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Budgets, SMS Texts, and Votes in Uganda[1]

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1 Political context

This study examined the effects of information about government performance on voters' choices in the February 2016 district elections in Uganda. It was designed to understand how new information promotes accountability when it is disseminated in ways that are difficult for politicians to control. As discussed previously in this volume, although political information has been theorized to enable voters to make choices in line with their interests, the empirical evidence in support of this proposition is decidedly mixed. The mixed evidence is particularly apparent in electoral autocracies, where incumbent politicians can use a variety of tactics to counter the information that voters access, including repression and alternative messaging.[1]

Contemporary Uganda was a fitting research site to deepen our understanding of the effects of political information since it is a low-income electoral authoritarian regime rated as "not free" by Freedom House at the time of our study.[2] Uganda holds elections for president, parliament, and local offices. However, the government of President Yoweri Museveni and his long-ruling party, the National Resistance Movement (NRM), have taken many steps to hamper opposition candidates and civil society. The elections held in 2016 – including the general elections for president and national parliament in February and the local elections that began six days later – were no exception. The repressive steps taken by the government around these contests included changes to electoral and civil society laws, restrictions on media freedom, and voter intimidation. As such, at the time of our study, Uganda fit the definition of electoral authoritarian regime provided by Steven Levitsky and Lucan Way: a country in which "formal democratic institutions exist... [but] incumbents' abuse of the state places them in a significant advantage vis-'a-vis their opponents."[3]

[1] Humphreys and Weinstein (2013); Driscoll and Hidalgo (2014); Ferree et al. (2015).

[2] Freedom House, "Uganda Country Report 2016". Available at https://freedomhouse.org/report/freedom-world/2016/Uganda. See also Tripp (2010).

[3] Levitsky and Way (2010, 5).

The common arm in our study focused on elections for Uganda's 111 district councils, which represent the highest tier of local government in the country and are equivalent to states or provinces elsewhere. Due to a decentralization process that began in 1992, Uganda has several tiers of local government, including districts (known locally as LC Vs), subcounties (LC IIIs), and villages (LC Is).[4] Each district council is led by a chairperson, who is elected by the district's voters and leads the council, and a group of councillors, who are elected by the constituencies of the smaller subcounties that comprise each district. Elections are held for these various councils sequentially over several weeks.

District elections provided a worthwhile case for studying information and voting since they are substantively important for Ugandans. Local governments are responsible for between 15 and 18 percent of the Ugandan national budget, and district-level politicians play a strong role in the management of local public works, health and education.[5] The primary responsibilities of LC V chairs and councillors include writing district legislation and development plans, supervising district staff, overseeing public programs and contracts, and executing budgets. In other words, the issues at stake in district elections matter for Ugandan voters' well-beings. In addition, voter decisions on the local level are likely less tied to the partisan and identity politics that shape national elections in Uganda.[6] As such, district elections may be considered a more-likely case to identify significant effects of new information on Ugandan voters' choices than national elections, such as for parliament or president, which have been explored by some previous studies that did not find positive evidence of information affecting voters' choices.[7] Finally, district elections are sufficiently localized as

[4]Grossman and Lewis (2014).

[5]Although appointed local bureaucrats are also key actors in local public service provision, LC V politicians must approve of bureaucrats' budgets and work plans. See Raffler (2016); Green (2015). A large proportion of subjects in our baseline survey indicated that good budget management was important in deciding how to vote for district officials.

[6]For example, Carlson (2015).

[7]Humphreys and Weinstein (2013).

to make it difficult for politicians to muzzle information sent privately by mobile phones, which facilitated our study.

We cooperated with Twaweza, a Ugandan non-governmental organization (NGO) that seeks to convey politically relevant information to citizens around both local elections and more generally as part of public affairs. The overall goal of Twaweza throughout the region is to improve governance by making more information available to the public and by improving the ability of citizens to engage in public life. By focusing on local as opposed to national elections, it was possible to conduct an information campaign without threatening the ruling NRM party in a way that might be dangerous for our partner (or participants).

Though local elections in Uganda are dominated by NRM candidates, they legally permit and in practice involve multi-party competition. In our sample (described in detail below), more than four-fifths of the elections for district chairs and councillors were contested (i.e., involved candidates from multiple parties).[8] However, NRM candidates win most district elections, prevailing in 77 percent of the elections for chair and doing so with an average margin of victory of 28 percent. But the electoral dominance of NRM should not be mistaken for widespread satisfaction with its local officials. Indeed, citizens tend to level broad criticisms at local governments, expressing especially serious concerns about the effective provision of public services and the prevalence of corruption within district councils.[9] For example, 54 percent of Ugandans reported that they thought their district councils were doing a bad job at maintaining local roads in the 2015 Afrobarometer survey.[10]

Yet there is ample evidence that Ugandan citizens do not have much credible information about politicians' performance at the district level.[11] As such, informational interventions

[8]In addition, the elections involved three candidates for district chair on average.

[9]Stohl, Stohl and Leonardi (2008).

[10]Afrobarometer (2015, questions 67a and 67b).

[11]Natamba et al. (2010).

around the district elections might enable voters to hold politicians accountable at the polls. Several factors contribute to the public's lack of understanding of local government. One factor is the restricted media environment, where opposition speech in radio and newspaper is restricted.[12]

Another factor is that it is difficult for the public to gain specific and accurate information about the performance of district and subcounty councils. When it comes to budget performance, information is available in principle to citizens, but it is not accessible to them. Although the Ugandan Auditor General provides annual information on budget discrepancies involving local governments, this information is presented in highly technical language and lengthy reports that are not easily digested by the public. In this way, the electoral authoritarian government in Uganda is obfuscating local government performance partly via information *overload*, in which voluminous data is released to the public in thousand-page reports containing difficult-to-decipher tables and graphs surrounded by jargon-filled text. Although politicians and the media might help make the information accessible to citizens, the reports are not debated in the national parliament or covered by the media.

In some ways, it is easier for citizens to observe local government performance in the area of public services, since they can monitor the quality of road, health clinics, water facilities, and schools that they encounter in their daily lives. However, understanding how those public services compare to other locations is more difficult for citizens. The lack of information on local governments' relative performance on public services also limits the potential for accountability.

We thus tested whether two kinds of information treatments could change whether voters turned out and who they chose at the polls. First, we sent a series of messages about irregularities in district budgets to voters in LC V elections (common arm). By making information that is otherwise obfuscated in technical volumes accessible to voters and in a

[12] Tripp (2010, 96-101).

way that is almost impossible for local politicians to counter, we hypothesized that we could enable them to hold district politicians accountable for their oversight of district budgets. Second, prior to beginning our experiment, we asked each subject to tell us the kind of public service that was most important for them when voting: roads, primary schools, water access, or health facilities. One reason why previous scorecard experiments around elections may not have had much of an effect on vote choice was that the information presented to voters was not salient. By explicitly allowing voters to opt into the kind of information that they will receive, we sought to use new communication technologies to personalize information and boost its salience. We expected that by providing information about salient public services to voters that we would heighten the potential to affect how voters made choices at the polls.

2 Research design

Our study involved a field experiment in which information about district budget management and local public services were transmitted to citizens via SMS text messages as part of two separate experimental arms. We also varied the density of the treatment with budget information to explore whether the creation of common knowledge about politicians' performance led to different treatment effects. The overall flow of the experiment for the budget information arm is provided in Figure 1.

