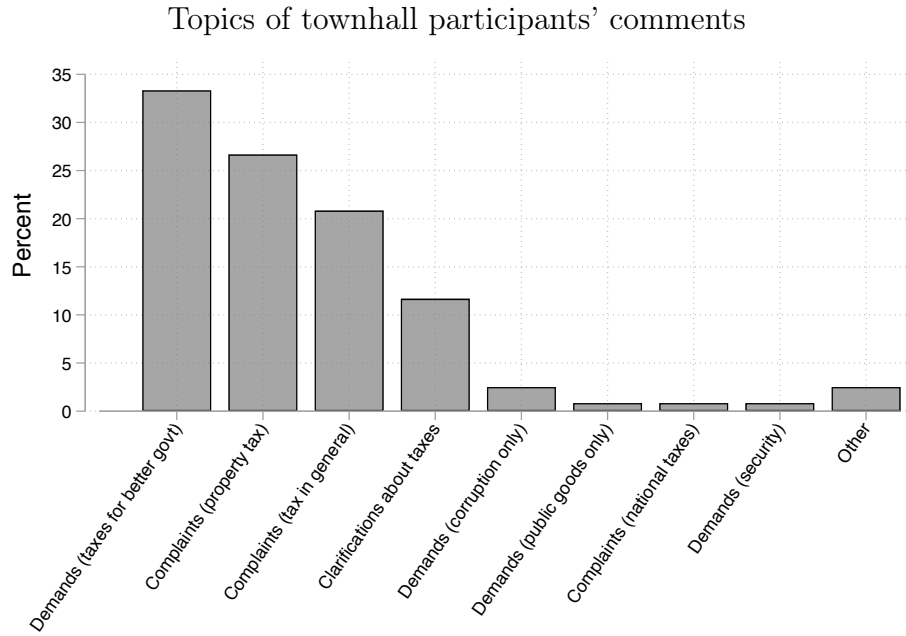


Figure II: Topics of citizens' comments at townhall meetings and on submitted government evaluations.

Notes: For details on the classification of townhall comment topics, see p. 44, as well as p. 17 in the online appendix. 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, e.g. '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.

Table VII: Topics of citizen comments at townhalls and written-in comments on submitted evaluations

<i>Panel I: Topics of citizen comments at townhall meetings</i>					
<i>Order</i>	(1)	(2)	(3)	(4)	(5)
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

<i>Panel II: Topics of written-in comments on submitted evaluations</i>					
<i>Order</i>	(1)	(2)	(3)	(4)	(5)
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	kasai	could

This table reports the first ten words in each of the five main topics identified by Latent Dirichlet Allocation (Blei et al., 2003) applied to two sources of text that offer insight into citizens’ reasons for choosing to participate. Panel I concerns a transcription of citizens’ comments during townhall meetings. Panel II concerns citizens’ written-in comments in the optional bottom section of evaluation forms. See p. 45 for further details on the method and its interpretation.