

Uninformed or Unmotivated?

A Field Experiment on Local Leader Responsiveness in Tanzania

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Abstract

When local leaders neglect their constituents' priorities, is it because they are uninformed or unmotivated? Interviews with 340 Tanzanian local leaders and 4,309 of their constituents reveal gaps in leaders' knowledge of, alignment with, and willingness to respond to their constituents' priorities. To test why these gaps persist, I conducted a preregistered field experiment that randomly assigned leaders across 109 villages to receive polling data on their constituents' priorities, a journalist's message that their conduct would be investigated and broadcast, both, or neither. Information raised leaders' ranking of their constituents' top priority by 0.29 standard deviations, their hypothetical budget for the priority by 18 percent, and their lobbying for the priority by 5.2 percentage points. The journalist's message raised lobbying from 9 to 16 percent. Against preregistered expectations, I find no evidence that the treatments interact. Informational and motivational obstacles limit local responsiveness under dominant-party rule, and priority polling and accountability journalism can each reduce them.

Keywords: Government responsiveness; Accountability; Information; Sub-Saharan Africa; Local politics; Media

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Introduction

Governments in developing countries often fail to deliver the services their citizens want. Many of these failures stem from structural barriers, such as limited budgets or the absence of effective electoral accountability ([Grossman and Slough 2022](#)). But blame may also lie with village and community leaders who fail to advocate for the needs of their constituents ([Auerbach 2020](#); [Baldwin 2016](#); [Kruks-Wisner 2022](#)). This failure is particularly consequential in decentralized systems, where district budgets are built in part from village requests for development assistance ([Carlitz 2017](#); [Tidemand 2015](#)).

Why do local leaders often fail to advocate for their constituents' priorities? One explanation holds that leaders are unresponsive because they lack reliable information about what their constituents want. An alternative view is that leaders know what their constituents want, but are insufficiently motivated by the prospect of electoral, professional, or social accountability. Both explanations have inspired rich theoretical and empirical literatures, but they are rarely evaluated experimentally in tandem. As a result, uncertainty remains about how informational and motivational barriers to government responsiveness compare and interact.

Untangling the barriers to government responsiveness has implications for the design of policies and programs to strengthen the link between what citizens want and what government provides. If the key barrier is information, promise lies in efforts to elicit and disseminate constituents' priorities, such as public polling. If the key barrier is motivation, attention should turn towards tools to help governments and electorates monitor and sanction under-performing officials, such as accountability journalism. And if the two mechanisms reinforce one another, programs to promote responsiveness would gain most from combining information and motivation.

I empirically evaluate information and motivation-based explanations for non-responsiveness, and their interaction, using a 2×2 field experiment among 340 village leaders (elected village chairpersons and councilmembers, and appointed village executive officers) across 109 village governments in rural Tanzania. In the *information* treatment, enumerators provided village leaders with representative public-opinion data about their constituents' development priorities. In the *motivation* treatment, local journalists told village leaders that their village would be included in a radio investigation of service delivery problems in their district. One month later, staff from the polling organization who had not

delivered either treatment gave every leader a costly opportunity to lobby their district council for a development project on their constituents' behalf.

Rural Tanzania offers a hard case for testing strategies to improve responsive local governance. Tanzania is a hegemonic party regime with a poor track record on rural service delivery and weak civil society institutions (Spurk and Katunzi 2023; Weghorst 2022). In such a context, it is far from obvious whether and why local leaders would respond to better information about their constituents' preferences or the prospect of public scrutiny in the absence of substantial electoral competition and bureaucratic oversight (Arkedis et al. 2021; Paget 2021). Yet even in this unpromising environment, mechanisms of accountability may still operate through intra-party electoral pressures, intra-village social pressures, or top-down professional pressures.

Descriptive evidence from surveys of 4,309 citizens and 340 village leaders across 109 villages confirms the existence of formidable barriers to local government responsiveness. Fewer than half of village leaders know their constituents' top policy priority, and there is a weak correlation between the expressed priorities and hypothetical budget allocations of village leaders and the priorities of the citizens they represent. Moreover, only a small fraction of leaders lobby district officials on behalf of their constituents' development priorities when given an explicit opportunity to do so.

In this challenging context, I find that providing credible information about constituents' development concerns causes local leaders to update their own priorities in line with those of their community (by 0.36 points for their constituents' top priority and 0.33 points for their second, on a 0 to 3 scale, one-tailed randomization-inference p -values = 0.003 and 0.004). Treated leaders also increased their budget allocations to their constituents' first priority by 18% in a hypothetical budget allocation (3.7 percentage points over a control mean of 20.2%, one-tailed RI p -value = 0.016). Finally, leaders who received information were 5.2 percentage points more likely to respond to an offer to lobby their district council for their constituents' top priority (one-tailed RI p -value = 0.042), with a smaller effect on lobbying of any kind (3.9 percentage points, one-tailed RI p -value = 0.142).

Turning to the motivation treatment, receiving the notification from a journalist raised the probability that the leader took advantage of an opportunity to lobby district officials on their constituents' behalf from 9 to 16 percent (6.9 percentage points, one-tailed RI p -value = 0.038). Treated villages were

16 percentage points more likely to have at least one leader lobby district officials and 13 percentage points more likely to have at least one leader lobby for the villagers' top priority.

However, against preregistered expectations, I do not observe evidence that information and motivation operate in synergy. The estimated interaction effect is small and imprecisely estimated for both primary outcomes, and the estimates are more consistent with additive effects. Treatment effects also do not vary detectably with ward competitiveness, elected office, or leaders' ties to their village.

This study makes three contributions. First, it contributes to the literature around information-based explanations of government unresponsiveness. The finding that providing information about citizens' development *priorities* influences their leaders' priorities and behavior complements research on the effects of informing officials about the issue *positions* of their constituents (Butler and Nickerson 2011; Dynes et al. 2022; García-Sánchez et al. 2023; Liaqat 2020; Sevenans et al. 2016), and points to a broad role for interventions that raise the salience of citizens' concerns. This focus on *priority responsiveness* is especially relevant to local governance in developing countries, where the central challenge is delivering core services such as health, water, and roads rather than resolving disagreements over policy (Grossman and Slough 2022).

Second, the study adds to scholarly debates about strategies to promote local government accountability. The findings offer novel experimental evidence that the mere *prospect* of local journalism coverage can motivate local leaders to act on their constituents' behalf, complementing existing evidence on the accountability effects of report-cards, social media, and citizens' groups (Anderson et al. 2019; Björkman and Svensson 2009; Grossman et al. 2024). The findings also add to observational research linking journalism to corruption reduction (Ferraz and Finan 2008; Larreguy et al. 2020), responses to natural disasters (Besley and Burgess 2002; Sen 1991), and public spending (Banerjee et al. 2019; Snyder and Strömberg 2010).

Finally, this study contributes to our understanding of local governance under hegemonic party regimes. Dominant parties survive in part by delivering services and by gathering information about what citizens want (Magaloni and Kricheli 2010; Malesky and Schuler 2011; Morse 2019). A growing literature shows that autocratic and dominant-party governments can be responsive through local legislatures, petitions, and online appeals (Chen et al. 2016; Chen and Xu 2017; Manion 2015; Truex 2016;

[Tsai 2007](#)), but most of this evidence concerns officials above the village level. Field experiments on local accountability in Africa, meanwhile, have mostly focused on interventions targeting citizens with report cards, civic information, or new channels to contact their representatives ([Gottlieb 2016](#); [Grossman and Michelitch 2018](#); [Grossman et al. 2018](#); [Raffler et al. 2019](#)). This study targets the village leaders who have been a foundation of Tanzania’s ruling party dominance in rural communities since independence ([Morse 2014](#)), and offers novel descriptive evidence about the extent of local leaders’ knowledge of, alignment with, and willingness to act on behalf of their constituent interests.

The design has several limitations that point towards opportunities for future research. The outcomes center on the knowledge, priorities, and behavior of local leaders rather than long-term service delivery, and future studies would do well to follow budgets and projects over a longer period. To keep the treatments realistic, I bundled several analytically distinct components. The information treatment asked leaders to guess their constituents’ top priority before sharing the polling results, and the motivation treatment asked leaders to talk through their plans for future action in addition to telling them about future media coverage. The stated-priority and budget outcomes were collected in the same interview that delivered the information treatment, so it is difficult to rule out demand effects for those outcomes. Finally, the sample size leaves estimates of the interaction between the treatments, and of heterogeneous effects by leader characteristics, imprecise.

Information, Motivation, and Government Responsiveness

The degree to which political representatives understand, share, and respond to constituent priorities is a foundational concern in political science ([Przeworski et al. 1999](#); [Sen 1991](#)). Scholars have long investigated institutions and interventions that tighten the link between what constituents want and what their political representatives provide ([Butler and Nickerson 2011](#); [Humphreys et al. 2019](#); [Malesky et al. 2012](#)).

This section develops a simple framework for understanding two potential barriers to responsive governance, information and motivation, as well as their interaction. I focus in particular on the theory and design of interventions to address information and motivation barriers.

Information Mechanism

Canonical theories of political representation hold that leaders can serve their constituents only if they know what those constituents want (Miller and Stokes 1963).¹ Lacking accurate information about constituents' priorities, leaders may respond to the wrong priorities or refrain from exerting effort altogether.

Yet a growing body of evidence shows that politicians routinely misperceive their constituents' preferences, often by large margins (Broockman and Skovron 2018; Butler and Nickerson 2011; Dynes et al. 2022; Kalla and Porter 2021; Loewen et al. 2022; Sevenans et al. 2023; Walgrave and Soontjens 2023). Recent syntheses conclude that such misperceptions are pervasive across countries and levels of government (Walgrave et al. 2024), including among local leaders in developing countries (Liaqat 2020).

Efforts to ameliorate information barriers to government responsiveness include petitions, citizen complaint boxes, village meetings, text-message feedback systems, and meet-your-representative media appearances. Perhaps the most common information intervention in industrialized democracies is public polling. The evidence for polling's effect on legislative outcomes is mixed. Butler and Nickerson (2011) show that constituency opinion changed how state legislators voted, while García-Sánchez et al. (2023) find that telling national legislators about citizens' support for a policy did not change the legislators' own support for it. In developing countries, Liaqat (2020) finds that informing local politicians in Pakistan about citizens' preferences moved their recommendations to party leadership closer to those preferences, while Golden et al. (2025) find that Pakistani politicians who received citizens' preferences did not change how they engaged voters or delivered services.

However, most of this evidence concerns leaders' perceptions of citizens' *positions* on policy questions. Less is known about whether leaders share their constituents' *priorities*: which of a menu of broadly supported policy options deserves *most* attention? The distinction between positions and priorities is especially important in local governance in developing countries, where national policy issues

¹Leaders may also come to reflect their constituents because voters select representatives who already share their views, rather than because representatives infer and act on constituent preferences (Miller and Stokes 1963).

are less salient and the desire for more services can be safely assumed (Grossman and Slough 2022). The central challenge of responsive governance in these contexts is prioritizing *which* basic services to deliver given resource constraints.

Hypothesis 1. *Providing village leaders with information about constituent priorities increases village leader responsiveness to constituent priorities.*

Motivation Mechanism

Even when leaders know what their constituents want, they may not act on that knowledge unless they anticipate benefits for good performance. In a classic principal-agent dynamic, a principal delegates authority to an agent to act on their behalf but cannot perfectly observe whether the agent exerts effort, and so must rely on the threat of sanction and the promise of reward to bring the agent's behavior in line with their interests (Barro 1973; Fearon 1999; Ferejohn 1986).

Local leaders in developing countries may respond to *electoral* incentives from their constituents (Besley and Burgess 2002; Bobonis et al. 2016; Grossman et al. 2018), *social* incentives from their immediate community (Kruks-Wisner 2022; Tsai 2007), or *professional* incentives from their superiors in government or the ruling party (Raffler 2022; Slough 2020). Each of these requires the local leader to believe that the principal can monitor their performance and issue meaningful punishments or rewards.

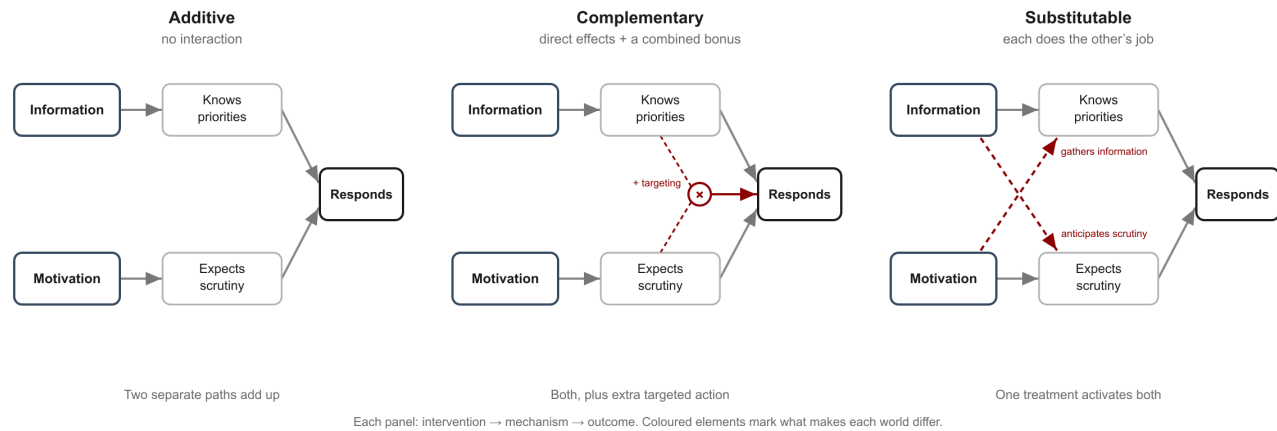
A large literature evaluates interventions that help various principals monitor leaders' performance. Prototypical motivational interventions include distributing report-card scores to citizens to activate electoral incentives, help community groups monitor and pressure service providers to activate social incentives, and supply political or bureaucratic superiors with information about leaders' performance to activate professional incentives.

Journalism has been celebrated as an especially useful tool for promoting accountability because it makes the performance of leaders common knowledge. The prospect of a journalistic investigation may therefore trigger any one of these electoral, social, or professional sanctioning concerns, or all three at once (Besley and Burgess 2002; Ferraz and Finan 2008; Larreguy et al. 2020; Sen 1991).

Hypothesis 2. *Telling village leaders about a future accountability journalism investigation on selected issue areas increases village leader responsiveness towards those issue areas.*

Information and Motivation Interaction

Figure 1: Three Predictions for How Information and Motivation Combine



How might interventions that address information and motivation barriers interact? [Figure 1](#) presents three possibilities. The most straightforward assumption is that interventions are *additive*. Because information and motivation address distinct obstacles to responsive governance, their combined effect would simply be the sum of the two interventions.