2.1 Common arm and mode of dissemination

We expected that information about local officials' management of budgets, which directly affects the quality of everyday services that citizens use, would be salient to citizens. That expectation was supported in our baseline survey, where more than 80 percent of subjects indicated that good budget management was important in deciding how to vote for district officials. We compiled information on the management of district budgets from the most

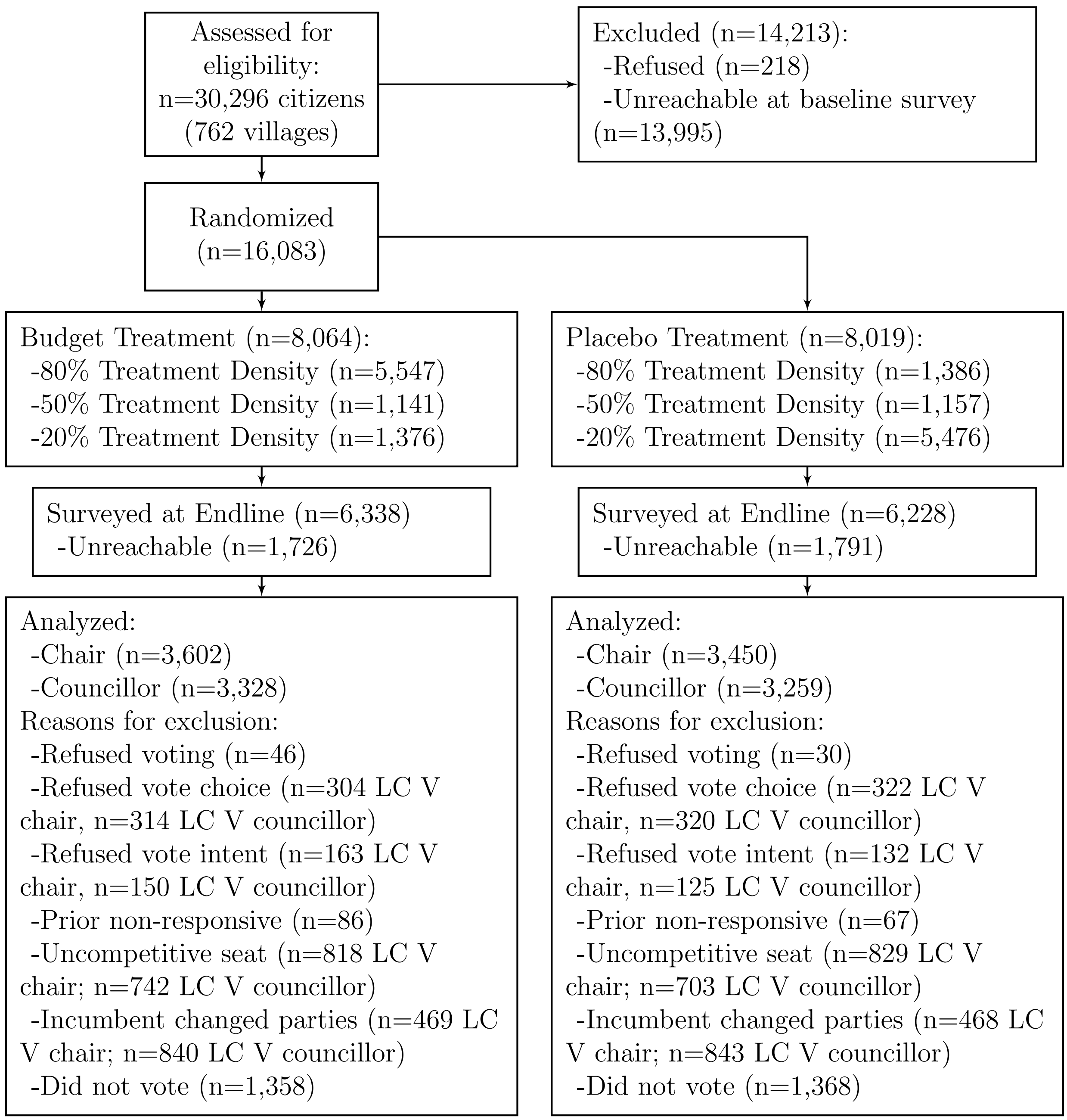

Figure 1: **Consort diagram.** Though we originally sampled 870 villages, we were refused permission to work in 30 villages and rain made 78 villages inaccessible during subject recruitment.

recent annual public reports of audits conducted on local government authorities and released by the Office of the Auditor General.[13] This little-known audit tracks and validates that the district council followed procurement rules, completed projects as specified, and accounted for expenditures. Our treatment sought to make make the detailed audit information compiled by this internationally-respected government office more accessible and meaningful.[14] We did so by simplifying it, adding brief comparative information, and delivering it directly to voters via SMS in the week before district elections. Specifically, we provided information about how the percentage of unaccounted-for funds in a citizen's district compared with unaccounted-for funds elsewhere.[15] We also included several concrete examples of unaccounted-for funds when the district ranked below the median on budget management as part of treatment.

For example, an individual living in a village where the district council performed much worse than expected could have received the following sequence of SMS messages.

- The Auditor General conducts yearly audits to record instances where LC5s could not satisfactorily explain how its money has been spent.

- Unexplained spending is often an indicator of mismanagement, fraud or poor quality services.

- Your LC5 did much worse than most other LC5s in the recent audit.

- In your LC5, the auditor found issues with 120 million UGX from its budget of 19

[13]Office of the Auditor General (2014).

[14]Note that the Office of the Auditor General is organizationally independent, even though its findings often lack follow-up by authorities and are limited in their public impacts in various other ways. See World Bank (2010). In our study, 51 percent of respondents trusted information from the Auditor General, whereas 19 percent did not trust it and 31 percent did not know.

[15]This percentage was obtained by calculating the total sum of "funds not accounted for," "procurement issues," and "payroll anomalies." This amount was divided by each district council's budget to compare financial irregularities across districts. For further examples of treatment and placebo messages, see the Appendix.

billion UGX. This is much worse than in other districts.

- This means that 6.3 out of 1000 UGX in your LC5 budget had issues. In most LC5s 2.2 out of 1000 UGX had issues. Your LC5 did much worse than average.
- One reason your LC5 did much worse than average is that payments of 98 million UGX were made without proper documentation.
- Another reason your LC5 did much worse than average is that a bid for borehole construction included unexplained expenditures.

To isolate the effect of new information on voting behavior from the increased salience that subjects might have attached to an issue when prompted to think about it, we also sent messages to subjects in a placebo group. These individuals received public service messages about good personal financial management without any information about the performance of their politicians at managing budgets.[16] We sent a total of 12 SMS text messages about budgets to treatment group respondents and 5 to placebo group respondents. We sent a total of 207,940 messages across all subjects and treatment arms. Messages were translated into one of Uganda's 11 primary languages, depending upon subjects' preferences. [17]

We cannot confirm that the respondents interpreted the messages as intended. Despite our best efforts to convey information from the Auditor General in a clear and salient way, it is possible that respondents did not interpret the "good" or "bad" news in the treatment messages as we intended, or that some ambiguity about which elected officials were responsible for budget management remained. As such, our estimates below might be viewed in some ways as "intention to treat" analyses. However, we do have a measure of whether the treatment messages were received, as our endline survey asked respondents whether they

[16] Our treatment and placebo subjects were well balanced across pre-treatment covariates. See Appendix.

[17] Messages were sent using a bulk SMS delivery service developed by SMSOne, a communications technology company based in Kampala.

recalled seeing our messages in the alternate arm. This measure provides an indicator of treatment compliance. We refer to respondents who responded that they did receive our messages in the analyses below as "verified recipients."[18]

Our main outcome variable (M1) captured whether the subject voted for the incumbent or a politician from the incumbent's party if the incumbent did not run for re-election.[19] Our secondary outcome variable was turnout (M3), which captured whether a subject reported voting in the district elections. In other words, and unlike the Benin, Mexico, and Uganda I Metaketa projects, we use self-reported data from the endline survey for these measures.[20] As we discussed below, we use official returns only for validation, since we did not treat at a high-enough density to have any reasonable ability to detect treatment effects in official returns. We made this choice for ethical and practical reasons, to which we return below.

2.2 Site selection and sampling

Our study took place in villages in 27 districts that comprised a nationally-representative sample assembled by Twaweza. Specifically, we used the sampling frame associated with Twaweza's Uwezo education initiative. This enabled Twaweza to expand its capacity to disseminate its original information about primary education outcomes to citizens through SMS messages. Figure 2 shows the sampled villages within the district boundaries of Uganda.

