



Working Paper 01

Participation, Legitimacy and Fiscal Capacity in Weak States: Evidence from Participatory Budgeting

Kevin Grieco, Abou Bakarr Kamara, Niccolò F. Meriggi,
Julian Michel and Wilson Prichard

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Participation, Legitimacy and Fiscal Capacity in Weak States: Evidence from Participatory Budgeting

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Abstract

Building durable fiscal capacity requires that states obtain compliance with their taxes—a persistent challenge for states with low enforcement capacity. One promising option for governments in weak states is to raise voluntary compliance by enhancing governmental legitimacy. This study reports results from a participatory budgeting policy experiment in Sierra Leone designed to increase legitimacy and tax compliance by inviting public participation in local policy decision-making. In phone-based town halls, participants shared policy preferences with neighbors and local politicians and then voted for public services that were subsequently implemented. We find that the intervention durably increased participants' perceptions of government legitimacy. However, contrary to influential models of tax compliance, we report a robust null effect on tax compliance behavior. Participants' partisan affiliation strongly conditions the treatments' effects on tax compliance and attitudes toward paying taxes: We find large, positive impacts among copartisans of the incumbent government but significant negative impacts among non-copartisans. Our results highlight that the legitimacy gains of participatory interventions may not increase voluntary tax compliance when participation politicizes compliance. *JEL: H20, D72*

Keywords: fiscal capacity, participatory budgeting, taxation, legitimacy, state building.

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1 Introduction

The weakness of many states in sub-Saharan Africa is a key barrier to economic development and political stability (Michalopoulos and Papaioannou 2020; Besley and Persson 2011).¹ Weak tax systems across the continent are, in turn, both effect and cause: state weakness limits the ability of states to raise revenue effectively, while weak revenue collection limits investments in state building (Besley and Persson 2013). How can governments in weak states break out of this pernicious cycle of low state capacity and insufficient revenue collection?

The conventional answer has been to focus on strengthening tax enforcement (e.g., Kleven et al. 2011; Slemrod 2019; Bergeron et al. 2024; Kapon et al. 2024). However, relying on enforcement alone has proven challenging due to limited capacity to pursue non-compliant taxpayers and political resistance, which has made expanding tax enforcement politically unattractive (Christensen and Garfias 2021; Dom et al. 2022).²

In this paper, we propose instead that governments can advance efforts to build fiscal capacity—and state capacity more broadly—by increasing their legitimacy. This focus on legitimacy reflects two key channels through which higher legitimacy could enable capacity building. First, citizens are more likely to comply with the demands of legitimate governments (Levi 1988; Besley 2020; Timmons and Garfias 2015). This is particularly important in weak states where, due to low enforcement capacity, governments must rely more on quasi-voluntary compliance.

Second, more legitimate governments are likely to face less political opposition to efforts to build state capacity (Besley and Dray 2024). Such opposition can come from taxpayers who reject higher tax burdens and lack trust that governments will deliver tangible benefits in return (Gottlieb and Hollenbach 2018; Prichard 2015; Christensen and Garfias 2021; Robinson 2023), entrenched government officials who benefit from the status quo (Prichard et al. 2019) or elites who worry that a stronger state may be turned against them (Garfias 2018; North et al. 2009).

In short, building government legitimacy can enhance fiscal capacity by increasing the political acceptability of efforts to strengthen it or by improving quasi-voluntary tax compliance.

One way that governments may cultivate legitimacy is by inviting public participation in political affairs, which is central to both classic notions of legitimate government (Locke 1690) and modern democratic theory (Pateman 1970). Surveying America's young democracy, Tocqueville concluded that when citizens participate in law-making, "law thereby acquires a great authority" (De Tocqueville 2010, pg. 393). Indeed, the link between public participation and tax compliance is central to seminal accounts of the development of fiscal capacity in early modern Europe, which posit that political leaders traded expanded political voice to elites in exchange for consistent sources of revenue (North and Weingast 1989; Bates and Lien 1985). In contemporary representative democracies, one method for expanding political voice is to allow citizens to *directly* shape policy outcomes, such as through participatory budgeting.

¹ According to Hanson and Sigman (2021), state capacity is lower in sub-Saharan Africa than in any other region in the world and has been since at least the 1960s. Conceptually, we follow Migdal (1988, pg. 4), who defines state capacity as the capability of the state to "achieve the kinds of changes in society that their leaders have sought through state planning, policies, and actions" (see also Hanson and Sigman 2021; Cingolani 2013).

² For this reason, an emerging literature explores how non-punitive service delivery interventions affect tax compliance (Kresch et al. 2023; Brockmeyer et al. 2024; Carrillo et al. 2021; Krause 2020).

Accordingly, this paper examines the relationship between political participation, legitimacy and tax compliance in Freetown, Sierra Leone, by leveraging a participatory budgeting field experiment designed and implemented in collaboration with the Freetown City Council (FCC). In doing so, we contribute to the emerging literature on institutional experiments (Callen et al. 2023) by providing the first field experimental study of whether participatory budgeting can facilitate state capacity building.³

The intervention sought to give participants greater voice in, and control over, policy decisions regarding local development projects. Program participants joined WhatsApp chat groups—referred to as *Digital Town Halls* (DTHs)—alongside up to 36 other property owners from their neighborhood. Within these groups, they discussed service preferences, shared these preferences with local politicians and then voted on the services (valued at approximately US\$1,500) they wanted to see implemented in their neighborhood. The selected services were implemented six months later, and participants were informed of this through a phone call. To identify causal effects, we use a matched-pair design (King et al. 2007) to randomize half of 3,618 property owners into the treatment group. We observed individual-level tax compliance through administrative records and surveyed study participants at three stages: before the process, after services were selected but before they were delivered and after services were delivered.

We find that participating in the DTHs durably increases perceptions of government legitimacy. In line with standard conceptualizations of legitimacy (Levi 1997; Levi et al. 2009), we measure citizens' perceptions of (i) their influence over policy, (ii) government service delivery performance, (iii) government administrative competence and (iv) politicians' performance in three survey waves. The intervention significantly increases all nine legitimacy outcomes at the endline survey, which was conducted soon after services were implemented and seven months after the conclusion of the DTHs. Importantly, while substantial legitimacy gains are observed at midline, citizens' perceptions of the government's administrative competency do not improve until endline, following successful service delivery. Observing how legitimacy evolves at these crucial junctures is a key design innovation of our study. Additionally, we demonstrate that these legitimacy gains are consistent across partisan groups.

Turning to tax compliance behavior, we find large and significant heterogeneous treatment effects conditional on participants' (pretreatment) partisan affiliation. Among copartisans of the mayor, the treatment *increases* compliance by 7.4 percentage points, which is a substantial 31.8% increase over the group's control compliance rate. By contrast, we find that the treatment *lowered* compliance for non-copartisans by 4.0 percentage points. In our case, these countervailing forces are relatively balanced, leading to no average effect on compliance. This null effect is robust to alternative estimation specifications or operationalizations of tax compliance.

Why does partisan affiliation moderate the intervention's effect on tax compliance? We propose a novel channel where participation impacts compliance through *partisan cue exposure*: through participation, citizens learn where political actors stand on specific issues, prompting them to update their expectations about the policy's benefits (Zaller 1992; Brockman and Butler 2017). When leaders from opposing parties have divergent views on a policy—as was the case in Freetown during the highly politicized tax reform—increased citizen participation leads individuals to adjust their policy preferences and related

³ Following Acemoglu et al. (2005), we define institutions as mechanisms through which social choices are determined and implemented (also see Alesina and Giuliano 2015).

compliance behavior differently, depending on their political allegiance. Consistent with this mechanism, we find that partisanship also moderates the intervention's effect on the expected benefits of taxation, as measured by participants' willingness to trade more taxes for improved services. We rule out the possibility that these conditional treatment effects are driven by differences between partisan groups in (a) economic owner characteristics, (b) normative beliefs about taxation, (c) attribution of who funded and organized the DTHs, (d) proximity to delivered services and (e) service preferences.

Ultimately, our study makes five key contributions. First, we contribute to the literature on participatory institutions and development (Putnam 1993; Acemoglu et al. 2001; North and Weingast 1989) by showing that direct democracy in the form of participatory budgeting increases government legitimacy in weak states. Previous field experimental research on whether participatory institutions in lower-income countries can increase political legitimacy has largely yielded null (Casey et al. 2012; Fearon et al. 2015; Humphreys et al. 2019; Khan et al. 2022) or mixed (Olken 2010) results.⁴ While Beath et al. (2017) find that increasing citizen participation in the selection of community development projects boosts approval of political leaders in Afghanistan, they cannot untangle the effects of participation and service delivery.⁵ By holding the delivery of selected services constant across treatment and control, our design allows us to isolate effects of participation. Ultimately, our findings suggest a more optimistic view of what participatory fora can achieve: governments can use participatory budgeting to increase participants' perceptions of government legitimacy.

Second, we offer one of the first experimental assessments of the impacts of participatory budgeting programs. Since gaining popularity in South America in the early 1990s, participatory budgeting has been adopted by thousands of local governments worldwide (Sintomer et al. 2010; Dias 2018).⁶ Despite its prevalence and promotion as a "best practice" by international organizations such as the World Bank (Goldfrank 2012; Shah 2007) and the United Nations (Cabannes 2004), there is limited robust evidence on the impacts of participatory budgeting. To our knowledge, the only prior experimental assessment is by Wu et al. (2024), who conduct a field experiment in China to examine the effects of participatory budgeting on civic engagement.⁷ They find that participation in budgeting increases engagement in areas beyond budgeting itself. We build on this work by studying a different set of policy-relevant outcomes: government legitimacy and tax compliance. Our experimental design improves upon previous observational studies focusing on tax compliance (Touchton et al. 2021; Gonçalves 2014) by addressing well-understood endogeneity concerns. In contrast to this existing work, we do not find that participatory budgeting has a consistently positive impact on tax compliance.

Third, we demonstrate that partisanship moderates impacts of direct democracy on tax compliance. Prominent lab experiments have identified a "democratic dividend," where individuals are more likely to comply with rules they had a role in creating (Bó et al. 2010; Sutter et al. 2010; Alm et al. 1993).⁸ Yet, a recent review of the experimental literature finds

⁴ A vast body of research on "community-driven development" studies forms of participatory interventions other than participatory budgeting: plebiscites (Olken 2010; Beath et al. 2017), village council elections (at times bundled with training in local democratic practices) (Fearon et al. 2015; Humphreys et al. 2019) and the solicitation of citizen service preferences (Khan et al. 2022). For a review, see Casey (2018).

⁵ Treated villages, with higher participation, place development projects further from elites' houses, making it unclear whether the increase in approval is driven by participation itself or increased equity in project allocation.

⁶ Dias (2018) estimates that as of 2018, there were between 7,059 and 7,671 instances of participatory budgeting worldwide; Sintomer et al. (2010) estimates between 1,269 and 2,778 instances as of 2012. The World Bank supported such initiatives with at least US\$280 million between 2002 and 2016 (Goldfrank 2012). A particularly well-funded PB program emerged in Paris where citizens decided over more than US\$100 million annually between 2014 and 2020 (Kotanidis 2024).

⁷ In related work, Beuermann and Amelina (2018) experimentally study the impact of training and technical assistance for settlement populations and local authorities on how to employ participatory budgeting in Russia.

⁸ Several observational studies also link participation in policymaking to tax compliance (Pommerehne and Weck-Hannemann

mixed evidence for the democratic dividend, casting doubt on simple narratives linking participation to enhanced compliance and arguing for greater attention to moderating factors that may shape “when and why dividends of democracy emerge” (Markussen and Tyran 2023, pg. 9). Consistent with this call for greater nuance, we demonstrate that partisanship can moderate the impact of participation on compliance. The idea that political participation may lead to backfiring effects among out-partisans is, while intuitive, to our knowledge, absent from the existing literature.⁹ More broadly, our results suggest the need to rethink models that view citizens’ tax compliance solely as a function of government performance (e.g., Besley 2020; Levi 1988) or enforcement capacity (Allingham and Sandmo 1972), without considering its partisan composition.¹⁰

Fourth, we contribute to the emerging literature on e-government and the role of technology in public administration and tax collection. Whereas existing research documents the potential of digital technology in facilitating tax collection and monitoring tax compliance (Okunogbe and Santoro 2023; Brockmeyer and Sáenz Somarriba 2022; Okunogbe and Tourek 2024; Dzansi et al. 2025), we show that phone-based DTHs increase the legitimacy of authorities seeking to expand the state. Our findings also emphasize how WhatsApp, a messenger service that figures prominently in discussions of mis- and disinformation (Badrinathan 2021; Garimella and Eckles 2020), can be effectively used as a platform for citizen engagement.

Finally, our results contribute to the recent literature on citizen attitudes—and specifically, perceptions of illegitimacy—as constraints on efforts to build state capacity (Besley and Dray 2024; Robinson 2023). It is increasingly understood that the electoral costs of capacity building can disincentivize and hinder effective reform (Besley and Dray 2024; Christensen and Garfias 2021; Dom et al. 2022). Yet, little is known about interventions that might reduce these costs. Our study highlights the potential of participatory budgeting to drive such improvements in government legitimacy and thus unlock the ability of governments to pursue reform. Our findings also reveal a previously undocumented tradeoff that state-builders may face: inviting public participation can bolster political support for leaders and their governments, but at the risk of politicizing compliance with existing government demands and directives. Overall, these results emphasize the need for further research into the politics of state capacity building and the partisan consequences of state-related development interventions.

2 Intervention: Digital Town Halls and Service Delivery

This research takes place in cooperation with the FCC and Mayor Aki-Sawyer of the All People’s Congress (APC), in the context of a city-wide property tax reform two of the authors helped lead. The reform broadened the tax base—less than 50% of the approximately 105,000 properties had been registered previously in the property cadastre—and made the tax burden more equitable through a more nuanced, consistent and transparent property valuation scheme (Grieco et al. 2019; Orgeira Pillai et al. 2024). It

1996; Torgler 2005; Touchton et al. 2021).

⁹ Our result that participatory budgeting decreases tax compliance and support for expanded taxation among political opponents is similar to “backfiring” effects documented for other common policy interventions. These include anti-corruption campaigns (Cheeseman and Peiffer 2022), interventions to correct political misperceptions (Nyhan and Reifler 2010) and tax bill nudges (Castro and Scartascini 2015; De Neve et al. 2021).

¹⁰ However, see Cullen et al. (2021) on political alignment and tax compliance in the United States.

resulted in large overall increases in taxation, with assessed tax liabilities increasing fivefold—concentrated among higher value properties—and revenue collection increasing threefold in the first year of the reform (Prichard et al. 2020).

The mayor publicly announced that Digital Town Halls (DTHs) would be held starting in January 2021. In her messaging, she emphasized that the DTHs would be key for securing citizen participation in decision-making about service delivery, in the context of expanding revenue generation. She also stressed that she intended to institutionalize the DTHs, with future DTHs being assigned 20% of property taxes raised per ward (Freetown City Council 2021, pg. 26).

In this study, the DTHs serve as part of a broader intervention that contains three components: (i) DTHs, (ii) service delivery and (iii) notification calls about delivered services. While only the treatment group was invited to participate in the DTHs, the projects implemented are *public* services and are thus available to members of both the treatment and control groups. However, only the treatment group received a phone call informing them that the selected service had been delivered. This implies that the estimand in our primary analysis is the effect of participating in a DTH plus having received a notification call, conditional on services being delivered.

2.1 Digital Town Halls

DTHs were WhatsApp group chats where property owners discussed pressing development challenges with other property owners in their ward and then communicated these challenges to their political representatives.¹¹ The groups then deliberated on how to allocate a budget of 15 million leones (about US\$1,500) for their ward. Treated participants were assigned to one of 58 ward-specific chat groups, with group sizes ranging from 17 to 37 (median: 24). The DTHs comprised four distinct phases, reflecting key elements of effective deliberative processes (Mansbridge 1999; Fishkin 2002):

1. Horizontal Deliberation (January 15–19, 2021):

Participants received introductory videos from the Mayor of Freetown and their respective ward councilor.¹² These videos explained the overall process, highlighted the link between property tax payments and service delivery and invited participants to start discussing development concerns within their group. Group moderators introduced themselves and prompted participants with the following question: *What do you think is the greatest development problem in your ward?* This phase involved purely horizontal deliberation, as participants were informed that political representatives would not be involved or have access to the discussions during this phase.

2. Preference Articulation and Aggregation (January 20–February 12, 2021):

After five days of horizontal deliberation, DTH participants received a video from the mayor of Freetown asking them to (i) identify the two greatest development challenges in their ward and (ii) propose projects to address these challenges. Participants were instructed to consider only projects that could be completed within a budget of US\$1,500 and to submit their proposals via written messages or

¹¹ We completed a pilot DTH in one ward before scaling the DTHs up to our 30 study wards. In Appendix A.1, we lay out potential advantages and disadvantages of *Digital* Town Halls vis-à-vis in-person Town Halls.

¹² Videos from political representatives were shared with DTH participants through the WhatsApp group and also indirectly via a Qualtrics link. The research team hired a local team to act as moderators, who were supervised by project research assistants. DTH facilitators requested that participants use the chat only between 7 a.m. and 10 p.m. daily to ensure a facilitator would be present at all times. Participants could choose their preferred form of communication (text, voice or video messages) but were asked to contribute in Krio or English.

voice recordings. The DTH facilitators, with the participants' knowledge, aggregated this information and presented memos outlining the concerns and proposed solutions to both the mayor and the ward councilor. This approach allowed participants to anonymously communicate their preferences to their representatives. Through this process, it became clear that water access was the most pressing concern for many communities.¹³

3. Vertical Interaction (February 13–16, 2021):

Participants received separate videos from both their councilor and the mayor. In these videos, the representatives responded to participants' proposals, justified their preferred services and explained past and future delivery goals. We opted for this mediated interaction between citizens and representatives to (i) avoid elite domination of the DTH process and (ii) make realistic time demands on representatives. The mayor and councilors explained that an engineering firm had been assessing the feasibility of their proposed projects and that five projects had been determined as feasible within the budget: (1) installing a new water hand pump, (2) fixing community water pipes, (3) road rehabilitation (e.g., fixing potholes), (4) installing fifty meters of truck tracks and (5) installing two new solar street lights. Participants were informed that voting would start in four days.

4. Decision Making (February 17–22, 2021):

In this phase, participants cast their vote for their preferred project anonymously through a Qualtrics survey (Appendix Figure A1).¹⁴ After four days of voting, the mayor announced the winning project for each ward with a ward-specific voice message, which was posted in each DTH alongside a picture of the mayor in her office. After the announcement of the winning projects, group moderators thanked participants for their contributions and halted participants' ability to post messages in the DTHs.

2.2 Service Delivery and Notification

The majority of DTH participants voted for water-related projects (73% of voters), which won in 28 of the 30 wards, while road improvement projects won in the remaining two (Appendix Table A1). Each study ward received a service project, benefiting both treated and control units. Construction began in most wards in October 2021 and was completed in all but one by the end of the year.¹⁵ Participants expressed satisfaction with the selected services both after the DTH (4.56/5, midline survey) and following project implementation (4.24/5, endline survey).

In all but one ward, the implemented project matched the winning service type (water provision or road repair). In one ward, however, a water-related project was ultimately deemed infeasible due to steep terrain, so a road improvement project was delivered instead. While the remaining wards received the type of project they voted for, over half that chose water-related projects received a water project other than the one they voted for due to technical constraints.¹⁶ Providing alternative projects does not appear to have

¹³ Other preferences included roads, street lights, dustbins, public toilets and the upgrading of drainage systems.

¹⁴ A "how to" video was posted in each group that provided step-by-step instructions for the voting process. We also gave participants the option to inform moderators of their vote in bilateral conversations.

¹⁵ In the remaining ward, construction was finished in February 2022.

¹⁶ Specifically, if a ward voted for new water hand pumps but no suitable water well was available for installation, the construction firm sought a location for a street tap instead. Conversely, if a ward selected street taps but no suitable location was found, the firm attempted to install hand pumps instead. If this alternative project was still not feasible, the construction firm provided a 5,000-liter community water tank with three months of refilling.

affected participants' satisfaction with the implemented service. DTH participants in wards that received a replacement project reported similar endline satisfaction (4.21/5) to those in wards that did not (4.30/5; Appendix Table A2).

