SMS texts on corruption help Ugandan voters hold elected councillors accountable at the polls

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Many politicians manipulate information to prevent voters from holding them accountable; however, mobile text messages may make it easier for nongovernmental organizations to credibly share information on official corruption that is difficult for politicians to counter directly. We test the potential for texts on budget management to improve democratic accountability by conducting a large (n = 16,083) randomized controlled trial during the 2016 Ugandan district elections. In cooperation with a local partner, we compiled, simplified, and text-messaged official information on irregularities in local government budgets. Verified recipients of messages that described more irregularities than expected reported voting for incumbent councillors 6% less often; verified recipients of messages conveying fewer irregularities than expected reported voting for incumbent councillors 5% more often. The messages had no observable effect on votes for incumbent council chairs, potentially due to voters' greater reliance on other sources of information for higher profile elections. These mixed results suggest that text messages on budget corruption help voters hold some politicians accountable in settings where elections are not free and fair.

voting | accountability | elections | communication technology | information

In many countries, voters lack the information they need to evaluate politician performance. Instead, they vote for their coethnics or in response to patronage (1, 2). These dynamics reduce incentives for politicians to provide public goods and increase the propensity for corruption (3). International organizations and civil-society activists have spent billions of dollars attempting to help voters make better informed choices and to encourage politicians to improve services (4–6). However, many studies have questioned whether new political information enables voters to hold politicians accountable in practice (7–11). The mixed evidence is especially apparent in contexts where politicians can manipulate or censor information about performance by muzzling the press, repressing civil society, or spinning public disclosures (12–14).

Messages on budget corruption sent via SMS text may be harder for politicians to control and easier for nongovernmental organizations to use. Although politicians can shut down telephone networks or the Internet, this can prove costly, even in repressive settings (15). Information on budget management by SMS can also be tailored to an individual's location, read privately, and sent quickly and widely. Thus, while politicians still have means to devise alternative messages, they may have more difficulty controlling and directly countering information about corruption transmitted before elections through SMS compared with newspapers, television, and radio, in which dissemination is public and less targeted.

To examine the effects of budget information conveyed by text messages on electoral accountability, we conducted a large randomized controlled trial (n = 16,083) around Uganda's February 2016 district elections. We transformed data on district budget irregularities from the Uganda Office of the Auditor General

into simple notices. Then, we worked with a Ugandan nongovernmental partner, Twaweza, to send the information via texts immediately before the elections. We treated and measured self-reported voting at the individual level, so we are able to account for voters' prior beliefs and voting intentions in our analysis. This study formed a part of a seven-study initiative and meta-analysis on information and vote choice called Metaketa I, which informed our design (9).

Uganda was an appropriate research site. Its elections are not free and fair, and public corruption is perceived to be high (16). Even in autocracies, scholars have hypothesized that better information can promote accountability (17, 18). Yet increased transparency does not always yield positive effects in such settings. One study in Zimbabwe found that better informed voters were less likely to participate in politics (19). Another study in Uganda found that disclosing information about the performance of legislators did not affect voters' choices at the polls, potentially because of officials' ability to spin public disclosures (13).

In our experiment, informing citizens that budget irregularities in their district were worse than expected decreased reported votes for incumbent parties' candidates for district councillor, whereas informing them that irregularities were fewer than expected increased reported votes for incumbent councillors. However, the messages did not significantly affect reported votes for district chairs. While these heterogeneous findings were not expected, we conjecture that the null results for district chairs stem from citizens already having more information about these

Significance

Text messages providing salient, nonpartisan, official information on budget corruption prompted Ugandan voters to take the performance of some politicians into account when voting. Holding politicians accountable via elections is a fundamental precursor to effective governance, economic development, and high-quality public services. The results indicate that communication technologies can combine with data on budget management to help voters make better informed choices at the polls and thus have the potential to enhance local electoral accountability by providing information that is difficult for politicians to control and manipulate.

higher level officials and thus being more likely to take factors beyond budget performance into account.