[18] 80 percent of respondents remembered receiving our messages, and only 12 percent said it was "difficult" or "very difficult" to distinguish them from other messages. Given that Ugandans receive many other SMS messages around elections, this finding is important since it might have been difficult to distinguish our nonpartisan factual messages from those that have a more political purpose.

[19] This is a variation of the measurement of our outcome variable registered in our pre-analysis plan (PAP). In that PAP, we used the post-treatment outcome to directly rescale the pre-treatment outcome measure.

[20] Adida et al. (this volume); Arias et al. (this volume); Platas and Raffler (this volume).

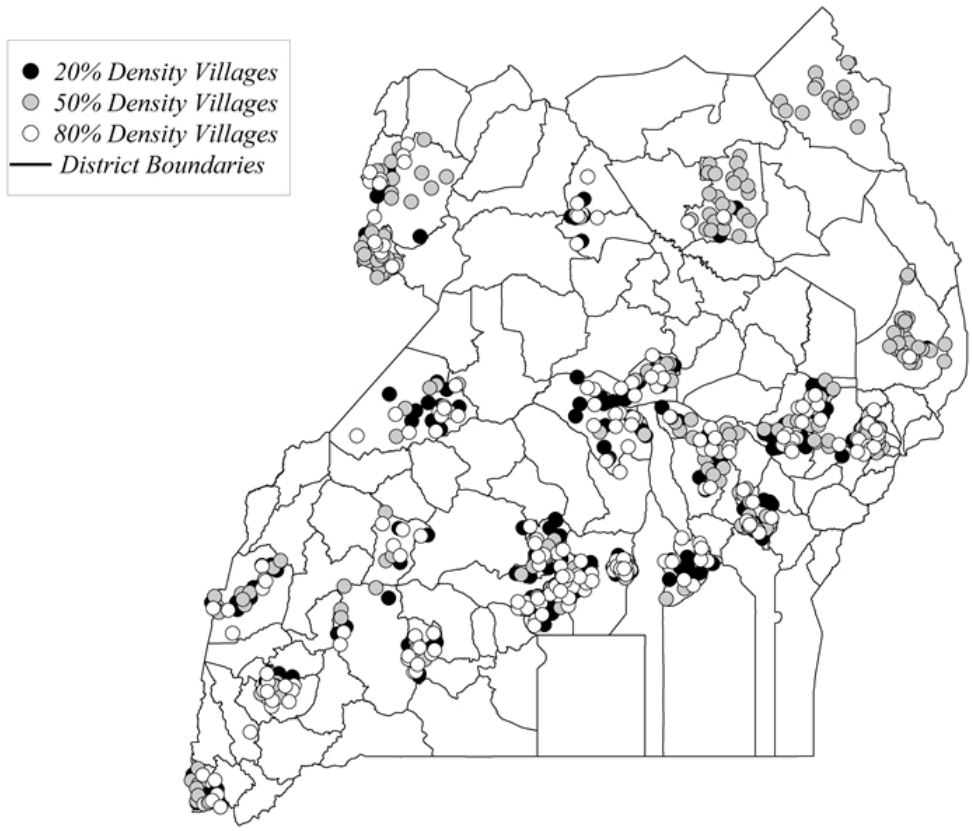

Figure 2: **Map of sampled villages in Uganda**.

Within the sampled villages, local enumerators recruited 31,310 individuals to participate using both collective gatherings and door-to-door visits between November 29 and December 19, 2015. To participate, individuals had to be Ugandan citizens, aged 18 years or older, own a mobile phone, and be willing to sign a consent form. Of those interested volunteers, 16,083 individuals completed the baseline telephone survey between January 11 and 16, 2016.[21] We

[21]The vast majority of the attrition from intake to study recruitment was due to call center staff being unable to successfully contact subjects who provided their contact information in the field, for example

refer to this group as our *experimental sample*. The study completed with a post-election endline survey between February 25 and 29, 2016. To encourage subjects in the experimental sample to participate in the entire study, we provided UGX 1,000 (roughly USD $0.30) of telephone airtime for each completed survey. Our efforts to encourage completion were fairly successful, as 12,581 individuals completed the endline survey successfully.[22]

To compare the experimental sample to the general population, we looked to the 2015 Afrobarometer survey.[23] In terms of age, our sample was representative; similar to the Afrobarometer, our median age was 33. Our subjects also had similar levels of NRM party affiliation (76 percent) to those in the Afrobarometer (78 percent). However, our use of mobile technology skewed the sample in favor of men and better-educated respondents. For example, 67 percent of our respondents were men, which is consistent with the gender imbalance in mobile phone ownership in Africa.[24] Moreover, whereas only 4 percent of our subjects had no formal schooling and 42 percent had completed some or all of secondary school, these figures were 14 percent and 30 percent, respectively, in the Afrobarometer.

We note that the composition of our experimental sample could have important implications for the interpretation of our findings. It is possible that our relatively well-educated subjects were more politically sophisticated than average Ugandans, making it more difficult to change their minds and suggesting that any average treatment effects identified by our study could be lower bounds on the true population effects. At the same time, better-educated subjects might have found it easier to understand and process the new information, implying that treatment effects might not be significant with a more representative national

because the provided number was invalid or because no one answered the phone after three attempts. Approximately 218 individuals were contacted and refused to participate in the survey and were dropped from the effective sample prior to randomization.

[22]Tests outlined in the meta-pre-analysis plan (MetaPAP) found no evidence of differential rates of attrition between the treatment and placebo conditions.

[23]Afrobarometer (2015).

[24]Pew Research Center (2015).

sample. In our analyses below, we tested for heterogeneous effects along the dimensions where our sample likely differed from a representative sample where possible.

2.3 Randomization strategy

Within sampled villages, we used complete randomization at the level of individual, with each subject receiving either treatment or placebo messages. We randomly assigned the treatment density between villages, which contained an average of 240 households. Within each district, sampled villages were partitioned by participation levels. In villages with less than 15 subjects, half were assigned to the treatment and half to the placebo. For villages with at least 15 subjects, we created paired blocks based on an ordered sorting of the number of participating subjects. Within each pair, we assigned one village to have 80 percent of subjects treated and the other village to have 20 percent of subjects treated.[25] This operationalization of "common information" sought to avoid allowing politicians the opportunity to spin budget management information and potentially undermine our main treatment. However, taking into account this concern meant that we did not disseminate information in a way most likely to achieve "common knowledge" – i.e., in a public setting.[26]

For ethical reasons, our design was purposefully crafted to have a very low probability of affecting aggregate election results. We sent messages to approximately 20 subjects per village on average, when villages typically have more than 1,000 eligible voters. Moreover, our sample only included 750 villages out of approximately 25,000 nationally.

[25] In villages where 80 percent of subjects were treated, the SMS message was modified to inform voters that "we are going to be sending you and many of your neighbors information." We did this in order to encourage coordination among recipients in high density villages.

[26] Arias (2016); Larson and Lewis (forthcoming). Thanks also to Etienne Poliquin.

2.4 Estimation

Estimation of vote choice in relation to priors generally conforms with the overall Metaketa PAP, with a few key exceptions documented in our individual PAP. To begin, we asked for voters' priors about candidate quality along a scale – "much worse," "a little worse," "better," or "much better" – since we did not believe that voters would have specific priors about the percentages of unaccounted-for funds. To determine the actual candidate quality Q_j, we divided the range of budget discrepancies at the district level into quartiles to roughly match the categories in the priors we elicited from subjects. We considered it ill-advised to map the strength of news onto a continuous, normalized scale as in the Metaketa PAP. Thus, we use a modified approach, maintaining the overall Metaketa notation to the extent possible.

To estimate the effect of good news, we collapse all types of good news into a single treatment indicator T_i^+, which equals one when the subject i is treated and is part of the relevant subset L^+. Thus, the good news subgroup included subjects who thought budget discrepancies were "much worse" than average at baseline and were eligible to receive information that they were only "a little worse." We follow a similar procedure for bad news.

