AIDDATA
A Research Lab at William & Mary

WORKING PAPER 41

June 2017

Escaping the Valley of Disengagement: Two Field Experiments on Motivating Citizens to Monitor Public Goods

Mark T. Buntaine
University California, Santa Barbara

Daniel L. Nielson
Brigham Young University

Jacob T. Skaggs
University California, Santa Barbara

Abstract

Governments cannot provide public goods effectively when they lack information about their delivery. Citizens, for their part, experience deficient or absent public services, but they lack incentives to provide monitoring when they do not expect governments to be responsive to their concerns. Over time, this reinforcing cycle creates what we term the valley of disengagement. We investigate how to activate and sustain citizen engagement in governance given the challenges posed by this vicious cycle. In two field experiments implemented in Kampala, Uganda, we recruited citizens to report on solid waste services to the municipal government. We find that neighbors? and leaders? nominations of reporters and public announcements about reporters? activity do not increase citizen monitoring. However, government responsiveness to reporters boosts participation over several months, highlighting the critical role of timely and targeted responsiveness by governments for sustaining citizen engagement in governance.

Author Information

Mark T. Buntaine
University California, Santa Barbara
buntaine@bren.ucsb.edu

Daniel L. Nielson
Brigham Young University
dan_nielson@byu.edu

Jacob T. Skaggs
University California, Santa Barbara
jskaggs@bren.ucsb.edu

Acknowledgments

The authors are grateful to Polycarp Komakech, Immaculate Apio Ayado and Catherine Tabingwa for contributions to the design and implementation of this research. This project has been carried out in partnership with the Kampala Capital City Authority, and we gratefully acknowledge the support and participation of Charles Herbert, Josephine Kitaka, James Semuwemba, Martin Ssekajja, Frank Batungwa Tumusiime, and Judith Tukahirwa. Experiment 1 was supported by AidData at the College of William and Mary and the U.S. Agency for International Development (USAID) Global Development Lab through cooperative agreement AID-OAA-A-12-00096. The views expressed here do not necessarily reflect the views of AidData, USAID, or the United States Government. Experiment 2 was supported by the Hellman Family Foundation through a fellowship to MB. All activities described in this paper received approval from the University of California, Santa Barbara Human Subjects Committee (protocol ESMS-BU-MA-031), the Uganda Mildmay Research Ethics Committee (protocol 0706-2015), and the Uganda National Council for Science and Technology (protocol SS 3840) and the Uganda Office of the President (ref: ADM/154/212/03). We pre-registered the hypotheses and our plans for testing them at the Evidence in Governance and Politics registry (20151103AA). The authors received helpful comments on previous versions of this paper from Matt Potoski, seminar participants at the University of California, Santa Barbara, and conference participants at the 2nd Annual Conference on Environmental Politics and Governance in Gerzensee, Switzerland and the 2016 Earth Systems Governance Conference in Nairobi, Kenya. The author contributions are as follows: MB is lead author. MB designed the research, with DN; JS managed implementation of the research, with MB; MB conducted the analysis; MB wrote the paper, with JS and DN.

Contents

1. Introduction

Governments often lack good information about where services should be extended, where existing public works are failing, and where contractors or government employees are shirking. These information problems contribute to the substandard provision of public goods. From their direct experience, citizens hold information about absent or deficient services, but when they do not expect a response from government, they lack incentives to share this information. We thus identify a key problem – the *valley of disengagement* – involved with initiating and sustaining citizen monitoring of governance. When citizens do not expect government to be responsive to their concerns, they have little incentive to provide useful information. Lacking data and public support, governments cannot easily improve services where they are in greatest demand, which over time reinforces distrust and disengagement and limits accountability.

We investigate how citizens can be motivated to provide information to governments in pursuit of public goods in light of this dilemma. In theory, governments could invest in self-monitoring systems, but it is usually more efficient to rely on reports from residents as a co-production strategy (Parks et al. 1981; Ostrom 1996), especially in the age of widely available information technologies. After all, citizens already possess the necessary information. We thus extend to the mass populace McCubbins' and Schwartz's seminal contribution about fire-alarm oversight (1984). Citizens can trip alarms that warn officials about problems with public services. The key is getting enough citizens to share their information in ways that take advantage of co-production potential.

We theorize that citizens will share information they possess about public goods and services when they have sufficiently positive beliefs about the responsiveness of government. Additionally, we hypothesize that neighbors' and leaders' nominations of monitors and public recognition of citizens' monitoring activities will increase engagement by identifying and motivating reporters who place a high value on public goods. We present a simple decision-theoretic model that captures how citizens' beliefs about the responsiveness of governments drives their participation in monitoring. The model highlights the importance for participatory governance of providing opportunities for citizens to positively update their beliefs about the responsiveness of government.