Alternatively, the interventions could be *complementary*. A leader who receives information but faces no scrutiny may sit on it, and a leader who faces scrutiny but does not know *which* priorities matter may delay acting or target the wrong problem. The combined effect of information and motivation would then exceed the sum of their separate effects.

Finally, the interventions could be *substitutes*. Information about constituent preferences may itself lead a leader to anticipate scrutiny, activating the motivation channel without any direct change in incentives. Scrutiny may likewise lead a leader to seek out information about constituent priorities on her own even without any direct provision of information. Each intervention activates the other's mechanism, so the combined effect falls short of the sum of their separate effects.

Hypothesis 3. *Village leaders will be more responsive to information about their constituents' priorities when they anticipate an accountability journalism investigation.*

Heterogeneous Treatment Effects

If leaders respond to information or motivation, we might still wonder *why* they do so. The research design cannot experimentally determine whether leaders respond due to the fear of electoral, social, or

professional costs of inaction. I marshal indirect evidence for these motivations by comparing treatment effects across leaders for whom each channel should be strongest.

First, if leaders respond because they fear *electoral* penalties for inaction, the treatments should be most effective among leaders in electorally competitive constituencies ([Besley and Burgess 2002](#)).

Hypothesis 4.1 *Village leaders who were elected and/or live in politically competitive areas will be more responsive to information about their constituents' priorities and the prospect of future media reports.*

On the other hand, if leaders respond to information and motivation because they fear *social* penalties from their community, we should expect the treatments to be most effective among leaders with strong social ties to their community.

Hypothesis 4.2 *Village leaders who were born in their community and/or lived longer in their community will be more responsive to information about their constituents' priorities and the prospect of future media reports.*

Finally, if leaders respond because they fear *professional* penalties from their superiors in government, we should expect the treatments to be most effective among bureaucrats who can be directly removed or promoted by their professional superiors ([Liaqat 2020](#)).

Hypothesis 4.3 *Village leaders who were appointed will be more responsive to information about their constituents' priorities and the prospect of future media reports.*

Such comparisons are not decisive tests of mechanisms, however. A difference in treatment effects across a moderator reveals which mechanism is at work only under the assumption that the moderator shapes that mechanism alone ([Fu and Slough 2025](#)), and elected and appointed leaders, for example, differ in their tenure and ties to the village as well as in their exposure to voters.

Context

The study is set in rural northeastern Tanzania. Three aspects of the setting are especially relevant for interpreting the study findings: the local government system, the dominance of the national ruling party, and the gaps in the current system of service delivery.

Local Government

Village governance in rural mainland Tanzania comprises village chairpersons (*mwenyekiti wa kijiji*, VC), village councilmembers (*wajumbe wa halmashauri ya kijiji*, VCMs), and village executive officers (*afisa mtendaji wa kijiji*, VEOs). Village chairpersons and councilmembers are directly elected by village residents aged 18 or older. The village chairperson serves as the “mayor” of the village. They convene village meetings where village policy is decided and represent the village to the ward and district council. Village councilmembers participate in council decisions and often serve on committees responsible for particular policy areas ([Mhina 2015](#)).

The VEO, by contrast, is an employee of the district council assigned to serve the village. The VEO serves as the “secretary” for the village and is responsible for maintaining records, overseeing village finances, and assisting with policy implementation. VEOs may be transferred among villages and are substantially less locally rooted than elected leaders. Field interviews also indicated that some VEOs maintain residences in nearby towns rather than living full-time in the villages where they serve.

Ruling-Party Dominance

Tanzania is a hegemonic party regime. Chama cha Mapinduzi (CCM) and its predecessor have governed since independence in 1961, and CCM has won every national election since multiparty competition returned in 1995 ([Morse 2019](#)). The space for opposition parties narrowed under President John Magufuli (2015–2021), and the 2020 general election ended in a landslide for the ruling party ([Paget 2021](#)). Village chairpersons and councilmembers are elected in separate local government elections, while ward councilors and members of parliament are elected as part of national general elections.

However, local leaders still have reason to worry about local elections even when the ruling party’s victory is close to certain. First, candidates compete for CCM nominations in party primaries. Second, poor performance in general elections can threaten politicians’ prospects for moving up within the ruling party hierarchy.

Service Delivery

The Tanzanian government formally implemented the Local Government Reform Program (LGRP), an effort to decentralize decision-making and delivery of services like education, water, and health care, in 1999 ([McLellan 2022](#); [Mdee and Mushi 2021](#); [Tidemand 2015](#)). Decentralization was intended

to improve responsiveness, service delivery targeting, and citizen participation (Tidemand 2015). In practice, decentralization has been both less effective and less complete than originally planned (Lange 2008; Mdee and Mushi 2021). Decentralization lacked buy-in from citizens, leaders, and Tanzania’s ruling party, so its implementation was often haphazard and poorly executed. Shifting financial and administrative power away from the national government also threatened a central pillar of the ruling party’s strategy for maintaining its dominance, the directed flow of government services (Tidemand 2015). The Tanzanian government intensified a process of “re-centralization” in 2015, in an effort to re-assert primary control over the flow of financial resources (McLellan 2025).

Today, village leaders in Tanzania can influence service delivery in three ways. First, they organize collective action at the village level (e.g., requesting that villagers contribute labor and resources to development projects). Second, they allocate small village-level discretionary development budgets. Third, and most relevant for this study, village leaders identify development priorities in their villages and lobby external actors (e.g. district councils, members of parliament, non-governmental organizations) for support to implement them. Much of this lobbying takes place through formal channels: village leaders meet with ward development councils on a quarterly basis to advocate for village priorities. But village leaders also engage in extensive informal lobbying efforts to ensure that their community’s needs receive attention (Mhina 2015). In in-depth interviews, village leaders describe this work to understand their constituents’ preferences and lobby ward and district councils for development assistance as the most important part of their jobs. “Our [village] budget is so small,” said one village chairperson, “it is only the district [government] that can build a clinic.”

Description of Treatments

Information Treatment

In the information treatment, village leaders were provided polling data about their constituents’ development priorities. The data came from public-opinion surveys of 40 randomly selected constituents in each village between 2021 and 2022. The surveys covered a wide range of social views and policy priorities. In one module, enumerators asked citizens to rank community priorities, such as water, roads, education, healthcare, and electricity, from most to least important (the Citizen Measures section below describes the citizen surveys and the priority-ranking measure in more detail).

In July and August 2022, a team of enumerators called all village leaders in the sample and offered to share findings from the public opinion surveys just described. Enumerators shared survey results on demographic information, radio ownership, and interest in politics with leaders in both the treatment and control groups. For leaders in the treatment group, enumerators also shared polling results about constituents' policy priorities. The information treatment script read:

Now I would like to talk about development issues in the village. We asked villagers in this village to say what their goals were for the village. This means that they listed what they thought were the biggest problems that they hoped local government could resolve in the coming year. First, what do you think was the issue citizens thought was MOST important?

After recording the leader's guess, enumerators continued:²

Thank you. When we surveyed people in your village, we found that most people thought that [First Ranked] issue was the most important issue in the village. The second most important issue to them was [Second Ranked] issue. That means that the issue citizens in YOUR village MOST want local leaders to help solve is [First Ranked] problem first and [Second Ranked] problem second.

Motivation Treatment

In the motivation treatment, local journalists called all the sampled leaders in treatment villages and told them about an upcoming news investigation of a specific service delivery issue (water, roads, electricity, health, or education) in the leaders' village. Journalists investigated the issue that constituents in the village had ranked as most important, so the treatment would not pull leaders' attention away from what mattered most to their community.

The journalists told leaders that the story would be broadcast on a popular regional radio station by the end of the year,³ and that they would conduct a follow-up over the next few months to find out what actions village leaders had taken to improve water/education/health/roads in their community. The loose script the journalists and I developed was as follows:

"Hello, my name is (insert name), I am a journalist from [local radio station]. This year we are producing a large series on how communities are responding to [insert top-ranked issue

²The script was delivered in Kiswahili, where "most people" reads *watu wengi* ("many people"). The top issue was the one with the highest average ranking. It was also the most common first choice in 89 of the 109 villages ([Village Priority Rankings](#) in the Appendix).

³the stories were broadcast after the outcome data collection.

in community] in this District. We are talking to leaders in many villages to understand how [issue] is affecting their village and what actions they are taking this year to solve the issue.

First, can you tell me about how problems with [issue] are affecting citizens in your village? Second, can you tell me some actions that you and other leaders in your village are planning to take to respond to [issue] this year?

We are going to follow up at the end of this year to put village stories on our radio program and website, so we hope that we can return and find out some of the actions that you have taken to respond to [issue] over the next few months. We will be very happy to include you as a positive story if actions are taken. If no actions are taken, we will report about the problems that are still happening in the village. Thank you very much for your time, and here is my number if you have any questions or concerns.”

Contact of this kind from journalists is realistic but far from routine for village leaders. Almost all local radio stations in Tanzania broadcast daily local news and employ teams of journalists (Katunzi and Spurk 2019; Rahmani 2025; Spurk and Dingerkus 2017). They report most consistently on new government development projects, local events, and citizens aggrieved by service delivery failures (Schiffrin et al. 2026). In one recent national survey of local leaders in rural Tanzania, about 30% indicated that a journalist had visited or reported on their village in the past year, and 25% recalled hearing such a report about their village on the radio (Groves 2023).

Research Design

Site Selection

The study sites were 109 rural villages distributed across 4 districts in Tanzania’s northeastern Tanga Region. Villages were eligible for inclusion if they met the following conditions: they did not touch a main or secondary road, they were at least 5km from a major town, and they were at least 3km from any other selected village.

Respondent Sampling

To sample constituents, the research team used satellite images to approximate each village’s boundary and then conducted a census of every household within it, recording the age and gender of members aged 18 to 65. In each village, the team randomly assigned 20 households to female interviews and 20 to male interviews, and randomly selected one household member of the assigned gender. If no eligi-

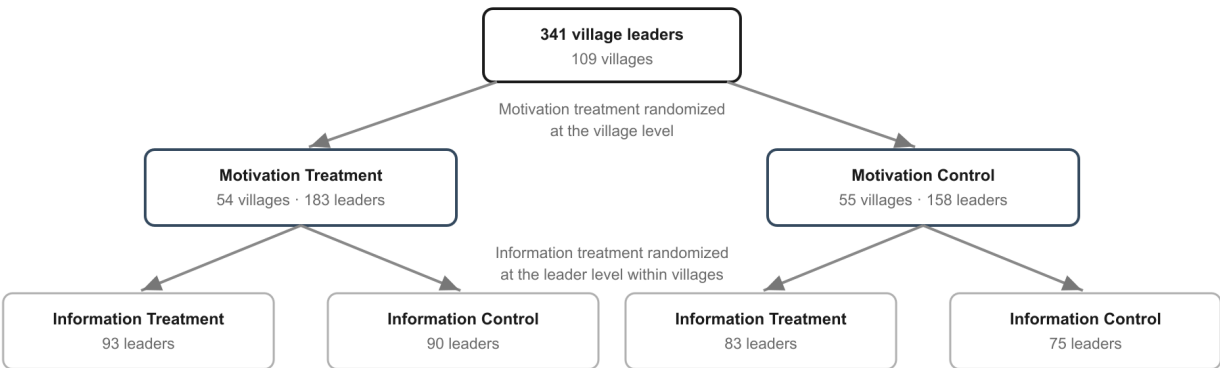
ble member was available over two days of interviews, the household was dropped and a replacement household was selected.

To sample village leaders, I selected the village chairperson (*mwenyekiti*), the village executive officer (VEO), and up to three village councilmembers per village. Selection occurred prior to randomization and so was unaffected by treatment assignment.

Randomization and Covariate Balance

Figure 2 visualizes the research design. I randomly assigned the motivation treatment at the village level to limit spillovers between leaders in the same village. I randomly assigned the information treatment across leaders within each village for two reasons. First, leader-level assignment maximized statistical power for a light-footprint intervention. Second, spillover risk was lower for the information treatment. The stated-priority and budget outcomes were collected in the same call that delivered the information, and the information was embedded in a feedback survey that shared a range of social and political results, which made the treatment itself less salient. Nonetheless, the estimated effect of the information treatment on lobbying behavior should be interpreted in light of potential within-village spillovers, which would tend to bias the information effect toward zero.

Figure 2: Overview of Random Assignment



One village executive officer assigned to the information treatment and motivation control did not respond to the endline survey (analysis sample: 340 leaders).

Table 1 describes the 340 leaders in the sample and tests covariate balance across experimental conditions. Leaders are 47 years old on average, and 79 percent are men. Like their constituents, most leaders are Muslim (60 percent). The typical leader has spent most of their life in the village they represent, but elected and appointed leaders differ sharply. Elected chairpersons and councilmembers

have lived in their village for 40 years on average, and half have lived there all their lives. Appointed village executive officers have lived there for seven years on average, and only 7 percent have lived there all their lives.

In the motivation experiment, no covariate differs across conditions at the 0.05 level and just three differ at the 0.1 level. In the information experiment, leaders who received information were less likely to be women (p -value = 0.040) and to be village councilmembers (p -value = 0.054). However, a joint test of balance across all baseline covariates is consistent with randomization (RI p -value = 0.19).

Table 1: Balance of Baseline Covariates Across Experimental Conditions

Variable	Motivation Experiment			Information Experiment			N
	Treat (N = 183)	Control (N = 157)	<i>p</i> -value	Treat (N = 173)	Control (N = 167)	<i>p</i> -value	
<i>Leader Information</i>							
Age	47.82	46.75	0.376	47.56	47.07	0.696	324
Female	0.20	0.22	0.739	0.17	0.26	0.040	339
Muslim	0.54	0.66	0.062	0.56	0.64	0.124	324
Tribe: Sambaa	0.42	0.36	0.439	0.43	0.35	0.104	340
Years in Village	31.15	33.10	0.235	30.90	33.26	0.295	324
Years in Position	5.36	6.03	0.219	5.61	5.74	0.833	324
Position: Village Chairperson	0.30	0.34	0.196	0.34	0.29	0.347	340
Position: Village Executive Officer	0.26	0.24	0.401	0.28	0.22	0.235	340
Position: Village Councilmember	0.45	0.41	1.000	0.38	0.49	0.054	340
<i>Village Preferences</i>							
Village First Preference: Education	0.21	0.31	0.420	0.25	0.25	0.952	340
Village First Preference: Electricity	0.20	0.10	0.196	0.16	0.14	0.643	340
Village First Preference: Health	0.08	0.12	0.295	0.10	0.09	0.659	340
Village First Preference: Roads	0.36	0.22	0.128	0.29	0.30	0.834	340
Village First Preference: Water	0.16	0.25	0.200	0.19	0.22	0.571	340
Village Second Preference: Education	0.30	0.34	0.449	0.30	0.34	0.493	340
Village Second Preference: Electricity	0.08	0.21	0.090	0.16	0.13	0.423	340
Village Second Preference: Health	0.16	0.17	0.937	0.17	0.16	0.766	340
Village Second Preference: Roads	0.28	0.17	0.122	0.20	0.26	0.184	340
Village Second Preference: Water	0.18	0.11	0.309	0.17	0.12	0.162	340
<i>Village Politics</i>							
2nd Place Vote, 2016 Ward Election	0.29	0.33	0.124	0.31	0.31	0.901	331
CCM Vote, 2016 Ward Election	0.70	0.66	0.108	0.68	0.68	0.817	340
Winner Vote, 2016 Ward Election	0.71	0.66	0.079	0.69	0.69	0.856	340
Total Parties, 2016 Ward Election	2.18	2.30	0.242	2.27	2.20	0.268	340

Note: Columns 2–3 report treatment and control means for the motivation experiment, and column 4 the two-sided p -value from a regression of the covariate on the motivation treatment, weighted by inverse probability of assignment with standard errors clustered by village. Columns 5–6 report treatment and control means for the information experiment, and column 7 the two-sided p -value from an unweighted regression of the covariate on the information treatment with heteroskedasticity-robust standard errors (information is assigned at the leader level). Column 8 reports the number of leaders with nonmissing data. Gender for 16 leaders whose endline gender question was skipped is coded from respondent names recorded at endline. One leader's gender remains missing.