This study tests the potential of text messages by mobile phone to enhance electoral accountability, reinforcing some prior positive findings using conventional media in established democracies (4, 20) but contrasting with other results (7, 9, 13). This study demonstrates a mixed accountability response to texts about corruption, with the findings on councillors indicating that better informed voters hold some politicians accountable for budgets.

Research Design

Setting. Similar to many developing countries, Uganda holds elections that are not free and fair and suffers from corruption and poor governance, especially locally (21, 22). Although there is competition, the electoral playing field favors the ruling National Resistance Movement (NRM) party. Moreover, the NRM restricts what media can report, particularly around elections (ref. 22, pp. 96–101). This setting creates severe challenges for electoral accountability.

We focus on council elections for district offices, analogous to provinces or states in other countries. District councils (also known as LC Vs or LC5s) are comprised of chairs and directly elected councillors that represent smaller constituencies. In our sample, 85% of elections for district chairs were contested, though the NRM won 77% of these elections.

District councils have primary responsibility for local public services such as health, roads, and education. They write legislation, monitor contracts, supervise public-service staff, formulate local development programs, and plan and oversee public budgets (23, 24). Chairs have significant responsibilities in these areas in both law and practice. However, when citizens observe shoddy services and mismanaged funds, councillors are often a first point of contact (23, 25). According to one study, two-thirds of councillors believe their efforts at monitoring public services and spending affect public-service quality (24). Yet the influence of bureaucrats and higher level officials clearly constrains councillors' ability to manage budgets (26).

Many Ugandans lack knowledge about district councils, which further hinders their ability to hold local officials accountable (ref. 27, p. 16). This dynamic contributes to what the Ugandan Auditor General has called poor local government performance (ref. 28, pp. 13–14). In our sample, 47% of subjects said they would be "not surprised" or "not too surprised" to learn about corruption in their district. Scandals of financial fraud have frequently implicated both chairs and councillors. We thus hypothesized that budget information delivered privately on mobile phones could help overcome more systemic barriers to gaining information about politicians' performance.

Experimental Treatment. District councils must submit to an annual audit by the internationally respected Ugandan Auditor General. The audit tracks whether councils followed procurement rules, completed projects as specified, and accounted for expenditures properly. The Auditor General presents its reports to parliament and posts them online. However, the reports are not widely known to the public or discussed. Respondents indicated limited knowledge of their contents; less than 25% correctly ranked their council's budget performance.

We used the audits to create district ratings for fraud and mismanagement. This information was salient: More than 80% of respondents in our sample indicated that good budget management was important in deciding how to vote for district officials. We made the audit information more accessible and meaningful by simplifying it and adding comparisons. For example, we explained how the percentage of unaccounted-for funds in a citizen's district compared with other districts. Additionally, to make the information more tangible, we sent two messages with local examples of budget performance that aligned with the treatment. Examples messages sent to treated subjects in a "much worse" district on day 2 of the experiment are

i) In your LC5, the auditor found issues with 120 million UGX from its budget of 19 billion UGX. This is much worse than in other districts.
ii) This means that 6.3 out of 1,000 UGX in your LC5 budget had issues. In most LC5s, 2.2 out of 1,000 UGX had issues. Your LC5 did much worse than average.
iii) One reason your LC5 did much worse than average is that a bid for borehole construction included unexplained expenditures.

Subjects not assigned to the treatment received placebo messages about the value of good personal financial management. This placebo design helped us isolate the effect of novel information from any increased salience of budgeting. Providing useful placebo information also helped avoid unequal attrition across conditions (see *SI Appendix*, Tables S8 and S9). Examples messages sent to control subjects in a "much worse" district on day 2 of the experiment are

i) Research suggests that households which open a savings account are better able to save for school books and fees.
ii) When youth learn to save and manage their money well, they often have higher savings and income later on in life.