We test our theory in two pre-registered field experiments conducted in close partnership with the Kampala Capital City Authority (KCCA) related to the monitoring of solid waste services. We prompted citizens of Kampala, Uganda to send reports over a number of months to the KCCA about the management of solid waste in their neighborhoods. Solid waste is a major challenge in Kampala, with only a minority of waste entering the formal waste stream (Kinobe et al. 2015). Most trash is burned or discarded into informal dump sites. A large majority of Kampala residents are personally concerned with the poor provision of waste services, as revealed in our baseline survey. The KCCA would like to improve solid waste services but lacks efficient ways to collect information about the locations where service delivery is substandard – information that citizens possess. The main outcome of interest in this study is the initial and sustained reporting of citizens about solid waste services in their neighborhoods. The outcome measure is the actual, on-topic reports of citizens sent to the KCCA from mobile phones.

We first test whether nomination of reporters by neighbors, nomination by community leaders, and leaders' public recognition of monitors increases reporting. Other studies have found that individuals' prosocial tendencies and non-financial rewards have a greater impact on prosocial behaviors than financial rewards (Ashraf et al. 2014). Yet, it is not clear whether community networks can be leveraged to select prosocial individuals through nomination and/or offer sufficient non-financial rewards like social recognition to encourage the sustained engagement of citizens in governance. Previous research on networks mostly tracks how the resources offered by network connections predict engagement (Berardo and Scholz 2010), rather than how networks can be actively leveraged to select and motivate citizens to participate in governance. Related work on political participation indicates that recruiting citizens based on social connections is often effective (Brady et al. 1999). We investigate whether nominations and recognition within neighborhoods yield similar effects related to participatory governance.

To preview our results, even though we saw higher rates of reporting than any any other citizen reporting platform of which we are aware in Uganda or elsewhere (e.g., Blaschke et al. 2013; Grossman et al. 2014), with approximately 20% of reporters sending reports during our study period, we did not find strong evidence that any of the recruitment or announcement conditions increased short-term or long-term engagement by citizen monitors. This is good news for policy, suggesting that

costly recruitment and social motivation treatments do not produce more engagement by citizens.

We also experimentally treated some citizen reporters with responsiveness by the KCCA to test our prediction that rapid, timely, and targeted responsiveness is key to activating and sustaining citizen engagement in governance. Reporters from neighborhoods in the responsiveness treatment received a weekly, targeted announcement about how their reports were translated into official action plans and used to improve solid waste collection in Kampala, allowing us to test core theoretical propositions about how trust in government and beliefs in responsiveness drive engagement (e.g., Sandström et al. 2014).

We find that government responsiveness significantly boosts engagement of reporters over months, as measured by actionable and usable reports. The effects of responsiveness become larger over time, corroborating our theory that citizen monitors are continuously updating their beliefs about whether government is listening to and acting on their demands. Building responsiveness into governance arrangements might significantly increase participation by citizens in improving the delivery of public goods. However, an endline survey that we fielded five weeks after the reporting period did not reveal increased trust in government or satisfaction with services among treated reporters, suggesting either that trust in government is hard to move or that the broader effects of responsiveness are short-lived.

The results of these field experiments are particularly significant given that information and communication technologies (ICT) improve prospects for low-cost, targeted, and timely responsiveness by governments around the world. Indeed, finding ways to engage citizens in governance is vitally important across a wide range of government activities. Community policing depends on building trust between citizens and the police and thereby improving information necessary to act on crime (Brogden and Nijhar 2005). Education is enhanced by involving parents in the local administration of schools and monitoring of teachers (Duflo et al. 2015). Water user boards augment the collaborative management of water resources (Berkes 2009). However, in places where the capacity of government is low and the management of public services is poor, building responsive relationships with citizens is especially difficult. Citizens do not engage because they perceive governments to be unresponsive. And governments cannot improve public services because they lack information about citizens' demands.

By harnessing the strengths of new modes of citizen-government interaction to foster the sharing of information and responsiveness, it may be possible to escape the valley of disengagement in a variety of settings. Governments around the world are building platforms to collect information from citizens to improve the provision of public services (Smith and Reilley 2013). Some evidence suggests that ICT can broaden public participation in governance (Grossman et al. 2014). Yet other efforts have failed to activate and sustain high-levels of participation (Evans & Campos 2013; McGee and Carlitz 2013; Grossman et al. 2015; Grossman et al. 2016). Our results show that building these new ICT platforms is not enough to sustain citizen engagement in governance; citizens need to understand how their effort is rewarded in terms of responses from public officials.

2. Background and Theory

Information that voters convey at the polls does not guarantee responsive governance. The binary nature of election returns fails to convey what public goods and services are demanded and where they are absent or deficient. Before the many goods and services that citizens demand from government are actually delivered, long chains of delegation between citizens, politicians, bureaucrats, and contractors organized into multiple layers must be formed and moved to action. Agent slack and slippage are likely at every link in the chain, and so extensive information must be gathered on the behavior and resulting outcomes of agents at each stage in order for governments to effectively deliver public goods and services. Monitoring and oversight from the top down can prove very expensive (Kiewiet and McCubbins 1991), so bottom-up information is often sought as a low-cost and efficient solution to learn how frontline government units and their contractors are performing (McCubbins and Schwartz 1984). The information requirements for good governance are especially intense at the local level, where in most countries thousands of local governments provide goods and services to millions of people. Citizens possess critical information about the status of public goods and services, but they need to be motivated to provide information to the responsible government office.