To ensure that DTH participants were aware of successful project implementation, we made notification calls on behalf of the FCC to all treated units. By contacting only treated units, we incorporated these notification calls into our treatment.¹⁷ We successfully reached 67.1% of DTH participants to inform them of the implemented services. These calls began in mid- November and were staggered across wards, starting only after service delivery was completed in a ward. The endline survey was similarly staggered, commencing after the notification calls were completed and never earlier than one week after service delivery.

While not the primary focus of this study, we also organized notification calls to a randomized subset of non-study property owners. We find no evidence that receiving information about recently implemented public service projects in their neighborhood affects property owners' tax compliance behavior (see Appendix Section F).¹⁸

3 Research Design

3.1 Sampling, Randomization and Balance

To estimate causal effects, we randomized an invitation to join a DTH across 3,618 property owners in Freetown. We constructed a sampling frame using the universe of properties in the FCC's administrative records, which provided information on property characteristics and owner contact details. To be eligible for the intervention, property owners needed to own property in one of the 30 study wards, have WhatsApp on their phone and be scheduled to receive a tax bill in the first year of the reform—properties below the median value were exempt from this tax due to COVID-19-related policy and therefore excluded from our study. We reached out to 10,503 property owners who had contact information on file at the FCC and verified that 4,860 had WhatsApp on one of their phones, making them eligible for the DTH intervention.¹⁹ We successfully completed a baseline survey (details below) with 3,859 of these property owners. Then, to mitigate potential spillover effects, we drew a restricted sample to ensure a minimum distance of 15 meters between sampled property owners. This resulted in a final sample size of 3,618 (see Appendix Figure B1 for a consort diagram).

We assigned treatment status using a matched-pair design, leveraging baseline survey data to match similar observations into groups of two (King et al. 2007). We created 1809 pairs and then assigned one observation in each matched pair to treatment and the other to control. This design preserves the unbiasedness of our estimates in the face of attrition under an assumption of equivalent potential outcomes within pairs. Appendix B provides more details on the restricted sampling, matching procedure and treatment assignment.

Table I reports balance across baseline attitudinal outcomes, immutable demographic covariates and property characteristics (29 covariates total). We observe imbalances in two

¹⁷ This decision was informed by Khan et al. (2022), who expressed concern that a lack of awareness about service delivery diminished the impact of their preference elicitation and service delivery intervention. We made notification calls to rule out this concern, thereby simplifying the interpretation of our findings.

¹⁸ However, see Montenbruck (2025) who reports positive effects of service delivery notifications on tax compliance.

¹⁹ It is important to note that the sample of property owners we contacted was *not* strictly random, as we filtered out some properties to limit geographic spillovers and could not reach owners whose contact information was missing from the FCC records.

variables, no more than is to be expected through chance. As our preregistered specification for survey-based outcomes includes the baseline measure of the dependent variable, we control for these slight imbalances when estimating treatment effects.

[Table I [here.](#)]

3.2 Data Collection

To capture property owner-level covariates and measure attitudinal outcomes, we conducted three rounds of phone-based survey data collection: prior to the DTH (100% response rate), following DTH participation but prior to service delivery (91.3%) and following service delivery (79.4%).²⁰ Conducting surveys before and after service delivery is a key design innovation of the study, as it allows us to capture the importance of subsequent service delivery in shaping response to participation. For our measure of tax compliance, we rely on FCC administration data, which allows us to observe individual-level tax compliance behavior for the universe of taxable properties in Freetown. Our preregistered measure of tax compliance is a dummy variable equal to 1 if a property owner makes any tax payment in 2022. The control group compliance rate is 29.1% and 31.5% in 2022 and 2021, respectively.

3.3 Estimation and Inference

The nature of our intervention allows for one-sided noncompliance, as property owners must voluntarily join the DTH groups. Of the 1,809 property owners assigned to treatment, 1,457 (80.5%) joined WhatsApp groups of the DTH. While intent-to-treat (ITT) estimators provide unbiased estimates of being assigned to treatment, the presence of one-sided noncompliance means they will underestimate the effect of *joining* the DTH. Therefore, we estimate the effect of a property owner joining the DTH using an instrumental variable regression framework. Our main equation is as follows:

$$Y_{ijt2} = \alpha_1 DTH_i + \lambda Y_{ijt1} + \sum_{j=1}^{1809} \theta_j PAIR_{ji} + \delta_w + \lambda X_i + \epsilon_i \quad (1)$$

where Y_{ijt2} is the endline (t_2) outcome of individual i in pair j , and DTH_i is an indicator variable equal to 1 if owner i *joined* the DTH. Y_{ijt1} is the baseline outcome for owner i in pair j . When Y is property tax compliance behavior, Y_{t1} refers to tax compliance in 2020. When Y is a survey outcome, Y_{t1} refers to the baseline survey outcome. $PAIR_j$ is an indicator variable equal to 1 if owner i belongs to pair j . \mathbf{X} is a set of preregistered property-level characteristics that we include for covariate adjustment only when Y is property tax compliance behavior.²¹ δ is a vector of ward fixed effects, and E_i is the error term.

Using two-stage least squares (2SLS), we jointly estimate:

²⁰ The response rate at baseline is 100% because only baseline respondents were eligible for the intervention.

²¹ Preregistered control variables include: (i) log total tax liability, (ii) number of properties with any liability, (iii) access to water, (iv) access to drainage, (v) property in an informal settlement, (vi) property has fencing or a gate, (vii) property has a garage, (viii) street condition, (ix) street type, (x) ease of property access, (xi) window quality and (xii) type of messaging appeal in received tax bill. Where covariate data is missing, including baseline values of the outcome, we impute missing data using the baseline mean of that variable. Note that Equation 1 controls for survey-based outcomes that we expect to predict compliance through the inclusion of block dummies.

$$DTH_{ij} = \beta_1 D_i + \eta Y_{ijt_1} + \sum_{j=1}^{1809} \mu_j PAIR_{ji} + \varsigma_w + \xi X_i + v_i \quad (2)$$

where D_i is the randomly assigned treatment indicator, which instruments for DTH_i in equation 1. Our quantity of interest is α_1 (equation 1), which captures the local average treatment effect among the set of individuals who comply with treatment—owners who joined the DTH. We report estimates with heteroskedasticity-robust standard errors (HC2). As randomization occurs at the level of the observation (property owner), we do not cluster standard errors.

We estimate treatment effects on various attitudinal outcomes, organizing them into hypothesis families. To adjust for multiple tests, we implement false discovery rate (FDR) corrections by [Anderson \(2008\)](#) and report the sharpened FDR q -values alongside conventional p -values for each indicator (see also [Benjamini et al. 2006](#)). A feature of sharpened q -values is that they can be *smaller* than unadjusted p -values when corrections are made within a set of hypotheses where there are many rejections.²² These corrections are applied within each hypothesis family. This paper combines analyses from two separate pre-analysis plans (PAPs), which include several shared indicators. For indicators appearing in only one PAP, we adjust them within the hypothesis family they were originally assigned to. For indicators linked to both PAPs, we assign them to the hypothesis family described in the more recent PAP for adjustment. Appendix B.7 provides further details on how we form hypothesis families.

4 Results

4.1 Participation in Digital Town Halls

Before presenting treatment effects, we first characterize participation in the DTHs. To participate, property owners had to actively choose to join, and 80.5% of those invited did so.²³ Once in the DTH, participants could engage in two main activities: sending messages and voting on service projects. In total, approximately 2,000 messages were exchanged, with most being text (55.25%) or voice messages (40.2%) and the remainder consisting of images and videos. Of those who joined a DTH, 63% sent at least one message, and the median number of messages sent was two. Additionally, over two-thirds (68%) of participants voted for their preferred service project.²⁴ The majority of DTH participants reported accessing the DTH daily (54%), and 84.3% accessed it more than once per week (see Appendix Table C5 for participation statistics).²⁵ To convey the substantive content of DTH messaging, we transcribed a random subset of 100 messages. These reveal that participants focused on development needs in their communities (for a word cloud, see

²² As [Anderson \(2008\)](#) notes in his code, “Sharpened FDR q -vals can be LESS than unadjusted p -vals when many hypotheses are rejected, because if you have many true rejections, then you can tolerate several false rejections too.” The intuition is that, since the FDR aims to control the proportion of false discoveries across all hypotheses, knowing that many hypotheses have little risk of false rejection (i.e., very low p -value) allows for a higher tolerance of false rejections in the remaining hypotheses, and for p -values to be adjusted downwards.

²³ We called all treated property owners and asked for their consent to join the WhatsApp group. Of these, 1,616 consented, and 1,457 actually joined. Individuals who chose not to join the DTH are generally similar to those who did, but they differ in two ways: (1) they perceive the FCC as less corrupt at baseline, and (2) they are two percentage points more likely to identify as politically independent. However, they are not less likely to report a partisan affiliation; rather, they are more likely to answer the question. See Appendix Table B2.

²⁴ Note that 25 people who did not join the DTH also voted, as we gave treated participants the option to communicate their vote bilaterally outside the DTH. The statistics regarding messages include all message formats. The median number of messages posted per DTH was 70, roughly evenly split across text and voice messages.

²⁵ Only 5% of joining respondents reported they never accessed the DTH, and 5% reported accessing it once.

Appendix Figure C1).

Demographics affected participation in the DTHs, with younger and more educated individuals being slightly more likely to send a message, vote and access the chat groups daily (see Appendix Tables C6, C7 and C8).²⁶ In addition, participants who report higher baseline levels of political interest are more likely to participate. For each one-point increase in political interest (four-point scale), participants are 2–3 percentage points more likely to have sent at least one message, voted, and accessed the DTH daily. In contrast, no significant differences in participation are observed between partisans of the two major political parties: APC and the Sierra Leone People’s Party (SLPP).

Participants reported that the DTHs were useful and safe spaces for exchanging views with representatives and community members. On average, participating respondents agreed that the DTHs allowed them to “let my political representatives know about my views” (3.94/5) and “better understand views from fellow members of my community” (4.04/5). Additionally, respondents generally agreed that “participants felt comfortable to make their views known even when their views differed from those of other participants” (3.82/5). While respondents were positive about their DTH experience, they were also realistic about its limitations. Many believed that the DTH budget was insufficient to significantly improve the delivery of the selected service (2.86/5) (Appendix Table C1).

While the service delivery budget was not drawn from the FCC’s regular revenue, this was not communicated to project participants, allowing the mayor and councilors to claim full credit for the participatory budgeting program and associated service provision.²⁷ Respondents overwhelmingly reported they believed that the FCC organized the DTH (89%), implemented services (96%) and funded the services (84%). Of the respondents who said the FCC funded the project, 87% thought it was funded through taxes, 6% from government transfers, 4% from development partners and 3% from foreign aid (Appendix Table C3). Participants’ partisan affiliation does not appear to influence these perceptions (Appendix Table C4) or their experience in DTHs more generally (Appendix Table C2).

4.2 Effects on Legitimacy

We first analyze impacts of the treatment on preregistered indicators of government legitimacy, which we expect, in turn, to drive changes in tax compliance. Following [Levi et al. \(2009\)](#), we classify those indicators into four categories: policy influence, service delivery and responsiveness, government administrative competence and approval of political representatives. We discuss results for each in turn (Table II).

We first examine citizens’ perceptions of their ability to influence policy ([Scharpf 1997](#); [Tyler 2000](#)), focusing on two outcomes: perceptions (1) that they have opportunities to voice their opinions about government matters to government officials and (2) that it is easy to directly engage in political activities. The intervention had large and durable effects on this first indicator, increasing reported *opportunities for voice* by 0.38 standard deviation units (SDUs) at the midline survey and 0.25 SDUs at endline. Given that the baseline standard deviation is roughly 1.00, these effect sizes can be interpreted as changes on a four-point Likert scale. The effect on *ease of participating in political activities* is positive at both midline ($\beta = 0.064$ SDUs) and endline ($\beta = 0.073$ SDUs), and, while the unadjusted *p*-values lie at the threshold of conventional levels, the adjusted *q*-values suggest these effects are

²⁶ Note that the sample for these relationships includes all treated units, not just those who joined the DTH.

²⁷ The budget allocated to the DTH came from the project’s research budget and not the FCC’s regular budget because of (1) the severity of the budget constraints the FCC faced and (2) the fact that property tax revenue would be accrued after the DTHs had taken place. For a discussion of research ethics, see Appendix G.

statistically significant.²⁸

Second, we study perceptions of service delivery and responsiveness (Scharpf 1997; Gilley 2006). We find that the intervention significantly increased treated citizens' perceptions that the local government was responsive to citizens' needs and demands both directly after the DTH (midline: $\beta = 0.141$ SDUs; p -value <0.001) and after service implementation (endline: $\beta = 0.116$ SDUs; p -value = 0.014). In addition, the intervention attempted to forge the social and fiscal contract between citizens and politicians by delivering local services that people demanded. We find that the intervention increased citizens' satisfaction with FCC service provision at both midline ($\beta = 0.182$ SDUs; p -value <0.001) and endline ($\beta = 0.146$ SDUs; p -value = 0.004). The adjusted q -values indicate that treatment effects on these outcomes are statistically significant at midline and endline.

Third, we explore perceptions of the ability of governments to administer their constituencies competently (Hutchison and Johnson 2011; Rothstein and Stolle 2008; Magalhães 2014). Before the intervention, respondents perceived the FCC as fairly incompetent: The average respondent perceived the FCC as *not* transparent (1.36/3) and of middling efficiency (2.86/4) and corruption (3.53/5). Note that for each measure, a higher score indicates better performance. The intervention improved respondents' perceptions of FCC administrative competence across all measures, though notably these improvements come largely at endline, after successful service delivery. While perceptions of transparency show a modest improvement at midline ($\beta = 0.085$ SDUs; p -value = 0.109; q -value = 0.070), this effect increases nearly fourfold by endline ($\beta = 0.319$ SDUs; p -value = 0.001). In terms of the perceived efficiency in the use of funds for public administration and development, we observe *no effect* at midline ($\beta = 0.037$ SDUs; p -value = 0.314; q -value = 0.181) but a clear positive impact by endline ($\beta = 0.129$ SDUs; p -value = 0.007; q -value = 0.008). Finally, for perceptions of corruption, we find a similar, though more extreme, change: At midline, the treatment *increases* participants' perceptions that the FCC is corrupt ($\beta = -0.141$ SDUs; p -value <0.001 ; q -value = 0.001), but after service delivery, treated participants *positively* update their views of FCC corruption relative to the control group ($\beta = 0.087$ SDUs; p -value = 0.075; q -value = 0.048). This shift in perception is likely due to citizens initially suspecting that new local development funds would be diverted to patronage and corruption, only to revise their expectations positively once services were actually delivered. The overarching message from these results is clear: For governments to reap the full legitimacy benefits of expanding participation, they must follow through on their service delivery promises. Citizens understand that talk is cheap; they respond to tangible action.

Fourth, we focus on approval of how both the mayor and ward councilor have performed over the past 12 months (Levi et al. 2009; Norris 2017). Our data show that the mayor is popular at baseline: Most respondents report they either "strongly approved" (43.4%) or "approved" (44.3%) of the mayor's performance. The intervention increases approval of the mayor by 0.15 SDUs (p -value <0.001) at midline and 0.19 SDUs (p -value <0.001) at endline. That we observe these effects is particularly impressive given that 43% of baseline respondents gave maximum approval ratings. By contrast, the modal respondents (41%) "disapproved" of their ward councilor's performance over the past year. While baseline approval for councilors was low, the intervention increased approval at both midline (0.19 SDUs; p -value <0.001) and endline (0.17 SDUs; p -value <0.001). Adjusted q -values indicate that treatment effects at midline and endline are statistically significant for both outcomes.

In summary, Table II provides unambiguous evidence that the intervention increases

²⁸ RI p -values are 0.096 and 0.113 at midline and endline, respectively.

perceptions of government legitimacy. Importantly, we find that the full impact of participation on legitimacy depends crucially on treated individuals seeing evidence of promised service delivery.²⁹ In the next section, we investigate whether this shift in legitimating beliefs led to a corresponding shift in tax compliance, as would be predicted by the literature (e.g., [Levi 1988](#)).

[Table II [here](#).]

4.3 Average Effects on Tax Compliance

Turning to the impacts on compliance, we first present the average effects (Table III), followed by an exploration of heterogeneous effects across key sub-groups in the next subsection. We report treatment effects on tax compliance for both 2021 and 2022, although our preregistered primary outcome of interest is 2022. While the DTHs were launched in 2021, service delivery was not completed until after the 2021 tax payment deadline, making 2022 the first tax season following the full treatment of participation and service delivery. We observe compliance behavior for all units.

Panel A of Table III reports average treatment effects for the full sample. Column 1 reports the control group mean compliance rate in 2021 and 2022, and Column 2 reports the effect of the intervention. Focusing first on 2022, the compliance rate in the control group is 29.1%. The estimated treatment effect in 2022 is negative 1.2 percentage points (pp), an effect that is statistically indistinguishable from zero (p -value = 0.5). In 2021, the point estimate on the treatment effect is again negative (-0.78 pp) and statistically insignificant (p -value = 0.72).

[Table III [here](#).]

These null effects are robust to different model specifications. Our main, preregistered specification includes ward-fixed effects and a set of property characteristics as control variables. Results are similar when we estimate effects using (i) only the treatment indicator and 2020 (pretreatment) compliance behavior; (ii) only ward-fixed effects and pretreatment compliance; (iii) only property characteristics and pretreatment compliance; and (iv) when we add to our primary specification a zero tax liability dummy (Appendix Table C11, Panel A).³⁰

The results are also robust to different operationalizations of tax compliance. While Table III presents our preregistered dependent variable, which is a dummy equal to one if the owner paid *any* tax, the results are robust to using (i) the total amount paid and (ii) the log total amount paid as the dependent variable (Appendix Table C11, Panel B).

These nulls are also precisely estimated, and we can rule out all but small effects: Estimated standard errors imply that the upper limit of the 95% confidence interval is 2.3 pp. Still, we might worry that a (small) true effect exists, but we are insufficiently powered to detect it. We can improve statistical power by pooling compliance behavior across 2021 and 2022, thereby leveraging all of our compliance data in a single estimate. In this case, the dependent variable is the mean of compliance dummies in 2021 and 2022.³¹ While the interpretation of this outcome is less intuitive—the group mean compliance, pooling across years—this effect

²⁹ ITT estimates for our legitimacy outcomes reveal similar patterns (see Appendix Table C10).

³⁰ Property owners can have no liability in a given year if they paid more than was due the previous year. In 2022, nine property owners had zero tax liability, and in 2021, 121 property owners had no liability.

³¹ This variable is equal to 0 if they paid in neither year, 0.5 if they paid in one and 1 if they paid in both years.

is causally identified. The point estimate is close to zero (−1.1 pp), is not statistically significant (p -value = 0.45) and has an upper limit of the 95% confidence interval of 1.8 pp. In summary, we find no evidence that the treatment, on average, impacts compliance behavior. Given the robustness of this finding and the precision of our estimates, any potential real impacts are almost certainly substantively small.

This null result runs against most existing research, which predicts a consistent link from increased participation and legitimacy to greater tax compliance (Levi 1988, 1997; Besley 2020). It is doubly surprising given that we *do* observe strong and durable positive impacts on government legitimacy. Why do we see positive impacts on legitimacy but not on compliance? One simple and mechanical explanation is that treated property owners *want* to pay more taxes but face sharp budget constraints. If this were the case, we should see positive impacts on respondents' willingness to pay more taxes for better services. However, as presented in Panel B in Figure III, we do not find evidence that the intervention increases property owners' willingness to engage in fiscal exchange. This finding also dispels the possibility that attitudinal effects are driven by experimenter demand (Zizzo 2010) rather than true changes in beliefs. If experimenter demand had shaped the results presented in Table II, we should have found that treated respondents *say* they would be more willing to pay taxes; we do not find this.