Additional examples and screenshots are provided in *SI Appendix*, Fig. S1 and Table S1.

The study was logistically complex, nationally representative, and a significant increase in scale from most prior studies on information and accountability. Just before district elections, we sent a total of 207,940 text messages in 11 languages to 16,083 subjects in 762 villages dispersed among 27 districts. Because both chairs and councillors share budget-management responsibilities, the texts appeared relevant for voter evaluations.*

To understand subjects' experiences with the text messages, we designed an additional survey and recontacted a random sample of 100 treated respondents after end line. Eighty-seven percent remembered receiving the texts, of whom 64% reported reading them fully and 33% in part. Seventy-four percent found them easy to understand and distinguish from other messages.

Voters may be more politically engaged when they are able to coordinate within social networks (29, 30). As a second experimental treatment, we therefore varied treatment density at 80% and 20% between village-pair blocks with at least 15 subjects. In the high-density condition, we also informed voters that many others in their village had received the treatment. We expected to observe stronger treatment effects and evidence of information sharing in the high-density condition.

Sample. We used a nationally representative sample of 27 out of 111 district councils, selected for a concurrent education audit by Twaweza. Within each sampled district, Twaweza chose 30 villages randomly in proportion to population, except Kampala, where 60 villages were chosen to account for greater population. In 762 accessible sample villages, enumerators recruited 31,310 individuals using gatherings and door-to-door visits. Recruitment involved substantial on-the-ground effort, since preexisting contact lists were unavailable. We were able to recontact 16,083 subjects via phone to confirm participation and administer a baseline survey. These subjects formed our experimental sample.

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*A concurrent, fully crossed treatment probed the effects of public-service quality. No interaction effect occurred between crossed treatments, as discussed in *SI Appendix, Pre-Registration*.

SI Appendix contains a detailed description of sampling procedures, reasons for exclusion, a CONSORT diagram (*SI Appendix, Fig. S3*), a map of the sample (*SI Appendix, Fig. S2*), and subjects' characteristics (*SI Appendix, Tables S6 and S7 and Figs. S10 and S11*).

We used the 2015 Afrobarometer survey to compare our experimental sample to the Ugandan population. In both samples, the median age was 33. However, our recruitment of people with mobile phones skewed the sample toward men and better educated individuals (31). If well-educated subjects are also more politically sophisticated, it may be more difficult to change their minds and our treatment effects may reflect lower bounds on population effects. However, better educated subjects might more easily understand new information, implying that these effects might not scale fully to voters without mobile phones.

Outcome of Interest. We measure vote choice using answers to questions from an endline survey. Self-reported voting measures may introduce biases, such as those related to social desirability and strategic misrepresentation. To combat such biases, previous studies have randomly assigned interventions at the level of precincts and measured aggregate support for given candidates using official returns (e.g., ref. 3).

We instead used a postelection survey for multiple reasons. First, given ethical considerations, we wished to avoid affecting election outcomes. Thus, we treated at a low density within constituencies, preventing the detection of effects in aggregate returns. Second, we valued the ability to account for individual characteristics like priors in our analysis, and this aligns with the goals of the Metaketa I initiative of which this study was a part (9). Third, studying aggregate-level outcomes necessitates cluster-randomized designs, which would dramatically decrease statistical power to identify the relatively small anticipated effects. Fourth, reports indicated that precinct-level results for district offices might not be released or might be unreliable. Fifth, survey-based data are often used to study vote choice in both experimental and observational studies (32, 33). Nevertheless, relying on reported vote choice introduced the possibility that subjects did not truthfully report behavior. Although the results from our endline survey correlate well with the official returns (see *SI Appendix, Fig. S5*), we used multiple approaches to detect possible misreporting.