Our primary estimating equation is given by Equation 1, which is notated for the good news subgroup. In it $y_{ij,t=1}$ indicates whether the subject voted for the incumbent party for the political office j, $y_{ij,t=0}$, indicates whether the subject intended to vote for the incumbent party, $\boldsymbol{\beta}$ is a vector of estimated coefficients, $\mathbf{Z_i}$ is a matrix of pre-specified, pre-treatment covariates, ν_j is a village fixed effect, and ϵ_{jh} is the error term clustered by politician (j) and district (h). As specified in our pre-analysis plan, we test our hypotheses on vote choice with and without pre-treatment covariates: perception of living conditions (M18); gender (M13); level of education (M17); age (M14); trust in information provided by our NGO partner (M24); perceptions that powerful people will learn about vote choice (M26); perceptions about the fairness of elections (M27); whether the subject voted for the incumbent in 2011

(M21); and intention to vote. Unlike the main Metaketa estimating equation, we do not interact our treatment indicator with the covariates in the main specification.

$$y_{ij,t=1} = \alpha + \tau_1 T^+_{ik} + \varphi y_{ij,t=0} + \boldsymbol{\beta}\mathbf{Z_i} + \nu_j + \epsilon_{jh} \tag{1}$$

All standard errors for treatment effects (τ) are computed by randomization inference on the relevant parameter of the estimating equation. We assume a sharp null hypothesis and create a sampling distribution of the parameter under 10,000 iterations of our exact randomization process described above. Our reported standard errors are the standard deviation the resulting sampling distribution and our p-values derive from comparing the estimated parameter value to the sampling distribution under the sharp null assumption. Thus, our characterizations of uncertainty derive from our design, rather than the particular model that we use to estimate treatment effects.

3 Results on the common arm (budget information)

3.1 Vote choice

First, we evaluate the effect of good and bad news on vote choice. Figure 3 presents our main estimates of the treatment effects, derived from Equation 1. As pre-registered and because our hypotheses are directional, we use one-tailed tests. Since we consider chair and councillor elections to provide two distinct tests of the hypothesis about vote choice, we apply the Benjamini–Hochberg correction for false discovery in the tests below.

We do not find that good news affected votes for district chairs, with subjects no more likely to vote for incumbents in the treatment condition compared to the placebo condition. However, good news caused an increase of 3 percentage points in reported votes for district councillor incumbents ($p = 0.12$), with a swing of 6 percentage points among verified recipients of the treatment messages ($p = 0.02$). The pattern repeats in the bad news condition. The bad news treatment caused no significant change in votes for district chairs, but bad news resulted in a 3 percentage-point decrease in votes for district councillors compared to placebo ($p = 0.03$) and a 6 percentage-point drop among verified recipients ($p = 0.01$).

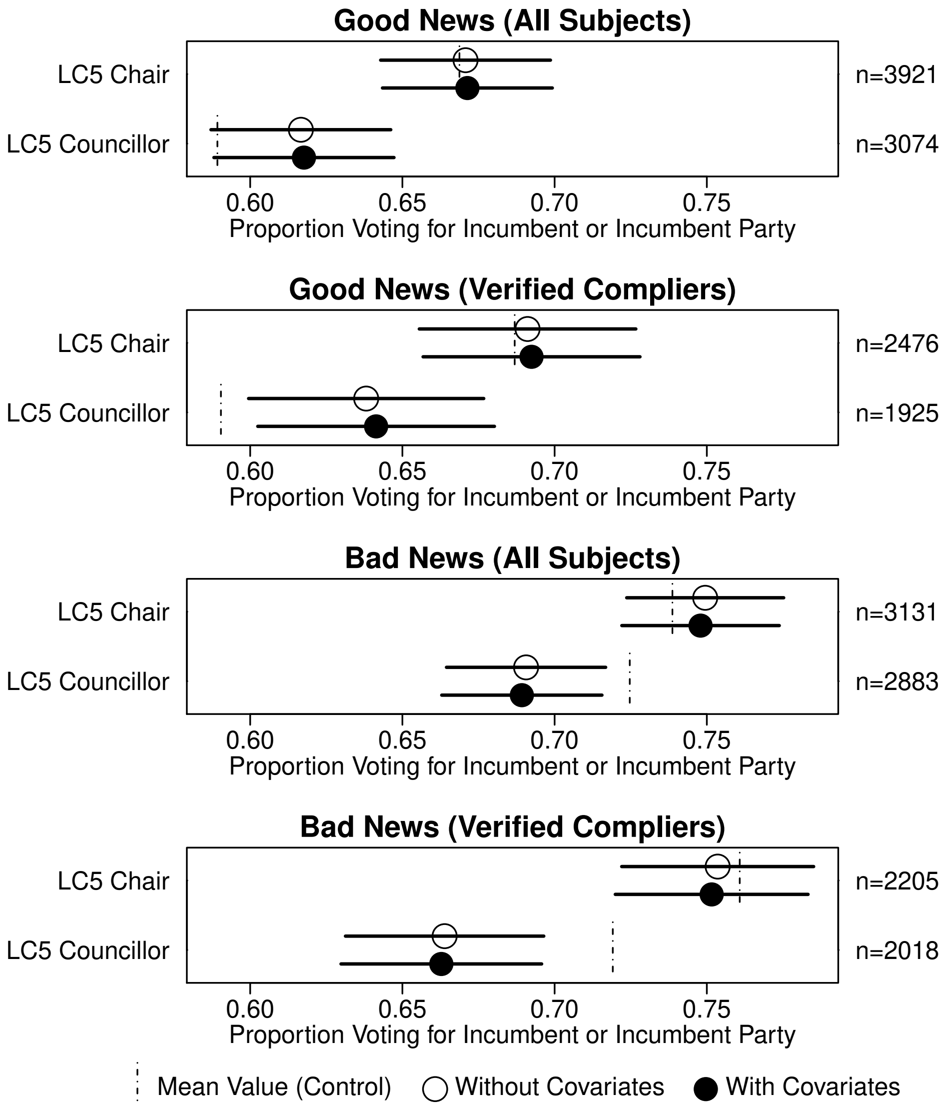

Figure 3: **Treatment effects of budget disclosures.** Bars show 90 percent confidence intervals, equivalent to one-tailed test at $\alpha = 0.05$. Figure displays treatment effects scaled to the control group mean, rather than levels directly.

We take a number of steps to rule out systematic bias in terms of reported vote choice.

For example, in the Appendix, we compare reported vote choice to official results. The patterns are consistent with accurate reporting. In other words, survey-reported voting for the incumbent party is correlated with the official election results, although people in low vote areas may have been somewhat more likely to report voting for the incumbent party than the official turnout data would suggest. This difference may be due to the fact that we over-sampled NRM voters relative to the population, and NRM was the incumbent party in all but six chair elections.

Moreover, in the Appendix, we explore whether our results hold for the subset of respondents who accurately recalled the basin color in their polling station. We find that none of the effect sizes change in magnitude in ways that are inconsistent with the main model, but that the effect of good news on vote choice is no longer inconsistent with random chance, whereas the effect of bad news remains statistically significant at the approximately p=0.03 in specifications both with and without covariates.

One potential reason for misreporting is social desirability. If respondents believed that our enumerators or NGO partner desired a particular response, then they might have over-reported voting for well-performing incumbents and under-reported voting for badly-performing incumbents. This dynamic is difficult to rule out, though we view it as unlikely. To evaluate it, we examined whether treatment effects were stronger among respondents who stated that they trusted Twaweza in the baseline survey. If responses are trying to tell Twaweza what they think it wants to hear, then we would expect stronger effects among this sub-group. As Table 3 shows, we do not.

A different form of reporting bias would arise if respondents over-reported voting for the official winner out of a desire to fit in or to avoid punishment from the government. However, under the reasonable assumption that the treatment and placebo groups were equally likely to over-report for the winner, this dynamic would not have affected the treatment estimates. To evaluate whether such over-reporting exists, we compared self-reported votes for the incumbent in surveys conducted prior to the release of the results and surveys conducted after the release. As the Appendix shows, we see no systematic difference.