If citizens do not believe that there will be a response to the information that they could provide, then they will not have incentives to engage in monitoring. Indeed, the record of transparency and

accountability initiatives that involve citizen monitoring of public goods provision is mixed (for recent reviews, see Joshi 2013; Fox 2015). Insofar as there is any consensus about the reasons why initiatives to solicit information and participation from citizens do not always improve service delivery, it is that citizens lack ways to motivate service providers to act on the information they offer (Banerjee and Duflo 2006, 124; Banerjee et al. 2004; Olken 2007), indicating that believing government will be unresponsive is correct in many settings. In contrast, studies about citizen monitoring in settings where rewards and punishment mechanisms are available to citizen monitors – whether through social pressure on providers (Bjorkman and Svensson 2009) or by enhancing enforcement by government (Caseley 2006) – have found citizen monitoring to be effective at improving public services.

In a recent reflection on this body of mixed evidence, Fox (2015) offers the important critique that many studies of citizen monitoring and government accountability are "tactical" rather than "strategic" – that is, they consider mostly how to get information flowing in one direction, rather than strengthening feedback loops between citizen monitors and governments. As Mansuri and Rao (2013) highlight in another review, the longer term success of social accountability schemes depends on governments using their abilities in sanctioning and oversight to act on the information gained through citizen engagement. However, considering the strategic context, where public services are substandard and, as a consequence, trust in government is low, the prior beliefs of most citizens will likely be that government is not responsive to their concerns. This will tend to decrease input of monitoring by citizens.

Consider the simple illustration of a government agency and k citizens C_1, C_2, …, C_k who might contribute to the supply of public goods by monitoring frontline providers. In this setting, each citizen has uncertain beliefs about whether government is *responsive* or *nonresponsive* to reports that they could submit. We denote the true probability that government will respond to a report by θ. Each citizen makes a decision about whether to report on public services (R_{kt}) as a function of their belief about the responsiveness of government at a given point in time $p_{kt}(\theta)$, the value that the individual places on a potential governmental response including prosocial considerations v_k, and the cost of the reporting c_k. Each citizen will report at time t if:

$$R_{kt} = \begin{cases} 1 & \text{if } p_{kt}(\theta)v_k > c_k \\ 0 & \text{if } p_{kt}(\theta)v_k \leq c_k \end{cases} \quad \textbf{(1)}$$

Before proceeding to simulate how this belief and incentive structure drives the dynamics of citizen engagement in governance, we consider each of the component parts and their place in existing theory:

2.1 Belief in Responsiveness of Government ($p_{kt}(\theta)$)

Even if government is responsive, citizens may fail to observe this and attribute proper credit. Low prior expectations worsen this dilemma. Citizens' combined experiences and beliefs thus often result in disengagement, which of course will provide few opportunities for updating. This vicious cycle of disengagement is poorly understood. In one of the few relevant studies, Tolbert and Mossberger show that citizens who interact online with governments in the United States generally have higher trust in government, perhaps through "interaction[s] with officials that are convenient and quick, potentially enhancing responsiveness" (2006, 357). They report increased citizen satisfaction after visits to government websites, but the mechanisms behind this effect are not well-identified, selection effects are a major concern, and ways that governments could foster positive beliefs are not explored.

In settings where institutional arrangements and political conditions make governments responsive, the core challenge is cultivating citizen beliefs about responsiveness that match the actual level of responsiveness from government. Closer to the setting of our own study, Grossman et al. (2016) ask how citizens can be motivated to report deficiencies in public services via SMS text to local politicians in Uganda. Low levels of perceived efficacy among citizens who experience service deficiencies creates significant challenges for participation. As an experimental treatment, the researchers sent messages to subjects from local officials encouraging reporting on deficient public services and find that the rate of ever-participation – citizens that use the platform at least once over a six-month period – rises from approximately 3.4 percent in control to 4.7 percent in treatment.

We take this idea further by experimentally informing citizen reporters, on a weekly basis, exactly what

the governmental agency receiving their reports is doing in response. Our responsiveness treatment included the KCCA's making weekly action plans for the mobilization and oversight of contractors in specific zones, organizing systematic zone-wide clean-ups, and engaging in new public outreach campaigns. Our treatment is designed to directly increase citizen beliefs about government responsiveness and therefore to perhaps increase "external efficacy." Our theory further predicts that we should observe greater effects of responsiveness as reporters are active for longer periods, since over time there will be more divergence between the beliefs of reporters who do and do not experience responsiveness.