A second possibility is that the intervention *negatively* impacted other key mediating mechanisms that, although not the primary targets, could plausibly have been affected. We preregistered two additional channels through which the intervention might influence compliance: perceptions of (i) fairness and equity and (ii) enforcement. If the intervention *diminished* participants' views of the tax system's fairness or reduced the perceived likelihood that noncompliers would be punished, this could have counteracted the positive effects on government legitimacy. However, at endline, we find no evidence of lasting treatment effects on either the fairness or enforcement outcomes (Appendix Table C15). While midline results are more varied, they do not persist to endline, making them unlikely explanations for the null results on compliance.³²

Third, we explore whether the discrepancy between the observed positive legitimacy effects and null compliance effects results from differences in the samples used. Since tax compliance is measured through administrative records, we estimate treatment effects on compliance using the full sample. In contrast, legitimacy effects are estimated using survey data where respondents can attrit. Appendix Figure C4 reanalyzes compliance treatment effects, excluding property owners who attrited from the survey. The point estimate for compliance *decreases* slightly, ruling out this potential explanation.

Another potential concern is that survey respondents may not be the individuals responsible for making property tax payment decisions. However, we guarded against this possibility by implementing a rigorous verification process.³³ Nor can the null effect on compliance be explained by incomplete reach of service notification calls. Even if the call were the entire intervention—which it is not—we would expect the observed estimate to be about two-thirds of the true effect, given that 67.1% of DTH participants received it. Therefore, if a real positive effect existed, we should observe a smaller but still positive effect; instead, we find a negative point estimate. Moreover, our results do not change if we limit the sample to only

³² These results are discussed in greater detail in Appendix Section C.5.

³³ In the initial verification survey, we compiled a list of confirmed property owners involved in financial decision-making for the property who used WhatsApp. We then independently verified that the respondent's phone number was linked to an active WhatsApp account. During the baseline survey, we re-verified that the respondent's name matched that of the verified property owner. Additionally, before enrolling participants in DTHs, we re-contacted owners to verify their identity, ownership status, WhatsApp usage and WhatsApp phone number.

participants who received a notification call (and their matched control unit) (Appendix Table C11, Panel C).³⁴

A final possibility is that true treatment effects exist but are washed out by spillovers. In Appendix Section E, we use a design-based approach to estimate spillover effects, leveraging compliance data from 74,352 owners outside of our study. While we find little evidence of spillovers using our preregistered specifications, there is suggestive evidence of small spillover effects for properties within 20 meters of a treatment property ($\beta = 1.8$ percentage points; RI p -value = 0.13). However, this is unlikely to meaningfully bias our estimates downward, as only 4.3% of control units are within 20 meters of a treated unit (Appendix Figure B3).³⁵

Given the absence of a simple explanation for the null effect on compliance despite significant increases in legitimacy, we investigate the possibility that our null result may disguise heterogeneous effects across groups.

4.4 Partisanship Moderates Participation's Impact on Tax Compliance

In this section, we demonstrate that participants' partisan affiliation moderates the treatment's impact on tax compliance and broader support for taxation. We argue that these heterogeneous effects arise because the intervention allowed participants to learn about political parties' positions on tax policy, prompting them to adjust both their policy preferences and compliance behavior to align with their party's stance.

Existing research suggests a relatively straightforward link between participation, legitimacy and compliance (Levi 1988; Besley 2020; Beath et al. 2017; Bó et al. 2010; Alm et al. 1993). In this body of work, a key reason why citizens are more likely to comply with government policy when leaders open channels for public participation is that it strengthens their belief that leaders will implement policies in their best interests and that they will benefit from compliance. From this perspective, a homogeneous citizenry responds to a unitary political actor; increased participation boosts tax compliance by raising citizens' expectations that government will use revenue to benefit citizens. This could be called the *government legitimacy* channel through which participation increases compliance.

Real-world politics, however, are often contentious. In the context we study, the central government opposed the property tax policy that the mayor championed, and the mayor publicly clashed with the Ministry of Finance over the FCC's legal authority to adjust property tax rates without central government approval (Luke 2020). Party politics were at the center of this conflict. While the mayor's party, the APC, controlled the FCC, their primary political opposition, the SLPP, controlled the central government and therefore the Ministry of Finance.

We argue that these partisan dynamics shape how citizens form policy preferences. According to seminal work in political science, citizens often have weak or malleable policy preferences and rely on cues from trusted political elites to form them (Zaller 1992). A large body of research documents that supporters of Party A often increase their support for a policy when they learn Party A backs it, while supporters of Party B may decrease their support when they find out Party B opposes it (Broockman and Butler 2017; Flores et al. 2022; Tappin et al. 2023).

³⁴ This discussion implies that legitimacy results (Table II) may have a slight *downward* bias.

³⁵ Nor do we think social network-based spillovers from treatment to control units are likely, as less than 10% of control units had even heard of the DTHs at endline.

The importance of partisan cues for preference formation suggests an alternative way that participation may affect compliance. Through political participation, citizens learn where political actors stand on specific issues, which prompts them to update their policy preferences. We refer to this as the *partisan cue exposure* channel through which participation can impact compliance. Importantly, this channel implies that when party leaders disagree on their support for a policy, increased citizen participation leads individuals to adjust their preferences about the policy in different ways, depending on their political allegiance.

The DTHs directly exposed participants to APC leaders' support for the controversial tax policy. Within the DTHs, the mayor strongly advocated for property taxation, emphasizing its importance for improving local public services. For instance, in the mayor's first DTH video, she encouraged participants to pay taxes, assuring them that the FCC would use the revenue to deliver services to Freetown residents.³⁶

In addition to increased exposure to the mayor's views, it is likely that the DTHs indirectly exposed participants to the central government's opposition to the tax policy by enhancing their political interest and engagement (Appendix Table C13). For instance, in the midline survey, DTH participants reported greater overall interest in politics ($\beta = 0.161$ SDUs; p -value < 0.001 ; q -value = 0.001) as well as more interest in FCC activities specifically ($\beta = 0.428$ SDUs; p -value < 0.001 ; q -value = 0.001), suggesting they became more attuned to the politicized debate surrounding property taxation in Freetown.

Given the APC's support for the tax reform, the partisan cue exposure channel predicts that participation leads APC partisans to increase their support for taxation, thereby also making them more likely to increase their tax compliance. This channel also predicts a corresponding drop in tax support and compliance for non-copartisans. Figure I presents clear evidence in line with these predictions. Plot A shows that participants' (pretreatment) partisan affiliation conditions how the treatment impacts their tax compliance behavior. Plot A presents predicted marginal effects from a model that interacts treatment with a copartisan indicator variable.³⁷ The interaction between treatment and co-partisanship is statistically significant ($\beta = 0.11$; p -value = 0.033). For copartisans of the mayor (i.e., APC supporters), the treatment increases compliance by 7.4 percentage points, which is a substantial 31.8% increase over the group's control compliance rate of 23.3%. In contrast, treatment effects are *negative* for non-copartisans: Treatment lowers compliance by 4.0 percentage points. These results are robust to alternative partisanship codings and to disaggregating opposition groups.³⁸

If participation leads to heterogeneous effects on compliance by shaping participants' policy preferences regarding taxation along partisan lines, we should also observe that

³⁶ Specifically, the mayor promised that the "FCC will use [tax revenue] to deliver services to the people of Freetown." In a separate video, the mayor also reminded participants, "If everyone pays their property rate, you can imagine what type of investment we can make in your ward."

³⁷ Other model specifications remain the same as in our main specification. The copartisan variable is equal to 1 for respondents who self-report affinity toward the APC; all other respondents are coded as 0. In our baseline survey, we asked respondents which political party (if any) they "personally support and feel close to." Just under half of all respondents reported having a partisan leaning (47.7%), with 24.3% and 19.9% declaring themselves for the APC (the incumbent party at FCC) and SLPP, respectively. Less than 3% of all respondents declared themselves for a party other than the APC or SLPP, with the majority of third-party partisans being affiliated with the NGC. The modal respondent claimed they did not support any party (30.1%), and an additional 22.2% of respondents opted not to answer this question and are labeled as "missing."

³⁸ Our preferred specification uses the full sample, coding respondents supportive of and close to the APC as "copartisan," with all other respondents coded as "opposition." Appendix Figure C3 shows similar results when excluding from the "opposition" group respondents who did not answer the partisanship question. Additionally, Appendix Table C12 reports treatment effects by five partisan sub-groups, demonstrating that the results are not driven by a single non-copartisan group: sub-group treatment effects are negative for *all* non-copartisan groups.

partisan affiliation moderates the impact of the intervention on these policy preferences. One way to measure individuals' support for taxation (i.e., their tax policy preference) is by assessing their willingness to exchange taxes for services. Prior to the DTH, the majority (57.4%) of surveyed respondents reported that they "strongly approved" of expanding taxation for improved services, while a significant minority (14%) opposed this idea.

Figure I (Panel B) shows predicted marginal effects from an interaction model, where the outcome of interest is the respondent's support for expanding taxation. To increase power for estimating this interaction, the predicted outcome is the respondent's *average* support for expanded taxation across the midline and endline surveys. The interaction between treatment and co-partisanship is statistically significant ($\beta = 0.223$ SDUs; p -value = 0.063), and again we see heterogeneous impacts by partisanship: Copartisans increase their support for expanding taxation for improved services, while non-copartisans decrease their support for this policy. Appendix Figure C2 shows estimates from an interaction model using midline or endline data separately. Estimated marginal effects display similar patterns. Overall, our results indicate that participants update their tax policy preference in line with their party's position and ultimately shift their compliance behavior conditional on their partisan affiliation.³⁹

[Figure I [here](#).]

As partisanship is not randomly assigned, these observed heterogeneous effects could be driven by confounding factors associated with partisan affiliation. While APC supporters do differ statistically from non-copartisans along several demographic dimensions, these differences are small (Appendix Table D2). APC partisans in our sample are three percentage points less likely to be female and five percentage points less likely to have a degree from an institution of higher education; APC partisans own slightly more properties on average, but with slightly lower taxable value. The small magnitudes of these differences make them unlikely candidates for driving the observed heterogeneity by partisanship. More importantly, Appendix Table D3 (Columns 3–4) shows that these variables do not moderate treatment effects on compliance.

Another potential confounder is normative beliefs about taxation, such as views on the ideal distribution of wealth, which are correlated with partisanship in some contexts (Stantcheva 2021). However, we find no evidence of such a relationship in our context: Partisanship is not associated with the belief that the wealthy should be taxed more to fund services for everyone (Appendix Table D1). We also find little indication that service preferences vary meaningfully by partisanship (Appendix Figure D1). Further, copartisans and non-copartisans have indistinguishable perceptions of the FCC's role in organizing, implementing and funding the DTHs (Appendix Table C4). We do see some evidence that DTH projects were implemented closer to APC supporters (Appendix Table D4).⁴⁰ Yet, DTH-level treatment effects are not correlated with the magnitude of partisan targeting (Appendix Figure D2).⁴¹

³⁹ Consistent with our account of partisan cue exposure, we find some evidence of affective polarization in the midline survey (Appendix Table C16). At midline, DTH participants, on average, report that it is more difficult to be friends with individuals who support the rival party ($\beta = -0.178$ SDU; p -value = 0.001; q -value = 0.011) and that they find it harder to trust statements made by non-copartisans about politics ($\beta = -0.068$ SDU; p -value = 0.093; q -value = 0.076). These results highlight that, given our intervention's emphasis on citizen-citizen interaction, partisan cues may be amplified by fellow participants. Yet, these effects dissipate by endline.

⁴⁰ The locations of the public service projects in our study were determined by our research team in collaboration with a construction firm, with input from local politicians. Research assistants reported that politicians strongly advocated for projects in specific locations in a few instances.

⁴¹ Specifically, we find no evidence that treatment effects are greater for APC supporters in town halls where projects exhibited stronger APC targeting (i.e., where APC partisans were, on average, closer to implemented projects than SLPP partisans).

In this section, we presented exploratory analyses suggesting that partisanship moderates the effect of participation on tax compliance. We argue that these heterogeneous effects are driven by divergent partisan shifts in policy preferences—specifically, support for taxation—resulting from participants’ enhanced comprehension of their party’s stance on the issue. Our interpretation that partisan cues can polarize policy attitudes and behaviors aligns with research indicating that, while cues from political leaders may be persuasive for the political ingroup, they can generate backlash from the outgroup (Haas and Khadka 2020; Nicholson 2012). We understand this mechanism, which we call *partisan cue exposure*, as occurring alongside the government legitimacy channel that is commonly posited in the existing literature. Not only do participants positively update about government legitimacy on average (Table II), Appendix Table C9 shows that positive updating occurs for both copartisans and non-copartisans.⁴² Our observation of negative treatment effects on compliance for non-copartisans suggests that partisan cues outweigh the impacts of increased legitimacy in shaping short-term compliance behavior for this sub-group.

5 Conclusion

It is well known that poor countries collect less taxes than richer ones (Lee and Gordon 2005; Besley and Persson 2014). Property taxes, in particular, have emerged as the most underperforming major tax type across lower-income countries (Brockmeyer et al. 2021).⁴³ Underperforming revenue collection is a key barrier to the development of strong social contracts. With little revenue, governments struggle to meet citizens’ needs, and because unresponsive governments are seen as illegitimate, citizens are less likely to support higher taxes or advocate for increased fiscal capacity. Many governments in poor countries appear mired in this pernicious equilibrium of low government legitimacy, low tax compliance and limited political support for strengthening tax systems. How can governments break out of this vicious cycle?

In this paper, we propose that governments can use direct democracy to overcome legitimacy constraints on state capacity building. We report results from a digital town hall intervention in Freetown, Sierra Leone—the first field experiment examining whether participatory budgeting can facilitate state capacity building. It was designed to support a weak local government to build fiscal capacity by increasing its legitimacy. In the short term, it aimed to raise tax compliance. In the medium term, it sought to enhance public perceptions of government legitimacy to enable the government to pursue and sustain policy reform efforts (Besley and Dray 2024).

We present two primary findings. First, our results demonstrate that participatory budgeting can increase citizens’ perceptions of government legitimacy. These positive effects are consistent across political supporters and opponents of the mayor. Whereas influential research on community-driven development reported either mixed (Olken 2010) or no impacts on legitimacy for other types of participatory interventions (Casey et al. 2012; Fearon et al. 2015; Khan et al. 2022), we find that participatory budgeting yields legitimacy gains.

Second, despite those relatively universal and durable impacts on legitimacy, we find that

⁴² We observe positive point estimates for non-copartisans on all legitimacy outcomes at endline. For the only outcome where treatment effects between copartisans and non-copartisans are statistically distinguishable (Mayor approval), effects are larger for non-copartisans, likely because copartisans face ceiling effects.

⁴³ While higher-income countries collect 2 to 3% of GDP in recurrent property taxes, lower-income countries appear to collect less than 0.2% of GDP from those same taxes (Bahl and Vazquez 2008).

effects on compliance are heavily moderated by partisanship. We argue that these heterogeneous impacts emerge due to *partisan cue exposure*: Through participation, citizens learn where political actors stand on specific issues, which prompts participants to update their policy preferences and related compliance behavior. This adds substantial nuance to influential models of tax compliance (Levi 1988, 1997), the literature on the democratic dividend (Bó et al. 2010; Sutter et al. 2010) and studies on participatory budgeting (Pommerehne and Weck-Hannemann 1996; Touchton et al. 2021), all of which suggest a simpler link from expanded participation to increased tax compliance.

While the state features prominently in many development interventions (Callen et al. 2023; Pomeranz and Vila-Belda 2019; Finan et al. 2017), the government's partisan character is often overlooked. Our results call for greater attention to the possibility that state-related development interventions have partisan impacts, particularly when they are led by political actors.

What do our results imply for governments considering similar participatory interventions? Focusing first on revenue collection, while our intervention did not increase participants' compliance on average, there are several reasons why the *total* compliance effects of similar interventions may be positive. First, we studied the impact of participation itself, distinct from the possibility that participatory budgeting processes may increase compliance across the population. We find suggestive evidence that our intervention led to positive compliance spillover effects on people who did not directly participate (Appendix E). Second, the long-term compliance impacts of participatory interventions are not limited to their short-term effects. While we observed no immediate impact on compliance, the increases in government legitimacy indicate that the long-term effects could be more promising. Third, there is the question of participant selection. Given the large treatment effect heterogeneity that we document, interventions in which participants self-select into eligibility or which are targeted at populations that are most likely to react positively may produce more positive average treatment effects.

Of course, governments considering implementing similar interventions care deeply about outcomes other than compliance, such as how they are perceived by voters. We find large, durable treatment effects on perceptions of government legitimacy. Thus, participatory budgeting can be used to create more supportive environments for governments that want to make ambitious, politically contentious investments in fiscal capacity.

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Tables and Figures

TABLE I. Balance Table

Measure	Mean		SD	Difference			Observations	
	C	T1	C	Raw	Std.	p-val	C	T1
<i>Survey Outcomes</i>								
Opportunities for voice	2.12	2.13	1.00	0.01	0.01	0.75	1,719	1,736
Ease of participating in political activities	1.76	1.74	1.14	-0.02	-0.02	0.62	1,794	1,793
FCC responsiveness to citizens' demands	3.17	3.17	1.18	0.00	0.00	0.91	1,712	1,719
Satisfaction with FCC service provision	3.64	3.64	1.17	0.00	0.00	0.96	1,790	1,796
FCC transparency	1.37	1.35	0.69	-0.02	-0.03	0.34	1,732	1,726
FCC efficiency	2.86	2.87	0.70	0.01	0.01	0.77	1,530	1,577
FCC corruption	3.50	3.57	1.01	0.07	0.07*	0.06	1,481	1,482
Mayor approval	4.23	4.22	0.89	-0.01	-0.01	0.76	1,770	1,774
Councilor approval	2.73	2.74	1.22	0.01	0.01	0.90	1,751	1,751
Willingness to pay more taxes for better services	4.19	4.18	1.22	-0.01	-0.01	0.78	1,805	1,804
Reform improves tax system fairness	2.12	2.11	0.79	-0.01	-0.01	0.83	1,112	1,129
Number of neighbors who will pay property tax	5.13	5.07	2.41	-0.06	-0.02	0.54	1,138	1,105
Likelihood detected noncompliers are punished	4.06	4.06	1.11	0.00	0.00	0.90	1,788	1,781
<i>Political Party Affiliation</i>								
APC	0.24	0.25	0.43	0.01	0.02	0.59	1,809	1,809
SLPP	0.20	0.20	0.40	0.00	0.00	0.62	1,809	1,809
Other party	0.02	0.03	0.16	0.01	0.06	0.36	1,809	1,809
No affiliation	0.32	0.29	0.47	-0.03	-0.06*	0.03	1,809	1,809
Did not respond	0.22	0.23	0.41	0.01	0.02	0.34	1,809	1,809
<i>Property Characteristics</i>								
Tax compliance 2020	0.07	0.07	0.25	0.00	0.00	0.74	1,809	1,809
Number of properties with tax liability (2021)	1.93	1.89	1.48	-0.04	-0.03	0.37	1,809	1,809
Total property tax owed (USD, 2021)	95.83	93.15	175.59	-2.68	-0.02	0.66	1,809	1,809
Received tax bill (2019 or 2020)	0.80	0.80	0.40	0.00	0.00	0.89	1,791	1,789
Property has water	0.47	0.47	0.50	0.00	0.00	1.00	1,809	1,809
Property has drainage	0.36	0.36	0.48	0.00	0.00	0.81	1,809	1,809

In informal settlement	0.06	0.06	0.23	0.00	0.00	1.00	1,809	1,809
<i>Demographics</i>								
Female	0.31	0.30	0.46	-0.01	-0.02	0.91	1,809	1,809
Age	51.65	51.88	13.00	0.23	0.02	0.60	1,803	1,804
Higher education	0.39	0.40	0.49	0.01	0.02	0.32	1,685	1,694
Married	0.72	0.72	0.45	0.00	0.00	0.68	1,804	1,805

Note: Table I reports balance across baseline survey outcomes, immutable demographic covariates and property characteristics. Columns 1-2 report group means; Column 3 reports the control group standard deviation; Columns 4-5 report raw and standardized differences, respectively. Column 6 reports the (unadjusted) p -value on this difference. We convert local currency (SLL) to USD at a rate of 10,000:1, which reflects the exchange rate in January, 2021. A respondent is coded as receiving higher education if they have a university degree, or a degree from a polytechnic school or teacher college. Receiving a tax bill in 2019 and 2020 is self-reported.