A final concern is that our results are idiosyncratic to particular parties, or biased in favor of one party. For instance, if only NRM voters were eligible for good news and only opposition voters were eligible for bad news, one might be skeptical about the generalizability of the effects. In the Appendix, we show the distribution of good and bad news by party. The results illustrate that these effects were not driven by voters or incumbents of a particular party, suggesting the broad applicability of using information technologies to decrease the information advantage of incumbents in electoral autocracies, particularly when it comes to functions of government that are directly attributable to politicians like overseeing public budgets.

3.2 Turnout

Contrary to our expectations, subjects treated with good news were not more likely to turn out for the district elections. Similarly, subjects treated with bad news were not less likely to turn out. Figure 4 presents these results. In addition, and as Table 1 shows, we do not find evidence to support our expectation that the effect of information on turnout would be moderated by voters' alignments with the incumbent part.

As previously acknowledged, an important concern is that respondents might not have accurately reported turnout. As shown in Figure 4 below, more than three-quarters of the experimental sample reported voting. This figure is quite a bit higher than the officially-reported turnout in the districts sampled in this study, which averages around 45 percent. This difference could be due to people in our studying saying that they voted when they did not because voting was seen as socially desirable. It could also be due to the ways that our experimental sample was not representative of Uganda as a whole (as discussed above), and may have over-represented voters by relying on mobile technology or recruiting subjects who

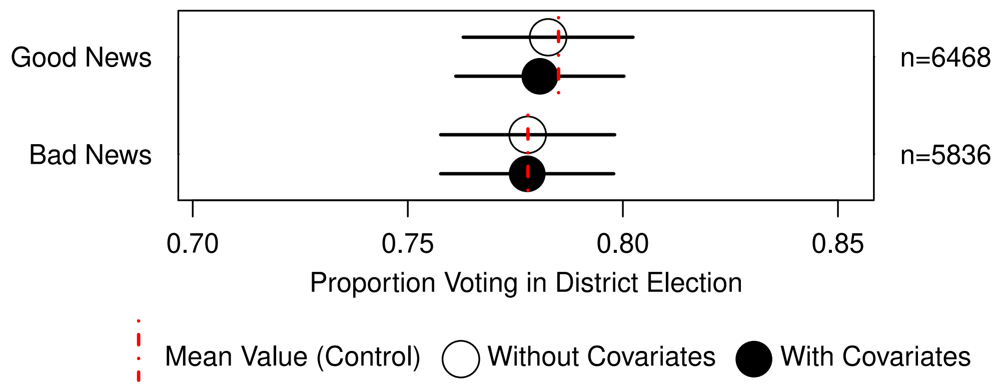

Figure 4: **The unconditional treatment effect of good and bad news on turnout.** Bars show 90 percent confidence intervals, equivalent to one-tailed test at $\alpha = 0.05$.

were interested in local governments and public services.

To assess whether respondents accurately reported turnout, we asked respondents to name the color of the water basin at their polling stations. Most respondents were able to answer this question in internally consistent ways, which suggests that they were accurately reporting their turnout. 63 percent of respondents recalled a basin or basin color in their polling station, and among those individuals, 77 percent were able to name the color in an internally consistent manner.[27]

3.3 Attitudes about politicians' integrity and effort

Although we consider vote choice and turnout to be our primary outcomes, we pre-specified that we would test for the effect of the treatment on a number of other outcomes, which are also reflected in the Chapter 3 meta-analysis. In particular, we hypothesized that the treatment would change voters' evaluations of candidate integrity and effort. Specifically, we asked voters how surprised they would be to hear about corruption involving the district

[27]For more details on this analysis, see the Appendix.

	DV: Turnout for LC V Election	
	Good News	Bad News
	(1)	(2)
Budget Treatment	-0.018	0.021
	(0.026)	(0.028)
LC V Chair Alignment	0.019	0.0003
	(0.019)	(0.017)
LC V Councillor Alignment	0.024	0.021
	(0.027)	(0.018)
Budget Treatment x LC V Chair Alignment	-0.003	-0.014
	(0.020)	(0.021)
Budget Treatment x LC V Councillor Alignment	0.025	-0.024
	(0.033)	(0.026)
Polling station fixed effects	Yes	Yes
Covariates	Yes	Yes
Observations	5,200	5,404
Adjusted R^2	0.092	0.143

Note: SEs clustered by politician; one-tailed tests; contested elections only. $*p < 0.10; **p < 0.05; ***p < 0.01$.

Table 1: **Conditional effect of budget treatment on turnout based on alignment with incumbents**.

chair and whether their district councillor put in more or less effort to get things done compared to other councillors in the district. We fail to find evidence that the treatment affected voters' attitudes about their district officials in ways that are inconsistent with the null hypothesis as shown in Table 2. The null result of the integrity measure for the chair in particular tracks the null result for the main vote choice outcome reported above.

	Dependent Variable:							
	Chair Integrity				Councillor Effort			
	Good News		Bad News		Good News		Bad News	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Budget	−0.012	−0.009	−0.002	−0.005	−0.014	−0.016	0.025	0.022
Treatment	(0.029)	(0.032)	(0.025)	(0.023)	(0.022)	(0.022)	(0.017)	(0.018)
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
Village FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,175	6,039	5,557	5,433	5,897	5,776	5,507	5,386
Adjusted R^2	0.162	0.177	0.180	0.189	0.148	0.154	0.170	0.176

Note: SEs clustered by politician; one-tailed tests; contested elections only. $*p < 0.10; **p < 0.05; ***p < 0.01$.

Table 2: **Effects of budget treatment on evaluations of candidate integrity and effort**

3.4 Heterogeneous treatment effects

We pre-registered a number of conditions under which we expected information to be more likely to shape vote choice, as reflected also in the Chapter 3 meta-analysis (this volume). Similar to the meta-analysis, we fail to find evidence that the following factors lead to larger effects for the information treatment (see Table 3):

1. gap between voters' prior beliefs about candidates and the information provided;

2. less certainty of priors about politician performance;

3. co-partisanship;

4. importance of information about budget management for voting;

5. weaker partisan attachment;
6. trust in the Auditor General (source of information treatment);
7. trust in Twaweza (implementing partner sending information); and
8. lower expectation of clientelist benefits.