Citizens who place a greater value on public goods should be more likely to act in ways that will lead to their production. Two factors should initiate and sustain citizen engagement: (1) attracting the participation of individuals who value a public good more highly and (2) raising the salience of the public good for the community. Indeed, past research consistently indicates that individuals with relatively high prosocial motivations undertake the bulk of online participation in governance (Budhathoki and Haythornthwaite 2013; Chandler and Kapelner 2013; Brabham 2009; Blaschke et al. 2013). While research on the role of networks – the pre-existing set of relationships and ties between members of a community – in governance finds that networks create opportunities for engagement by citizens and civil society (Berardo and Scholz 2010), there is little research that deals with actively leveraging social and community networks to select and incent participation in governance.

We consider two ways that community networks might be used to increase reporting. First, we expect that reporters selected through nomination by neighbors or community leaders will place a higher value on public goods than randomly-recruited reporters. As Brady et al. (1999) theorize, people who are closer to prospective participants in public affairs have advantages both in selection and motivation. Lab-in-field experiments in Uganda show that individuals with strong group attachments and community leadership positions display greater prosocial behavior in dictator games (Baldassarri and Grossman 2013). Because individuals who make nominations can maximize public goods by naming reporters with prosocial tendencies and leadership attributes, we expect that nomination will enhance the provision of citizen monitoring. Yet existing research on referrals and nominations have mainly been studied in labor markets where private rather than public-goods incentives dominate (Fafchamps et al. 2015; Beaman and Magruder 2012). Closest to the present study, Kim et al. (2014)

find that using "friend nominations" to select community members to distribute coupons for subsidized health-related goods results in higher uptake compared to relying on randomly-selected individuals or individuals with the most social ties. However, questions persist regarding both causal mechanisms and sustainability over time. Second, we expect that public announcements can motivate reporters to be more active. Previous work has found that non-financial rewards are more effective at motivating prosocial behavior than financial rewards (Ashraf et al. 2014). Providing potential actors with recognition for their contributions to collective goods may thus increase prosocial behavior (Karlan and McConnell 2014).

Across a variety of places and settings, "opportunity structure" influences citizen participation (Stevenson and Greenberg 2000; Leifeld and Schneider 2012; Vráblíková 2014), and costs to citizens undermine their engagement in public life (e.g., Speer 2012). Information technologies can thus alter the opportunity structure, primarily by substantially decreasing the costs of sharing and processing data relevant to public life (Oates 2003; Grossman et al. 2014; McGuire 2006; Charalabdis et al. 2012; Linders 2012; Rotberg & Aker 2013). Unlike engagement in governance via traditional means, which often involves significant time and costs for citizens, mobile phones allow for instantaneous and, in many deployments, toll-free access to public officials.

Considering the model above and its component parts, it is straightforward that decreasing the cost of reporting will encourage more engagement in governance. What is missing and more interesting is the time path of $p_{kt}(\theta)$ as each reporter perceives responsiveness or a lack thereof to their engagement. Consider a standard Bayesian updating model where the prior $p_{kt}(\theta)$ takes the form of a beta distribution, where $p(\theta) = \theta^{\alpha-1}(1-\theta)^{\beta-1}$. In each period t, if and only if a reporter submits a report, then they will have the opportunity to perceive with error whether government responds to the report.[1] More formally, after submitting a report, the citizen views the outcome of a Bernoulli trial screened by an error function $\varepsilon(\theta)$, which might not have an expected value of θ, such as when any response is not easy to attribute to the government or if the government is able to gain positive credit even though it is not responsive. We now have the machinery to simulate the reporting behavior of citizens over time.

[1] In our experiments, each period is 2-3 days, since reporters were sent prompts for reports 2-3 times per week. In principle, a period can be any interval where it is possible to submit monitoring *and* receive a response.

Consider a set of citizen reporters who have various prior beliefs about the responsiveness of government, when the true value of θ indicates that government is in fact the responsive type.[2] In the case of a government that has difficulty quickly enacting a response to public reports, we use the simple adjustment where $\varepsilon(\theta)$ is the outcome of the Bernoulli trial with probability θ, filtered by some number of periods of delay in the opportunity to observe responsiveness to reports. Of course, $\varepsilon(\theta)$ may take on a variety of forms that we do not attempt to fully anticipate here, but a delayed or unobserved response is perhaps the most typical type of error process that citizens experience when they interact with government. Recall that once the value of $p_{kt}(\theta)$ drops below a certain level, reporters will not submit reports and will therefore not have the chance to update their beliefs about responsiveness. This is the *valley of disengagement*: once reporters have sufficiently low beliefs in responsiveness, they will not update their beliefs about responsiveness because they will cease to provide monitoring. To avoid falling into this valley, it is necessary to either find monitors who place a great value on the public service or provide opportunities for monitors to positively update their beliefs that government is listening and responding to their concerns.