First, we asked respondents about the basin color in their polling station and checked for internal consistency of responses.[†] Second, due to social desirability, respondents may have overreported voting for incumbents who performed well on budgets and underreported voting for incumbents who performed poorly. Yet this possibility is inconsistent with our finding that information affected reported votes for councillors but not for chairs. Moreover, under the reasonable assumption that the treatment and placebo groups were equally likely to overreport votes for the ruling party, this dynamic would not have altered estimated effects. Our check for reporting bias toward the election winner did not show any differences between responses conducted before and after the release of official results (*SI Appendix, Fig. S4*). Additional checks show that reported vote choice is not conditional on perceptions of our partner (*SI Appendix, Table S2*) or expectations about privacy (*SI Appendix, Table S3*).

Findings

Turnout. The treatment could have influenced which citizens reported voting (3, 11), so we first consider how information affected reported turnout. The treatment had no detectable average effect on turnout (*SI Appendix, Table S10*). We further show that treatment effects reported below on votes for the incumbent are robust to including a lack of turnout as a nonpositive outcome (*SI Appendix, Table S11*). Further explanation is in *SI Appendix, Turnout and Estimates of Vote Choice*.

Vote Choice. We expected that when citizens received good (bad) news about politician performance, they would be more (less) likely to vote for incumbents. Like other Metaketa I studies (9), we examine the effects of receiving good news and bad news separately. The subsets of subjects are defined by whether the information that voters were eligible to receive was better or worse than what they believed at baseline or, if the budget information matched their pretreatment beliefs, whether it was above or below the median district performance (see *SI Appendix, Table S5* for analysis without priors). A detailed accounting of the match between this analysis and our preanalysis plan is in *SI Appendix, Pre-Registration*.

Fig. 1 displays the core results on reported vote choice and confidence intervals on the estimated treatment effects among subjects who reported turning out and who voted in elections without party switching or redistricting. This is to minimize ambiguities to the definition of incumbency (see *SI Appendix, Tables S18–S21* for robustness).

For subjects eligible for good news, treated subjects were no more likely to report voting for the incumbent *chair* than placebo subjects. In contrast, treated subjects were ∼3% points more likely to report voting for the incumbent councillor than the placebo subjects.[‡] Likewise in the other direction, treated subjects eligible for bad news were not less likely to report voting for the incumbent chair, but they were around 3% points less likely to report voting for the incumbent councillor. In an augmented analysis, we find the effects were larger among verified respondents—subjects who said that they had seen our messages—and in the range of 5–6% points. *SI Appendix, Tables S12 and S13* show the treatment effect by party and voter–incumbent alignment.

Information might not have affected votes for chairs because they represent larger constituencies and, unlike councillors, are not the primary contacts for constituents. Incentives for performance-based voting are often greatest in small constituencies because voters can more easily coordinate (2). Councillors' smaller constituencies and their role as primary contacts at the district level might be the reason why citizens hold councillors responsible for budget management, despite chairs' greater role in principle. Thus, our findings on citizens' reward or punishment of councillors are consistent with literature on attribution in consolidated democracies (34, 35).

Additionally, elections for chair involve higher profile campaigns that disseminate information via rallies, flyers, and public media. Indeed, independent audits assess chairs as communicating better with their constituents than councillors (25). This dynamic could decrease voters' likelihood of reacting to new budget information and increase chairs' ability to diffuse responsibility for budget mismanagement. Also, chairs evince more stable tenure, perhaps due to increased resources for vote buying. Voters may thus feel less able to impact chairs' elections and be more likely to rely on ethnicity or party as a coordination

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[†]Seventy-seven percent of responses were consistent with the modal color selected by respondents in a village. The nonmodal answers may also happen due to difference in polling station assignment or the presence of multiple basins. The percentage correct does not vary meaningfully between treatment and control, or between good and bad news-eligible subjects (*SI Appendix, Fig. S6*).