Moderation Effect	Dependent variable:			
	Chair (Good)	Councillor (Good)	Chair (Bad)	Councillor (Bad)
Baseline Category: Accurate Prior				
Actual One Better	−0.016 (0.044)	0.018 (0.053)		
Actual Two Better	−0.113** (0.044)	0.063 (0.060)		
Actual Three Better	−0.022 (0.045)	0.062 (0.059)		
Actual Four Better	−0.086* (0.045)	0.043 (0.077)		
Baseline Category: Accurate Prior				
Actual One Worse			0.011 (0.029)	0.066* (0.034)
Actual Two Worse			0.059* (0.033)	0.027 (0.041)
Actual Three Worse			0.023 (0.059)	0.047 (0.052)
Actual Four Worse			0.034 (0.164)	−0.160 (0.173)
Baseline Category: Very Certain Priors				
Certain Priors	0.011 (0.022)	0.047 (0.039)	−0.060* (0.034)	−0.008 (0.042)
Not Certain Priors	0.068 (0.056)	0.019 (0.049)	−0.067 (0.050)	−0.024 (0.044)
Very Uncertain Priors	0.008 (0.116)	0.009 (0.125)	−0.028 (0.060)	−0.044 (0.089)
Missing Certainty Priors	0.181 (0.169)	−0.171 (0.294)	−0.354 (0.234)	0.747** (0.339)
Baseline Category: Budget Management Not Important				
Budget Not Very Important	−0.085 (0.087)	−0.060 (0.086)	−0.134 (0.085)	0.013 (0.069)
Budget Somewhat Important	−0.064 (0.085)	−0.055 (0.086)	−0.058 (0.074)	−0.030 (0.070)
Budget Very Important	−0.048 (0.089)	−0.010 (0.078)	−0.078 (0.062)	0.017 (0.055)
Missing Budget Importance	−0.514** (0.260)	−0.401 (0.315)	−0.068 (0.303)	−0.408 (0.257)
Baseline Category: Not Aligned LC V Chair				
Aligned LC V Chair	−0.014 (0.040)		−0.031 (0.049)	
Missing LC V Chair Alignment	−0.020 (0.061)		−0.054 (0.068)	
Baseline Category: Not Aligned LC V Councillor				
Aligned LC V Councillor		0.044 (0.040)		0.009 (0.041)
Missing LC V Councillor Alignment		−0.083 (0.077)		−0.045 (0.079)
Baseline Category: Not Same Tribe LC V Councillor				
Same Tribe LC V Councillor		0.016 (0.035)		0.009 (0.041)
Missing Same Tribe LC V Councillor		–		0.914*** (0.168)
Baseline Category: Do Not Trust Auditor General				
Don't Know Trust Auditor General	0.059** (0.027)	0.100* (0.056)	0.016 (0.032)	0.028 (0.045)
Trust A Little Auditor General	0.062 (0.042)	0.104** (0.052)	0.044 (0.046)	0.012 (0.044)
Trust A Lot Auditor General	0.070 (0.048)	0.092 (0.068)	−0.017 (0.057)	−0.015 (0.046)
Missing Trust Auditor General	0.011 (0.198)	0.126 (0.225)	−0.099 (0.082)	0.032 (0.083)
Baseline Category: Do Not Trust Twaweza				
Don't Know Trust Twaweza	−0.135* (0.074)	−0.136 (0.093)	0.014 (0.100)	−0.147* (0.085)
Trust A Little Twaweza	−0.083 (0.082)	−0.178* (0.098)	0.040 (0.081)	−0.107 (0.073)
Trust A Lot Twaweza	−0.091 (0.085)	−0.165* (0.094)	0.004 (0.078)	−0.152* (0.077)
Missing Trust Twaweza	0.715*** (0.176)	−0.158 (0.279)	−0.021 (0.197)	−0.137 (0.148)
Baseline Category: Vote Buying Very Likely				
Vote Buying Somewhat Likely	0.003 (0.060)	0.053 (0.064)	0.001 (0.044)	−0.009 (0.062)
Vote Buying Somewhat Unlikely	−0.065 (0.072)	−0.001 (0.079)	0.036 (0.066)	0.043 (0.062)
Vote Buying Very Unlikely	−0.012 (0.045)	0.029 (0.061)	−0.014 (0.051)	0.086 (0.060)
Vote Buying Don't Know	0.008 (0.057)	0.078 (0.082)	0.060* (0.035)	0.066 (0.073)
Missing Vote Buyings	−0.088 (0.143)	−0.271 (0.192)	0.024 (0.297)	−0.032 (0.191)
Baseline Category: Very Strong Party Attachment				
Party Attachment Strong (6)	0.019 (0.049)	−0.117** (0.049)	0.063 (0.048)	−0.009 (0.043)
Party Attachment (5)	0.012 (0.037)	−0.042 (0.052)	0.016 (0.044)	0.013 (0.045)
Party Attachment (4)	0.047 (0.048)	−0.108 (0.085)	−0.054 (0.058)	0.022 (0.058)
Party Attachment (3)	0.031 (0.092)	−0.077 (0.102)	0.132 (0.096)	−0.091 (0.091)
Party Attachment (2)	−0.191 (0.174)	−0.057 (0.152)	−0.010 (0.108)	−0.097 (0.093)
Party Attachment Very Little (1)	−0.048 (0.092)	−0.128 (0.116)	0.038 (0.063)	0.034 (0.057)
Party Attachment Not Applicable	−0.020 (0.040)	−0.095* (0.057)	−0.036 (0.071)	−0.062 (0.046)
Missing Party Attachment	0.026 (0.039)	0.086 (0.096)	0.157** (0.072)	0.062 (0.097)
Observations	3,921	3,074	3,131	2,883
Adjusted R^2	0.214	0.330	0.347	0.399

$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table 3: Moderation of Information Effects on Vote Choice

3.5 Treatment density

Finally, our study's variation in treatment density allowed us to evaluate whether common knowledge reinforced information effects. It also allowed us to assess the extent to which voters shared messages with their neighbors. If subjects shared messages with each other, then we expected control subjects in high-density villages to be more likely to vote according to treatment information than control subjects in low-density villages.

To estimate the conditional effect of the good news and bad news treatments based on treatment density, we use the modified estimating equation given by Equation 2, which includes a density treatment indicator D_j assigned at the village level. Because the density treatment is assigned at the level of the village, we use a paired-village fixed effect b_j to mirror our assignment strategy.[28] For analysis of the density treatment, we only consider the subset of subjects from villages with at least 15 subjects:

$$y_{ij,t=1} = \alpha + \tau_1 T^{+}_{ik} + \tau_2 D_j + \tau_3 T^{+}_{ik} D_j + \varphi y_{ij,t=0} + \boldsymbol{\beta} \mathbf{Z_i} + b_j + \epsilon_{jh} \tag{2}$$

Figure 5 displays results for the density condition among treated subjects. They are null, suggesting that treating 80 percent of subjects at a polling station did not significantly alter votes for district chairs or councillors compared to the control condition with 20 percent of subjects treated. Although we varied the density of treatment among subjects, the individuals who volunteered to participate were often a relatively small subset of the local population, so even 80 percent treated may have failed to induce common knowledge. Moreover, given the generally unfree political environment, Ugandans reported taking great care when discussing politics, so they may have failed to pass along the budget information regardless of the local size of the experimental sample.

Table 4 displays the results of the village-level density treatment combined with the

[28]We mistakenly omitted the notation for paired block fixed effects in our PAP.

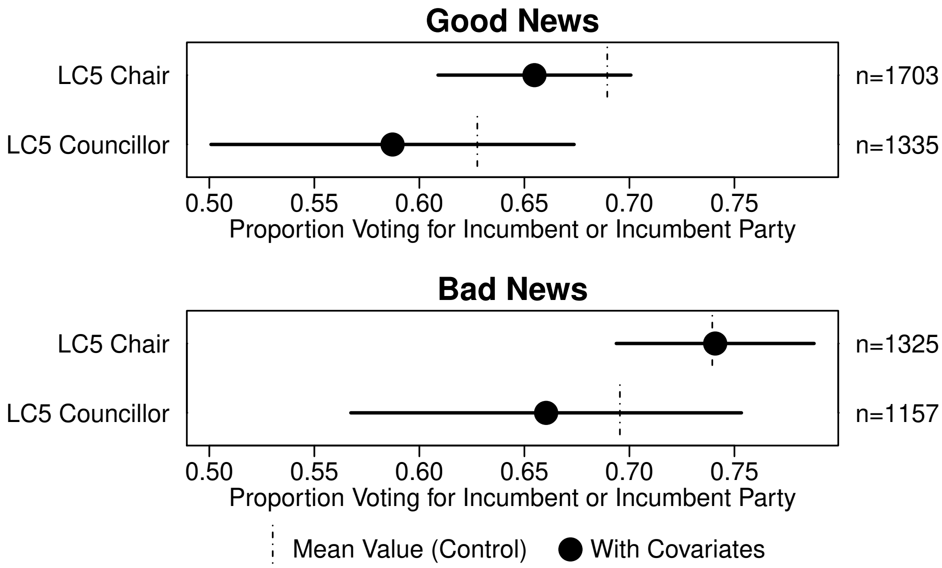

Figure 5: **The treatment effect of higher treatment density among treated subjects.** Bars show 90 percent confidence intervals, equivalent to one-tailed test at $\alpha = 0.05$.

subject-level budget information treatment. This specification combines the two binary treatment conditions into the four possibilities with the comparison condition being subjects receiving placebo information in a low-density treatment village. The coefficients show the effects of the combination of the density and information treatments on vote choice when compared to the placebo and low-density conditions as the baseline. We include fixed effects for the paired villages.