Data Collection and Outcome Measures

Citizen Measures

Prior to the experiment, enumerators visited 109 villages in Tanzania's Tanga region and asked 40 citizens a battery of questions about their social values and political views. To measure constituents' prioritization of development issues, enumerators laid out a set of cards visualizing potential goals for a village and asked respondents to stack the cards from most to least important.⁴

Leader Measures

Knowledge. Enumerators measured village leaders' *knowledge* of their constituents' priorities just before delivering the information treatment by asking leaders to guess their constituents' top development priority. Specifically, enumerators asked, *"Now I would like to talk about development issues in the village. We asked citizens in this village to say what their priorities were for the village. This means they listed what they thought were the biggest problems that they hoped the local government could resolve in the coming year. First, what do you think was the issue citizens thought was MOST important?"* Respondents chose one of the five major development priorities in Tanzania (education, electricity, health, roads, and water) or named another issue.

Prioritization. Enumerators measured village leaders' *priorities* at two points. First, as part of the baseline survey of constituents, enumerators asked two or three village leaders in each village to complete the same card-sorting exercise as citizens. Leaders completed this exercise in only 83 villages, because research clearance to speak with leaders was not granted until the second wave of village surveys. I use these data only to describe the concordance of leader and constituent priorities.

Second, at the end of the phone survey in which the information treatment was delivered, enumerators asked village leaders to name the three issues they believed were most important in their village: *"Now, think about your own views of what the most important problems are in your community. What are the three most important goals for your community in the coming year? What is the MOST important issue? What is the SECOND most important issue? What is the THIRD most important issue?"* I code each response 3 if the leader ranked their constituents' top priority first, 2 if second, 1 if third, and 0

⁴While additional options were included in some of the surveys, all surveys included water, roads, education, healthcare, and electricity and I focus on these core service delivery goals.

otherwise, and apply the same coding to constituents' second-ranked priority.

Leaders then divided a hypothetical budget among the five development issues. Enumerators asked: *"Now, imagine that you had 10,000,000 TZS to spend on development projects in your village this year. I would like to know how much you would spend on each of the following items: health, education, water, roads, and electricity."* Any money left over went to leader salaries and office support. I measure the share of the budget that each leader gave to their constituents' first- and second-ranked priorities.

Behaviors. I measured village leaders' *responsiveness behaviors* one month after the information treatment and two weeks after the motivation treatment. An enumerator from the research organization that had conducted the polling, who had not delivered either treatment, sent a text message to every leader in the sample, inviting leaders to send a voice or text message requesting budgetary support from district officials for development projects in their village. The text message read:

"Hello, this is [redacted] from [redacted]. We are collecting comments from village leaders about their goals for this year's Tanzanian budget. We will share these messages with the District to show what actions leaders are taking to help their communities. If you would like, please send a message or audio recording about your top priorities for development in your village this year. Send a (1) audio recording, (2) WhatsApp message, or (3) call this phone number: [redacted]."

Enumerators sent one reminder several days later, and I recorded replies for two weeks. I code two outcomes. *Any message* takes the value 1 if the leader sent any message in reply and 0 otherwise. *Top-priority message* takes the value 1 if the leader's message referenced their constituents' top-ranked priority and 0 otherwise. Constituents' top priority was also the issue journalists raised in the motivation treatment.

Estimation

I used ordinary least squares to estimate the effect of the information and motivation treatments. Let Y_i denote the outcome for subject i , let $T1_i$ denote this subject's assigned information treatment status (1 if information treatment, 0 if information control), and $T2_i$ denote the subject's assigned motivation treatment status (1 if motivation treatment, 0 if motivation control). The primary specification for Hypotheses 1 and 2 is the preregistered additive model

$$Y_i = \beta_0 + \beta T1_i + \gamma T2_i + u_i,$$

in which β and γ estimate each treatment's *average* effect over assignment to the other treatment. To test Hypothesis 3, I estimate the full factorial model, which adds the interaction term $\alpha (T1_i \times T2_i)$. The sign of α distinguishes the three accounts in [Figure 1](#): $\alpha = 0$ implies the treatment effects are additive, $\alpha > 0$ that the treatments are complements, and $\alpha < 0$ that they are substitutes.

I report specifications with LASSO-selected covariates in the Appendix ([Table A2](#) and [Table A1](#)), together with estimates that adjust for office type and gender. Both specifications are weighted by inverse probability of motivation assignment (village-level assignment probabilities vary mechanically with the number of sampled leaders, see [Table A3](#) in the Appendix), with standard errors clustered by village. In keeping with the pre-analysis plan, I generate p -values using randomization inference, with one-tailed tests for directional hypotheses.

Descriptive Results

I begin by describing how leaders' knowledge, priorities, and behavior compare with their constituents' priorities.

Table 2: **Baseline Gaps in Leader Knowledge, Alignment, and Responsiveness**

			Leader Type		
	Chance	All	VC	VCM	VEO
Knowledge (pre-treatment; N = 169)					
Leader correctly guesses top village priority (%)	20%	38%	43%	29%	46%
Average villager ranking of leader guess (1–5)	3.00	3.82	3.89	3.62	4.00
Priority Alignment (baseline; N = 157)					
Leader’s top priority matches villagers’ (%)	20%	36%	35%	37%	33%
Correlation between leader and villager rankings (–1 to 1)	0.00	0.37	0.47	0.32	0.32
Budget Allocation (information control; N = 167)					
Share of budget to villagers’ top priority (%)	20%	20%	22%	21%	18%
Share of budget to villagers’ second priority (%)	20%	21%	21%	21%	22%
Correlation between leader budget and villager rankings (–1 to 1)	0.00	0.16	0.19	0.16	0.11
Lobbying (pure control; N = 75)					
Leader lobbies for development assistance (%)	–	7%	14%	6%	0%
Leader lobbies for villager top priority (%)	–	5%	10%	3%	0%

Note: VC = village chairperson (elected), VCM = village councilmember (elected), VEO = village executive officer (appointed). *Chance*: the value expected if the leader guessed between the five core priorities (20%, 3.00, 20%, 0.00). The alignment measures compare each leader’s own ranking to their constituents’ ranking. *Knowledge*: whether the leader guessed constituents’ top priority, and the villager priority ranking (1–5, higher values indicate higher priority) of the issue the leader guessed (available for 168 of the 169 guesses). *Priority alignment*: whether the leader’s own top priority matches their constituents’, and the correlation between each leader’s ranking of the five issues and their constituents’ ranking. *Budget allocation*: leaders were asked how they would divide a hypothetical 10,000,000 TZS among the five issues. Rows report the share given to constituents’ first- and second-ranked priorities and the within-leader correlation between the five amounts and constituents’ ranking, among leaders who did not receive the information treatment. Chance is the value expected if leaders’ allocations were unrelated to their constituents’ ranking: each issue receives 20% of the budget on average, and the expected correlation is 0.00. *Lobbying*: weighted by inverse probability of motivation assignment, like the experimental estimates. Whether the leader responded to an opportunity to lobby their district council for development assistance, and whether the response referenced constituents’ top priority.

I first consider whether village leaders *know* which issue their constituents care most about. Before delivering the information treatment, enumerators asked the 169 leaders in the information treatment group whom they reached to guess which issue their constituents considered most important.⁵ Like their constituents, village leaders focused overwhelmingly on development issues: water (35%), health-care (30%), education (20%), roads (11%), and electricity (4%).

Table 2 shows that 38% of village leaders correctly guessed their constituents’ top priority. A leader

⁵Enumerators did not ask village leaders in the information control group to predict the priorities of their constituents in order to avoid priming control group leaders to think about constituent interests.

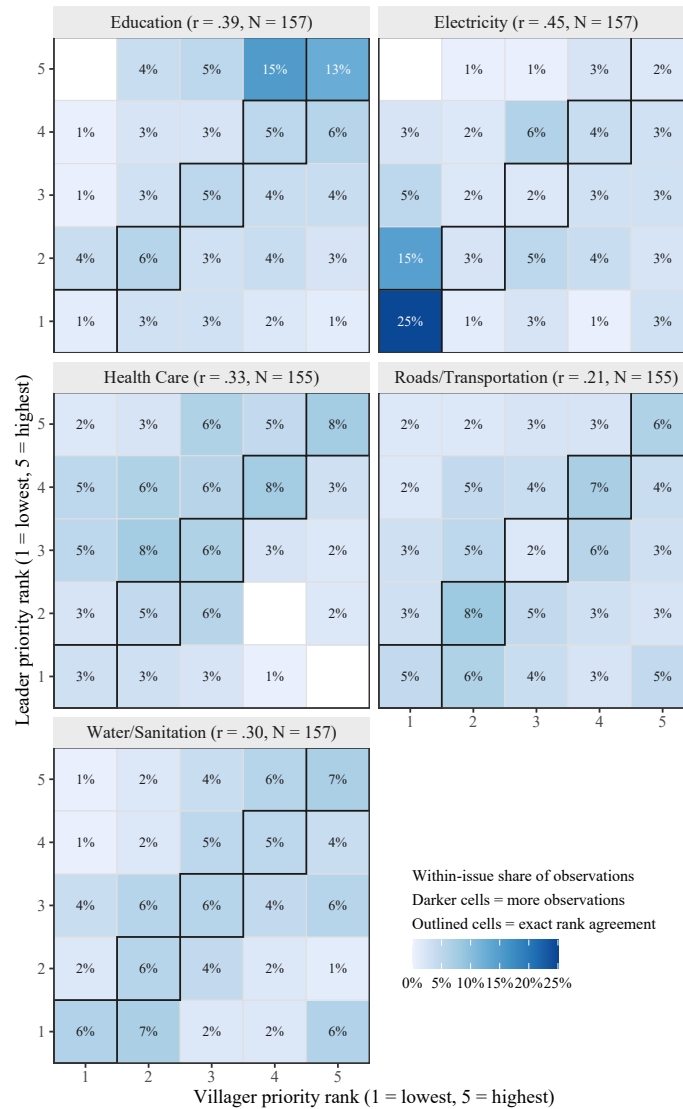
guessing at random among the five main issues would have been correct 20% of the time. On average, the issue leaders guessed ranked 3.82 out of 5 among their constituents' priorities, against 3.00 under random guessing. Village chairpersons (43%) and village executive officers (46%) were more likely to guess their constituents' top priority than village councilmembers (29%), although the differences fall short of conventional levels of statistical significance (χ^2 p -value = 0.14).

I next consider whether village leaders *prioritize* the same issues as their constituents. In the months before the information treatment, enumerators visited 83 villages and asked village leaders to sort priority cards from most to least important. Enumerators made clear that leaders should sort according to their priorities as a leader, rather than their priorities as a private citizen or their beliefs about their constituents' priorities. As with citizen priorities, each issue is scored from 5 (highest) to 1 (lowest). I compare each leader's ranking to their constituents' ranking, leader by leader, rather than averaging ranks across the leaders of a village.

Figure 3 shows the alignment between leaders' rankings of the five issues and their constituents'. Alignment is better than chance but far from exact: the average within-leader correlation between leader and villager rankings is 0.37 (Table 2). Agreement is strongest for education (correlation = 0.39) and electricity (correlation = 0.45), where observations cluster along the perfect-agreement diagonal, and weakest for roads (correlation = 0.21). The agreement on electricity is concentrated at the bottom of the scale: leaders and villagers agree that electricity is the *least* important of the five issues.

The correlation between the priorities of leaders and their constituents is stronger for village chairpersons (correlation = 0.47) than for village councilmembers (correlation = 0.32) or village executive officers (correlation = 0.32). Descriptively, local bureaucrats appear as knowledgeable about constituent priorities as elected village chairpersons but less likely to share those priorities, while village councilmembers appear both less knowledgeable and less aligned than chairpersons.

Figure 3: Rank Agreement Between Villager and Leader Priorities



Note: Each cell shows the share of leader-issue observations with the given combination of villager rank (x-axis) and leader rank (y-axis). Outlined cells mark perfect agreement. Ranks are per-leader integer rankings, and villager ranks repeat across leaders from the same village. Strip labels report the issue-specific Pearson correlation of ranks and the number of leaders.

Leaders' hypothetical budgets track their constituents' priorities even less closely. Among the 167 leaders who did not receive the information treatment, the average leader gave their constituents' top priority 20% of the budget and their second priority 21%, almost exactly what would be expected if leaders divided the budget evenly across the five issues. The average correlation between a leader's budget and their constituents' ranking of the five issues is 0.16, less than half the correlation for stated priorities.

Finally, I evaluate whether village leaders take costly actions to respond to their constituents' pri-

orities. One month after the delivery of the information treatment, a representative of the research organization that had conducted the polling contacted each village leader and offered to include a message from the leader in a formal report to district officials. The message was sent shortly before district councils met to develop formal budget requests to submit to parliament. To respond, village leaders needed to take the time to write the message and pay the standard cost of sending a text or audio message (typically 1,000 TZS, about 0.40 USD). Village leaders may also have risked some backlash by speaking out about development failures to district officials and party leaders. Some representative short-form responses appear below:

“Budget priorities in the island village of Muheza district are (1) Construction of 2 classrooms primary school (2) Construction of a village dispensary room. First finish the village office building and then we look forward to building a kindergarten class”

“The main attention of the village of [redacted] is the original primary school for developing and reaching 7 rooms and one teacher’s house and possibly adding 6 toilets for this budget season 2022 2023 we ask for cooperation thank you”

Overall, 7% of village leaders in the pure control condition took up the offer to lobby the district council for budgetary support. Among the 41 leaders across all conditions who sent a message, 68% requested assistance for their constituents’ top-ranked priority. In the pure control condition, village chairpersons were much more likely to lobby the district council (14%) than councilmembers (6%) or village executive officers, none of whom sent a message.