[‡]Estimates for good news attenuate to 2.3% points under a weighted estimator that accounts for potential heterogeneity in treatment effects across randomization blocks (*SI Appendix, Table S25*). Across robustness checks, there is more uncertainty about good news estimates than bad news estimates.
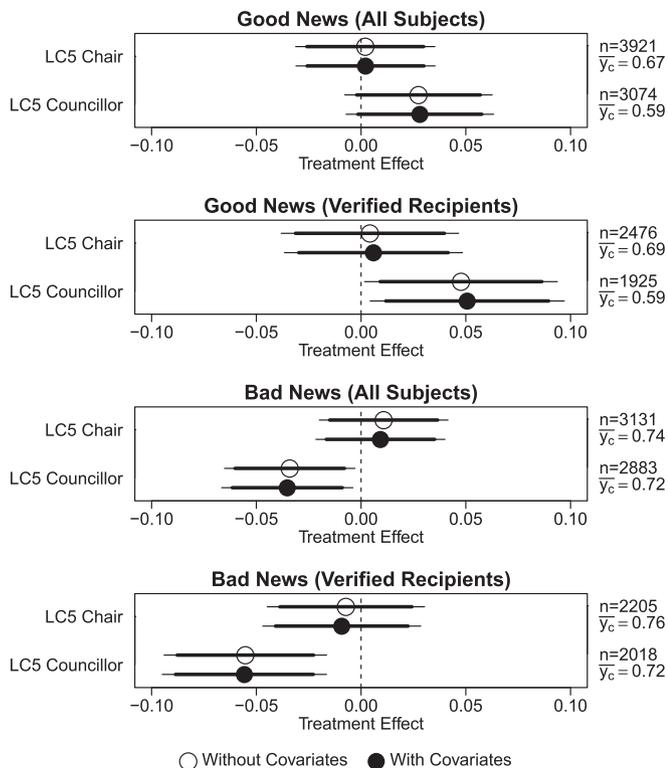
Fig. 1. Treatment effects of budget disclosures by news type. The figure displays estimated treatment effects for all subjects and for respondents who reported seeing messages in the endline survey. Subgroup sample sizes and control group means are printed at right. All estimates exclude uncontested elections, elections with party-switching incumbents, and redistricted constituencies. Thick and thin bars show 90% and 95% confidence intervals, respectively. Full results are in *SI Appendix*, Tables S14 and S15.

device. Our treatments may therefore have been too weak to overcome barriers to coordination or to compete with other sources of information in the chairs' campaigns.

Treatment Density. We expected larger treatment effects among those in high-density villages compared with low-density villages. Yet as Fig. 2 shows, we did not find evidence inconsistent with the null hypothesis for treatment density.

There is, however, some evidence of information spillover. Table 1 shows the crossed impacts of the density treatment and the budget information treatment combined into one indicator with four levels. The baseline condition is a placebo subject in a low-density village. For placebo subjects in high-density villages, there is some evidence of negative spillover in the bad news condition, since they are less likely to vote for the incumbent than placebo subjects in the corresponding low-density villages. The treatment effects of bad news are similarly sized within low- and high-density subgroups. Given the indications of spillover to the placebo subjects, these results indicate that a 9% reduction in votes for the incumbent is the upper bound on the treatment effect of bad news disseminated at high density. These results provide some support for our hypothesis that higher treatment density encourages communication and coordination. The good news subgroup, however, does not yield results that are inconsistent with the null hypothesis.

The mixed effects of encouraging common knowledge to overcome coordination problems are largely consistent with the findings of related experiments. An experiment in Kenya showed that privately disseminated information about children's education outcomes did not affect parents' private or public actions, and

the researchers hypothesized that publicly disseminated information would have also yielded null effects unless it raised expectations of collective action (7). Another study found an impact of information on vote choice only when many others were receiving the same information and when voters were provided civic education messages (36).