Significance: * $p < 0.10$

TABLE II. Effect on Legitimacy

Outcome	Baseline		Midline				Endline				
	Mean (1)	Mean (2)	Effect (3)	p -val (4)	N (5)	q -val (6)	Mean (7)	Effect (8)	p -val (9)	N (10)	q -val (11)
Policy Influence											
Opportunities for voice	2.13 (1.00)	2.33 (0.92)	0.377*** (0.038)	0.000	3,288	0.001	2.16 (0.92)	0.251*** (0.046)	0.000	2,849	0.001
Ease of participating in political activities	1.75 (1.14)	1.62 (1.02)	0.064 (0.040)	0.113	3,298	0.040	1.63 (1.02)	0.073* (0.046)	0.096	2,863	0.034
Service Delivery and Responsiveness											
FCC responsiveness to citizens' demands	3.17 (1.19)	3.36 (1.06)	0.141*** (0.038)	0.000	3,251	0.001	3.31 (1.16)	0.116** (0.048)	0.014	2,830	0.015
Satisfaction with FCC service provision	3.64 (1.17)	3.61 (1.06)	0.182*** (0.040)	0.000	3,302	0.001	3.47 (1.21)	0.146*** (0.050)	0.004	2,864	0.009
Govt. Administrative Competence											
FCC transparency	1.36 (0.69)	1.42 (0.77)	0.085 (0.052)	0.109	3,288	0.070	2.16 (1.34)	0.319*** (0.101)	0.001	2,834	0.002
FCC efficiency	2.86 (0.71)	2.86 (0.56)	0.037 (0.038)	0.314	3,233	0.181	2.79 (0.70)	0.129*** (0.048)	0.007	2,791	0.008
FCC corruption	3.53 (1.00)	3.62 (0.90)	-0.141*** (0.043)	0.000	3,177	0.001	3.45 (0.93)	0.087* (0.048)	0.075	2,736	0.048
Approval of Political Representatives											
Mayor approval	4.23 (0.89)	4.08 (0.82)	0.149*** (0.042)	0.000	3,296	0.001	3.91 (0.94)	0.194*** (0.051)	0.000	2,855	0.001
Councilor approval	2.73 (1.22)	2.73 (1.17)	0.193*** (0.040)	0.000	3,278	0.001	2.74 (1.22)	0.171*** (0.047)	0.000	2,841	0.001

Note: Table II reports the effect of the treatment on political attitudes. Columns 1, 2 and 7 report the control group mean for each indicator for the baseline, midline and endline surveys, respectively, with the standard deviation in parentheses. Column 3 presents treatment effects estimates at the midline survey and Column 8 presents treatment effects estimates at the endline survey. Stars refer to randomization inference p -values (1,000 simulations), which are reported in Columns 4 and 9. Columns 5 and 10 reports the number of non-missing observations in the midline survey and endline survey, respectively. Columns 6 and 11 report corrected q -values, which adjust for multiple hypothesis testing, following [Anderson \(2008\)](#). Reported effects are standardized effects.

Significance: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

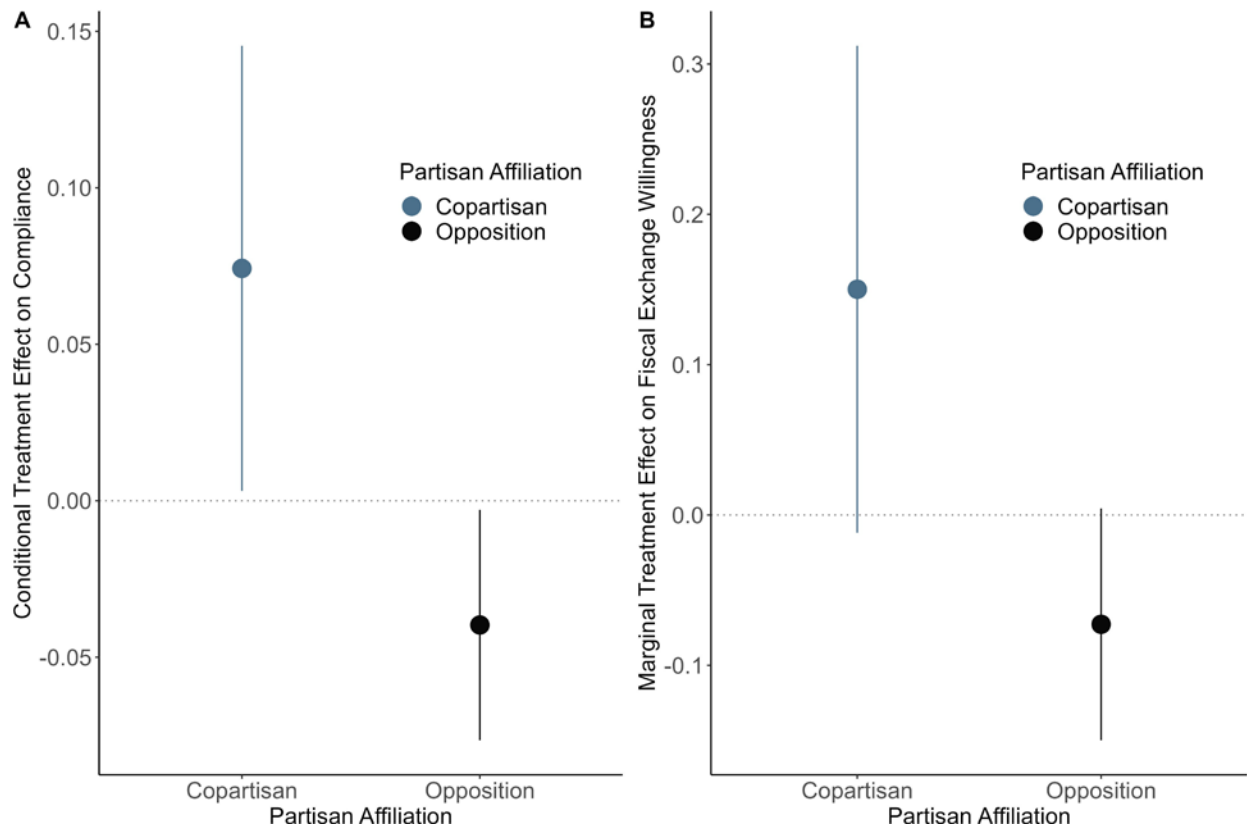
TABLE III. Effect on Tax Compliance

Outcome	Mean (1)	Effect (2)	<i>p</i> -value (3)	N (4)
Panel A: Tax Compliance Behavior				
<i>Did the owner pay any taxes?</i>				
2022	0.291	-0.012 (0.018)	0.496	3,618
2021	0.315	-0.007 (0.019)	0.723	3,618
Panel B: Fiscal Exchange Attitudes				
<i>Willingness to pay more taxes for better services</i>				
Midline	4.001 (1.253)	0.066 (0.047)	0.163	3,296
Endline	4.030 (1.293)	-0.075 (0.053)	0.155	2,872

Note: Table III reports treatment effects on tax compliance behavior (Panel A) and attitudes towards fiscal exchange (Panel B). Column 1 reports control group means. Column 2 presents treatment effects estimates. In Panel A these effects are reported in raw percentage points; in Panel B presented effects are standardized effects. Column 3 reports *p*-values and Column 4 reports the number of non-missing observations.

Significance: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

FIGURE I. Treatment Effects Conditional on Partisan Affiliation



Note: Panel A reports marginal treatment effects on tax compliance behavior, conditional on partisan affiliation. Panel B reports marginal treatment effects on attitudes towards fiscal exchange, conditional on partisan affiliation. In both panels, respondents who self-report affinity towards the All People's Congress are coded as "copartisans." All other respondents are coded as "opposition." Point estimates are presented with 90% confidence intervals.

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A Intervention Appendix

A.1 Digital Town Halls: Pros and Cons

To begin with, participation can be less costly: If access to WhatsApp already exists, participants only need to invest a modest amount of time and mobile data to enter the DTH. Whereas offline THs enable participation only for a short and fixed time period, DTHs can be accessed for weeks and whenever it is convenient for participants. This flexibility reduces the oft significant opportunity costs of participation (Casey 2018). Intuitively, transportation costs—traditionally a barrier to participation especially in rural settings (Sexton 2017, p.35)—are not incurred. Remarkable improvements in internet activity in developing countries—31 % of Sierra Leoneans in 2018 own a phone with internet access (Afrobarometer 2018)—have led to an explosion in social media usage (21.5% of Sierra Leonean report obtaining news through Facebook or Twitter at least “a few times a week” (Afrobarometer 2018)). As our study population is property owners in the capital city, we expect these numbers to be even higher in our setting. In our model of mediated interaction through WhatsApp, participation is less costly for political representatives too: All that is required of them is to read a summary of participant contributions and to respond in a limited number of video and voice messages.

Second, perhaps counterintuitively, we argue that DTHs hold more deliberative promise: In the Habermasian ideal type of deliberative democracy, participants engage in potentially endless communicative action (an exchange of reasoned arguments) as equals until the best argument prevails (Habermas 1975). In offline THs, attendants regularly find themselves unable to make their views known in front of representatives as time constraints only allow for a limited number of contributions. Statements, especially from members of marginalized groups, are often interrupted by other participants (Parthasarathy et al. 2019). In contrast, DTHs allow all participants to make their views known without running the risk of interference by others. Importantly, DTHs alleviate the constraint of limited attention spans on successful argumentative reasoning: While it is easy to forget what a participant argued a few minutes ago in an offline TH, participants in WhatsApp can just scroll back. Whereas immediate reactions are required offline to ensure that the conversation stays on topic, DTHs enable participants to first reflect on their statement—in theory for multiple days—before posting it. Therefore, the longer time frame in a DTH should increase the argumentative quality of contributions and facilitate perspective taking (as the need for immediate reactions in offline DTHs precludes taking the time to reflect on where someone else’s argument is coming from). Finally, we can avoid face-to-face interactions which in group settings under time constraints lend themselves to emotionalized exchanges (more cues are visible—e.g., body language and facial expressions—which make it harder to focus on the merits of the argument alone).

Third, DTHs can alleviate one dimension of the well-known gap in political participation by targeting the relatively young who usually are less likely to participate in conventional forms of political engagement. Yet, DTHs—just like their offline analogue—display additional participation biases (higher ability and willingness to participate among those able to afford smart phones and internet usage, the more educated and literate, those with higher political efficacy (on self-selection in offline TH participation, see Boulianne 2019; Neblo et al. 2010)).

However, there are also potential relative disadvantages to the DTH format: The relative anonymity decreases the (reputational) cost of disruptive behavior as participants can choose how much identifying information they provide through their WhatsApp profile.

Furthermore, moderating chats can be costly, constrained by the functionalities provided by WhatsApp (messages can only be deleted by who wrote them) and, if done poorly, runs the risk of altering the conversation. The absence of face-to-face interactions can lead to questioning that one is actually talking to ones' representatives and fellow community members. Fortunately, this is less of a concern here as political representatives have prominently associated themselves with the DTH intervention in public. One may argue that voice- and text-based communication is less rich when other cues cannot be observed (e.g., the eyes as an indicator of the sincerity of the speaker). Perhaps most crucially, while DTHs reduce participation costs for many, those lacking internet/WhatsApp access cannot participate. Finally, the brevity of text messages may not be conducive to the articulate elaboration of arguments ([Jaidka et al. 2019](#)). However, there are no length limitations in WhatsApp and participants have the option to record voice and video messages as well.

A.2 Voting and Project Implementation

FIGURE A1. Menu of Services

Q1.

Which project would you like to be implemented in your ward?

Each project is worth 15 million leones.

Fixing of potholes

A new water hand pump

2 new solar street lights

Fixing of water pipes

50m of truck tracks



TABLE A1. Project Votes, Winning Projects, Implemented Projects

Projects	Votes Won	Built
Water Projects		
Fixing Pipes (Street Taps)*	429	19
Water Hand Pump	313	9
<i>Replacement: Water Tank</i> [†]		8
Road Projects		
Truck Tracks (50-meters)	138	2
Road Rehabilitation (Fixing Potholes)	51	0
Light Projects		
Solar Street Lights	83	0

Note: Table A1 reports participants' votes by project type (Column 1), the number of wards where each project won (Column 2) and the number of wards where each project was implemented (Column 3). This table includes the votes of 25 property owners in the treatment group who did not join the DTH.

*The project "fixing of water pipes" was implemented by constructing a street water tap connected to community water pipes.

[†]In some wards, neither street taps nor hand pumps could be implemented due to technical constraints. Instead, these wards received a 5,000-liter water tank with three months of refills.

TABLE A2. Self-Reported Satisfaction with Selected Service

	<i>All Participants</i>	<i>Exact</i>	<i>Type</i>	<i>Different</i>	<i>Did Not Vote</i>
Panel A: All Wards					
Satisfaction (midline)	4.56	4.68	4.49	4.50	4.59
Satisfaction (endline)	4.24	4.32	4.23	4.12	4.28
N	1457	271	460	258	468
Panel B: Wards Receiving Exact Projects					
Satisfaction (midline)	4.56	4.65	4.31	4.52	4.57
Satisfaction (endline)	4.30	4.36	4.30	4.30	4.22
N	549	203	66	118	162
Panel C: Wards Receiving Replacement Projects					
Satisfaction (midline)	4.56	4.77	4.52	4.50	4.59
Satisfaction (endline)	4.21	4.22	4.22	4.00	4.30
N	908	68	394	140	306

Note: Table A2 reports DTH participants' satisfaction with the selected service before (midline) and after (endline) implementation. Panel A includes all DTH participants, Panel B focuses on participants in wards that received the exact project they voted for and Panel C covers participants in wards that received a replacement project. In each panel, Column 1 reports overall satisfaction, while Columns 2-5 break it down by participant sub-groups. Column 2 includes individuals whose ward received the exact project they voted for. Column 3 covers those whose ward received a similar project type (but not the exact project they voted for). Column 4 reports satisfaction for participants whose ward received a different project type. Column 5 reports satisfaction for participants who did not vote.

FIGURE A2. Street Tap Implemented in Ward 442



FIGURE A3. Truck Tracks Implemented in Ward 404



FIGURE A4. Water Tank Implemented in Ward 399



FIGURE A5. Hand Pump Implemented in Ward 406



FIGURE A6. Street Tap (Alternative Design) Implemented in Ward



B Research Design

B.1 Sampling

At the time of our intervention there were 95,572 residential properties in the Freetown City Council's administrative records.⁴⁴ To be eligible to participate in the Digital Town Hall a property owner must (i) have been scheduled to receive a tax bill (RDN) in 2020, (ii) have contact information on file in FCC records and (iii) own a property in one of the 30 study wards.

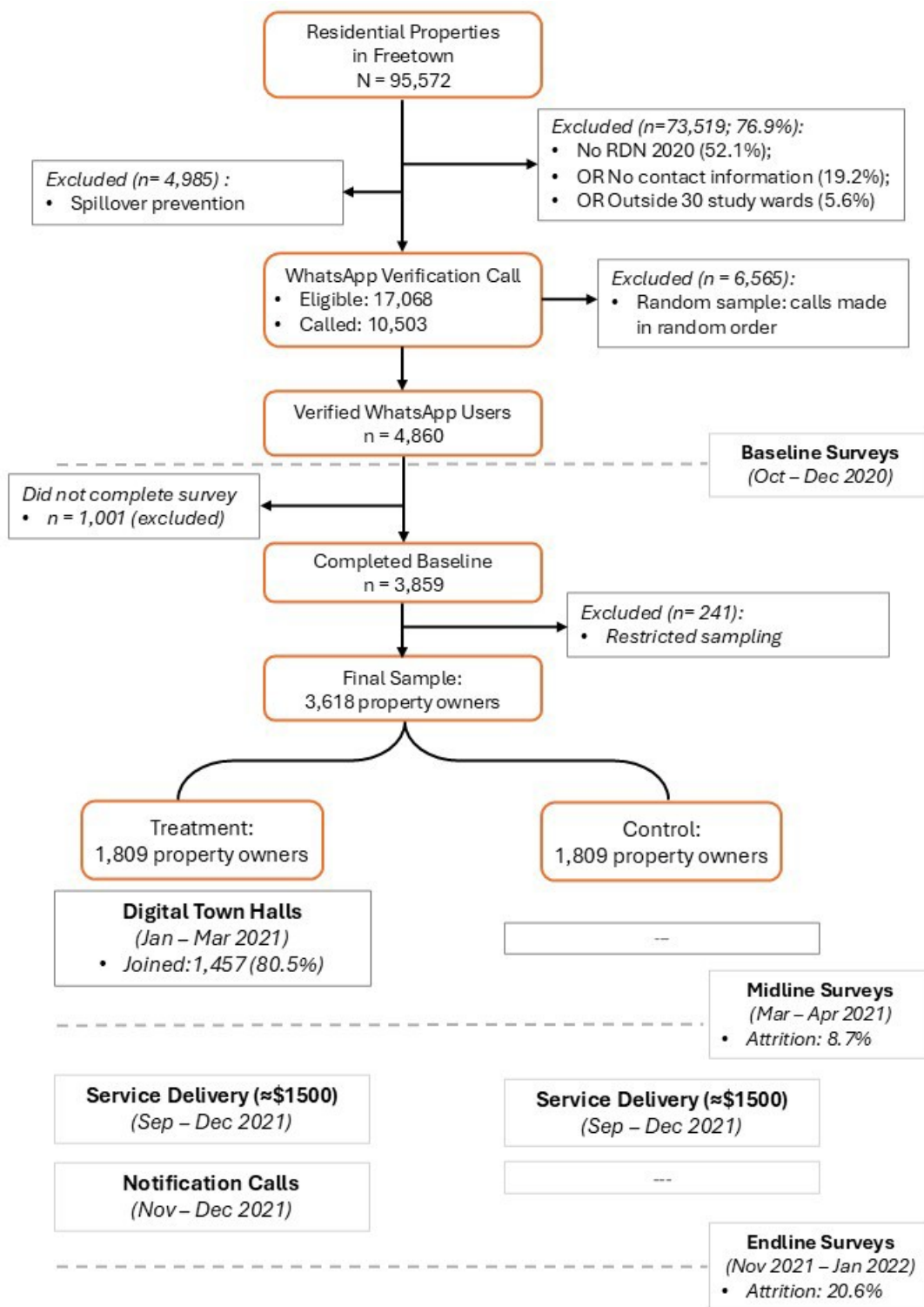
The first eligibility criterion warrants further explanation. Because tax compliance is a primary outcome in our study, eligibility required that property owners be scheduled to receive a tax bill. The intervention was originally planned for 2020; however, in response to COVID-19, the FCC waived property taxes that year for properties below the median assessed value. As a result, we excluded these properties from our study, although political delays related to tax reform postponed the DTH intervention until early 2021.

From the 22,053 properties that met these criteria, we removed an additional 4,985 to limit geographic spillover. Specifically, we overlaid all eligible properties with a spatial grid and excluded properties located within five meters of a grid cell boundary.

Of the 17,068 eligible properties, we contacted 10,503 property owners, placing calls in random order. We verified that 4,860 of these owners had WhatsApp on their phone and later recontacted them for a baseline survey. We completed the baseline survey with 3,859 property owners. To mitigate potential spillover effects, we drew a restricted sample from these 3,859 respondents, ensuring that each property was at least 15 meters from the nearest study property. This yielded a final sample of 3,618.

⁴⁴ For property owners that own multiple properties, we coded them as being exclusively eligible for the DTH in the study ward that contains their highest-value property (i.e., highest tax rate).

FIGURE B1. Research Design



B.2 Survey Data Collection

Baseline: Between October 28 and December 2, 2020, we attempted to survey the 4,860 property owners we had verified as eligible for the study and completed baseline surveys with 3,859 individuals (79.4%). Only baseline survey respondents were eligible to receive treatment and were attempted to be surveyed in subsequent rounds.⁴⁵

Midline: After the completion of the DTHs (between March 4 and April 17, 2021) we conducted midline surveys with all study property owners. Importantly, this survey round took place *before* services were implemented. We completed midline surveys with 3,304 study property owners (91.3%).⁴⁶

Endline: After the implementation of the selected services (between November 11, 2021 and January 2022) we conducted endline surveys with all study property owners. We completed endline surveys with 2,872 study property owners (79.4%).