If reporters are selected who place higher value on the public good or if social motivation can raise the value of the public good, the value of $p_{kt}(\theta)$ will be lower before a reporter is deactivated. Figure 1 shows simulated paths of the mean value of $p_{kt}(\theta)$ at each period, which is equal to the posterior value of the previous period after taking into account the result of $\varepsilon(\theta)$, which in this case delays the observation of any response. In the left column of Figure 1, no selection process is present and no social benefits are added, which raises the value of $p_{kt}(\theta)$ needed to sustain reporting as compared to the right column, where the selection of prosocial reporters and the presence of social benefits lower the value of $p_{kt}(\theta)$ needed to sustain reporting, resulting in more reporting. If it is possible to identify monitors who place a higher value on the service in question, fewer will fall into the valley of disengagement.

[2] In the simulations below, we set $\theta = 0.8$, $c_k = 1$, draw the starting beliefs of reporters $p_{k,t=0}(\theta)$ randomly from a uniform distribution of mean values [0.1,0.9], and draw the value placed on the public good v_k from a uniform distribution of [1,5] for the case with no social motivation and [3,7] for the case with social motivation.

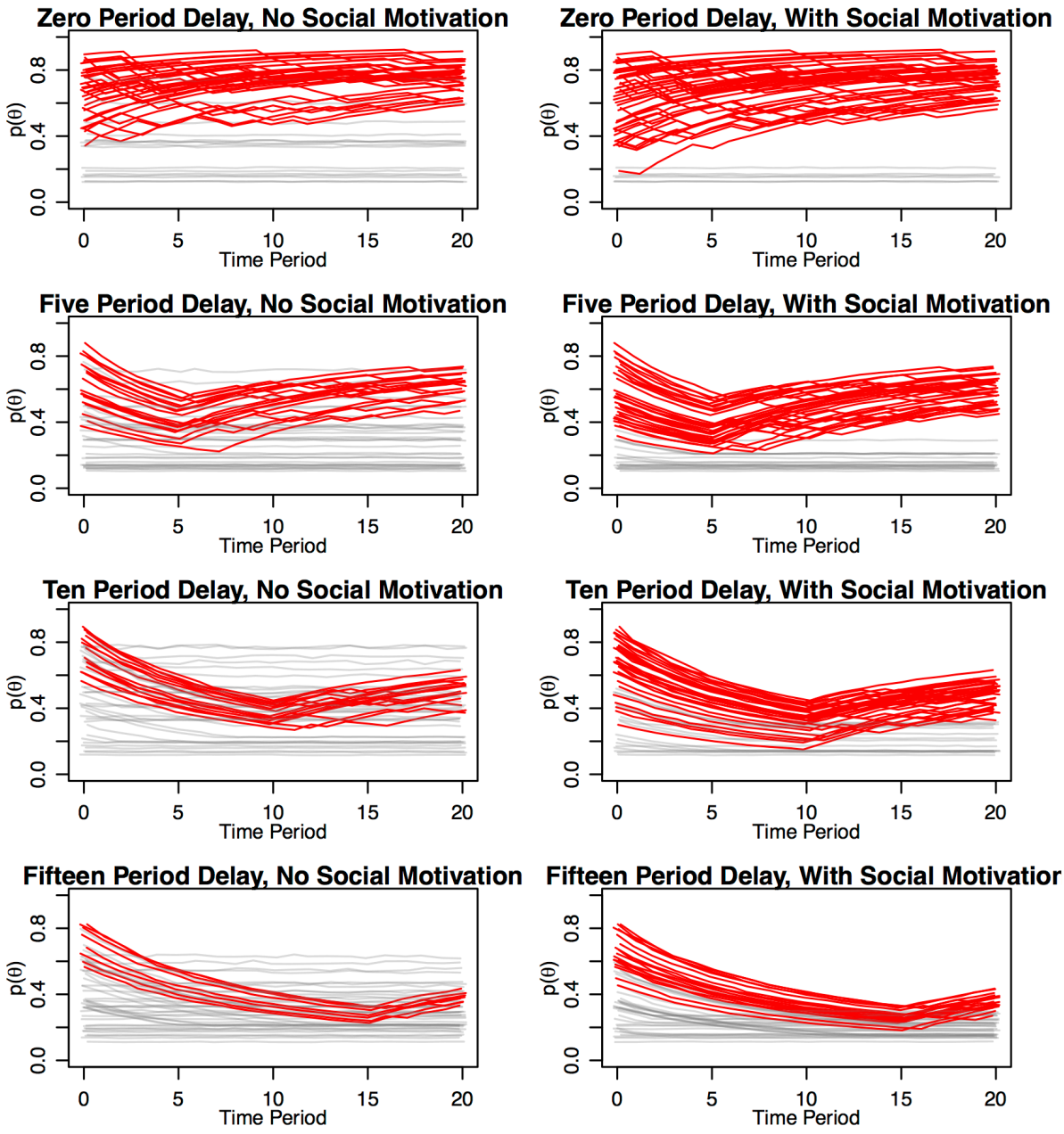

Figure 1. The posterior beliefs about the responsiveness of government when observations about responses to citizen reports are delayed by the number of periods indicated. Red lines indicate reporters who do not fall below the activation threshold during the reporting period and grey lines indicate reporters who fall below the activation threshold.