Discussion

Political accountability depends on voters' gaining and responding to information about the performance of elected officials. Our findings suggest that factual information about budget management increased voter support for councillors when performance was above expectations and decreased support for councillors when performance was below expectations but had no effects for chairs inconsistent with the null hypothesis. Our mixed results partly contrast with related studies that have found null or even negative effects of information, including the Metaketa initiative of which this study formed a part (7, 9, 11–14, 37). The results also contrast with a parallel, factorial intervention focused on information regarding road, water, education, and health services at the village level for subcounty elections, which evinced null results on vote choice—perhaps indicating the challenges of attribution for local services (38). The predicted, positive effects of budget information on vote choice for district councillors may reflect the combined influence of private SMS texts with news on corruption and high statistical power.

While we intentionally treated at low density to avoid changing electoral outcomes, we estimate that between 16 and 39 out of 890 contested LC5 councillor elections could have flipped in a hypothetical scenario in which all voters were treated with information and assuming homogeneous treatment effects (see *SI Appendix*, Figs. S12 and S13). Although the information environment is continually changing, access to factual information on budgets by SMS text that politicians could not directly counter promoted electoral accountability for some local offices.

However, we did not find that voters responded to new information when voting for chairs. This unexpected result may indicate that information on corruption is less effective at promoting accountability in higher profile elections, possibly because there is more information available or because factors like ethnicity or party identification are more salient. In keeping with this intuition, some of the most promising findings about information campaigns come from more local elections (2, 5), whereas the
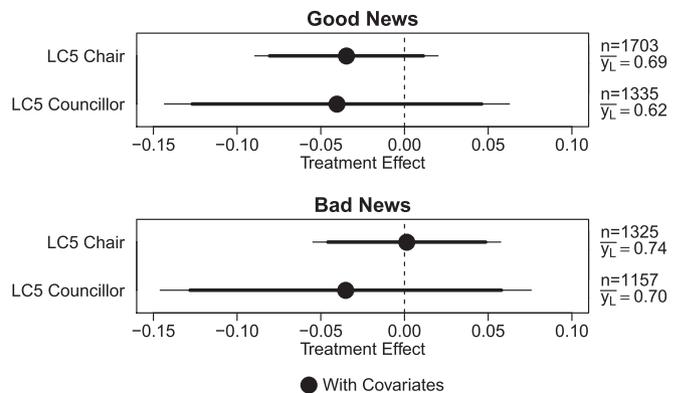

Fig. 2. The treatment effect of higher treatment density among treated subjects. The figure displays estimated treatment effects of being assigned to a high-density village among treated subjects. Subgroup sample sizes and control group mean are printed at right. All estimates exclude uncontested elections, elections with party-switching incumbents, and redistricted constituencies. Thick and thin bars show 90% and 95% confidence intervals, respectively. Full results are in Table 1.

Table 1. Effects of budget treatment and treatment density

Condition	Good news		Bad news	
	LC5 Chair (1)	LC5 Councillor (2)	LC5 Chair (3)	LC5 Councillor (4)
Treated, low density (RI)	0.023	0.012	−0.027	−0.054
	(0.026)	(0.030)	(0.025)	(0.028)
	$P = 0.182$	$P = 0.351$	$P = 0.141$	$P = 0.027$
Control, high density (RI)	0.004	−0.028	−0.034	−0.044
	(0.029)	(0.050)	(0.029)	(0.055)
	$P = 0.450$	$P = 0.718$	$P = 0.124$	$P = 0.208$
Treated, high density (RI)	−0.010	−0.020	−0.028	−0.091
	(0.021)	(0.045)	(0.021)	(0.050)
	$P = 0.691$	$P = 0.669$	$P = 0.091$	$P = 0.035$
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	0.211	0.244	0.286	0.264

Dependent variable: Vote choice for the incumbent. Standard errors were derived from randomization inference (RI). One-tailed tests were performed in the direction of the hypothesized relationship. Sample only includes subjects eligible for the density treatment (≥ 15 respondents per village). Statistical models are numbered 1–4 as indicated in column headings.

effects of information campaigns for higher level offices even in Uganda have been disappointing (13).