⁴⁵ Appendix Figure B5 documents the broader data collection and project timeline.

⁴⁶ We incentivized midline and endline survey responses by offering packages of mobile data.

B.3 Matching for Treatment Assignment

We match property owners using the following covariates:

- Unconditional tax morale
- Service conditional tax morale
- Perceived probability of punishment for non-compliance
- Satisfaction with FCC service provision
- Tax reform awareness and support
- RDN received in 2019 or 2020
- Opportunities to voice opinion about FCC governance
- Willingness to believe member of opposing party
- Mayor approval
- FCC councilor approval
- Gender
- FCC responsiveness
- Age
- Property value
- Education

We generated matched pairs using the *blockTools* package in *R*. We use the Optimal Greedy (“optGreedy”) matching algorithm to find best matches along Mahalanobis distance. We weight certain variables higher than others when matching, as we expect that certain variables are a stronger predictor of our outcomes of interest. We place the greatest weight on unconditional tax morale which we expect to be the strongest predictor of tax compliance, in line with its common use as a proxy for tax compliance behavior. We place equal weight on another set of six measures from our baseline survey. Three of these measures are important factors in the literature on tax compliance: (i) service conditional tax morale, (ii) perceived likelihood of punishment for non-compliance, and (iii) satisfaction with FCC service provision. We also place equal weight on the (iv) gender of the property owner, (v) their awareness and support of the property tax reform,⁴⁷ and (vi) the number of these five variables that were imputed.

Table B1 presents descriptive statistics and match weights for our matching variables. If a respondent refused to answer a question or said they “did not know” we imputed the value as the unconditional mean of the variable. The last column displays the number of observations that were imputed for matching. Note that in general, the number of imputed responses is low.

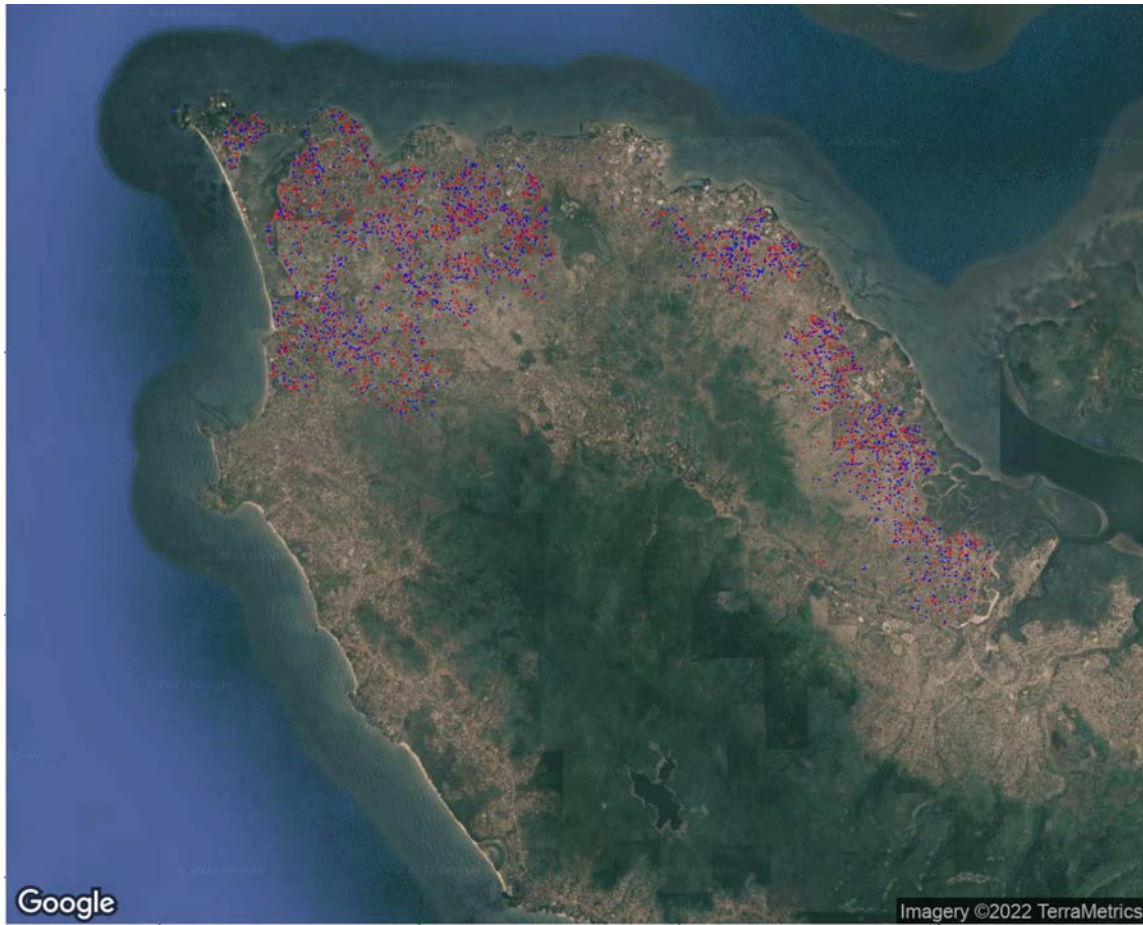
⁴⁷ We create a three-level ordinal variable based on two survey items. A first group consists of respondents who have heard of the reform and strongly/somewhat support it; a second group consists of respondents who (a) have heard of the reform and feel neutral towards it and (b) have not heard of the reform; a third group consists of respondents who have heard of the reform and somewhat/strongly oppose it.

TABLE B1. Summary Statistics of Matching Variables

Variable Name	Weights	Mean	SD	Min	Max	N Imputed
Unconditional tax morale	1.10	3.77	1.55	1.00	5.00	25
Service conditional tax morale	1.00	1.96	0.96	1.00	3.00	11
Perceived probability of punishment	1.00	4.06	1.11	1.00	5.00	52
Satisfaction with FCC service provision	1.00	3.64	1.17	1.00	5.00	35
Gender (female = 1)	1.00	0.31	0.46	0.00	1.00	0
Reform awareness / support	1.00	2.38	0.67	1.00	3.00	19
RDN delivered 2019 or 2020	0.90	0.83	0.38	0.00	1.00	0
Opportunities for voice	0.10	2.13	0.99	1.00	4.00	174
Mayor approval	0.10	4.23	0.89	1.00	5.00	79
Councilor approval	0.10	2.73	1.22	1.00	5.00	122
FCC responsiveness	0.10	3.17	1.19	1.00	5.00	199
Believe out-party member	0.10	3.00	1.55	0.00	5.00	132
Age	0.09	51.77	12.93	20.00	100.00	11
Property tax value (USD)	0.09	60.25	87.45	2.88	1281.85	0
Education [0-2]	0.09	1.31	0.62	0.00	2.00	259

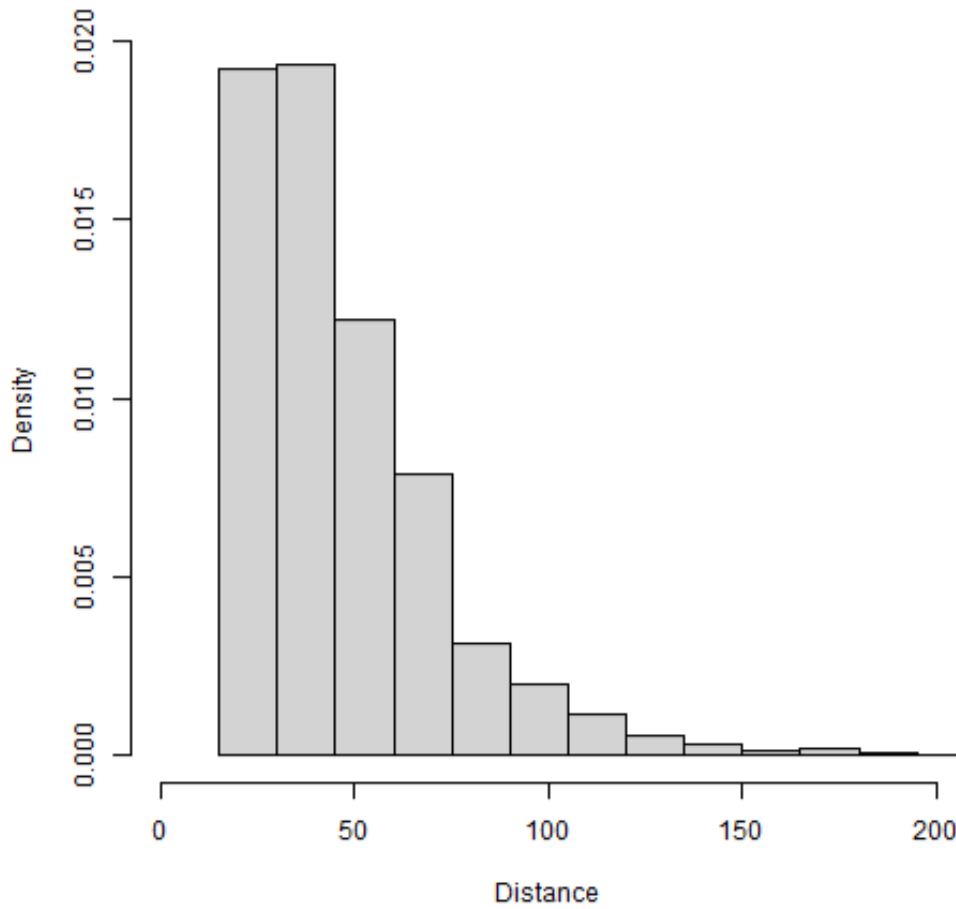
B.4 Treatment Assignment Map

FIGURE B2. Digital Town Hall Treatment Assignment in Freetown (Red = Treatment)



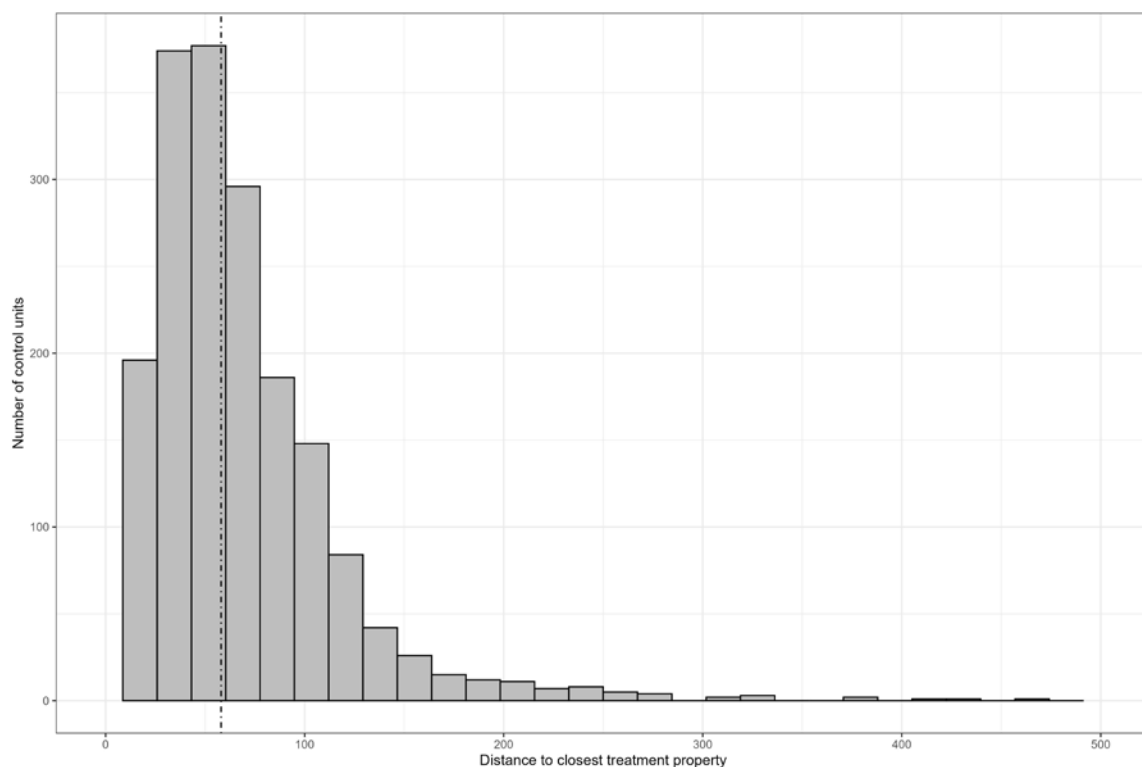
B.5 Distance to Closest Study Property

FIGURE B3. Histogram of Minimum Distance (in Meters) Between Study Properties



Note: Figure B3 shows the distribution of the distance from each property to the closest study property in the sample.

FIGURE B4. Distribution of Distance Between Control Units and the Closest Treatment Unit



Note: Figure B4 plots the distribution of distance between control units and the closest treatment unit. Eight control units have a minimum distance greater than 500 meters and are excluded from Figure B4. The dotted vertical line shows the median distance (58 meters)

TABLE B2. Comparing DTH Participants to Owners That Opted Out

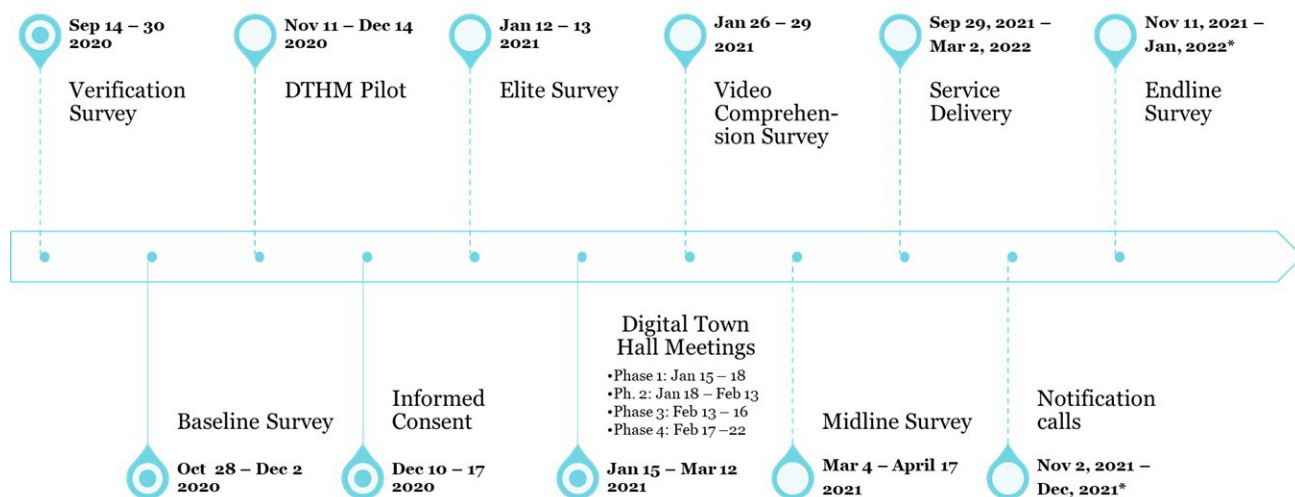
Measure	Mean		SD	Difference			Observations	
	Out	Joined	Out	Raw	Std.	p-val	Out	Joined
<i>Survey Outcomes</i>								
Opportunities for voice	2.16	2.12	1.03	-0.04	-0.04	0.75	332	1,404
Ease of participating in political activities	1.61	1.77	1.09	0.16	0.15	0.62	349	1,444
FCC responsiveness to citizens' demands	3.20	3.16	1.16	-0.04	-0.03	0.91	337	1,382
Satisfaction with FCC service provision	3.74	3.62	1.14	-0.12	-0.11	0.96	349	1,447
FCC transparency	1.27	1.37	0.59	0.10	0.17	0.34	337	1,389
FCC efficiency	2.92	2.85	0.72	-0.07	-0.10	0.77	305	1,272
FCC corruption	3.63	3.55	0.94	-0.08	-0.09*	0.06	289	1,193
Mayor approval	4.27	4.21	0.89	-0.06	-0.07	0.76	343	1,431
Councilor Approval	2.78	2.73	1.27	-0.05	-0.04	0.90	340	1,411
Willingness to pay more taxes for better services	4.21	4.17	1.21	-0.04	-0.03	0.78	350	1,454
Reform improves tax system fairness	2.06	2.12	0.77	0.06	0.08	0.83	216	913
Number of neighbors who will pay property tax	5.13	5.05	2.47	-0.08	-0.03	0.54	206	899
Likelihood detected noncompliers are punished	4.04	4.06	1.04	0.02	0.02	0.90	348	1,433
<i>Political Party Affiliation</i>								
APC	0.25	0.25	0.43	0.00	0.00	0.59	352	1,457
SLPP	0.20	0.20	0.40	0.00	0.00	0.62	352	1,457
Other party	0.03	0.03	0.17	0.00	0.00	0.36	352	1,457
No affiliation	0.31	0.29	0.46	-0.02	-0.04*	0.03	352	1,457
Did not respond	0.21	0.23	0.41	0.02	0.05	0.34	352	1,457
<i>Property Characteristics</i>								
Tax compliance 2020	0.06	0.07	0.24	0.01	0.04	0.74	352	1,457
Number of properties with tax liability (2021)	1.93	1.88	1.59	-0.05	-0.03	0.37	352	1,457
Total property tax owed (USD, 2021)	93.33	93.10	136.49	-0.23	0.00	0.66	352	1,457
Received tax bill (2019 or 2020)	0.80	0.80	0.40	0.00	0.00	0.89	349	1,440
Property has water	0.47	0.47	0.50	0.00	0.00	1.00	352	1,457
Property has drainage	0.36	0.36	0.48	0.00	0.00	0.81	352	1,457
In informal settlement	0.05	0.06	0.23	0.01	0.04	1.00	352	1,457
<i>Demographics</i>								
Female	0.34	0.30	0.47	-0.04	-0.09	0.91	352	1,457
Age	53.32	51.53	12.80	-1.79	-0.14	0.60	351	1,453
Higher education	0.40	0.41	0.49	0.01	0.02	0.32	334	1,360
Married	0.73	0.72	0.45	-0.01	-0.02	0.68	352	1,453

Note: Table B2 compare treated individuals who joined the DTHs to those who did not across baseline survey outcomes, immutable demographic covariates and property characteristics. Columns 1-2 report group means; Column 3 reports the control group standard deviation; Columns 4-5 report raw and standardized differences, respectively. Column 6 reports the p -value on this difference (not adjusted for multiple comparisons). We convert local currency (SLL) to USD at a rate of 10,000:1, which reflects the exchange rate in January, 2021. A respondent is coded as receiving higher education if they have a university degree, or a degree from a polytechnic school or teacher college. Receiving a tax bill in 2019 and 2020 is self-reported.

Significance: * $p < 0.10$

B.6 Project Timeline

FIGURE B5. Project Timeline



Note: Notification calls and endline surveys in one ward, Tengbeh Town, were delayed by two months due to contractual issues with the construction firm.

B.7 Inference

This paper integrates analyses from two separate pre-analysis plans (PAPs). The first PAP investigates the effectiveness of DTHs as tools for enhancing political accountability during COVID-19 (registered at: <https://osf.io/cg738>). The second PAP focuses on tax compliance. Notably, five indicators from the COVID-19 PAP were reshuffled into new hypothesis families in the tax compliance PAP.⁴⁸

To adjust for multiple comparisons, we use the two-step correction method outlined by [Anderson \(2008\)](#), which involves grouping hypotheses into families and then applying corrections within these families. For outcomes that appear in only one PAP (either COVID-19 or tax compliance), we adjust them within the hypothesis family they were originally assigned to. For indicators linked to both PAPs, we assign them to the hypothesis family described in the tax compliance PAP for adjustment. Appendix Table B3 maps the indicators to their respective hypothesis families for adjustment. Column 1 lists all attitudinal outcomes from both PAPs; Column 2 indicates whether the outcome is included in the COVID-19 PAP (C), the tax compliance PAP (T), or both; Column 3 shows the hypothesis family used for adjusting p -values, and Column 4 identifies the table number where results for each indicator are presented.