As displayed in Figure 1, any delay in observing the response of government to citizen reports also decreases posterior beliefs that government is the responsive type. The greater the delay between reports and response, the more reporters fall into the valley of disengagement. This means that only

citizens with very high prior beliefs about the responsiveness of government will persist in reporting (red simulated paths), while all others will fall below their activation threshold and fail to update further (grey simulated paths). Furthermore, a comparison of the top and bottom rows of Figure 1 highlight how the beliefs of reporters who do and do not experience rapid responsiveness display greater divergence over time. These dynamics illustrate the critical role that targeted responsiveness is likely to play in keeping beliefs about responsiveness high enough among at least some citizens to sustain engagement.

2.2 Pre-Registered Hypotheses

Based on the expectation that nomination can enhance the provision of reporting about solid waste services by raising the value of the public good (v_k, H1-H3) or by enhancing beliefs about the responsiveness of government ($p_{kt}(\theta)$, H4), we pre-registered the following hypotheses prior to randomly assigning experimental conditions or collecting any data (SI, Appendix D contains the exact wording of pre-registered hypotheses, which are shortened here for readability):

H1: Nomination by neighbors will increase reporting.

H2: Nomination by the local council chair will increase reporting.

H3: Announcement by the local council chair about the reporters will increase reporting.

H4: Responsiveness to citizen reports will increase reporting.

3. Experimental Design

We designed and carried out two randomized field experiments to understand whether community networks and government responsiveness can initiate and sustain participation by citizens in the governance of public services. In particular, the treatments that we employ are meant to raise either beliefs about responsiveness ($p_{kt}(\theta)$) or the value of the public good (v_k) among reporters who might engage in governance. We focus on citizen reporting about solid waste management, which generates

high levels of citizen concern, with 90% of residents in our study area personally concerned with the state of solid waste management as of 2014 (see SI, Appendix A for results of a pre-experimental survey).

Kampala, Uganda faces similar problems of monitoring and accountability for solid waste management as many other parts of the world (Bhuiyan 2010; Okot-Okumu and Nyenje 2011). Private companies contracted to remove solid waste often provide services of lower quality to groups of people that are not able to share monitoring information (Oteng-Ababio et al. 2010; Katusiimeh et al. 2012). Since most of Kampala is contracted to private collectors, city managers find themselves in a challenging position, especially given information asymmetries, pressures toward corruption, and wealth disparities across communities.

Our close partner in this project, the Kampala Capital City Authority (KCCA), has prioritized improving solid waste management to boost resident satisfaction and promote public health. Kampala is also one of the key strongholds of opposition support in Uganda, and the nationalized KCCA has a strong political mandate from the ruling party to improve resident satisfaction with government services. Additionally, the KCCA has been supported by international donors for more than a decade to improve waste management, but still finds it difficult to engage the public in actionable ways. Despite having used public resources to develop an interactive SMS platform and a mobile application to exchange information with citizens, the KCCA struggles to use its technological investments to exchange useful information with the public. They now seek to understand whether mobile technologies can enhance public engagement and encourage more accountable provision of public services.

In Phase 1, we recruited 1034 citizen reporters from a sample of 90 administrative zones to provide feedback on solid waste removal services and disposal practices at the spatial scale of neighborhoods.[3] In November 2015, our team of enumerators carried out a recruitment drive over a period of two weeks to form our experimental sample of reporters. The KCCA provided us with a list of

[3] One of the 90 zones was dropped from the sample due to a failure to conduct recruitment activities by the field team as assigned. Two of the zones are duplicates due to an error in the administrative files received from the KCCA that was discovered only after the project was out of the field. For analysis, the duplicate zone is considered two separate zones as this was how we allocated treatment assignment.

all zones (LC I) inside the capital city jurisdiction of Kampala and the associated shapefiles outlining their boundaries. At the time of the first experiment, there were a total of 755 zones (LC I) contained within 97 parishes (LC III) and 5 divisions used to manage waste services. We randomly selected 90 zones for our experimental sample. We dropped 11 zones from the original sample because they were demolished, lacked residencies, or gated communities that barred access. We replaced these 11 zones with another random sample to form the final experimental sample.

After selecting the experimental sample, we randomly assigned each zone to one of two reporter recruitment conditions using complete randomization (Figure 2, Panel A). In each zone, we then aimed to recruit 12 citizen reporters according to the recruitment condition assigned at the zone level, a process which yielded 1,034 unique reporters (see SI, Appendix B for detailed recruitment protocols):

(**Recruitment Baseline**) *Random Citizen recruitment*: Following a random walk pattern, the enumeration team approached adults walking or sitting outside of their homes or businesses and asked whether they would sign up to be a reporter.