We find that control subjects in high-density villages were less likely to vote for incumbent chairs in the bad news condition, possibly indicating that they were affected by spillover. Note that the effect of the budget treatment remains significant in both the low-density ($p = 0.03$) and high-density ($p = 0.04$) subgroups. In the bad news condition, all treated subjects for both elections were less likely to vote for the incumbent than their counterpart placebo subjects in the low-density villages. This finding provides some potential insight into the divergent results between chair and councillor elections in the bad-news subgroup, since negative spillover to control subjects may be muting the treatment effect when using village fixed-effects in our estimating equation. However, the average effect of the high-density treatment across treated and control subjects is not inconsistent with random chance for either chair ($p = 0.18$) or councillor ($p = 0.13$) elections. In contrast, the good news subgroup does not yield any results on the budget treatment or treatment density that are inconsistent with the null hypotheses.

	DV: Vote Choice for the Incumbent			
	LC V Chair	LC V Councillor	LC V Chair	LC V Councillor
	Good News		Bad News	
	(1)	(2)	(3)	(4)
Control, High Density (RI)	0.004	-0.028	-0.034	-0.044
	(0.029)	(0.050)	(0.029)	(0.055)
Budget Treatment, Low Density (RI)	0.024	0012	-0.027	-0.054**
	(0.026)	(0.030)	(0.025)	(0.028)
Budget Treatment, High Density (RI)	-0.011	-0.019	-0.028*	-0.092**
	(0.021)	(0.045)	(0.021)	(0.050)
Control, High Density	0.004	-0.028	-0.034*	-0.044
	(0.032)	(0.036)	(0.024)	(0.040)
Budget Treatment, Low Density	0.024	0.012	-0.027*	-0.054*
	(0.042)	(0.028)	(0.019)	(0.025)
Budget Treatment, High Density	-0.011	-0.019	-0.028*	-0.092**
	(0.022)	(0.031)	(0.019)	(0.034)
LC V Chair Intent	0.081*		0.023	
	(0.025)		(0.029)	
LC V Councillor Intent		0.049**		0.007
		(0.016)		(0.021)
Paired village fixed effects	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Observations	3,281	2,585	2,633	2,391
Adjusted R^2	0.212	0.244	0.286	0.265

Note: SEs clustered by politician; one-tailed tests; contested elections only. $*p < 0.10; **p < 0.05; ***p < 0.01$.

Table 4: **Effect of budget treatment density on vote choice for district (LC V) incumbents**. Data subset excludes constituencies that were redistricted or where incumbents switched parties.

4 Alternate arm: public services information

The management of budgets is an important issue in Uganda, as explained above. Yet it remains fairly distant issue for many voters. In contrast, the quality of public services is directly relevant to their well-being and often the most tangible interaction that they have with their local governments.

At the local level, politics often turns on the ability of politicians to deliver public services effectively, especially given the decentralized political system in Uganda. Although recent legislation has made local governments more dependent on the central government both administratively and financially, local governments by law have substantial responsibility to provide a variety of essential public services as part of the country's "radical and early decentralization."[29] Furthermore, surveys indicate that Ugandan citizens hold subcounty officials partly responsible for providing public services such as water access – although not as much as the central government.[30] In light of the potential salience of local public services to voter behavior, our study also included an alternate arm that involved collecting and disseminating novel information about the relative quality of local public services.

4.1 Research design

Concurrent to the subject recruitment activities described above, we conducted independent audits of three public services that are managed locally and that focus groups indicated would be especially salient to voters in local elections in Uganda. These services were access to improved water sources, road conditions, and the conditions of local health facilities. For the water audit, we recorded the round-trip walking time to the nearest improved water

[29]Ndegwa (2003); Green (2015).

[30]Grossman and NDI (2011). Given that many Ugandans blame the central government first for poor public services, we note that it may have been difficult to identify a significant effect of new information within the context of local elections.

source from a central location, tallied the wait time to access the water source, and recorded whether fees were charged for access. For the road audits, we used the tablet application *RoadLab* to record the roughness encountered by vehicles that attempted to traverse a local county road. For the health audit, we recorded the cleanliness and availability of medicines in local health clinics that are run at the district and subcounty levels.

We also utilized the results of the Twaweza Uwezo education audits. Twaweza independently tested school children for scholastic achievement outside of the school environment to avoid manipulation. Thus, we have independent data on a total of four public services that are likely to be especially important to voters. All of these services fall under the combined responsibilities of district and subcounty governments.

For each audit, we created an index of "service quality" and then normalized that value within the district. The process produced our treatment, which indicates whether services in the village where each of the subjects live were "much better," "better," "a little worse," or "much worse" than other villages in the same district. These categories were similar to those used in the budget information treatment. The intent of the public services treatments were to provide subjects with some perspective about whether politicians were doing a good job or bad job as compared to other areas of the district, which was likely to make the information more meaningful than if it simply contained the raw scores. Normalizing the treatment information within districts also helped us avoid problems in analysis that would arise from differences in district-level attributes that are relevant for public service provision, such as MP effectiveness and revenue transferred from national government ministries.

Prior to assigning the treatment, we used the same baseline survey described above both to collect key data for later analysis related to individuals' priors and also to determine the public service (i.e., roads, water, education, health) that each individual considered most important when deciding how to vote in local elections. During this survey, we asked each person what kind of information s/he would like to hear about prior to the upcoming district and subcounty elections. With our partner Twaweza, we deployed the same informational treatments related to public services prior to both the district and subcounty elections, which were held in February and March 2016 approximately three weeks apart.[31] In other words, subjects in our experimental sample received messages prior to the district elections about *both* budget irregularities and public services, and they received further messages prior to the subcounty elections about *only* public services. For district elections, the public services treatment was assigned independently of the budget treatment. For subcounty elections, only the public service treatment was deployed since the management of district budgets was not relevant for LC III voting.

We randomly assigned half of the subjects within each village to receive information on public services. The subjects not assigned to receive the public services information treatment instead received placebo messages about the importance of the four public services that we audited. These placebo messages contained information about the welfare of Ugandans without any information about the actual state of public services as revealed in the audits that we conducted. The placebo messages were sent to ensure that any treatment effects that we detected were not just a reflection of priming effects.

4.2 Finding for the "salient public service" treatment

The overall result for the alternative arm is that we fail to reject the null hypothesis for any of the pre-specified hypotheses on main effects, including vote choice, turnout, evaluations of candidate quality. This finding is consistent across all offices and all elections, as reported in Table 5. In the interest of space, we do not report or discuss the specific results here that are analogous to the primary arm results for the budget treatment reported in the sections

[31]To the extent that we did not have data on the public service that an individual indicated was most important to them when voting, we gave them information on the public service they indicated was the second most important. For the small subset of subjects for whom we did not have audit data for either the first or second choice, we provided information randomly on a public service for which we do have data.

above.

Office	Treatment Effect (Good News)	Treatment Effect (Bad News)
	DV: Vote Choice for the Incumbent	
LC V Chair	-0.008	0.020
	(0.011)	(0.017)
	[0.76]	[0.88]
LC V Councillor	-0.039	-0.008
	(0.018)	(0.018)
	[0.99]	[0.32]
LC III Chair	-0.003	0.013
	(0.014)	(0.017)
	[0.58]	[0.78]
LC III Councillor	0.013	0.016
	(0.020)	(0.024)
	[0.25]	[0.74]
	DV: Turnout	
LC V Election	0.005	0.027
	(0.010)	(0.014)
	[0.29]	[0.97]
LC III Election	0.002	0.012
	(0.012)	(0.014)
	[0.45]	[0.80]
	DV: Evaluation of Chair Integrity	
LC V Chair	0.038*	-0.034
	(0.025)	(0.028)
	[0.07]	[0.11]
LC III Chair	0.027	0.027
	(0.031)	(0.034)
	[0.19]	[0.79]
	DV: Evaluation of Councillor Effort	
LC V Councillor	0.014	-0.021
	(0.026)	(0.029)
	[0.29]	[0.24]
LC III Councillor	-0.018	-0.024
	(0.026)	(0.026)
	[0.76]	[0.18]

Note: SEs clustered by politician; one-tailed tests; contested elections only. $*p < 0.10; **p < 0.05; ***p < 0.01$.