Experimental Results

In the previous section, I showed that village leaders in rural Tanzania often do not know, share, or act on the priorities of their constituents. I now turn to the results of overlapping interventions designed to test two explanations for non-responsiveness. The first *informed* village leaders about polling results on their constituents’ development priorities. The second used the prospect of a local news report to *motivate* leaders to act on those priorities.

Effects of Information on Leader Priorities

I begin by asking whether village leaders changed their priorities and hypothetical budget allocations as a result of hearing about their constituents’ priorities. Both outcome measures were collected later in the same call that delivered the information (see [Leader Measures](#)).

In the control group, 32% of village leaders listed their constituents' top priority first (average ranking = 1.53 on a 0 to 3 scale), and 16% listed their constituents' second-highest priority first (average ranking = 1.14), similar to the baseline alignment reported in the [Descriptive Results](#). As discussed there, control-group leaders allocated only 20.2% and 21.2% of the hypothetical budget to their constituents' first- and second-ranked priorities.

Did leaders update in response to accurate information about their constituents' priorities? [Table 3](#) reports the effect of the information treatment on how highly leaders ranked their constituents' first and second priorities, and on the share of the budget that leaders allocated to those priorities.

Treated leaders ranked their constituents' top priority 0.360 points higher (one-tailed RI p -value = 0.003) and their constituents' *second-highest* priority 0.325 points higher (one-tailed RI p -value = 0.004) than leaders in the control group. [Figure A1](#) in the Appendix decomposes these effects. Information raised the share of leaders who ranked their constituents' top priority as their own top priority from 32% to 43%, and cut the share who left their constituents' second priority out of their top three nearly in half, from 41% to 22%.

Turning to the budget allocation outcome, the information treatment raised the share of the budget leaders allocated to their constituents' top priority by 3.7 percentage points over a control mean of 20.2%, an 18% increase (one-tailed RI p -value = 0.016). However, it did not raise the share given to constituents' second priority (−1.7 percentage points; one-tailed RI p -value = 0.83). Exploratory estimates by office type ([Table A6](#) in the Appendix) suggest updating is broadly similar across roles.

Table 3: **Effect of Information Treatment on Leader Priorities**

	Leader Priority Ranking		Leader Assigned Budget Share	
	Citizen 1st priority (1)	Citizen 2nd priority (2)	Citizen 1st priority (3)	Citizen 2nd priority (4)
Information Treatment	0.360	0.325	0.037	-0.017
Standard Error	(0.162)	(0.152)	(0.021)	(0.023)
One-tailed RI <i>p</i> -value	0.003	0.004	0.016	0.830
Hypothesis	+	+	+	+
Control mean	1.527	1.138	0.202	0.212
Control SD	1.241	1.124	0.170	0.186
Possible range	0–3	0–3	0–1	0–1
Village fixed effects	Yes	Yes	Yes	Yes
Leaders	340	340	340	340

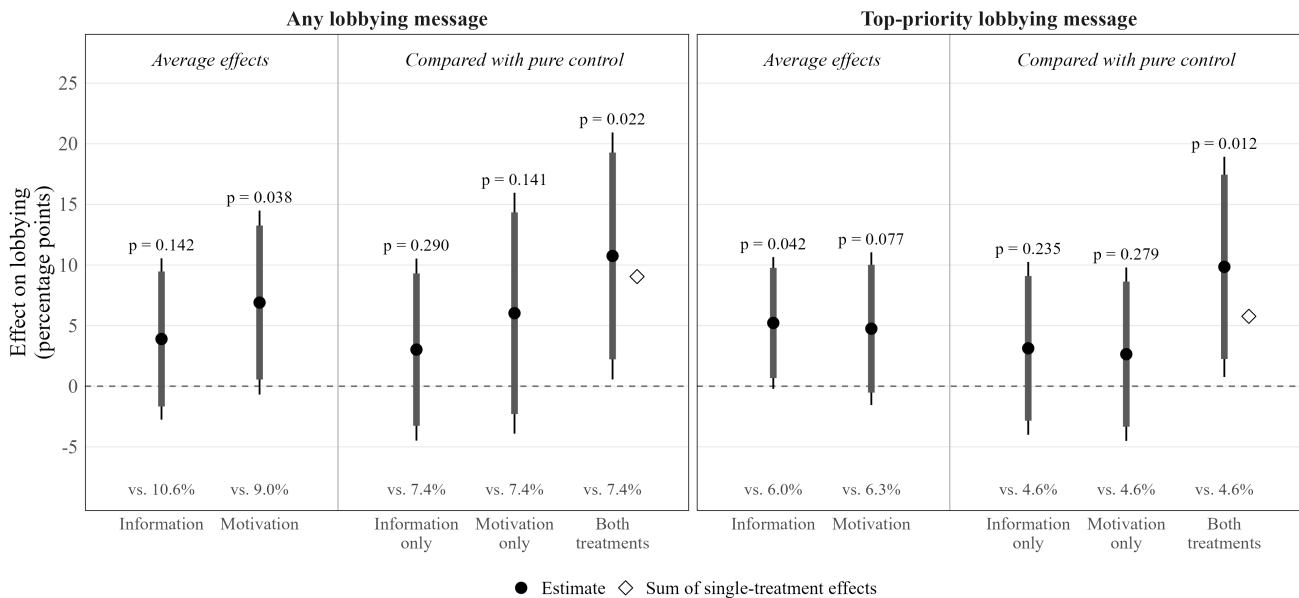
Note: Village fixed effects (the information experiment’s randomization blocks) are partialled from the outcome and the information assignment by Frisch–Waugh–Lovell residualization, with no other covariates and with standard errors clustered by village in parentheses. *p*-values are one-tailed randomization-inference *p*-values in the preregistered (+) direction, computed over 2,000 re-draws of the information assignment. [Table A1](#) in the Appendix adds the LASSO-selected covariates or office-type and gender indicators. The 1st-priority measure is coded from leaders’ answers to: “Now, think about your own views of what the most important problems are in your community. What are the three most important goals for your community in the coming year?...What is the MOST important issue?...What is the SECOND most important issue?...What is the THIRD most important issue?” It takes the value 3 if the villagers’ top priority was the leader’s most important issue, 2 if it was the leader’s second, 1 if the leader’s third, and 0 otherwise. The 2nd-priority measure applies the same coding to the villagers’ second-ranked priority. The budget shares (columns 4 and 5) come from: “Now, imagine that you had 10,000,000 TZS to spend on development projects in your village this year. I would like to know how much you would spend on each of the following items: health, education, water, roads, and electricity,” with any money left over going to leader salaries and office support. Each is the share of the 10,000,000 TZS given to the villagers’ first- or second-ranked priority.

I did not measure the effect of the motivation treatment on leaders’ self-reported priorities for theoretical and practical reasons. Theoretically, the motivation mechanism runs through leaders’ beliefs about the consequences of public attention rather than their beliefs about what issues are most important for the village. Practically, one of the design’s strengths is that village leaders were not aware of the connection between the motivation treatment and the collection of outcome data. Following the motivation treatment with a survey on the same topic would have compromised this feature of the design.

Effects on Lobbying the District Council

I now turn to the outcome of primary interest, village leader lobbying behavior. One month after delivering the information treatment and two weeks after delivering the motivation treatment, a representative of the research organization that had conducted the polling sent a message to village leaders asking them to call, text, or send an audio-recording about the development needs of their village. Their messages were included in a formal report delivered to the village leaders' district council about district-level development priorities. I collected two measures of village leader behavior: whether leaders replied with a text or audio message advocating for development assistance, and whether leaders asked for help with the pre-specified top development priority in their village.

Figure 4: Effects of Information and Motivation on Lobbying the District Council



Note: Points are effects in percentage points. *Average effects* are the estimates in Panel A of Table 4: each treatment compared with leaders who did not receive it, pooling over assignment to the other treatment. *Compared with pure control* shows each treated group's difference from the pure-control group: "Information only" and "Motivation only" are the single-treatment effects in Table 5, and "Both treatments" is Panel B of Table 4. All estimates are weighted by inverse probability of motivation assignment. Thick bars are 90 percent and thin bars 95 percent confidence intervals, from standard errors clustered by village. Labels above each estimate are one-tailed randomization-inference p -values, and labels below give the lobbying rate of the comparison group. All rates are weighted like the estimates. The hollow diamond is the sum of the two single-treatment effects, the value expected for leaders who received both treatments if the effects were additive, so its distance from the "Both treatments" estimate is the interaction in Table 5.

Figure 4 summarizes the lobbying results. Table 4 reports the estimates for the preregistered hypotheses. As the descriptive results above show (Table 2), few village leaders in the pure control condition sent a message of any kind (7.4%) or a message advocating for support for their constituents' top

priority (4.6%).

Table 4: Average Effects of Information and Motivation on Village Leader Lobbying

	Any message (1)	Top-priority message (2)
<i>A. Average effects (all leaders)</i>		
Information Treatment	0.039	0.052
Standard Error	(0.034)	(0.027)
One-tailed RI <i>p</i> -value	0.142	0.042
Hypothesis	+	+
Motivation Treatment	0.069	0.047
Standard Error	(0.038)	(0.032)
One-tailed RI <i>p</i> -value	0.038	0.077
Hypothesis	+	+
Leaders	340	340
Villages	109	109
<i>B. Both treatments vs. pure control</i>		
Both treatments	0.107	0.098
Standard Error	(0.051)	(0.046)
One-tailed RI <i>p</i> -value	0.022	0.012
Hypothesis	+	+
Leaders	166	166
Villages	103	103
Pure-control mean	0.074	0.046
Possible range	0–1	0–1

Note: Panel A reports each treatment’s average effect over assignment to the other, from a regression of the outcome on both treatment indicators for all leaders ($Y = \alpha + \beta \text{ Info} + \gamma \text{ Motiv}$), the estimand described by the pre-analysis plan’s hypotheses. Panel B is a separate regression on the two groups being compared, leaders who received both treatments and the pure-control group ($Y = \alpha + \delta \text{ Both}$). Its estimate equals the difference between those two groups and does not depend on whether the effects add up. All estimates are weighted by inverse probability of motivation assignment, with no covariates or block fixed effects, and standard errors are clustered by village. Randomization-inference *p*-values are one-tailed in the preregistered direction and use 2,000 re-draws of the information assignment and 1,000 stored village-level draws of the motivation assignment, with weights recomputed per draw. Panel B re-randomizes both assignments jointly. The pre-analysis plan states no separate hypothesis for the combined effect, but a positive combined effect follows from Hypotheses 1 to 3 (see the deviations from the pre-analysis plan). “Any message” is whether the leader sent any message to the district council. “Top-priority message” is whether the message referenced their constituents’ top priority. The pure-control mean is weighted like the estimates. [Table A2](#) in the Appendix adds the LASSO-selected covariates specified in the pre-analysis plan, or office-type and gender indicators.

How did the information treatment influence the responsiveness of local leaders? Leaders who received information about their constituents’ priorities were 5.2 percentage points more likely to send a message about their constituents’ top priority issue (one-tailed RI *p*-value = 0.042), nearly doubling

the 6.0% rate among leaders who did not receive information, while the estimated effect on sending a message of any kind is smaller (3.9 percentage points, one-tailed RI p -value = 0.142). The effect of the information treatment on lobbying that did *not* concern the top priority is –1.3 percentage points (two-sided RI p -value = 0.53). The estimates are consistent with information directing lobbying effort toward constituents’ top concern rather than raising its volume.

Turning to the motivation treatment (Hypothesis 2), leaders contacted by journalists were on average 6.9 percentage points more likely to send a message lobbying for development assistance (one-tailed RI p -value = 0.038), raising the share who lobbied from 9.0% among leaders whom journalists did not contact to about 16%. Motivated leaders were also 4.7 percentage points more likely to send messages targeting the issue that mattered most to their constituents, although the p -value falls short of the conventional standard of statistical significance (one-tailed RI p -value = 0.077). In sum, the prospect of media coverage appears to increase the effort that village leaders expend to advocate for their constituents’ interests.

At the village level, motivation raised the share of villages from which at least one leader lobbied the district council from 23% to 39% (one-tailed RI p -value = 0.033), and the share from which at least one leader lobbied for constituents’ top priority from 17% to 30% (one-tailed RI p -value = 0.048).

Combined Effects and Interaction

Did the two treatments reinforce each other? [Figure 4](#) and Panel B of [Table 4](#) show that leaders who received both treatments were 10.7 percentage points more likely than pure-control leaders to send any lobbying message and 9.8 percentage points more likely to send one about their constituents’ top priority, against a base rate of 4.6% (one-tailed RI p -values = 0.022 and 0.012). The estimates in [Table 5](#) are more consistent with additive effects. The interaction coefficients are small for both outcomes and imprecisely estimated (0.017 and 0.041; two-sided RI p -values = 0.804 and 0.502).

Table 5: **Interaction of Information and Motivation**, Factorial Model

	Any message (1)	Top-priority message (2)
Information, without motivation	0.030	0.031
Standard Error	(0.038)	(0.036)
One-tailed RI <i>p</i> -value	0.290	0.235
Hypothesis	+	+
Motivation, without information	0.060	0.026
Standard Error	(0.050)	(0.036)
One-tailed RI <i>p</i> -value	0.141	0.279
Hypothesis	+	+
Information $\times$ Motivation	0.017	0.041
Standard Error	(0.067)	(0.055)
Two-sided RI <i>p</i> -value	0.804	0.502
Hypothesis	+	+
Pure-control mean	0.074	0.046
Possible range	0–1	0–1
Leaders	340	340
Villages	109	109

Note: Estimates from the factorial model $Y = \alpha + \beta_1 \text{Info} + \beta_2 \text{Motiv} + \beta_3 \text{Info} \times \text{Motiv}$, weighted by inverse probability of motivation assignment, with no covariates or block fixed effects and standard errors clustered by village. “Information, without motivation” is β_1 , the effect of information among leaders who did not receive the motivation treatment, and “Motivation, without information” is β_2 . Each uses about half the sample, so these effects are less precise than the average effects in Table 4, which pool over both arms of the other treatment. Their randomization-inference *p*-values re-randomize only that treatment, within the other treatment’s control arm, and are one-tailed in the pre-registered direction. The interaction is β_3 , the difference between the information effect among motivated and non-motivated leaders. Its randomization-inference *p*-value tests the null of additive effects: outcomes are imputed under additivity, using the estimated average effects of the two treatments, and both assignments are re-randomized jointly. A cluster-robust analytic test gives similar values (0.799 and 0.457). The test is two-sided: the pre-analysis plan predicted a positive interaction, but substitution is as theoretically relevant as complementarity (see the deviations from the pre-analysis plan). The combined effect of both treatments is reported in Panel B of Table 4. The pure-control mean is weighted like the estimates. Outcomes are defined as in Table 4.