(**Recruitment Treatment**) *Neighbor Nomination recruitment:* Following a random walk pattern, the enumeration team approached adults walking or sitting outside of their homes or businesses and asked whether they could nominate a "trustworthy and responsible" individual who lives in the zone to report on behalf of its residents. If the individual indicated willingness to make a nomination, the enumerator asked the citizen to make a face-to-face introduction to the nominated individual. This nominated individual was then asked whether they would sign up to be a reporter.

All reporters in this study were fully informed that the data they provided would be received by the KCCA without revealing the identity (mobile phone number) of any reporter, to avoid any concern that reporters would feel coerced into reporting. Over a 7-week period following the recruitment drive, all citizen reporters received prompts from the KCCA's interactive SMS messaging system in the same way. In Phase 1, reporters received a total of 17 prompts for information about waste pick-up schedules, waste burning practices in their zone, and the locations of waste piles that needed special attention by the KCCA or its contractors (See SI, Appendix C for a list of prompts). To make reporting

free for reporters, we sent an airtime credit initially to all reporters and then also sent replenishment credit each week to the phones of all reporters who submitted at least one response that week. To further encourage reporting, we held a lottery for a ~$10 prize in airtime each week for reporters in both the baseline and treatment condition in a uniform way. Further details and justification for the implementation procedures are contained in our publicly-available pre-analysis plan (EGAP design 20151103AA).

3.1 Phase 2 Experimental Design

In June 2016, our team of enumerators recruited an additional 1,905 reporters from 97 randomly selected administrative zones or local councils (LC I), again dropping five zones where research was impossible and replacing with six new random selections. In each zone, we aimed to recruit 20 reporters. Each zone was divided into four cells of roughly similar geographic size and five individuals were recruited to be reporters from each cell. Reporters were required to be adult residents of the zone and the primary user of their own mobile phone. The zones did not overlap with the Phase 1 sample (Figure 2).

With the zone as the unit of randomization, each Phase 2 zone was assigned one of eight different treatment combinations based on a three--arm experimental design. Two arms were recruitment and announcement conditions (Figure 2, Panel B). The third arm was responsiveness of government to citizen reports and was applied to both Phase 1 and Phase 2 zones (Figure 2, Panel C). The content of these treatments are as follows:

(**Arm 1, Recruitment Baseline**) *Random Citizen recruitment*: Following a random walk pattern, the enumeration team approached adults walking or sitting outside of their homes or businesses and asked whether they would sign up to be a reporter. This condition follows exactly the protocol from Phase 1 and serves as the baseline condition.

(**Arm 1, Recruitment Treatment**) *LC1 Nomination recruitment:* Reporters in these zones were recruited by the local council chairperson (LC1) or a delegated zone--level authority figure if the chairperson was not available. We chose LC1s to select citizen monitors because they are

typically well-connected with community members and able to select reporters willing to volunteer on behalf of the community. LC1s nominated reporters by introducing them to the recruitment team.

(**Arm 2, Announcement Treatment**) *Announcement of Reporters by LC1:* Reporters in these zones were informed that the LC1 would announce the citizen monitoring program and the names of reporters at an upcoming zone--wide meeting. After all 20 reporters were recruited, a list of the names of selected citizen monitors and information on the program were left with the LC1. The implementation team contacted LC1s by phone one week following the completion of the recruitment activity to remind the LC1s to make the announcement at a community meeting.[4] If the LC1 was not present during recruitment, we contact the LC1 by phone that day to inform him/her about the monitoring program and our request that they make an announcement about reporters at an upcoming zone meeting. Zones not assigned to treatment were assigned to a control condition where the LC1 was not requested to make an announcement.[5]

(**Arm 3, Responsiveness Treatment**) *Responsiveness from the KCCA:* We sent reporters in these zones weekly personalized text messages informing them that their responses had been sent to the KCCA Waste Management team and communicated to reporters the KCCA's action plans made on the basis of reports. In later weeks of the reporting phase, the KCCA took action to address solid waste based on reports, which may have been observable to reporters. In some weeks, we sent information listing the number of responses that individual reporters sent and the total number of responses by all citizen monitors in the reporter's zone, along with

[4] We collected data on compliance with the announcement treatment and found that only 38% of the community leaders in zones who were assigned to this condition and who we were able to contact at endline delivered the announcement treatment. The reporters in these zones still expected a community announcement, since they were fully informed about the upcoming announcement during recruitment, so we still consider them to have been treated. In SI Appendix G, we estimate complier average causal effects for the announcement treatment by 2SLS.