⁴⁸ We had analyzed effects of the DTH on midline survey outcomes at the time of writing the tax PAP, but we had not yet analyzed any endline data. We note this for transparency reasons, but do not believe that the analysis of midline outcomes impacted our analysis plan for the tax compliance study in any meaningful way.

TABLE B3. Inference

Outcome	PAP	Family	Table
Fiscal exchange attitudes	T	Fiscal Exchange	2
Satisfaction with FCC service delivery	Both	Fiscal Exchange	1
Opportunities to voice opinions to govt.	Both	Political Efficacy	1
Ease of participating in political activities	Both	Political Efficacy	1
FCC responsiveness to citizens' demands	Both	Political Efficacy	1
Reform improves tax system fairness	T	Fairness of Taxation	17
Number of neighbors who will pay property tax	T	Fairness of Taxation	17
Likelihood detected noncompliers are punished	Both	Enforcement	17
Mayor approval	C	Attitudes Towards Govt.	1
Councilor approval	C	Attitudes Towards Govt.	1
FCC efficiency	C	Attitudes Towards Govt.	1
FCC corruption	C	Attitudes Towards Govt.	1
FCC transparency	C	Attitudes Towards Govt.	1
Satisfaction with the political system	C	Attitudes Towards Govt.	N/A
Support for direct democracy	C	Attitudes Towards Govt.	N/A
Knows ward councilor name	C	Political Knowledge and Efficacy	14
Attempted to contact ward councilor	C	Political Knowledge and Efficacy	14
Attempted to contact MP	C	Political Knowledge and Efficacy	14
Level of interest in politics	C	Political Knowledge and Efficacy	14
Level of interest in FCC activities	C	Political Knowledge and Efficacy	14
Attended political meeting	C	Political Knowledge and Efficacy	14
Level of trust in neighbors	C	Polarization and Cohesion	18
Level of connection of neighbors	C	Polarization and Cohesion	18
Ease of befriending out-party member	C	Polarization and Cohesion	18
Ease of believing out-party member	C	Polarization and Cohesion	18

C Additional Results

C.1 Experience and Participation in DTH

TABLE C1. DTH Experience

Question	Agree [0-5]
DTH gave space to voice views to political representatives	3.94
DTH facilitated better understanding of community members' views	4.04
Budget (LE 15 Million) sufficient to meaningfully improve selected service	2.86
Participants comfortable making views known	3.82
Menu of services reflected services community wanted improved, given budget	3.33
Selected service will be delivered in the near future	3.58
Vote was fair and gave every participant the same influence	3.83

Note: Table C1 presents participants' experiences in the DTHs. We asked questions in both positive and negative forms, so as to limit confirmation bias in the average response. For example, we asked half of respondents if they agreed with the following statement: "The Town Hall allowed me to let my political representatives know about my views." And we asked the other half of respondents if they agreed with the negative version of that statement: "The Town Hall **did not** allow me to let my political representatives know about my views." Respondents were asked whether they agreed or disagreed with these statements, using a five-point Likert scale. Questions in the table are presented in the positive form. Appendix Table C1 reports the average agreement with each statement.

TABLE C2. DTH Experience by Partisan Affiliation

Question	Agree [0-5]	
	Copart.	Non Copart.
DTH gave space to voice views to political representatives	3.95	3.93
DTH facilitated better understanding of community members' views	3.96	4.07
Budget (LE 15 Million) sufficient to meaningfully improve selected service	2.91	2.84
Participants comfortable making views known	3.69	3.86
Menu of services reflected services community wanted improved, given budget	3.26	3.36
Selected service will be delivered in the near future	3.51	3.61
Vote was fair and gave every participant the same influence	3.78	3.85

Note: Table C2 presents participants' experiences in the DTHs, conditional on partisan affiliation. We asked questions in both positive and negative forms, so as to limit confirmation bias in the average response. For example, we asked half of respondents if they agreed with the following statement: "The Town Hall allowed me to let my political representatives know about my views." And we asked the other half of respondents if they agreed with the negative version of that statement: "The Town Hall **did not** allow me to let my political representatives know about my views." Respondents were asked whether they agreed or disagreed with these statements, using a five-point Likert scale. Questions in the table are presented in the positive form. Appendix Table C2 reports the average agreement with each statement.

TABLE C3. Perceptions of DTH Organization, Implementation and Funding

Responsible Actor	Organized (%)	Implemented (%)	Funded (%)
FCC	89.30	96.14	84.23
National Government	1.93	4.51	10.60
Researchers	12.55	1.77	2.33
Citizens	0.24	1.69	11.50
Other	1.37	0.56	5.55

Note: Table C3 reports participants' perceptions of which actor(s) organized, implemented and funded the DTHs. Values are the percent of respondents who named a given actor. Participants were allowed to name multiple actors. These data were collected during the midline survey.

TABLE C4. Perceptions of Organization, Implementation and Funding by Partisanship

Responsible Actor	Organized (%)		Implemented (%)		Funded (%)	
	Copart.	Non-Copart.	Copart.	Non-Copart.	Copart.	Non-Copart.
FCC	89.36	89.28	97.10	95.82	84.84	84.03
National Government	2.90	1.61	5.16	4.29	9.27	11.04
Researchers	13.55	12.22	1.94	1.72	2.26	2.36
Citizens	0.00	0.32	0.97	1.93	9.68	12.11
Other	1.29	1.39	0.32	0.64	6.45	5.25

Note: Table C4 reports participants' perceptions of which actor(s) organized, implemented and funded the DTHs, by partisan affiliation. Values are the percent of respondents who named a given actor. Participants were allowed to name multiple actors. These data were collected during the midline survey.

TABLE C5. DTH Participation

Panel A: Self-Reported Participation Frequency		%
Daily		53.6
Four to six times per week		8.0
Two or three times per week		22.7
Once per week		5.3
Never		5.0
Panel B: Behavioral Participation		
Voted for service [%]		0.68
Sent any message in DTH [%]		0.63
Median messages sent		2.00
Mean messages sent		3.84

Note: Table C5 reports DTH participation behavior. Panel A displays the self-reported frequency of accessing the DTH group, while Panel B presents behavioral participation measures, including voting and sending messages. The sample for these statistics consists of property owners who joined a DTH (i.e., participants).

TABLE C6. Correlates of Participation: Who Messages?

Explanatory Variable	<i>Dependent Variable: Sent at Least One Message</i>			
	(1)	(2)	(3)	(4)
<i>Demographics:</i>				
Female	0.018 (0.026)			0.019 (0.026)
Age	0.003*** (0.001)			-0.003*** (0.001)
Property value (log)	-0.033** (0.015)			-0.030** (0.015)
Education: Attended some school	0.088** (0.037)			0.073* (0.037)
<i>Partisan Affiliation: (Baseline group = APC)</i>				
SLPP		-0.019 (0.035)		-0.022 (0.035)
Third Party (NGC/other)		0.059 (0.072)		0.056 (0.073)
Independent		-0.004 (0.032)		0.021 (0.033)
Did not answer		0.069** (0.034)		0.077** (0.034)
Political Interest [1-4]			0.033*** (0.011)	0.034*** (0.011)
Num. Obs.	1801	1809	1794	1787
R^2	0.014	0.004	0.005	0.023

Note: Table C6 presents the results from OLS regressions, where the dependent variable is an indicator equal to 1 if the participant sent at least one message in the DTH, and 0 otherwise. Columns 1-3 report results from models that include only demographic characteristics (Column 1), only partisan affiliation (Column 2) and only political interest (Column 3). Column 4 presents results from a model that includes all three sets of variables. The *education* variable has three categories: some education, no education and missing value. Education data is missing for 115 out of 1809 participants in the DTH sample; missingness (i.e., the “missing” category) is positively correlated with participation.

Significance: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

TABLE C7. Correlates of Participation: Who Votes for Services?

Explanatory Variable	<i>Dependent Variable: Voted for Preferred Service</i>			
	(1)	(2)	(3)	(4)
<i>Demographics:</i>				
Female	-0.021 (0.026)			-0.021 (0.026)
Age		-0.003*** (0.001)		-0.003*** (0.001)
Property value (log)	0.000 (0.015)			0.004 (0.015)
Education: Attended some school		0.104*** (0.038)		0.096** (0.038)
<i>Partisan Affiliation: (Baseline group = APC)</i>				
SLPP		-0.033 (0.035)		-0.034 (0.035)
Third Party (NGC/other)		0.071 (0.070)		0.061 (0.069)
Independent		-0.058* (0.032)		-0.048 (0.033)
Did not answer		0.022 (0.034)		0.026 (0.034)
Political Interest [1-4]			0.026** (0.011)	0.019 (0.011)
Num. Obs.	1801	1809	1794	1787
R^2	0.013	0.005	0.003	0.021

Note: Table C7 presents the results from OLS regressions, where the dependent variable is an indicator that equals 1 if the participant voted for their preferred service, and 0 otherwise. Columns 1-3 report results from models that include only demographic characteristics (Column 1), only partisan affiliation (Column 2) and only political interest (Column 3). Column 4 presents results from a model that includes all three sets of variables. The *education* variable has three categories: some education, no education and missing value. Education data is missing for 115 out of 1809 participants in the DTH sample; missingness (i.e., the "missing" category) is positively correlated with voting.

Significance: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

TABLE C8. Correlates of Participation: Who Checks DTH Daily?

	<i>Dependent Variable: Accessed Chat Group Daily</i>			
	(1)	(2)	(3)	(4)
<i>Demographics:</i>				
Female	-0.038 (0.025)			-0.034 (0.025)
Age	-0.003*** (0.001)			-0.003*** (0.001)
Property value (log)	-0.013 (0.014)			-0.012 (0.015)
Education: Attended some school	0.156*** (0.033)			0.151*** (0.033)
<i>Partisan Affiliation: (Baseline group = APC)</i>				
SLPP		-0.037 (0.034)		-0.039 (0.034)
Third Party (NGC/other)		0.031 (0.072)		0.019 (0.071)
Independent		-0.045 (0.031)		-0.030 (0.032)
Did not answer		-0.015 (0.034)		-0.004 (0.034)
Political Interest [1-4]			0.023** (0.011)	0.017 (0.011)
Num. Obs.	1801	1809	1794	1787
R^2	0.021	0.002	0.003	0.023

Note: Table C8 presents the results from OLS regressions, where the dependent variable is an indicator equal to 1 if the participant accessed the WhatsApp chat group daily (as self-reported), and 0 otherwise. Columns 1-3 report results from models that include only demographic characteristics (Column 1), only partisan affiliation (Column 2) and only political interest (Column 3). Column 4 presents results from a model that includes all three sets of variables. The *education* variable has three categories: some education, no education and missing value. Education data is missing for 115 out of 1809 participants in the DTH sample; missingness (i.e., the “missing” category) is positively correlated with daily access.

Significance: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

C.2 Legitimacy

TABLE C9. Effects on Legitimacy Outcomes Conditional on Partisan Affiliation

Outcome	Midline (CATE)			Endline (CATE)		
	Copart.	Non-Copart.	Diff	Copart.	Non-Copart.	Diff
Policy Influence						
Opportunities to voice opinion to govt	0.398 (0.097)	0.368 (0.048)	0.030 (0.119)	0.186 (0.122)	0.266 (0.058)	-0.081 (0.148)
Ease of participating in political activities	0.186 (0.127)	0.037 (0.056)	0.149 (0.152)	0.217 (0.155)	0.037 (0.063)	0.180 (0.182)
Service Delivery and Responsiveness						
FCC responsiveness to citizens' demands	0.189 (0.112)	0.160 (0.058)	0.029 (0.140)	0.222 (0.157)	0.111 (0.072)	0.111 (0.190)
Satisfaction with FCC service provision	0.239 (0.120)	0.204 (0.059)	0.034 (0.147)	0.285 (0.144)	0.133 (0.074)	0.152 (0.177)
Government Administrative Competence						
FCC corruption	-0.067 (0.114)	-0.165 (0.054)	0.097 (0.138)	0.174 (0.117)	0.060 (0.061)	0.114 (0.144)
FCC efficiency	0.030 (0.066)	0.025 (0.034)	0.004 (0.083)	0.109 (0.088)	0.085 (0.044)	0.024 (0.108)
FCC transparency	0.259 (0.104)	-0.005 (0.045)	0.264 (0.124)	0.298 (0.197)	0.197 (0.087)	0.101 (0.236)
Approval of Political Representatives						
Mayor approval	0.104 (0.097)	0.141 (0.047)	-0.037 (0.119)	0.014 (0.116)	0.220 (0.058)	-0.206 (0.143)
Councilor approval	0.397 (0.135)	0.184 (0.060)	0.213 (0.162)	0.097 (0.147)	0.244 (0.071)	-0.147 (0.177)

Note: Table C9 reports treatment effects on legitimacy outcomes, conditional on partisan affiliation, at the midline and endline survey. Columns 1 and 2 report treatment effects at midline for copartisans and non-copartisans, respectively. Column 3 reports the difference in treatment effects between subgroups. Treatment effects are reported and standard deviation units and standard errors in parentheses. Columns 4-6 report similar estimates for the endline survey. Respondents who self-report feeling "close to" APC are defined as copartisans; all other respondents are coded as non-copartisans.

Significance: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

TABLE C10. Effects on Legitimacy Outcomes (ITT)

Outcome	Midline		Endline	
	Effect	N	Effect	N
Policy Influence				
Opportunities to voice opinions to govt	0.312 (0.032)	3,288	0.209 (0.039)	2,849
Ease of participating in political activities	0.053 (0.033)	3,298	0.061 (0.038)	2,863
Service Delivery and Responsiveness				
FCC responsiveness to citizens' demands	0.116 (0.031)	3,251	0.097 (0.040)	2,830
Satisfaction with FCC service provision	0.150 (0.033)	3,302	0.122 (0.042)	2,864
Government Administrative Competence				
FCC transparency	0.070 (0.043)	3,288	0.266 (0.084)	2,834
FCC efficiency	0.031 (0.031)	3,233	0.108 (0.040)	2,791
FCC corruption	-0.117 (0.036)	3,177	0.073 (0.040)	2,736
Approval of Political Representatives				
Mayor approval	0.123 (0.035)	3,296	0.162 (0.043)	2,855
Councilor approval	0.159 (0.033)	3,278	0.143 (0.040)	2,841

Note: Table C10 reports Intention-to-Treat (ITT) effects on legitimacy outcomes. Columns 1 and 3 present treatment effects estimates at the midline and endline survey, respectively, with the standard error in parentheses. Columns 2 and 4 report the number of non-missing observations in the respective survey round. Reported effects are standardized effects.

Significance: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

C.3 Tax Compliance

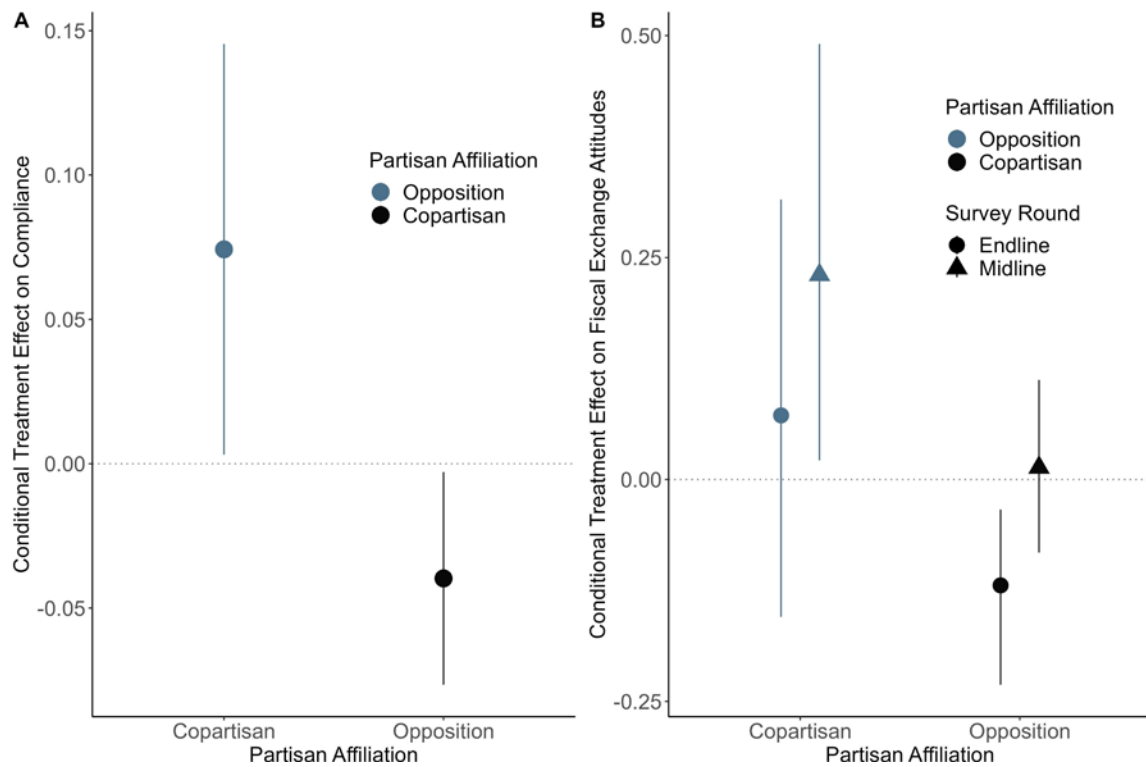
TABLE C11. Effect on Tax Compliance: Robustness

	2022			2021		
	Est	SE	<i>p</i> -value	Est	SE	<i>p</i> -value
Panel A: Alternative Model Specifications						
Baseline Compliance	-0.012	0.018	0.515	-0.010	0.019	0.584
Baseline Compliance + ward FE	-0.010	0.018	0.574	-0.008	0.019	0.679
Baseline Compliance + prop. covs	-0.012	0.018	0.512	-0.008	0.019	0.660
Main spec. + zero liability dummy	-0.011	0.017	0.530	-0.007	0.019	0.707
Panel B: Alternative Operationalizations						
Total paid (USD)	2.944	3.845	0.444	3.922	2.999	0.191
Log total paid (USD)	-0.005	0.011	0.664	-0.001	0.012	0.900
Panel C: Sample Subset						
Received notification call	-0.002	0.021	0.959	0.018	0.023	0.440
Panel D: ITT Estimator						
Preregistered controls	-0.010	0.014	0.496	-0.005	0.015	0.723

Note: Table C11 reports treatment effects on tax compliance behavior for 2022 and 2021 for several robustness conditions. Columns 1 and 4 present treatment effects estimates for 2022 and 2021, respectively. Columns 2 and 5 report standard errors; Column 3 and 6 report *p*-values. Panel A reports treatment effects in raw percentage points, using alternative model specifications. Panel B reports treatment effects using alternative operationalizations of the dependent variable. We convert local currency (SLL) to USD at a rate of 10,000:1, which reflects the exchange rate in January, 2021. Panel C reports treatment effects when sub-setting the sample to only DTH participants that received a notification call (and their matched control unit). Panel D reports Intention-to-Treat effects of the intervention on tax compliance, using the preregistered set of controls.