Heterogeneous Treatment Effects

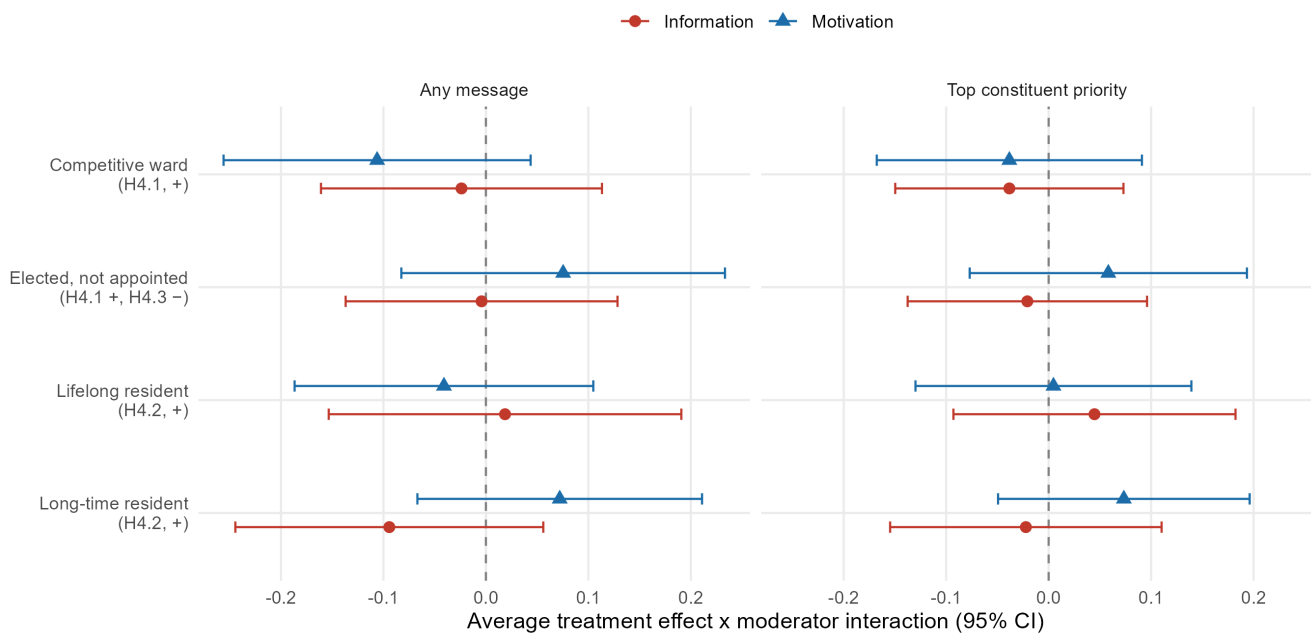
Finally, I tested whether treatment effects varied by features of the village and/or leader to whom it was delivered. Figure 5 shows the interaction of each treatment’s average effect with four potential moderators.

First, I preregistered that the treatments would be most effective in wards with historically close elections, consistent with an electoral accountability mechanism (Hypothesis 4.1). I also tested whether

either treatment was more effective on leaders holding elected office (village chairperson or village councilmembers), consistent with an electoral accountability mechanism, or appointed bureaucrats, consistent with a professional sanctioning mechanism. Finally, I tested whether treatment effects were stronger among leaders who have lived in the village all their lives, or for most of them, consistent with a social accountability mechanism (Hypothesis 4.2).

None of the hypotheses finds substantial support. None of the 16 interaction coefficients in [Figure 5](#) is significant at the 5 percent level, and the point estimates do not consistently support any mechanism.

Figure 5: **Treatment-by-Moderator Interaction Estimates, by Hypothesis**



Note: Each point is the coefficient on the interaction of a treatment’s average effect with a binary moderator, from the additive model $Y = (\text{Info} + \text{Motiv}) \times M$ without covariates, weighted by inverse probability of motivation assignment. Whiskers are village-clustered 95 percent confidence intervals. Labels give the hypothesis each moderator tests and the predicted sign of the interaction. The elected-versus-appointed row tests Hypothesis 4.1 (positive) against Hypothesis 4.3 (negative). Moderators are defined in [Table A9](#) in the Appendix, which reports the same estimates. These are interaction coefficients, not treatment effects.

Conclusion

Local leaders in Tanzania face both informational and motivational barriers to responsiveness. Fewer than two in five leaders knew their constituents’ top development priority, and only 17% of villages had at least one leader lobby the district for their constituents’ top priority when given the chance. Interventions to address informational and motivational barriers improve responsiveness. Polling data changed what leaders prioritized: they ranked their constituents’ top priority higher, gave it 18% more

of a hypothetical budget, and were 5.2 percentage points more likely to lobby for it. The prospect of a journalist's investigation changed whether leaders acted at all, raising lobbying from 9 to 16 percent and the share of villages with at least one lobbying leader by 16 percentage points. Leaders who received both treatments were 9.8 percentage points more likely to lobby for their constituents' top priority, about the sum of the two separate effects.

I expect these findings to apply where local leaders sit between citizens and a higher level of government that controls the budget, as Tanzanian village leaders depend on district councils for most development spending. Three features of this study are likely to matter for whether the results travel. Leaders had reason to believe the polling data, the journalists' threat of coverage was credible because the journalists followed up, and lobbying cost leaders little because their messages were delivered for them. Where any of these is missing, I would expect smaller effects. For governments and funders working to improve local representation in such settings, citizen polling and accountability journalism both provide promising opportunities.

The paper offers a template for design-based research on local government responsiveness built on collaboration with professional journalists and civil society organizations ([Stiglitz et al. 2024](#)). These collaborations raise ethical questions, especially in dominant-party regimes, where reports can damage leaders' standing in their communities and journalists can face pressure and threats from public officials. I discuss how this study handled each of these in the [Ethics Appendix](#). Nonetheless, working directly with journalists and civil society organizations to separate and test the components of their work represents a promising strategy for identifying and responding to the drivers of local government unresponsiveness.

Why leaders respond to data on their constituents' priorities and to the prospect of public attention remains an open question. Treatment effects are not detectably larger in wards with competitive elections, among elected leaders, or among leaders with stronger ties to their village. One suggestive exploratory finding ([Table A7](#) in the Appendix) is that the motivation treatment raised the share of chairpersons who sent a message about their constituents' top priority by 14 percentage points, compared with 0.8 percentage points for village councilmembers and 0.5 percentage points for village executive officers. The information and motivation treatments were also complements among chair-

persons, while the interaction was negative but imprecise for other leaders. Although these results were not preregistered and rest on small sample sizes, they point towards future work focused on understanding the roles and motivations of different types of village leaders.

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Supplemental Materials for *Uninformed or Unmotivated? A Field Experiment on Local Leader Responsiveness in Tanzania*

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Priorities and Budget Allocations

Village Priority Rankings

Each village's ranking of the five issues orders the issues by their average score among the 40 citizens surveyed. In 14 of the 109 villages, two issues had exactly the same average score. The original ranking code gave one of the tied issues a rank and left the other unranked. For the descriptive comparisons in this paper, every village receives a full ranking: ties are broken at random, with the other tied issue taking the next rank, except where the tie involves the village's top two issues. There the order matches what leaders in the information treatment were told.

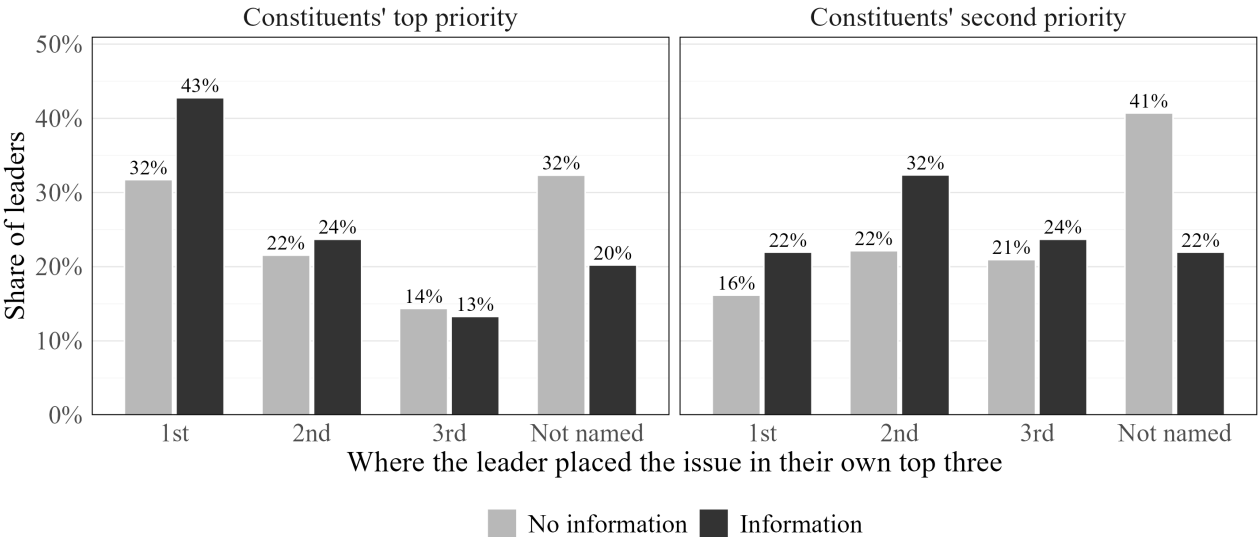
The tie involved the top two issues in 8 villages (24 leaders). In 6 of them, two issues tied for second, and the information treatment named one of the two as the second priority. In the other 2 villages (9 leaders), two issues tied for first. The information treatment named one of them as the top priority and, as the second priority, named the issue ranked third rather than the other issue tied for first. I coded the experimental outcomes against the priorities that leaders were told, so treatment and measurement agree in every village. Excluding these 8 villages leaves the estimates essentially unchanged: the information effect on leaders' ranking of their constituents' top priority is 0.318 (against 0.360 in the full sample) and of their second priority 0.329 (against 0.325), the effect on the budget share for the top priority is 0.034 (against 0.037), and the lobbying effects of both treatments change by less than 0.012.

The information script told leaders that "most people" in the village considered the top issue most important. In Kiswahili, the language of delivery, the phrase was *watu wengi*, "many people." The top issue was the one with the highest average ranking, not necessarily the most common first choice. It was the most common first choice, or tied for it, in 89 of the 109 villages. A median of 36% of the citizens surveyed in a village ranked it first, and a majority did so in 17 villages.

Priority Rankings by Position

Figure A1 shows where leaders placed their constituents' top two priorities in their own list of the three most important issues. Table 3 scores these positions 3 (first), 2 (second), 1 (third), and 0 (not named).

Figure A1: Where Leaders Placed Their Constituents' Top Two Priorities

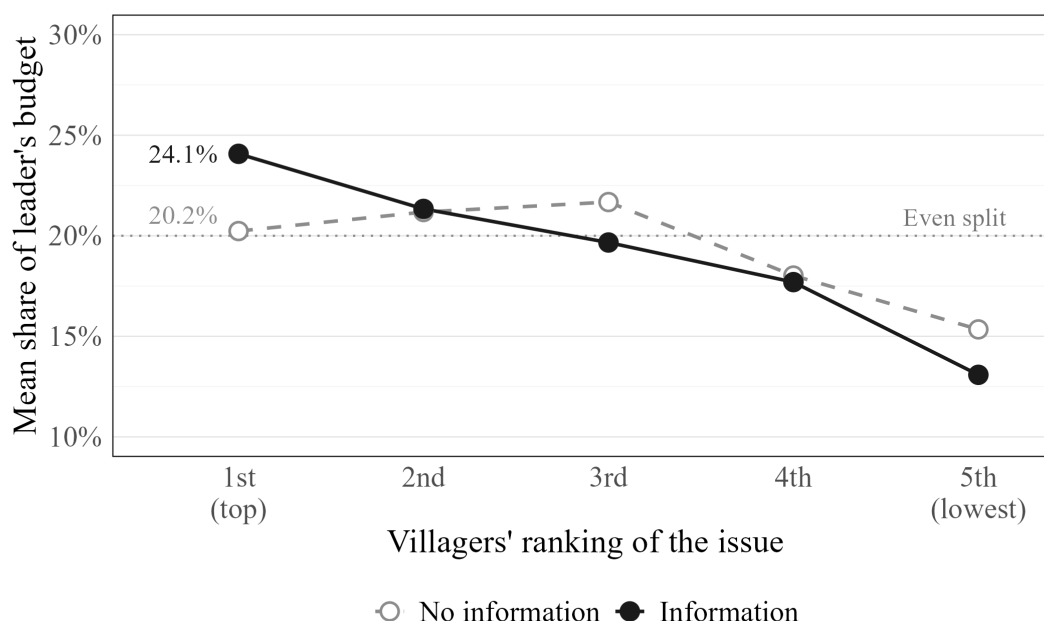


Note: Share of leaders who placed constituents' top priority (left) and second priority (right) first, second, or third in their own list of the three most important issues in their village, or did not name it, by information treatment. Shares are raw and unweighted (no information, $N = 167$; information, $N = 173$). Treatment effects with village fixed effects and randomization inference are in Table 3.

Budget Allocation

Figure A2 shows how leaders divided the hypothetical 10,000,000 TZS budget, by where villagers ranked each issue. Leaders who did not receive information gave villagers' top three priorities about the same share, close to an even split, and gave somewhat less to the two issues villagers ranked lowest. Leaders who received information gave more to villagers' top priority (24.1 against 20.2 percent) and less to their lowest priority (13.1 against 15.3 percent). The share given to villagers' second priority is unchanged. Table 3 reports the treatment effects on the first- and second-priority shares.

Figure A2: **Share of Hypothetical Budget Allocated to Each Issue, by Villagers' Ranking of the Issue**



Note: Each point is the mean share of the 10,000,000 TZS that leaders allocated to an issue, among leader–issue pairs with the given villager ranking (1 = villagers' top priority). Means are raw and unweighted. Every rank includes all 340 leaders; tied village rankings are resolved as described in the section on ties in village priority rankings. The dotted line marks an even split across the five issues.