Future work could examine information campaigns across different levels of elections and vary the amount of information voters must process. Our hypothesis about information saturation could be directly tested by mixing treatment information with additional information that is either relevant or irrelevant for electoral accountability. A further possibility is that information campaigns should be accompanied by civic education that emphasizes the importance of programmatic voting and clarifies politicians' responsibilities (36).

Finally, we note that despite our use of mobile technology, subjects were engaged in our study. We had uptake and treatment effects similar to those associated with more traditional methods of information dissemination, such as public-service broadcasts, leaflets, or door-to-door canvassing as cited above. Yet assembling and disseminating information via text messaging is simpler and cheaper for the many civil society organizations—such as Twaweza—that have already collected such contact information as part of their programing. Thus, the costs of information campaigns such as the one in our study might reasonably be borne by community-based groups in the future (see *SI Appendix*, Table S22 for examples). These opportunities will increase as more people own mobile phones.

Materials and Methods

Measurement. Local enumerators conducted baseline and endline phone surveys. The baseline survey occurred in early January 2016, before treatment assignment. Enumerators collected information about subjects' demographics, partisanship, prior voting, intended participation in the elections, political knowledge, interest in budget information, and beliefs about incumbents' performance.

To measure priors, we asked respondents at baseline to compare their LC5's record of managing its budget expenditures and contracting to other districts in Uganda. Using these priors, we divided respondents into good and bad news subgroups based upon whether their priors were more positive or negative than the true value from the council audit. For instance, if a respondent believed that his or her council was doing "better" than other councils but the council had in fact done worse than others in the

audit, then the respondent would be in the bad-news subgroup. We provide more details on the coding of priors in *SI Appendix*, show the distribution of prior and true values in *SI Appendix*, Figs. S7–S9, and display robustness of estimates to unsure priors in *SI Appendix*, Table S4.

We conducted an endline survey immediately following the district elections. We were able to recontact more than 80% of the experimental sample at endline, with attrition balanced across experimental groups (see *SI Appendix*, Tables S8 and S9 and Fig. S11). Approximately 10% of the endline surveys were completed before the election results were announced, with subjects contacted in a random order.

Defining incumbency was complicated because 19% of chairs and 30% of councillors in our sample switched parties, which was unanticipated in our preanalysis plan. In many cases, party switchers had lost a primary and ran as independents, raising the question of whether the candidate or party should be considered the incumbent. In the main results, we exclude elections where the incumbent switched parties and ran for re-election, which seemed the most reasonable adjustment best aligned with prespecified theoretical goals. *SI Appendix* shows different effects of treatment for party-switching incumbents (*SI Appendix*, Table S18). We explore the implications of different exclusion criteria and incumbency definitions in further analyses: (1) the subset of constituencies where incumbents ran as a member of their party from the 2011 elections (*SI Appendix*, Table S19); (2) all observations, with incumbency defined as the party elected to the relevant office in 2011 (*SI Appendix*, Table S20); and (3) all observations, with incumbency defined as the potentially updated party of all individuals who ran for re-election in 2016 (*SI Appendix*, Table S21). Treatment effects for good news are larger for (1) and (2) and smaller for (3), while treatment effects attenuate slightly for bad news with all alternative definitions of incumbency. Inconsistency with the null hypothesis varies by specification.

Treatment Density. Within each district, villages were partitioned into those that had at least 15 subjects and those that did not. For villages with at least 15 subjects, we created paired blocks of villages with similar numbers of subjects. Within each block, we assigned one village to have 80% of subjects treated and the other to have 20% of subjects treated. In villages where 80% of subjects were treated, the messages also informed subjects that "we are going to be sending you and many of your neighbors information."