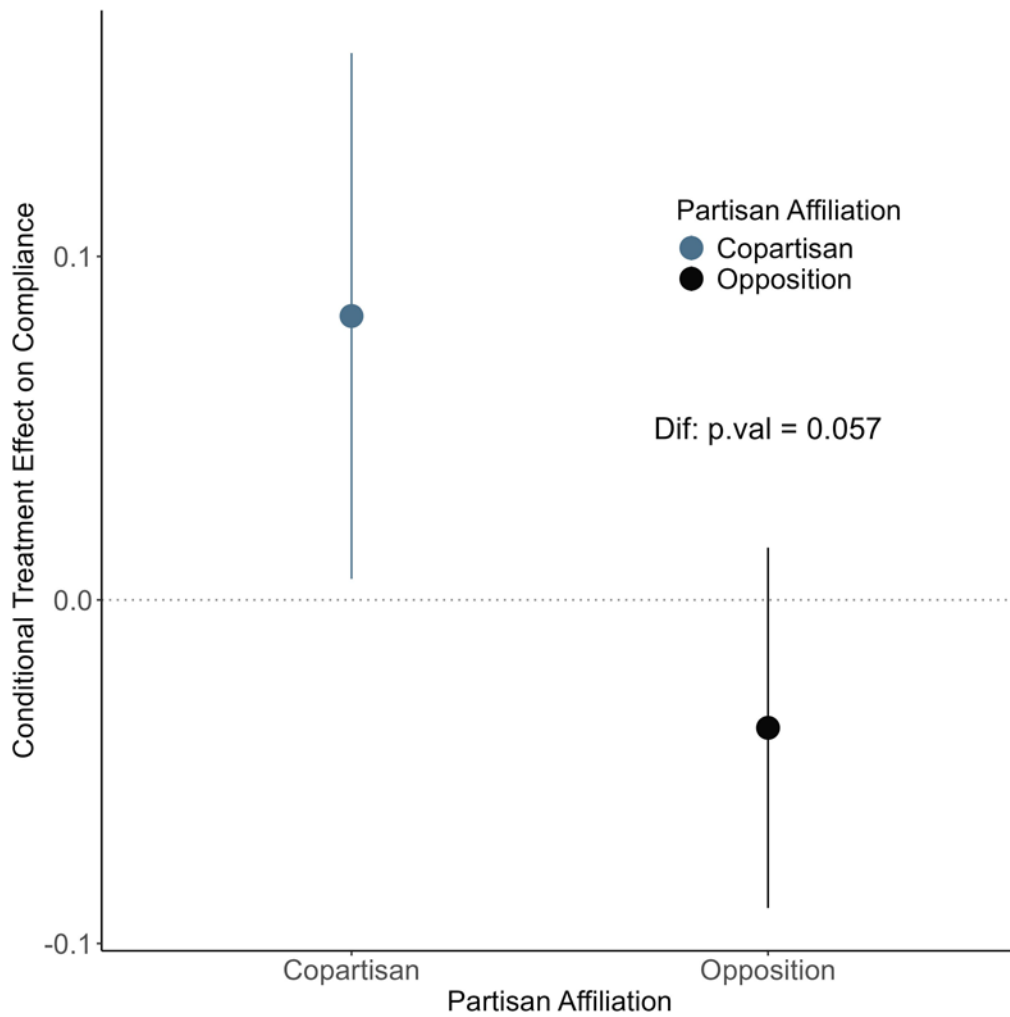
Significance: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

FIGURE C2. Treatment Effects Conditional on Partisan Affiliation



Note: Panel A reports marginal treatment effects on tax compliance behavior, conditional on partisan affiliation. Panel B reports marginal treatment effects on attitudes towards fiscal exchange, conditional on baseline attitudes towards fiscal exchange. In both panels, respondents who self-report affinity towards the All People's Congress are coded as "copartisans." All other respondents are coded as "opposition."

FIGURE C3. Treatment Effects Conditional on Partisan Affiliation (Alternative Coding)



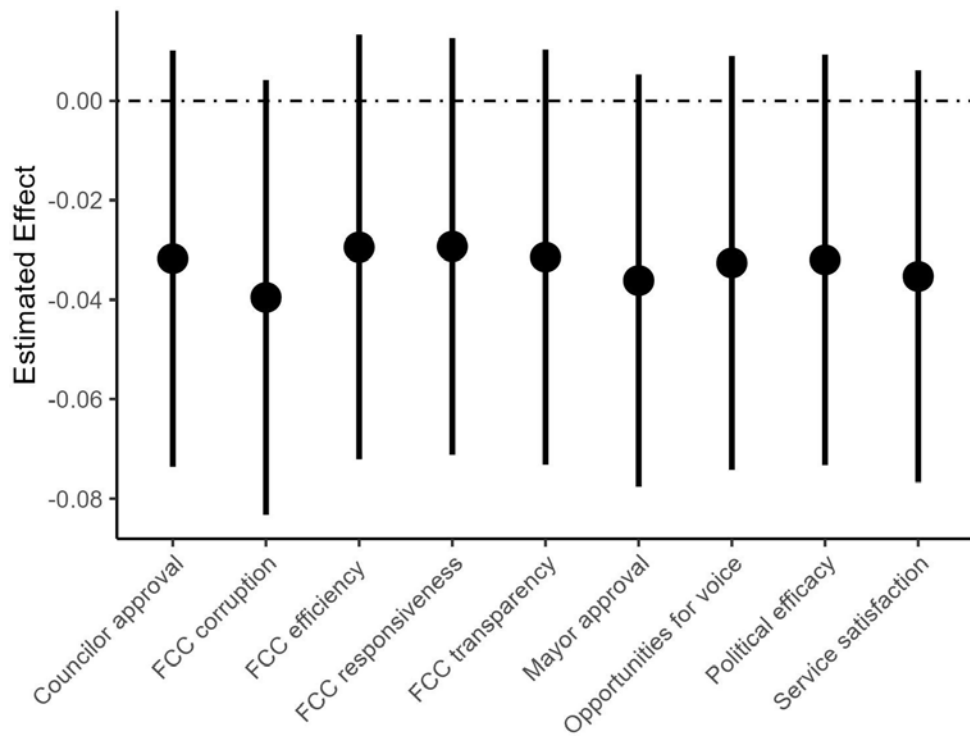
Note: Figure C3 presents treatment effects on tax compliance behavior, conditional on partisan affiliation using an alternative coding. Respondents who self-report affinity towards the All People’s Congress are coded as “copartisans.” Respondents who report affinity to a party other than APC, or report no affinity towards any party, are coded as “opposition.” Respondents who do not answer this question are dropped.

TABLE C12. Effect on Tax Compliance: By Party

Partisan Group	Sub-group Estimates			Dif APC
	CATE	SE	n	ρ -value
APC	0.057	0.034	880	0.095
SLPP	-0.035	0.041	720	0.104
Third Party (NGC/Other)	-0.100	0.120	99	0.212
Independent	-0.012	0.033	1115	0.170
No Response	-0.036	0.039	804	0.085

Note: Table C12 reports treatment effects on compliance by partisan group. Column 1 shows the sub-group treatment effect, Column 2 reports the standard error and Column 3 displays the number of observations in each sub-group. For all groups other than APC, Column 4 reports the ρ -value for the difference with the APC sub-group treatment effect. For the APC effect, Column 4 reports the ρ -value for the difference from zero.

FIGURE C4. Reanalysis of Compliance Effect Dropping Attritors



Note: Figure C4 reanalyzes treatment effects on compliance, excluding property owners who attrited from the survey. Since the sub-sample of respondents varies by survey question, Figure C4 presents compliance effects separately for each question's responding sub-sample.

C.4 Political Engagement

TABLE C13. Political Engagement

Outcome	Baseline	Midline				Endline			
	Mean	Mean	Effect	N	<i>q</i> -val	Mean	Effect	N	<i>q</i> -val
Knows ward councilor name	0.360 (0.480)	0.383 (0.486)	0.104*** (0.040)	3,618	0.004	0.313 (0.463)	0.044 (0.039)	3,618	0.292
Attempted to contact ward councilor	0.193 (0.395)	0.188 (0.391)	0.214*** (0.044)	3,299	0.001	0.264 (0.440)	0.084 (0.052)	2,865	0.292
Attempted to contact MP	0.112 (0.316)	0.092 (0.289)	0.142*** (0.043)	3,297	0.002	0.159 (0.365)	-0.023 (0.055)	2,865	0.693
Level of interest in politics	1.841 (1.090)	1.818 (1.001)	0.161*** (0.040)	3,299	0.001	2.028 (1.096)	0.091* (0.055)	2,575	0.292
Level of interest in FCC activities	2.952 (1.097)	3.103 (0.896)	0.428*** (0.034)	3,300	0.001	3.157 (0.954)	-0.009 (0.043)	2,871	0.704
Attended political meeting	1.157 (0.501)	1.175 (0.545)	0.051 (0.049)	3,301	0.052	1.150 (0.516)	0.104* (0.058)	2,558	0.292

Note: Table C13 reports the effect of the treatment on political engagement measures. Columns 1, 2 and 6 report the control group mean for each indicator for the baseline, midline and endline surveys, respectively, with the standard deviation in parentheses. Column 3 presents treatment effects estimates at the midline survey and Column 7 presents treatment effects estimates at the endline survey. Columns 4 and 8 report the number of non-missing observations in the midline and endline surveys, respectively. Stars refer to randomization inference *p*-values. Columns 5 and 9 report corrected *q*-values, which adjust for multiple hypothesis testing, following [Anderson \(2008\)](#). Reported effects are standardized effects. Attempts to contact MP or Councilor, or attendance at political meeting, are for last six months.

Significance: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

TABLE C14. Effects on Political Engagement Outcomes Conditional on Partisan Affiliation

Outcome	Midline			Endline		
	Copart.	Non Copart.	Diff	Copart.	Non Copart.	Diff
Knows ward councilor name	0.023 (0.051)	0.058 (0.024)	-0.035 (0.062)	-0.025 (0.049)	0.036 (0.024)	-0.060 (0.061)
Attempted to contact ward councilor	0.055 (0.049)	0.094 (0.022)	-0.039 (0.058)	0.065 (0.055)	0.022 (0.026)	0.042 (0.066)
Attempted to contact MP	0.033 (0.040)	0.049 (0.017)	-0.016 (0.048)	0.040 (0.046)	-0.023 (0.022)	0.063 (0.056)
Level of interest in politics	0.292 (0.123)	0.139 (0.053)	0.153 (0.145)	0.289 (0.165)	0.036 (0.075)	0.253 (0.199)
Level of interest in FCC activities	0.541 (0.099)	0.446 (0.047)	0.095 (0.120)	0.153 (0.123)	-0.060 (0.059)	0.214 (0.150)
Attended political meeting	0.071 (0.069)	0.011 (0.030)	0.060 (0.081)	0.070 (0.083)	0.046 (0.035)	0.024 (0.098)

Note: Table C14 reports treatment effects on political engagement, conditional on partisan affiliation, at the midline at endline survey. Columns 1 and 2 report treatment effects at midline for copartisans and non-copartisans, respectively. Column 3 reports the difference in treatment effects between sub-groups. Treatment effects are reported in standard deviation units and standard errors are in parentheses. Columns 4-6 report similar estimates for the endline survey. Respondents who self-report feeling “close to” APC are defined as copartisans; all other respondents are coded as non-copartisans.

Significance: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

C.5 Fairness and Enforcement

At endline, we find no evidence of persistent treatment effects on either fairness or enforcement mechanism outcomes (Table C15). However, at midline, treatment effects on alternative mechanisms are more varied. We see contradictory results for the fairness and equity mechanism. Before services are delivered treatment respondents believe (i) that the tax system is more fair and (ii) that their neighbors are less likely to pay, compared to respondents in the control condition. However, after services are delivered, these results both vanish towards zero. With respect to enforcement, at midline we see strong evidence that the treatment group believes they are *less* likely to be punished if they don't pay property tax, relative to control. Again, by the time services have been delivered, this difference in beliefs about enforcement disappears. In summary, while we do see short-term effects on these alternative mechanisms, we see no evidence that these effects persist after services have been delivered, which is the period that directly precedes tax compliance behavior.

TABLE C15. Effect on Fairness and Enforcement

Outcome	Base	Midline				Endline			
	Mean	Mean	Effect	N	q-val	Mean	Effect	N	q-val
Fairness									
Reform improves tax system fairness	2.113 (0.796)	2.152 (0.691)	0.125** (0.057)	2,252	0.017	2.381 (0.782)	-0.005 (0.049)	2,852	1.000
Number of neighbors who will pay property tax	5.100 (2.381)	5.971 (2.289)	-0.209*** (0.052)	2,878	0.001	5.919 (2.448)	-0.006 (0.060)	2,489	1.000
Enforcement									
Likelihood detected noncompliers are punished	4.060 (1.105)	4.241 (0.983)	-0.316*** (0.044)	3,301	0.001	4.136 (1.042)	0.043 (0.046)	2,857	0.493

Note: Table C15 reports the effect of the treatment on the alternative mechanisms of fairness and enforcement. Columns 1, 2 and 6 report the control group mean for each indicator at baseline, midline and endline, respectively (with the standard deviation in parentheses). Column 3 presents treatment effects estimates at the midline survey and Column 7 presents treatment effects estimates at the endline survey. Column 4 and 8 report the number of non-missing observations in the midline survey and endline survey, respectively. Reported effects are standardized effects. Stars refer to randomization inference p -values. Columns 5 and 9 report corrected q -values, which adjust for multiple hypothesis testing, following [Anderson \(2008\)](#). Reported effects are standardized effects.

Significance: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

C.6 Polarization and Cohesion

TABLE C16. Affective Polarization and Social Cohesion

Outcome	Baseline		Midline			Endline			
	Mean	Mean	Effect	N	<i>q</i> -val	Mean	Effect	N	<i>q</i> -val
Level of trust in neighbors	3.23 (0.81)	3.15 (0.69)	0.131*** (0.038)	3,225	0.011	3.17 (0.74)	-0.017 (0.045)	2,799	1.000
Level of connection of neighbors	4.56 (0.70)	4.49 (0.68)	0.016 (0.044)	3,228	0.193	4.53 (0.70)	-0.053 (0.049)	2,798	1.000
Ease of befriending out-party members	3.45 (0.94)	4.22 (0.94)	-0.178*** (0.052)	3,214	0.011	4.13 (1.04)	-0.008 (0.058)	2,738	1.000
Ease of believing out-party members	2.98 (1.50)	2.52 (1.31)	-0.068 (0.041)	3,206	0.076	2.62 (1.31)	-0.053 (0.046)	2,672	1.000

Note: Table C16 reports treatment effects on affective polarization and social cohesion at the midline and endline surveys. Columns 1, 2 and 6 report the control group mean for each indicator for the baseline, midline, and endline surveys, respectively, with the standard deviation in parentheses. Column 3 presents treatment effects estimates at the midline survey and Column 7 presents treatment effects estimates at the endline survey. Columns 4 and 8 report the number of non-missing observations in the midline survey and endline survey, respectively. Stars refer to randomization inference *p*-values. Columns 5 and 9 report corrected *q*-values, which adjust for multiple hypothesis testing, following [Anderson \(2008\)](#). Reported effects are standardized effects. **Significance:** * *p* < 0.10 ** *p* < 0.05 *** *p* < 0.01

TABLE C17. Effects on Affective Polarization and Social Cohesion by Partisan Affiliation

Outcome	Midline			Endline		
	Copart.	Non-Copart.	Diff	Copart.	Non-Copart.	Diff
Level of trust in neighbors	0.193 (0.084)	0.078 (0.038)	0.115 (0.101)	0.100 (0.100)	-0.014 (0.044)	0.114 (0.118)
Level of connection of neighbors	0.107 (0.087)	-0.019 (0.039)	0.125 (0.105)	0.051 (0.082)	-0.067 (0.042)	0.118 (0.099)
Ease of befriending out-party members	-0.264 (0.133)	-0.136 (0.062)	-0.128 (0.163)	0.068 (0.152)	-0.033 (0.068)	0.100 (0.182)
Ease of believing out-party members	-0.050 (0.163)	-0.118 (0.077)	0.068 (0.197)	-0.114 (0.172)	-0.069 (0.088)	-0.045 (0.211)

Note: Table C17 reports treatment effects on affective polarization and social cohesion, conditional on partisan affiliation, at the midline and endline surveys. Columns 1 and 2 report treatment effects at midline for copartisans and non-copartisans, respectively. Column 3 reports the difference in treatment effects between subgroups. Treatment effects are reported in standard deviation units and standard errors are in parentheses. Columns 4-6 report similar estimates for the endline survey. Respondents who self-report feeling “close to” APC are defined as copartisans; all other respondents as non-copartisans.

Significance: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

D Comparing Partisan Groups

D.1 Differences by Partisan Group

TABLE D1. Approval of Political Representatives by Partisan Group

Measure	Mean		SD	Difference			Observations	
	Opp.	APC	All	Raw	Std.	<i>p</i> -val	Opp.	APC
Approval of APC Political Representatives								
Mayor approval	4.17	4.41	0.89	0.24	0.27***	0.00	2,669	875
Councilor approval	2.66	2.96	1.22	0.30	0.25***	0.00	2,633	869
FCC Service Delivery and Responsiveness								
Satisfaction with FCC service provision	3.58	3.83	1.17	0.25	0.21***	0.00	2,710	876
FCC responsiveness to citizens' demands	3.14	3.27	1.19	0.13	0.11***	0.01	2,575	856
FCC Administrative Competence								
FCC corruption	3.49	3.65	1.00	0.16	0.16***	0.00	2,176	787
FCC efficiency	2.82	2.98	0.71	0.16	0.22***	0.00	2,310	797
FCC transparency	1.37	1.33	0.69	-0.04	-0.06	0.11	2,594	864
Ideology								
Tax rich more for services	2.08	2.07	0.95	-0.01	-0.01	0.69	2,710	878

Note: Table D1 presents approval of APC political leaders and APC-led government institutions by partisan group. Columns 1 and 2 report group means for supporters of the opposition ("opp.") and APC partisans, respectively. Column 3 reports the standard deviation (pooled). Column 4 reports the raw difference in means between APC supporters and opposition supporters and Column 5 standardizes this difference by the standard deviation. Column 6 reports the *p*-value on this difference. Columns 7 and 8 report non-missing observations for opposition and APC supporters, respectively. All respondents who self-report feeling "close to" APC are defined as APC supporters; all other respondents are coded as opposition. A higher value of the outcome *Tax Rich More for Services* indicates *disagreement*.

Significance: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

TABLE D2. Demographics by Partisan Group

Measure	Mean		SD	Difference			Observations	
	Opp.	APC	ALL	Raw	Std.	<i>p</i> -val	Opp.	APC
Demographics								
Female	0.31	0.28	0.46	-0.03	-0.07**	0.05	2,738	880
Age	51.84	51.52	12.96	-0.32	-0.02	0.52	2,728	879
Married	0.71	0.74	0.45	0.03	0.07	0.15	2,729	880
Higher education	0.41	0.36	0.49	-0.05	-0.10**	0.03	2,539	840
Property Characteristics								
Total property tax owed (USD, 2021)	96.91	86.95	183.81	-9.96	-0.05	0.11	2,738	880
Number of properties with tax liability (2021)	1.89	1.99	1.47	0.10	0.07*	0.10	2,738	880
Property has water	0.48	0.44	0.50	-0.04	-0.08*	0.07	2,738	880
Property has drainage	0.36	0.35	0.48	-0.01	-0.02	0.35	2,738	880
In informal settlement	0.06	0.06	0.23	0.00	0.00	0.63	2,738	880

Note: Table D2 presents demographic variables and property characteristics by partisan group. Columns 1 and 2 report group means for supporters of the opposition (“opp.”) and APC partisans, respectively. Column 3 reports the standard deviation (pooled). Column 4 reports the raw difference in means between APC supporters and opposition supporters and Column 5 standardizes this difference by the standard deviation. Column 6 reports the *p*-value on this difference. Columns 7 and 8 report non-missing observations for opposition and APC supporters, respectively. All respondents who self-report feeling “close to” APC are defined as APC supporters; all other respondents are coded as opposition. The variable *Higher education* is an indicator variable equal to 1 if the respondent has a degree from a university, polytechnic school or teachers college.