Robustness of the Main Estimates

Covariate Adjustment

The pre-analysis plan specifies LASSO-selected covariates in all regressions. The main-text estimates use no covariates (see the deviations from the pre-analysis plan below), and this section reports the LASSO-adjusted estimates. The original selection was implemented in Stata's `lasso linear`, but the selection is implemented in R (`cv.glmnet`) over the preregistered covariate pool, with the penalty that minimizes the 10-fold cross-validation error averaged over 10 runs. [Table A1](#) reports the estimates for all four outcomes in [Table 3](#), the two priority rankings and the two budget shares, with village fixed effects alone (the main-text specification), with the LASSO-selected covariates added, and with office-type and gender indicators added instead. The estimates are similar across all three specifications.

Table A1: **Effect of Information Treatment on Leader Priorities**
Robustness to Covariate Adjustment

	Leader Priority Ranking		Leader Assigned Budget Share	
	Citizen 1st priority (1)	Citizen 2nd priority (2)	Citizen 1st priority (3)	Citizen 2nd priority (4)
<i>A. Village fixed effects only (main-text specification)</i>				
Information Treatment	0.360	0.325	0.037	-0.017
Standard Error	(0.162)	(0.152)	(0.021)	(0.023)
One-tailed RI <i>p</i> -value	0.003	0.004	0.016	0.830
<i>B. Adding LASSO-selected covariates (pre-analysis plan)</i>				
Information Treatment	0.388	0.350	0.037	-0.018
Standard Error	(0.157)	(0.149)	(0.021)	(0.023)
One-tailed RI <i>p</i> -value	0.002	0.002	0.016	0.849
Covariates selected	6	15	3	6
<i>C. Adding office type and gender</i>				
Information Treatment	0.339	0.347	0.037	-0.017
Standard Error	(0.159)	(0.152)	(0.021)	(0.023)
One-tailed RI <i>p</i> -value	0.004	0.003	0.017	0.838
Possible range	0–3	0–3	0–1	0–1
Leaders	340	340	340	340

Note: Every panel partials village fixed effects from the outcome and the information assignment by Frisch–Waugh–Lovell residualization, with standard errors clustered by village in parentheses. Panel A is the main-text specification ([Table 3](#)). Panel B includes each outcome's LASSO-selected covariates, chosen from the preregistered pool. Panel C partials adjusts for office-type and gender indicators instead. *p*-values are one-tailed randomization-inference *p*-values in the preregistered (+) direction over the same 2,000 re-draws of the information assignment used in [Table 3](#). The selected covariate set is held fixed across draws rather than re-selected. Outcomes are defined as in [Table 3](#).

[Table A2](#) re-estimates the average-effects model of [Table 4](#) under three specifications: without any covariates (the design-only, main-text specification), with the LASSO-selected covariates (the pre-analysis plan's adjustment), and with office-type and gender indicators instead. The estimates are similar across specifications. No covariates were selected for the top-priority outcome, so its first two columns coincide.

Table A2: **Effect of Motivation and Information Treatments**, Robustness to Covariate Adjustment

	Any message			Top-priority message		
	(1)	(2)	(3)	(4)	(5)	(6)
Information Treatment	0.039	0.023	0.030	0.052	0.052	0.047
Standard Error	(0.034)	(0.033)	(0.033)	(0.027)	(0.027)	(0.027)
One-tailed RI p -value	0.142	0.283	0.217	0.042	0.042	0.063
Motivation Treatment	0.069	0.063	0.070	0.047	0.047	0.049
Standard Error	(0.038)	(0.041)	(0.038)	(0.032)	(0.032)	(0.032)
One-tailed RI p -value	0.038	0.054	0.037	0.077	0.077	0.066
LASSO-selected covariates	No	Yes	No	No	Yes	No
Office type, gender	No	No	Yes	No	No	Yes
Possible range	0–1	0–1	0–1	0–1	0–1	0–1
Leaders	340	340	340	340	340	340

Note: Average effects from the additive model, weighted by inverse probability of motivation assignment, with standard errors clustered by village in parentheses and one-tailed randomization-inference p -values in the hypothesized direction. Columns 1 and 4 include no covariates (the main-text specification, reproducing [Table 4](#)). Columns 2 and 5 add each outcome’s LASSO-selected covariates (the pre-analysis plan’s adjustment). Columns 3 and 6 add office-type and gender indicators instead.

Motivation Assignment Weights

The village-level randomization process for the motivation treatment took the following steps: (1) Assign a random number to every village leader, (2) Assign the village the highest random number from among leaders of the village, (3) Assign the motivation treatment to the villages with the highest random numbers. This gave villages with more leaders in the sample a higher probability of being assigned to treatment. The number of leaders sampled in each village was a function of the amount of time that team members had available after finishing the community survey rather than any important features of the village government. Because the assignment process is known, I account for the differing probabilities of assignment with inverse probability weights, which depend only on the number of leaders sampled in the village. [Table A3](#) shows the average effects of [Table 4](#) without the weights, and without the weights but with fixed effects for the number of leaders sampled. The estimates are similar. Without weights, the motivation effect on any message is 7.1 percentage points (one-tailed $p = 0.044$), against 6.9 with them. With village-size fixed effects it is 5.6 points ($p = 0.067$). The information effect on top-priority messages is 4.5 points without weights ($p = 0.056$ to 0.057), against 5.2.

Table A3: **Effect of Motivation and Information Treatments**, Robustness to Inverse-Probability Weighting

	Any message			Top-priority message		
	(1)	(2)	(3)	(4)	(5)	(6)
Information Treatment	0.039	0.027	0.026	0.052	0.045	0.045
Standard Error	(0.034)	(0.033)	(0.034)	(0.027)	(0.025)	(0.026)
One-tailed RI p -value	0.142	0.233	0.242	0.042	0.056	0.057
Hypothesis	+	+	+	+	+	+
Motivation Treatment	0.069	0.071	0.056	0.047	0.048	0.043
Standard Error	(0.038)	(0.037)	(0.040)	(0.032)	(0.030)	(0.034)
One-tailed RI p -value	0.038	0.044	0.067	0.077	0.077	0.093
Hypothesis	+	+	+	+	+	+
Inverse-probability weights	Yes	No	No	Yes	No	No
Village-size fixed effects	No	No	Yes	No	No	Yes
Possible range	0–1	0–1	0–1	0–1	0–1	0–1
Leaders	340	340	340	340	340	340

Note: Average effects from the additive model of [Table 4](#), without covariates, with standard errors clustered by village. Columns 1 and 4 weight by inverse probability of motivation assignment (the main-text specification, reproducing [Table 4](#)). Columns 2 and 5 are unweighted. Columns 3 and 6 are unweighted and add fixed effects for the number of leaders sampled in the village (one to five), the only input to the assignment probability. p -values are one-tailed randomization-inference p -values in the preregistered (+) direction, using the same stored draws as [Table 4](#).

Additional Lobbying Results

Second-Priority Lobbying

The pre-analysis plan lists lobbying for constituents' *second*-ranked priority as a behavioral outcome alongside the top-priority measure, and predicts a positive information effect on it. The plan's motivation hypotheses are only stated for any lobbying and for top-priority lobbying (because the journalists' call only focused on the top-ranked issue). Both estimates are positive and neither is statistically significant.

Table A4: **Effect of Motivation and Information Treatments on Second-Priority Lobbying**

	Second-priority message
Information Treatment	0.012
Standard Error	(0.029)
One-tailed RI <i>p</i> -value	0.373
Motivation Treatment	0.035
Standard Error	(0.031)
Two-sided RI <i>p</i> -value	0.268
Control mean	0.053
Possible range	0–1
Covariates	None
Leaders	340

Note: Average effects from the additive model without covariates, weighted by inverse probability of motivation assignment, with standard errors clustered by village in parentheses and randomization-inference *p*-values. The information *p*-value is one-tailed in the preregistered (+) direction; the pre-analysis plan states no directional motivation hypothesis for this outcome, so the motivation *p*-value is two-tailed. The outcome is whether the leader sent a message referencing their constituents' second-ranked priority.

Village-Level Effects of Motivation

The motivation treatment was assigned by village, and several leaders in the same village could lobby. Table A5 therefore estimates its effect with one observation per village: whether any leader in the village sent a lobbying message, whether any sent one about constituents' top priority, and the number of each kind of message. Panel A weights villages by the inverse probability of their motivation assignment, which depends on the number of leaders sampled. Panel B adds fixed effects for the number of leaders sampled and the share of the village's leaders assigned to the information treatment.

Table A5: Effect of the Motivation Treatment on Village-Level Lobbying

	Any leader in the village lobbied		Messages from the village	
	Any message (1)	Top priority (2)	Any message (3)	Top priority (4)
<i>A. Weighted by village assignment probability</i>				
Motivation Treatment	0.164	0.129	0.240	0.162
Standard Error	(0.091)	(0.085)	(0.122)	(0.101)
One-tailed RI <i>p</i> -value	0.033	0.048	0.028	0.052
Hypothesis	+	+	+	+
<i>B. Adding village-size fixed effects and the share of leaders informed</i>				
Motivation Treatment	0.124	0.108	0.177	0.131
Standard Error	(0.095)	(0.088)	(0.120)	(0.102)
One-tailed RI <i>p</i> -value	0.086	0.113	0.073	0.107
Hypothesis	+	+	+	+
Control mean	0.227	0.169	0.269	0.187
Possible range	0–1	0–1	0–5	0–5
Villages	109	109	109	109

Note: One observation per village. Columns 1 and 2 indicate whether at least one sampled leader in the village sent any lobbying message or a message about constituents' top priority. Columns 3 and 4 count those messages. Panel A weights villages by the inverse probability of their motivation assignment. Panel B adds fixed effects for the number of leaders sampled in the village and the share of its leaders assigned to the information treatment. HC2 standard errors in parentheses. *p*-values are one-tailed randomization-inference *p*-values in the preregistered (+) direction over the 1,000 stored village-level draws of the motivation assignment, with weights recomputed per draw. The control mean is weighted.

Heterogeneity

Heterogeneity by Office Type

None of the office-type breakdowns was preregistered, and all should be read as exploratory. [Table A6](#) reports the information effect on leader priorities by office type. [Table A7](#) repeats [Table 4](#) separately for each office type: each treatment's average effect from the additive model, with the main-text specification and inference. [Table A8](#) adds, for each office type, the factorial interaction and the combined-treatments-versus-control contrast.

Table A6: **Effect of Information Treatment on Leader Priorities, by Office Type**
(Exploratory)

	Leader Priority Ranking		Leader Assigned Budget Share	
	Citizen 1st priority (1)	Citizen 2nd priority (2)	Citizen 1st priority (3)	Citizen 2nd priority (4)
<i>All leaders (village fixed effects; one-tailed RI p-values)</i>				
Information Treatment	0.360	0.325	0.037	-0.017
Standard Error	(0.162)	(0.152)	(0.021)	(0.023)
One-tailed <i>p</i> -value	0.003	0.004	0.016	0.830
Control mean	1.527	1.138	0.202	0.212
Leaders	340	340	340	340
<i>Village chairpersons</i>				
Information Treatment	0.306	0.261	0.017	-0.021
Standard Error	(0.230)	(0.211)	(0.037)	(0.035)
One-tailed <i>p</i> -value	0.093	0.110	0.328	0.725
Control mean	1.592	1.061	0.217	0.206
Leaders	108	108	108	108
<i>Village councilmembers</i>				
Information Treatment	0.271	0.668	0.031	0.015
Standard Error	(0.212)	(0.192)	(0.025)	(0.031)
One-tailed <i>p</i> -value	0.101	<0.001	0.107	0.314
Control mean	1.593	1.074	0.206	0.211
Leaders	147	147	147	147
<i>Village executive officers</i>				
Information Treatment	0.619	0.163	0.080	-0.013
Standard Error	(0.273)	(0.246)	(0.043)	(0.044)
One-tailed <i>p</i> -value	0.013	0.254	0.033	0.619
Control mean	1.297	1.378	0.175	0.222
Leaders	85	85	85	85
Equal effects across office types (<i>p</i> -value)	0.538	0.219	0.516	0.762
Possible range	0–3	0–3	0–1	0–1

Note: VC = village chairperson (elected); VCM = village councilmember (elected); VEO = village executive officer (appointed). The first panel (all leaders) reproduces the estimates of [Table 3](#), with village fixed effects partialled out and one-tailed randomization-inference *p*-values. The office-type panels are exploratory and use no covariates or village fixed effects, because most villages contribute only one leader of each office type (analytic one-tailed *p*-values). Joint tests of equal information effects across the three office types, from a model interacting the treatment with office type, do not reject for any outcome (bottom row). Standard errors clustered by village in parentheses. Outcomes are defined as in [Table 3](#).

Table A7: **Average Effects of Motivation and Information Treatments, by Office Type** (Exploratory)

	Any message			Top-priority message		
	VC (1)	VCM (2)	VEO (3)	VC (4)	VCM (5)	VEO (6)
Information Treatment	0.075	0.004	0.043	0.043	0.044	0.069
Standard Error	(0.073)	(0.049)	(0.059)	(0.066)	(0.042)	(0.047)
One-tailed RI p -value	0.168	0.472	0.284	0.278	0.160	0.117
Hypothesis	+	+	+	+	+	+
Motivation Treatment	0.181	0.023	0.014	0.140	0.008	0.005
Standard Error	(0.074)	(0.061)	(0.062)	(0.067)	(0.046)	(0.053)
One-tailed RI p -value	0.007	0.353	0.449	0.010	0.439	0.461
Hypothesis	+	+	+	+	+	+
Pure-control mean	0.138	0.061	0.000	0.095	0.033	0.000
Possible range	0–1	0–1	0–1	0–1	0–1	0–1
Leaders	108	147	85	108	147	85
Villages	108	101	85	108	101	85

Note: VC = village chairperson (elected); VCM = village councilmember (elected); VEO = village executive officer (appointed). Each column estimates the additive model of [Table 4](#) among leaders of one office type: no covariates or block fixed effects, weighted by inverse probability of motivation assignment, with standard errors clustered by village. p -values are one-tailed randomization-inference p -values in the preregistered (+) direction, using the same stored draws as [Table 4](#) restricted to the office type. The breakdown was not preregistered and should be read as exploratory.