Estimation. Information can represent either good or bad news depending on respondents' prior beliefs. Thus, as preregistered and coordinated in Metaketa I, we split our sample into two subsets for analysis defined by whether individuals were eligible to receive good or bad news. We define P_i as each subject i's pretreatment belief about the quality of the incumbent candidate j, where the true value is Q_j. Additionally, we define $\widehat{Q}$ as the median value of quality for incumbents in all other districts k. We then define the subset of subjects eligible for good news (L^+) as all subjects where $P_i < Q_j$ or where $P_i = Q_j$ and $Q_j \geq \widehat{Q}$. We define the subset of subjects eligible for bad news (L^-) as all subjects where $P_i > Q_j$ or where $P_i = Q_j$ and $Q_j < \widehat{Q}$.

To estimate the effect of good news, we collapse all types of good news into a single treatment indicator T_i^+, which equals 1 when the subject i is treated and is part of the relevant subset L^+. For example, the good news subgroup includes subjects who thought budget discrepancies were much worse than average at baseline but were eligible to receive information that they were only "a little worse." Likewise, we collapse all types of bad news into an indicator that equals 1 when subject i is treated and part of the subset L^-.

Our primary estimating equation is Eq. **1**, which in this case 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 stated that he or she intended to vote for the incumbent party during the baseline survey; β is a vector of estimated coefficients; $\mathbf{Z}_i$ is a matrix of prespecified, pretreatment covariates; ν_j is a village fixed effect; and ϵ_{jh} is the error term clustered by politician (j).

$$y_{ij,t=1} = \alpha + \tau_1 T_{ik}^+ + \varphi y_{ij,t=0} + \beta \mathbf{Z}_i + \nu_j + \epsilon_j. \qquad [1]$$

To estimate the conditional effect of the good news and bad news treatments based on treatment density, we use the modified estimating equation in Eq. **2**, which includes a density treatment indicator D_j assigned at the village level. Because the density treatment is assigned at the village level, we use a paired-village fixed effect b_j for blocks to mirror our assignment strategy:

$$y_{ij,t=1} = \alpha + \tau_1 T_{ik}^+ + \tau_2 D_j + \tau_3 T_{ik}^+ D_j + \varphi y_{ij,t=0} + \beta Z_i + b_j + \epsilon_j. \qquad [2]$$

By using the pretreatment intention measure as a predictor variable, we deviate slightly from our preregistered equations. In the prespecified strategy, we use the difference between intended and realized voting outcomes as the dependent variable. The results from that strategy are nearly the same (see *SI Appendix*, Tables S16 and S17). All standard errors for treatment effects in the main text are standard deviations of the randomization distribution created by assuming a sharp null hypothesis and recording values that would have been realized under 10,000 iterations of random assignment. All *p* values are one-sided in the direction of the hypothesized relationship. The adjusted models include the following preregistered covariates: perception of living conditions, gender, education, age, trust in information from Twaweza, perception that powerful people will learn about vote choice, perception that vote counting will be fair, and voted for incumbent in 2011. Additional analyses in *SI Appendix* cover manipulation checks (*SI Appendix*, Fig. S14 and Table S23); multiple, joint, and pooled testing (*SI Appendix*, Table S24); and fixed-effects weights (*SI Appendix*, Table S25).

Data and Preanalysis Plan. Buntaine's Dataverse page provides replication data and code. Preregistration materials filed with Evidence in Governance and Politics (EGAP, 20151119AA) include an initial plan before baseline, a revised plan before treatment, and an amendment before analysis.

This project was approved by the University of California, Santa Barbara (UCSB) Human Subjects Committee (15-0690); institutional review boards (IRBs) at Brigham Young University (15381), William and Mary (2015-09-10-10589), and Temple (via IRB Authorization Agreement); the London School of Economics and Political Science Research Ethics Committee; the Uganda Mildmay Research Ethics Committee (0309-2015); the Uganda National Council for Science and Technology (SS 3943); and the Ugandan Office of the President (ADM 154/212/03).

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