Significance: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

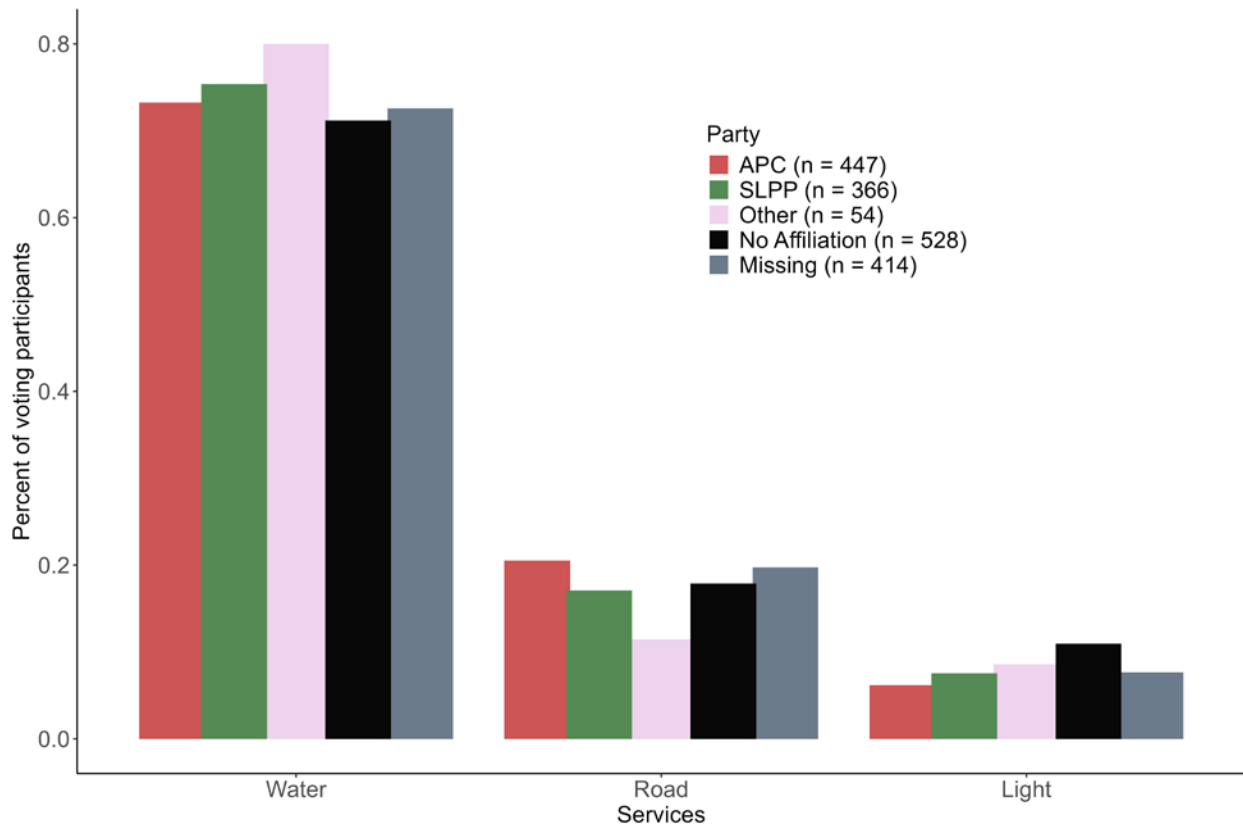
TABLE D3. Effects on Compliance Conditional on Demographic Variables

Outcome	Conditional Effect		Difference	
	CATE (1)	SE (2)	Dif (3)	<i>p</i> -value (4)
Gender				
Male	-0.001	0.021		
Female	-0.038	0.032	-0.036	0.345
Higher Education				
No	-0.015	0.026		
Yes	0.019	0.034	0.034	0.457
Total Tax Owed				
Below Median	-0.024	0.028		
Above Median	0.000	0.030	0.024	0.603
Water Access				
No	-0.029	0.029		
Yes	0.008	0.033	0.037	0.469
Number of Properties with Tax Liability				
One	-0.048	0.031		
More than one	0.011	0.030	0.059	0.233

Note: Table D3 reports conditional treatment effects for five demographic variables where baseline levels differ by partisanship (see Appendix Table D2). Column 1 reports conditional average treatment effects for the two groups comprising each variable; Column 2 reports the standard error. Column 3 reports the difference between sub-group conditional effects and Column 4 reports the *p*-value of that difference. Note that to estimate treatment effects conditional on *Number of Properties with Tax Liability* we drop the 121 property owners that had no tax liability for 2021.

Significance: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

FIGURE D1. Votes for Services by Partisan Affiliation



Note: Figure D1 presents votes for each service, broken out by partisan affiliation. There is little indication that votes for services differ meaningfully by partisan group. Vote share calculated from voting participants. Whether a participant votes is also similar across partisan groups, and is as follows: APC = 58%; SLPP = 54%; No affiliation = 52%; Other = 65%; Missing = 60%.

D.2 Partisan Targeting

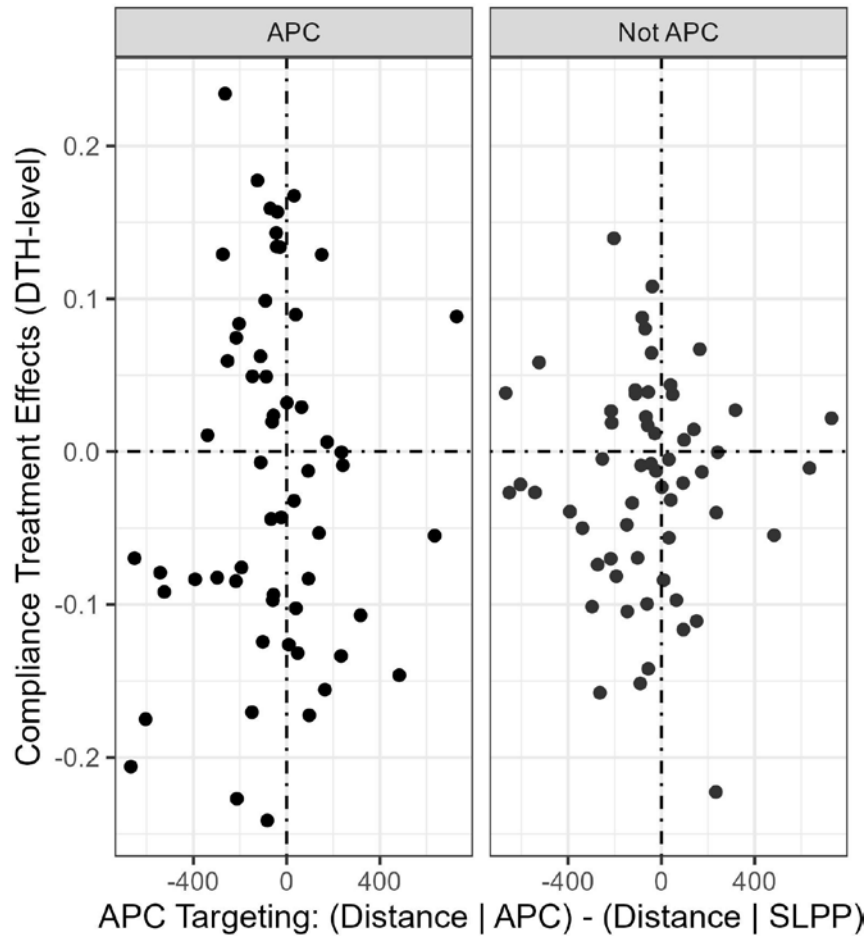
TABLE D4. Distance to Services

Party	Model 1		Model 2		Model 3	
	EST	SE	EST	SE	EST	SE
SLPP	78.96***	23.73	78.82***	23.73	51.60**	20.10
Independent (no reported party)	15.25	20.88	12.40	20.87	0.78	18.26
Third Party Supporter	18.10	47.14	16.67	47.36	2.12	44.31
Did not answer	22.08	22.32	20.53	22.29	14.63	19.15
<i>Controls:</i>						
Property Value logged			✓		✓	
Ward Fixed Effect					✓	

Note: Table D4 reports the relationship between respondents' partisan affiliation and their distance from services implemented by the DTH intervention. The reference category is APC partisans. Point estimates represent the average distance (in meters) that a respondent from a given partisan group is from the implemented service, relative to an APC supporter. Positive point estimates indicate that respondents are further away. These estimates exclude 25 respondents identified in the administrative data as being over 3.5 kilometers from the implemented service, as this suggests incorrect geo-location. The estimates are not sensitive to the threshold used for dropping respondents.

Significance: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

FIGURE D2. APC Service Targeting and Compliance Effects



Note: Figure D2 plots the DTH-level treatment effects against the degree of APC partisan targeting exhibited by the projects implemented in each town hall. APC targeting is defined as the difference between the average distance of APC supporters to the service and the average distance of SLPP supporters to the service. If APC targeting were responsible for the observed heterogeneous treatment effects, we would expect a positive relationship between APC targeting and treatment effects for APC partisans (left panel), and a negative relationship between APC targeting and treatment effects for non-APC supporters (right panel). However, we do not observe these patterns.

E Spillover Analysis

We use a design-based strategy to estimate spillovers that occur due to geographic proximity between properties. For this analysis, we focus on tax compliance spillovers from treated properties to 74,352 properties outside of our study.⁴⁹ Our approach compares non-study properties geographically proximate to treated study properties to non-study properties proximate to control study properties.⁵⁰ We estimate spillovers with the following equation:

$$Y_{i,2022} = \beta_1 SPILL_i + \gamma Y_{i,2020} + \lambda X_i + \delta_w + \epsilon_i \quad (3)$$

Where Y_{2022} is the binary tax compliance outcome of non-study property owner i in 2022; $SPILL_i$ is a dummy variable equal to 1 if there is at least one treated study property *close* to non-study property owner i . Therefore, β_1 captures the spillover effect on tax compliance of being close to a treated property owner. Y_{2020} is the tax compliance behavior of property owner i in 2020; δ is a vector of ward fixed effects; \mathbf{X} is the set of property-level characteristics described in Section 3.3, included as covariate adjustment.

As the density of buildings varies across the city, the probability of being assigned to “spillover treatment” (i.e. the probability that $SPILL_i$ is equal to one in equation 3) varies across properties. That is, non-study properties in denser areas are more likely to be assigned to spillover treatment because they are more likely to be close to more study units. In this context, unweighted regressions can be biased because building density (and therefore treatment assignment) may also be correlated with compliance behavior.⁵¹ To address this, we weight observations by the inverse probability of being assigned their spillover treatment condition, where assignment probability is calculated by re-simulating treatment assignment of study properties (Blattman et al. 2021; Gerber and Green 2012; Chen et al. 2010). Note that this implies non-study properties that are not close to a study property are weighted zero (i.e., not used to calculate spillover effects).

Estimating spillovers crucially depends on choosing a distance threshold to define non-study properties as “close” to study properties. We pre-specified this distance as 64 meters, believing that it would maximize the precision of our estimates, without downward biasing them.⁵²

Table E1 shows spillover effects on compliance behavior at this preregistered threshold distance. Column 1 shows results for compliance behavior in 2022, our preregistered primary dependent variable for the spillover analysis. The point estimate at this threshold is positive but small, about a third of a percentage point, and is statistically insignificant (RI p -values in Column 2). As noted in our pre-analysis plan, the selection of this preferred distance threshold is somewhat arbitrary; the additional results in Column 1 show the estimated spillover effect when the distance thresholds change, both above and below our

⁴⁹ While we observe compliance outcomes for 95,769 properties that are not eligible for the intervention, some individuals own multiple properties. Intuitively, the effects of the DTH should only spill over to affect the compliance behavior of a proximate non-study property when the property owner is living there. As we lack data on the residence of property owners who own more than one property, we assume that these multiple property owners are living in their highest value property. Therefore, our spillover analysis is restricted to the set of 74,352 non-study properties that are the highest value property registered to a given property owner.

⁵⁰ See Miguel and Kremer (2004) for a prominent example of using non-experimental units (i.e., units that are not themselves part of the randomization) to estimate spillovers.

⁵¹ Imagine, for example, potential differences in compliance behavior between densely packed informal settlements and spacious affluent neighborhoods.

⁵² Absent a theory-driven procedure for selecting the threshold distance (D), we opt for a pragmatic approach.

preregistered threshold. The estimated effect is positive at all thresholds, but substantively small and not statistically significant at any threshold. There is some suggestive evidence of a positive spillover for properties within 20 meters of a treated unit ($\beta = 1.8$ percentage points; RI p -value = 0.13), but at that threshold estimates are noisy and we cannot reject the null of no spillover effect.

TABLE E1. Spillover Effects

Threshold Distance (meters)	2022 Compliance		N Observations	
	Est	RI p -val	Treatment	Control
20	0.018	0.133	3,212	2,885
30	0.010	0.273	7,259	5,860
40	0.008	0.247	12,103	8,378
64 (Preregistered Threshold)	0.003	0.597	24,214	10,585
70	0.004	0.483	26,885	10,441
80	0.005	0.437	31,016	9,782
90	0.008	0.233	34,679	8,814
100	0.008	0.227	37,729	7,906

Note: Table E1 reports spillover effects on the compliance behavior of non-study property owners, at different distance thresholds for defining spillover units (Column 1). Column 2 reports spillover treatment effects where the dependent variable is a dummy indicating if the owner paid any tax in 2022. Treatment effects are reported in raw percentage points. Column 3 reports randomization inference p -values from 300 simulations. Columns 3 and 4 refer to the number of observations in treatment and control, respectively, at a given distance threshold.

Significance: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$.

While the overall number of non-study properties used in the spillover estimation increases with higher values of D , the number of spillover control units is maximized when D equals 64 meters. Values of D greater than 64 have increasing units in the spillover treatment condition, but decreasing units of spillover control units. Given that the motivation for selecting higher values of D is to increase precision, selecting a value of D greater than 64 meters requires that the loss of precision brought on by the decline of units in the control arm is outweighed by increase in precision due to additional units entering into the treatment arm. When D is equal to 64 meters the treatment spillover arm has 24,177 units, compared to 10,637 units in spillover control; therefore, we privilege maintaining control units over gaining treatment units.

F Notification Calls

As part of our intervention, we called treated property owners to notify them about the implementation of services selected through the DTH (see Section 2.2). Since this information was provided only to treatment units, our primary analysis does not cleanly isolate the effect of this information alone from the other components of the intervention. To estimate the pure effect of the information about services, we randomized the delivery of this information to property owners outside of our study sample.

Sample: We constructed a sample frame of 15,217 non-study property owners who met the following criteria: (i) they owned a property in one of the 30 study wards, (ii) had a phone number on file at the FCC, (iii) had not been contacted as part of the initial verification process that selected property owners for the study and (iv) had not paid taxes in either 2020 or as of October 23, 2021.⁵³

Randomization: The 15,217 property owners were randomized into treatment and control groups, where treatment is defined as receiving a call notifying the property owner about implemented services. Randomization was performed by blocking on tax rate decile within each ward. Within each block, units were assigned to the treatment group with a probability of 0.588.

Treatment Text: Treated respondents receive a call from a surveyor who identifies themselves as calling on behalf of the FCC. After confirming the respondent's personal information, the surveyor provides the respondent with the following information: *"Recently, in your ward [WARD NAME], [PROJECT DESCRIPTION] has been built by a construction firm on behalf of the Freetown City Council. This is at [PROJECT LOCATION]. This project was funded by resources associated with the FCC's property tax reform."*

Surveyors then ask the property owner if they have heard of this project, and if so, if they had visited it. Then, surveyors conclude the call with the following text: *"We're looking forward to continuing to work with people in your community to better understand the most pressing local development needs. This is one of the steps the FCC is taking to develop the city as part of the FCC's ambitious plan to Transform Freetown. If you have any further questions about the project in your ward, you may contact us at the following phone numbers: XXX or XXX."*

Estimation: We estimate ITT treatment effects:

$$Y_{ikt_2} = \beta_1 T_i + \gamma Y_{ijt_1} + \sum_{j=1}^{299} \theta_k \text{Block}_k + \delta_w + \lambda X_i + \epsilon_i \quad (4)$$

Where Y_{ikt_2} is the post-treatment tax compliance behavior of property owner i in randomization block k ; T_i is an indicator variable equal to 1 if owner i is assigned to treatment and β_1 captures the average treatment effect of the Notification Call. Y_{ikt_1} is the pretreatment compliance measure for owner i . As service delivery calls were made starting in November 2021, the pretreatment compliance variable is an indicator coded as 1 if a property owner had paid tax in 2020 prior to November, when the campaign started. Block_k is an indicator variable equal to 1 if owner i belongs to randomization block k ; δ is a vector of ward fixed effects and E_i is the error term. X is the same set of property-level

⁵³ 2020 tax compliance was only about 3%.

characteristics that we use in our main analysis (Section 3.3), included for covariate adjustment.

Results: Table F1 presents the results. The estimated treatment effects are small in magnitude, statistically insignificant, and robust to alternative specifications. Information about service provision does not appear to have any effect on compliance behavior.

TABLE F1. Effects on the Service Information Campaign on Tax Compliance

Model	EST	SE	<i>p</i> -value	N
Preregistered	0.0032	0.0056	0.57	15,202
Baseline compliance only	0.0024	0.0056	0.67	15,202
Ward FE only	0.0023	0.0056	0.68	15,202
Covariates only	0.0032	0.0055	0.56	15,202

Note: Table F1 presents results of the information campaign that called property owners to notify them of service delivery in their wards. The top row presents the preregistered model that contains pre-treatment compliance behavior, ward fixed effects, match-pair dummies and a set of preregistered controls. As service delivery calls were made starting in November 2021, the pretreatment compliance variable is an indicator coded as 1 if a property owner had paid tax in 2020 prior to November, when the campaign started. The estimated average treatment effect is substantively small and not statistically distinguishable from zero. Rows 2-4 show the robustness of this result to different specifications.

Significance: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

G Research Ethics

We find it important to reflect ethically on several dimensions of this project. First, much of this project was carried out during COVID-19. Therefore, we took several steps to minimize in-person contact and the risks associated with that contact. Most fundamentally, we shifted the project's primary intervention—the town hall—to an online platform, after having originally conceptualized the intervention as a set of in-person town halls. In addition, we conducted data collection through phone interviews, rather than in-person interviews. Phone surveying followed guidelines from the International Growth Centre for conducting research during the pandemic. For example, while our enumeration team met in person to conduct phone interviews, they followed social distancing and sanitation protocols. Finally, all members of our enumeration team received COVID-19 vaccines. We weighed the risks to our enumeration team against the costs associated with calling the project off. Through conversations with research assistants and project supervisors, we believed that much of our enumeration team would be without a paying job during the pandemic if the project were canceled. We reasoned that the costs to enumerators of canceling the project outweighed the risks associated with continuing the project.⁵⁴

Second, only a subset of property owners were eligible to take part in the intervention. We believe that valid equity concerns can be raised about the fact that Freetown citizens who do not own property were not eligible to participate in a participatory budgeting intervention. In addition, eligibility was restricted to property owners (i) with WhatsApp and (ii) with a property above median property value. These latter restrictions were for practical reasons. As the original intervention was originally planned for 2021, and the outcome of interest would be tax compliance in that year, we could only focus on the subset of property owners who received an RDN in 2020.⁵⁵ We believe that restricting the intervention to property owners is justified by the scientific goal of the study and because we believe the project has increased the likelihood that all residents of Freetown have a chance to participate in future participatory budgeting programs. Scientifically, we are primarily interested in the relationship between participation in DTH and property tax compliance. Given budget constraints, including citizens not owning property in the intervention would weaken our ability to learn about the effect of the DTH on tax compliance. Moreover, future iterations of the DTHs, to which the Mayor of Freetown has publicly committed, promise to be less restrictive. Freetown residents who were not eligible for this iteration of the DTH are now more likely to be eligible for future participatory budgeting programs, compared to if this DTH project had never taken place. Finally, we do not believe it to be the case that the selected public services only benefit, or even are more likely to benefit, property owners. For example, community water pumps or street taps benefit everyone in the community, not just property owners. Third, we purposefully did not inform participants that the funds for selected services came from donors. While we were generally ambiguous about the source of the funding, in at least one instance, scripted messages from moderators to participants in DTH referred to DTHs as a way to decide on the allocation of some of "FCC's budget". Placards placed at the site of completed projects list the Freetown City Council as the sole implementing partner and the FCC's logo is the only logo on these placards. We believe this deception to be justified by the scientific benefits of the project. While external donors often play a significant role in bankrolling poor local governments,

⁵⁴ When making our decision to continue with the project during COVID-19, our research team primarily considered the risks and benefits to our enumeration team. However, we can also point out the additional project benefit of delivering key services (totalling over \$45,000) in Freetown.

⁵⁵ As described previously, as part of a COVID-19 policy to reduce tax burdens on lower-income households, only property owners in the top half of the assessment distribution received RDNs in 2020.

our goal is to study the fiscal contract between government and citizens. We reasoned that acknowledging the external source of funding would make our results more difficult to interpret. Finally, we note that we are not aware of evidence showing that donor credit claiming for donor-funded projects leads to positive outcomes for citizens; in the absence of such evidence, we follow our instinct that donor credit claiming for our project is not an *ex ante* normatively superior decision.



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