Table A8: **Effect of Motivation and Information Treatments, by Office Type** (Exploratory)

	VC (1)	VCM (2)	VEO (3)
Any message			
Information Treatment (average)	0.075	0.004	0.043
Standard Error	(0.073)	(0.049)	(0.059)
Two-sided <i>p</i> -value	0.307	0.929	0.471
Motivation Treatment (average)	0.181	0.023	0.014
Standard Error	(0.074)	(0.061)	(0.062)
Two-sided <i>p</i> -value	0.016	0.705	0.829
Information $\times$ Motivation	0.365	-0.164	-0.167
Standard Error	(0.141)	(0.098)	(0.115)
Two-sided RI <i>p</i> -value	0.007	0.100	0.219
Both treatments vs. control	0.236	0.022	0.081
Standard Error	(0.121)	(0.061)	(0.057)
Two-sided <i>p</i> -value	0.055	0.722	0.161
Control mean	0.125	0.054	0.000
Possible range	0–1	0–1	0–1
Top-priority message			
Information Treatment (average)	0.043	0.044	0.069
Standard Error	(0.066)	(0.042)	(0.047)
Two-sided <i>p</i> -value	0.519	0.297	0.151
Motivation Treatment (average)	0.140	0.008	0.005
Standard Error	(0.067)	(0.046)	(0.053)
Two-sided <i>p</i> -value	0.039	0.859	0.922
Information $\times$ Motivation	0.213	-0.061	-0.048
Standard Error	(0.131)	(0.085)	(0.094)
Two-sided RI <i>p</i> -value	0.091	0.457	0.638
Both treatments vs. control	0.171	0.050	0.081
Standard Error	(0.109)	(0.055)	(0.057)
Two-sided <i>p</i> -value	0.121	0.365	0.161
Control mean	0.083	0.027	0.000
Possible range	0–1	0–1	0–1
Leaders	108	147	85

Note: VC = village chairperson (elected); VCM = village councilmember (elected); VEO = village executive officer (appointed). This breakdown was not preregistered and should be read as exploratory; cells contain between 14 and 44 leaders. Within each office type, “average” rows report each treatment’s average effect from the additive model, and the interaction and combined rows come from the full factorial, all without covariates, weighted by inverse probability of motivation assignment with standard errors clustered by village and two-sided analytic *p*-values, except for the interaction rows. Interaction *p*-values are two-sided randomization-inference *p*-values under the null of additive effects within each office type, as in Table 5. A randomization-inference test that the information $\times$ motivation interaction is equal across the three office types, under a null of office-specific additive effects plus one common interaction, gives *p*-value = 0.001 for any message and 0.122 for top-priority messages (a joint Wald test from a single model with treatment-by-treatment-by-office terms gives 0.006 and 0.21).

Competitiveness and Other Moderators

Table A9 reports treatment-by-moderator interaction estimates for the dichotomized moderators, including the exploratory social-embeddedness measures discussed in the main text.

Table A9: **Heterogeneous Treatment Effects**, Interaction Estimates

	Any message		Top priority	
	Mot. $\times$ M	Info. $\times$ M	Mot. $\times$ M	Info. $\times$ M
<i>Electoral (H4.1)</i>				
Competitive ward	-0.106 (0.076)	-0.024 (0.070)	-0.038 (0.066)	-0.038 (0.057)
<i>Electoral vs. professional (H4.1, H4.3)</i>				
Elected leader	0.075 (0.080)	-0.004 (0.067)	0.058 (0.069)	-0.021 (0.059)
<i>Social embeddedness (H4.2)</i>				
Lifelong resident	-0.041 (0.074)	0.019 (0.087)	0.005 (0.068)	0.045 (0.070)
Long-time resident	0.072 (0.071)	-0.094 (0.076)	0.073 (0.062)	-0.022 (0.067)
Possible range	0–1	0–1	0–1	0–1

Note: Each cell reports the coefficient on the interaction of a treatment’s *average* effect with a (dichotomized) moderator, from the additive model $Y = (\text{Info} + \text{Motiv}) \times M$ with no information-by-motivation term and no covariates, weighted by inverse probability of motivation assignment and clustered by village. “Mot. $\times$ M” and “Info. $\times$ M” denote the motivation-by-moderator and information-by-moderator interactions. Standard errors in parentheses. Moderators are binary. *Competitive ward*: the runner-up’s vote share in the leader’s ward councilor election is at or above the sample median of 0.30 (171 of the 331 leaders with ward election results). *Elected leader*: village chairperson or councilmember, versus appointed village executive officer (255 of 340). *Lifelong resident*: reported years living in the village is at least the leader’s age (130 of 324). This measures life-long residence rather than birthplace. *Long-time resident*: years in the village divided by age is at or above the sample median of 0.82 (162 of 324). Medians are taken over the estimation sample; leaders missing a moderator are dropped from that row’s regressions.

Table A10 probes the robustness of the null competitiveness result, interacting each treatment’s average effect with the winner’s and runner-up’s ward vote shares (each continuous and median split, signed so higher values indicate a more competitive ward) and with the runner-up median split within elected leaders only.

Table A10: **Interaction Effects**, by Electoral Competitiveness

Competitiveness measure	Info. $\times$ M	Mot. $\times$ M	N
Winner share (reversed, continuous std.)	0.032 (0.029)	-0.045 (0.037)	340
Winner share $\leq$ median	0.053 (0.068)	-0.106 (0.075)	340
Runner-up share (continuous std.)	0.016 (0.032)	-0.053 (0.039)	331
Runner-up share $\geq$ median	-0.024 (0.070)	-0.106 (0.076)	331
Runner-up share $\geq$ median, elected only	0.035 (0.079)	-0.197 (0.093)	249
Possible range	0–1	0–1	

Note: Each cell reports the coefficient on the interaction of a treatment’s average effect with the competitiveness measure, from the additive model $Y = (\text{Info} + \text{Motiv}) \times M$ without covariates, weighted by inverse probability of motivation assignment and clustered by village. The outcome is whether the leader sent any message. Standard errors in parentheses.

Deviations from Pre-Analysis Plan

[Table A11](#) summarizes every known deviation between the pre-analysis plan and the final analysis.

Table A11: **Deviations from the Pre-Analysis Plan**

PAP provision	Final implementation	Reason
118 villages (Sec. II)	109 villages	Typing mistake; Sec. III of the PAP correctly specifies 109.
Baseline leader card sorts in 95 villages (Sec. II)	83 villages (157 leaders)	Error in the PAP. Leaders completed the card sort in 83 villages.
Provisional treatment allocation (Sec. III)	Realized assignment counts	The PAP was written before the final random assignment.
342-entry assignment roster	341 valid assignments	One councilmember was listed from a village outside the study sample (no citizen surveys, never surveyed).
Information-treatment variable	Delivered assignment: in-form draw for interviews on the first two fieldwork days, preloaded roster assignment thereafter	Prior versions used the in-form draw for everyone, and an intermediate version used the roster for everyone; see Note A.
Randomization inference over stored draws	Randomization inference restored, approximated by Monte Carlo: 2,000 draws of the delivered information assignment (recovered roster algorithm, fair coin flips for in-form-draw interviews), 1,000 stored village draws with per-draw weights (motivation), joint draws for the interaction	The deployed assignment algorithm was recovered and verified; see Note B.
Block (ward) indicator in all regressions (Sec. IX)	Village fixed effects for the information-only alignment analyses; no block fixed effects where the motivation treatment enters	Neither experiment was blocked by ward. The information treatment was randomized within villages, so the village is its block; the motivation treatment was randomized across villages without blocking.
Outcome-specific selected covariates (Sec. IX)	Main-text estimates use no covariates; the LASSO adjustment, implemented as planned (penalty minimizing 10-fold cross-validation error averaged over 10 runs, selected set held fixed across re-randomizations), is reported in Table A2 and Table A1	The design-only estimates rely only on the randomization and are simpler to interpret. The largest change from the LASSO adjustment is to the motivation effect on any message: 6.3 rather than 6.9 percentage points (one-tailed RI p -value 0.054 rather than 0.039).

Table A11, continued

PAP provision	Final implementation	Reason
–	Gender name-coded for 16 respondents (one missing)	The endline gender question was skipped for 17 respondents with no baseline contact.
One-week response window for lobbying messages	Responses accepted for two weeks	Field operations kept the window open longer.
Second-priority lobbying outcome	Reported in Table A4	Retained as an appendix outcome; the plan predicts a positive information effect on it but states no directional motivation hypothesis, so the motivation contrast is tested two-tailed.
Social-attitudes treatment and outcome	Omitted	The social-attitudes treatment was programmed incorrectly and gave leaders the wrong figures on villagers' attitudes toward domestic violence, so I report no estimates. Its assignment was independent of the priorities treatment (chi-squared $p = 0.70$), so the priorities estimates average over it.
Budget-allocation measure	Reported as planned in Table 3	Earlier versions omitted it as unreliable. Respondents found the exercise confusing, in part because leaders in these roles do not allocate budgets of this kind, enumerators were not adequately trained to administer it, and 84 of 340 leaders allocated less than the full amount. But allocations track leaders' own stated rankings (correlation 0.42), so the measure is informative and I report it.
LASSO-selected covariates (Stata)	Re-executed in R (cv.glmnet); Table A1	Archived Stata selection not exactly re-executable and pre-dates the assignment correction; conclusions unchanged.
One-tailed test of the interaction	Two-sided	Opted to text for substitution as well as complementarity. One-tailed p -values are preserved in the replication files.
No hypothesis stated for the combined effect of both treatments versus pure control	One-tailed test in the positive direction	A positive combined effect follows from Hypotheses 1 to 3 (each treatment raises responsiveness, and the interaction is predicted to be non-negative).
"Village screenings" language	No screenings in this study	Vestigial template language from the umbrella project's registration.

Note A (information-treatment variable). Analyses prior to this paper version used an incomplete information-treatment variable. The endline survey ran on two sets of form versions. The forms used on the first two days of fieldwork (22 and 25 July 2022) contained only an in-form random draw. The forms used from 26 July onward added a preloaded assignment (`treat_info`, pulled from the case roster attached to the survey software). The original cleaning code built the treatment variable from random assignment that occurred within the survey form. The randomization-inference draws follow the assignment mechanism of the analysed submission. The replication archive documents the correction and preserves the superseded variables.

Ethics

Equipoise

According to the “policy equipoise” standard, treatment arms in a randomized evaluation are ethically justified if there is reasonable uncertainty that treated subjects will be made better off than other subjects (MacKay 2018). The current project satisfies the “policy equipoise” standard because there are no experimental studies about the effects of delivering information or motivation through journalists to rural village leaders. More broadly, evidence about the effect of accountability interventions in developing countries remains limited (Grossman and Hanlon 2014). It was also unclear whether treated leaders were better or worse off.

Scarcity

Research may also be ethically justified on the grounds of resource scarcity. The information treatment was not constrained by resource scarcity. Delivering information about villager preferences to leaders is cheap. The research team therefore provided information to village leaders in the control group after endline data collection.

The motivation treatment was meaningfully constrained by resource scarcity, because journalists needed to follow up on their promises in the villages where they said they would conduct reports. Random assignment of the motivation treatment may therefore be justified by resource scarcity.

Researcher Role with Respect to Implementation

This intervention would not have occurred in the researcher’s absence. The level of researcher involvement in designing, funding, and implementing the intervention generates a unique ethical responsibility on the part of the researcher to ensure that participants are treated with dignity.

Potential Harms to Participants or Non-participants

Journalists and Media Houses. Journalists and media houses may have been subject to harassment and intimidation by government officials if the topics of their proposed reports were threatening. To respond to this concern, I worked closely with local journalists to craft topics and messages that would not generate significant risks. We also received ethical approval for the intervention from both district and regional offices in Tanzania.

Community Members. It is difficult to imagine how community members may have been harmed for participating (or not participating) in the research project. All information gathered from community members was aggregated to the community level and I never shared individualized responses with leaders. It is possible that the treatment caused some village leaders to lobby more effectively for government resources, leading to fewer government resources being directed towards non-participating villages. However, these effects are highly uncertain, and it is equally plausible that the treatment generated positive spillover rather than substitution effects.

Village Leaders. It is somewhat easier to imagine how village leaders may have been harmed by participating (or not participating) in the research project. Being subject to negative news reports may harm leaders' standing in their communities and party. The news reports were factual stories, and leaders were told about the reporting in advance, so they had the opportunity to act before any story aired. Reporting on problems that remained unresolved could still have hurt the reputation of leaders. Leaders' lobbying messages were also forwarded to district officials. The leaders in the study had good working relationships with their districts, district officials took the messages into account, and no leader faced sanctions for sending one.

Potential Harm from Data Collection or Research Protocols

Informed Consent for Randomization. Because of the nature of the intervention, community members were not given informed consent over the randomization process. However, community members were informed that the aggregate data from surveys would be used to help with village development projects in their area. Village leaders were informed that they were part of a research study that involved randomization and information about their constituents' needs. Their consent did not cover the journalism treatment, which was delivered by working journalists as part of their reporting. The journalists followed up in every village where they had promised to.

Informed Consent for Interview Participation. All subjects were provided with both written and oral explanations of the consent process and given the option to refuse participation. All data was collected on password-protected tablets using SurveyCTO software. Upon completion of the survey, survey data was synced to an encrypted SurveyCTO server and downloaded to an encrypted laptop held by the research team.

Community Consent. Survey enumerators secured government consent at the national, regional, and district levels before conducting any surveys. This consent is both legally mandatory and ensures the safety of enumerators and research participants.

Potential Harms to Field Staff. Because of the geographic coverage of the study, field staff were required to travel long distances, sometimes on public transportation. All staff were provided with medical insurance in case of accidents, and with per diems sufficient to cover overnight stays in safe lodgings so that they did not need to travel at night. All staff were paid salaries in compliance with Tanzanian employment law, and their salaries were reported to the government.

Financial or Reputational Conflicts of Interest

The research team reports no financial or reputational conflicts of interest in this study.

Intellectual Freedom

The research team reports no contractual limitations to reporting the results of the study. The Tanzanian government requires that any research be submitted to their research compliance office before publication, but there are no restrictions on what findings can be published.

Feedback to Participants or Communities

Because this study involves collaboration with radio stations, there is a built-in opportunity for sharing research findings with participating communities. Radio stations have agreed to broadcast reports about the findings, which will enable treatment, control, and non-sampled communities to hear about the results.

Pre-Analysis Plan

Motivation and Information: Mechanisms of Media Influence on Local Government Responsiveness

I. Purpose

The purpose of this project is to evaluate whether and why local political leaders respond to the preferences of their constituents. We are particularly interested in investigating two potential explanations of responsiveness: leaders receiving *information* about their constituents' preferences and leaders being *motivated* to respond to constituents' needs by the prospect of media reports.

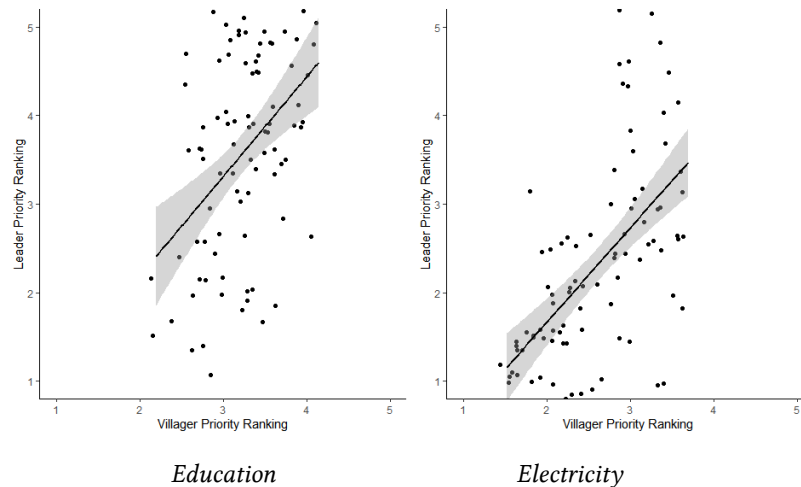
This pre-analysis plan is submitted after the implementation of the treatment but before observing outcome measures.