Table 5: **Main effects of salient public services treatment**. Data subset excludes constituencies that were redistricted or where incumbents switched parties. Table shows estimated treatment effect, (clustered standard errors), [one-tailed p-value]. None of the treatment effects displayed have adjustments for covariates.

5 Discussion and conclusion

5.1 Common arm

To summarize, our findings with respect to the common arm present a mixed picture with respect to the effect of budget information on accountability. On the one hand, and as expected, we found that bad news about budget discrepancies led to fewer votes for incumbent district councillors. Similarly, we found that good news about budget discrepancies led to increased votes for the incumbent district councillors. These findings support the overall Metaketa project's theoretical framework about the ways that new information can improve political accountability. They also support one of intuitions motivating our specific intervention: salient political information disseminated via mobile phones can significantly affect voter behavior even in the context of an electoral authoritarian regime (Uganda) where previous studies have not always found the expected effects of information.

On the other hand, our study also had some unexpected null findings that are in keeping with the Metaketa project's overall null results. In our view, most important among the unexpected null findings from our study was the limited influence of the informational treatments on voter behavior for district chairs. Whether presented with good news or bad news, our treatments did not affect voter behavior. If we had found more consistent results for chairs and councillors, then we could draw conclusions with more confidence about the significant role of information in shaping voter choice and the applicability of using mobile technology to make information available to voters in settings like Uganda.

Developing and testing a full explanation for our mixed and null findings is beyond the scope of this chapter. As with any study involving null results, the findings may be the result of specific design choices or the research context – or they may reflect a "true null," i.e., in our case, they could suggest that information does not affect accountability at all on average.[32] In our chapter's earlier section on research design, we reflected on some of the factors (e.g., the design of the treatment and the sample) that may have led to a null result in terms of the chairs. Below, we suggest several broader explanations related to the overall research context that could account for the null result for chairs but not councillors.

First, it is possible that voters do not understand which level of government or which official is most responsible for government performance and the public services that they care about. Governance is complex and citizens in many settings, including Uganda, do not have a good understanding of who is ultimately responsible for delivering public goods and services.[33] In general, voters are less able to sanction incumbents when lines of responsibility for outcomes are not clear, as we suspect may be the case in post-decentralization Uganda.[34] There is evidence that information about responsibility for governance conditions how citizens react to government performance.[35] It is possible that the voters in our sample underestimated the responsibility of chairs and overestimated the responsibility of councillors in managing district budgets. Adding a civic education component to the intervention, similar to the civics message used in Benin, may have been a useful design modification.[36]

Second, it is possible that higher-level elections take place within a more saturated information environment. Indeed, NGO audits of district level officials in 26 districts scored chairs as better than councillors, on average, in contacting their electorates.[37] Furthermore, races for district chair positions are more professional affairs that involve a larger amount of campaigning through both traditional and social media outlets. In contrast, races for district councillor positions involve lower budgets and less traditional campaigning in ways

[32] Bush et al. (2016, 1731).

[33] Hobolt and Tilley (2014); Hobolt, Tilley and Wittrock (2015); Grossman and Michelitch (2016).

[34] Tavits (2007).

[35] Gottlieb (2015).

[36] Adida et al. (this volume).

[37] Bainomugisha (2015).

that might get information into the hands of voters. Consequently, voters for councillor may have been more receptive to new information about budget performance. This logic not only might explain the difference between our findings about chairs and councillors but might also offer insight as to why we uncovered some significant effects of new information in a local election in Uganda but studies focusing on national elections have been largely null.[38]

Third, and relatedly, it is possible that higher-level elections are decided further in advance and as a consequence of a longer-term accumulation of knowledge and information. In higher-level elections, voters who have already made up their minds might engage in more motivated reasoning about their choices when presented with new information toward the end of a political contest. As such, future studies could investigate not only the content and mode of information treatments, but also the timing. Most existing studies of this type have been carried out using survey experiments rather than deploying treatments in field settings, yet timing is a crucial variable facing organizations who seek to empower voters.[39]

The limited influence of informational treatments on voter behavior for district chairs was not our only unexpected finding. Although we hypothesized that new information would drive turnout, we did not find positive evidence for that in any case. This null effect was surprising because most canonical models of voter behavior highlight how informed voters should be more likely to turnout because they face less uncertainty in their choices.[40] Being able to identify whether a politician is managing a budget better or worse than expected should provide more information about politician type and thus increase the utility that voters derive from participating, since conditional on participating they will be able to make a choice more effectively. We also hypothesized that the effect would depend on the alignment of the voter: a voter that received good news about an incumbent with which they align

[38] E.g., Humphreys and Weinstein (2013).

[39] Chong and Druckman (2010).

[40] Lassen (2015).

would be more likely to turnout, whereas a voter that received good news about that same incumbent when they align with the challengers would be less likely to turnout.

One reason why we may not have detected the effect that we expected is that voters were deciding on how to participate in multiple races during the district elections. It is possible that moving voters' estimates of candidate quality on only the lowest office being contested was not strong enough in the context of an election where the contest for district chair brings people to the polls. Indeed, in many other contexts up-ballot races drive turnout, and it may be these up-ballot races that are most immune to having information treatment change voter choices.

5.2 Alternate arm

The results on the alternate arm of our study are perhaps the most surprising. That voters did not respond to information that they personally stated was most important to them when they go to the polls suggests a number of possibilities. First, this result may simply echo the results of the meta-analysis (Chapter 3, this volume) that suggest information is not likely to be used by voters on average. For public services in particular, it is possible that it was difficult to attribute the information in our treatments to any particular politician or that voters did not link public services to local politicians in the way that we hypothesized they would. If we were to run this experiment again, we would have collected more information from voters about who they held responsible for performance as it related to budget management and the provision of public services.

5.3 Concluding thoughts

Information presented to voters privately, as in our study, is likely to be considerably harder for politicians to uncover and counter. Information presented publicly – such as through the media, fliers, community meetings or public screenings – is subject to constant spin or downright falsification by politicians. Other projects in the Metaketa initiative reveal these dynamics; in Mexico, politicians actively developed strategies to falsify information;[41] in India, politicians shut down the unwanted information completely;[42] and in the Uganda I study, politicians attempted to distort how citizens process information.[43] Of course, politicians may still use other strategies to counter mobile messages, such as overloading voters with irrelevant or fake information via SMS or even shutting down mobile networks. For an NGO-initiated information campaign about accountability to impact citizens' actions, organizations must take steps to protect their own credibility and the credibility of the information they provide in the eyes of the citizens against potential efforts to discredit them. However, on average it should be harder for politicians to prevent voters from accessing the information they need to vote when information is conveyed through text blasting and quickly evolving encryption techniques.

Text messages have other advantages as well. Targeted information can be deployed via SMS messages during the final days of elections or in response to critical events. New technologies allow NGOs to get information to voters during debates, after campaign appearances, and following candidate statements, which may help voters make choices according to their interests. One constraint of SMS delivery is the challenge of condensing credible, new, and salient information into a short message that fits within the 160-character limit. Future research will be needed to offer guidance on how and when a similar organization might effectively disseminate information with mobile technology. We suspect the form of the most effective messages could be highly contextual.

Widespread and growing mobile phone ownership, nationwide network coverage, and the

[41] Arias et al. (this volume).

[42] Sircar and Chauchard (this volume).

[43] Platas and Raffler (this volume).

relatively low cost of SMS technology makes our approach to providing information feasible for many NGOs and citizens' groups to use to spread the information they are already gathering. Our study has demonstrated the feasibility and potential impact of using cheap, SMS text messaging to empower citizens with credible information to use to hold accountable local elected officials for the quality of their budget management. This approach holds some promise even in an electoral authoritarian regime. Yet, that the same information on budget management did not affect citizens' votes for incumbent district chairs, who are on paper those officials most accountable, strongly suggests that development experts and donors need to give more attention to educating citizens in decentralized systems about political responsibilities for budget management and different public services.

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