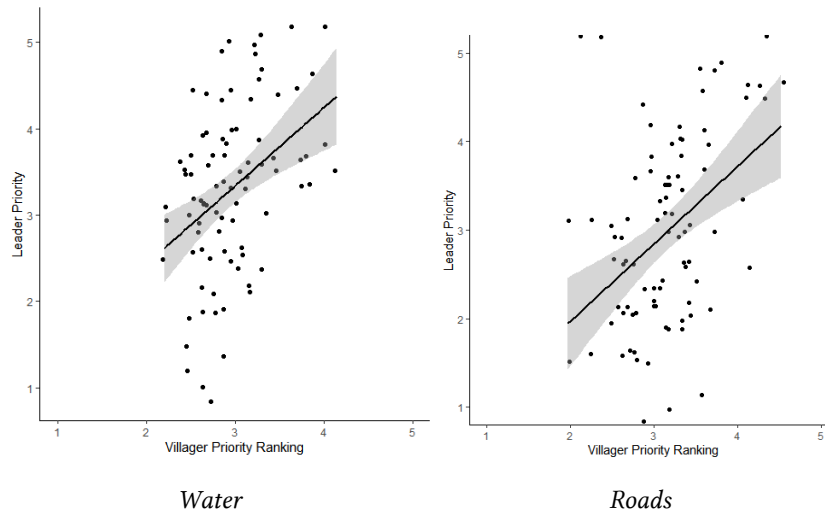
II. Context

The study originates from data collected from 40 randomly selected citizens (20 men and 20 women) in 118 villages and data from the village chairperson and 1-2 randomly selected village council members in 95 of the 118 villages.

In every citizen and leader survey, enumerators gave the respondent stack of cards with different village problems pictured and asked them to place them in order from most important problem in their community to least important problem in their community. In leader surveys, leaders were also given a hypothetical budget allocation of 10,00,000 TZS (4,000 USD) and asked how they would distribute the budget among the problems in their village.

The correlation between average citizen and average leader priorities are shown in the Figures below. Each dot represents one village. The y-axis is the average priority ranking of a particular issue by leaders in that village (5 = highest priority, 1 = lowest priority). The x-axis shows the average ranking of the same issue by 40 randomly selected citizens in the village. Leaders prioritized the same problems as their citizens on average, but the correlation was not perfect, suggesting substantial room for interventions to tighten the link between public and politician priorities.

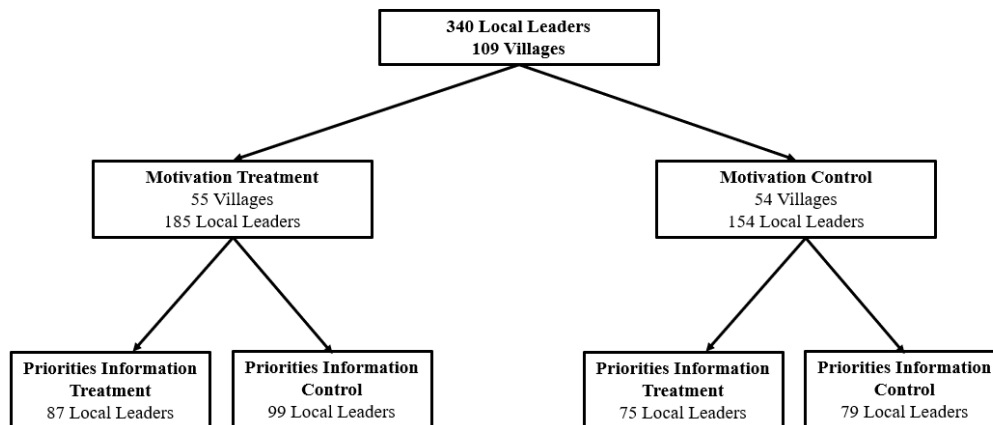




III. Overview

I conduct two overlapping field experiments. In the first experiment, I randomly assigned $\frac{1}{2}$ of all village leaders to receive accurate polling information about the development priorities of citizens in their village. I randomly assign the information treatment at the individual leader level.

In an overlapping experiment, we randomly assign a local news reporter to contact leaders in $\frac{1}{2}$ of villages to conduct reports on actions that leaders are taking to respond to a development issue. I randomly assign the motivation treatment at the village level.



IV. Information Treatment

The focus of the “information” experiment is on whether leaders update their social attitudes, priorities, and behavioral intentions in response to learning about the social attitudes and policy priorities of their constituents. To deliver information about constituent opinion, we draw on public opinion data collected from 4,400 citizens in 110 villages between 2020 and 2022. Enumerators collected information about a wide range of social views and policy priorities (see [redacted] [redacted]). The sampling and data collection process are described in Appendix XX.

The core of the information treatment was to share specific findings from these surveys with village leaders. Enumerators from [redacted] [redacted] the organization that conducted the original surveys, called leaders in both the treatment and control groups, and told the leaders that [redacted] [redacted] wanted to follow up on the survey they recently conducted in their community. [redacted] [redacted] enumerators then shared some basic findings from the survey, including the demographic profile of the respondents, the percentage of respondents in the survey that owned a radio, and the percentage of respondents that were interested in politics.

Political Attitudes

We randomly assigned 1/2 of village leaders to a political priorities information treatment condition. In the political priorities information treatment condition [redacted] [redacted] enumerators told leaders about villagers' political priorities. In the original survey, enumerators asked citizens to rank order priorities such as water, roads, education, healthcare, and electricity, from most to least important.¹ We calculated the average rank of each issue, then ranked the issues from most to least important on average. Enumerators told leaders in the political priorities information treatment:

How I would like to talk about development issues in the village. We asked villagers in this village to say what their goals were for the village. This means that they listed what they thought were the biggest problems that they hoped local government could resolve in the coming year. First, what do you think was the issue citizens thought was MOST important?

After recording the leaders' guess, enumerators continued:

Thank you. When we surveyed people in your village, we found that most people thought that X issue was the most important issue in the village. The second most important issue to them was Y issue. That means that the issue citizens in YOUR village MOST want local leaders to help solve is X problem first and Y problem second.

Social Attitudes

We assigned 1/3rd of village leaders to a social attitudes information treatment. [redacted] [redacted] enumerators told leaders about the views that citizens in their village hold towards women's political participation. In the original survey, [redacted] [redacted] enumerators asked citizens "do you agree or disagree with the following statement: there should be an equal number of male and female political leaders in Tanzania." In the social attitudes information treatment condition, enumerators told leaders:

"In our survey, we asked about people's views on issues in the community. The first question that we asked was about women participating in politics. We asked if they agreed with the statement: There should be an equal number of women political leaders and male political leaders?" Out of 40 people in your village, how many do you think said that women are just as good political leaders as men?"

After recording the leaders' guess, enumerators continued:

"Thank you. When we surveyed 40 people in your village, we found that X out of 40, or Y percent, said that there should be an equal number of female political leaders as men. That means [most / about half / less than half] of the village think women are just as good leaders as men."

V. Motivation Treatment

We randomly assigned leaders in 1/2 of villages to receive "motivation" treatment. The focus of the "motivation" treatment is on whether leaders increase their efforts to promote community development in response to the prospect of news coverage about development issues in their village. To create the expectation of negative news coverage, I collaborated with four independent journalists to contact leaders targeted villages and inform the leaders that the journalist was planning to produce a news report about the leaders' village,

with a focus on a specific service delivery issue (water, roads, electricity, health, or education). Journalists focused on the top ranked issue among citizens of the targeted community but did not inform local leaders that they had any unique knowledge about the service delivery problem in their area. Instead, journalists told leaders that they were planning to broadcast a report about the state of water/education/health/roads in a number of villages in Tanga Region,

The journalists indicated they would broadcast the story on a popular regional radio station *Tanga Kunani*, in the coming month, and that they would follow up with all villages in November to find out what actions local leaders had taken to improve water/education/health/roads outcomes in their community. The implication was that leaders would be celebrated if they took positive actions but somewhat embarrassed if they took no action and the problem remained unresolved. The script that I developed in collaboration with the team of journalists reads as follows:

“Hello, my name is (insert name), I am a journalist from Tanga Kunani FM. This year we are producing a large series on how communities are responding to (insert issue) in this District. We are talking to leaders in many villages to understand how (insert issue) is affecting their village and what actions they are taking this year to solve the issue.

First, can you tell me about how problems with (insert issue) are affecting citizens in your village? Can you tell me some actions that you and other leaders in your village are planning to take to respond to (insert issue) this year?

We are going to follow up at the end of this year to put village stories on our radio program and website, so we hope that we can return and find out some of the actions that you have taken to respond to (insert issue) over the next few months. We will be very happy to include you as a positive story if actions are taken. If no actions are taken, we will help spread the word about the problems that are still happening in the village. Thank you very much for your time, and here is my number if you have any questions or concerns.”

VI. Outcome Measures

Social Attitudes

We measured the effect of learning about constituent's views towards women's participation in politics on two measures of leaders' own views about women in politics. To measure general attitudes, we asked leaders “Do you agree or disagree with the following statement: In general, women are equally good political leaders as men.” If the leader agreed, we asked “Do you strongly agree, or just agree?” If leaders disagreed, we asked “Do you strongly disagree or just disagree?” We also sought to measure leaders' behavioral intentions.

We asked “Would you encourage your daughter or niece to run for political office, or would you say there are better things for them to do with their time?” If the leader indicated that they would encourage their daughter or niece to run for public office, we asked “would you strongly encourage her, or just encourage her?” If the leader said they would not encourage their daughter or niece, we asked “would you strongly discourage her, or just discourage her?”

For all questions, answered were scored as 3 for strongly endorsing the progressive position, 2 for endorsing the progressive position, 1 for rejecting the progressive position, and 0 for strongly rejecting the progressive position.

Political Priorities

Turning to outcomes of the political priorities information treatment, we gave leaders the opportunity to share their own list of the three most important issues in the village. We said: “Now, think about your own views of what the most important problems are in your community. What are the three most important goals for your

community in the coming year? What is the MOST important issue? What is the SECOND most important issue? What is the THIRD most important issue?” For both the top villager priority and the second villager priority, we coded the response as 3 if the villager priority was ranked first by the leader, 2 if the villager priority was ranked second, 1 if it was ranked third, and 0 otherwise.

Finally, we measured whether learning about villager priorities affected a hypothetical budget allocation by the leader. We told leaders “Now, imagine that you had 10,000,000 TZS to spend on development projects in your village this year. I would like to know how much you would spend on each of the following items: health, education, water, roads, and electricity.” We informed leaders that any money left over would be spent on leader salaries and office support. We recorded how much leaders chose to spend on each issue, and created a simple percentage outcome for the top villager priority and the second villager priority.

Behavioral Outcome

Finally, we collected a costly behavioral outcome measure. One month after the information treatment and approximately two weeks after the motivation treatment, a representative for [redacted] sent a message to every leader in the sample. The message asked leaders to share a voice or text message requesting budgetary support from the district for any development projects in the leaders' community. The text message read:

“Hello, this is [redacted] We are collecting comments from village leaders about their goals for this years Tanzania budget. We will share these messages with District to show what actions leaders are taking to help their communities. If you would like, please send a message or audio recording about your top priorities for development in your village this year. Send a (1) audio recording, (2) WhatsApp message, or (3) call phone”

We waited one week for leaders to reply to the message and then coded the responses as follows. First, we coded an “any response” variable which takes the value 1 if the leader sent any meaningful response and 0 otherwise. Second, we coded a “first priority” response which takes the value 1 if the leader sent a message that specifically referenced the top priority among villagers and 0 otherwise. Recall that the top priority among villagers was also the topic that journalists emphasized in the motivation treatment. Third, we coded a “second priority” response which takes the value 1 if the leader sent a message that referenced the second villager priority. The second highest priority among villagers was shared in the information treatment but not mentioned by journalists in the motivation treatment.

VII. Hypotheses

Priorities Information Treatment

- Increase village leader prioritization of their citizens’ first and second-ranked priorities (within-survey)
- Increase village leader messages to District officials about citizens’ top and second-ranked priorities (one-month after survey)
 - Effect will be strongest in electorally competitive villages

Motivation Treatment

- Increase village leader messages to District official advocating for budget support
 - Effect will be strongest in electorally competitive villages
- Increase village leader messages to District official advocating for citizens’ top ranked priority
 - Effect will be strongest in electorally competitive villages

Interaction

- The effect of the motivation treatment will be strongest among village leaders who also receive the priorities treatment.

Social Values Information Treatment

- Increase village leader belief that there should be equal number of women as men in politics
- Increase village leader support for women's participation in politics

VIII. Estimation

Our main analysis will focus on the individuals who attend the village screenings. Unless otherwise specified, all hypotheses are one-tailed tests.

Primary Specification

For all analyses, we will estimate treatment effects using ordinary least squares regression, with fixed effects for block (ward). Additional covariates will be selected using the process outlined in Section 5.6.

Randomization Inference

For all specifications, we will calculate p -values using OLS with robust clustered standard errors (clustered at the level of randomized assignment) and randomization inference. We will calculate cluster-robust standard errors using the *hc3* option in STATA, but will not rely on these standard errors for inference.

Heterogeneous Treatment Effects

We will analyze heterogeneous treatment effects for given covariates by including an interaction with the treatment variable on the right-hand side of the regression.

IX. Covariates

We will include an indicator for block (ward) in all specifications.

We will use lasso regression to select the minimal number of covariates that best predict each outcome and include only these covariates in our estimation. We draw potential covariates from the individual-level covariates from the baseline survey (and a selected few from the follow-up survey that are implausibly affected by treatment). The lasso procedure that we plan to use features a generalized linear model with lasso penalization and is implemented using the lasso package in STATA. The loss function requires selecting a regularization parameter that determines the severity of the penalty for including extra covariates. Since this regularization parameter cannot be optimally chosen in advance, we will select it using 10-fold cross-validation. Specifically, for each outcome, we will choose the regularization parameter that minimizes the 10-fold cross-validation error averaged over 10 runs (since the folds are chosen at random). Only the covariates retained by the lasso will be included in the specification. This implies that for each outcome, a different number of covariates may be included on the right-hand side.

Each covariate will be recoded as missing when the respondent answers "Other (specify)", "Refuse to answer" or "Don't know". Moreover, when the covariate data has item-level missingness, we will input for that respondent the mean of their village.