[5] All reporters who were recruited in zones assigned to the announcement condition were fully informed at recruitment that their names would be announced at a community meeting and could decline the invitation to participate. Additionally, the LC1 only announced that reporters had agreed to report on behalf of the community and never had access to the reports or aggregate data on the number of reports submitted. For ethical reasons, we did not risk subjecting reporters to social punishment for low levels of reporting and instead think of this treatment as allowing reporters to gain recognition from their community for reporting on behalf of the zone. This procedure was approved by both international and local review boards.

an offer to answer questions.[6] Any questions were answered during a call center held each week.

In this arm, program representative also contacted subjects one month after the start of the reporting period through voice calls in which they discussed the quantity of the subject's responses, reminded them of objectives and expected results of reporting, and explained how the reports were being used to improve waste management. Both active and inactive reporters received the responsiveness outreach. Reporters in zones assigned to control did not receive any messages or phone calls about what the KCCA was doing with their reports. For zones in the control condition, the KCCA asked only to receive a digest of reports at the end of the reporting period and did not respond weekly to reports.

The responsiveness treatment has several components, all of which were included to raise the belief of reporters that the government was receiving, processing, and responding to the reports that were submitted (an accurate belief in our setting). Responsiveness requires that government is attentive to the reports of monitors and that citizens know that government is attentive. We are not able to parse contributions of the different components of responsiveness, but like many field experiments conducted with organizational partners, our treatment was designed to maximize the chances of detecting an effect of an omnibus responsiveness treatment. This is a necessary first step in probing the effects of government responsiveness before parsing individual mechanisms.

[6] In SI Appendix H, we show that rates of reporting are unconditional on the number of reports that the subjects were informed as being received throughout the zone.

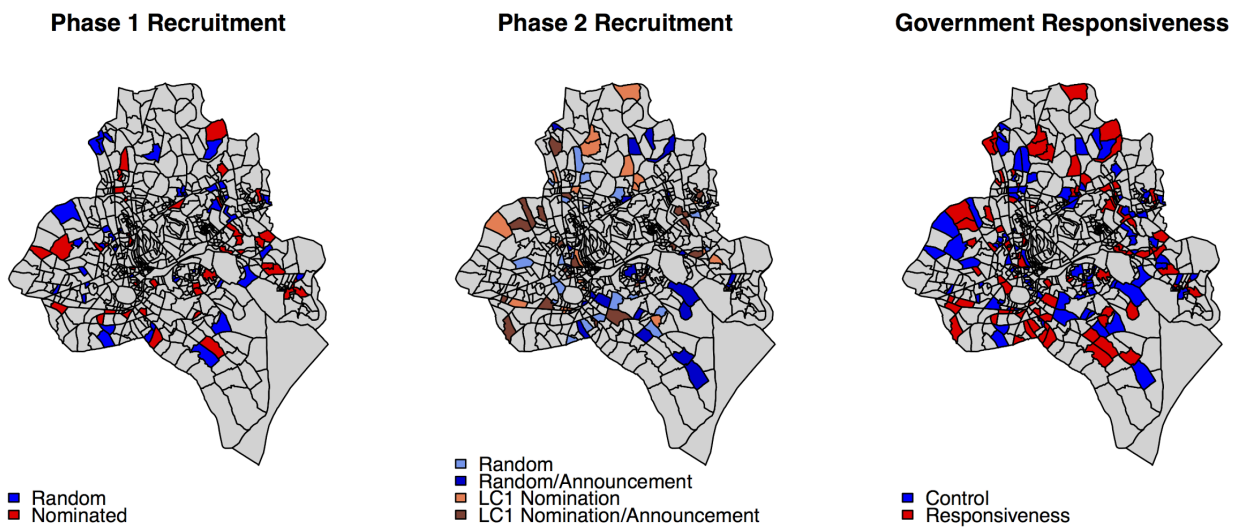

Figure 2. Final sample of zones within the jurisdiction of the Kampala Capital City Authority for Phases 1 and 2 recruitment drives and the combined responsiveness treatment condition. This is the final random sample, after replacing zones that did not contain residences or that were inaccessible.

During the 8-week Phase 2 reporting period between July 2 and August 29, 2016, all subjects recruited during Phase 1 and Phase 2 were sent 15 prompts to supply reports. The questions we asked reporters were based on information that the KCCA identified as most useful in monitoring the quality of services provided by its waste management contractor. Prompts included general questions about zone-level waste conditions and the quality, frequency, and proximity of waste collection services provided to the zone, along with several open-ended questions (See SI Appendix C for the list of prompts used in Phase 2). As in Phase 1, we encouraged reporters to answer prompts by running a lottery each week for ~$10 in airtime for all reporters in all treatment conditions in a uniform way. Five weeks after the end of the Phase 2 reporting period, we implemented a short survey to understand whether responsiveness increased trust in government and satisfaction with waste services, which would indicate longer-lasting shifts in more general attitudes as a result of responsiveness.

Summary of experimental design and conditions

There was a break of almost six months between the two phases when no prompts were sent to reporters from Phase 1. Figure 3 summarizes the combined design of the two experiments described above and displayed in Figure 2.

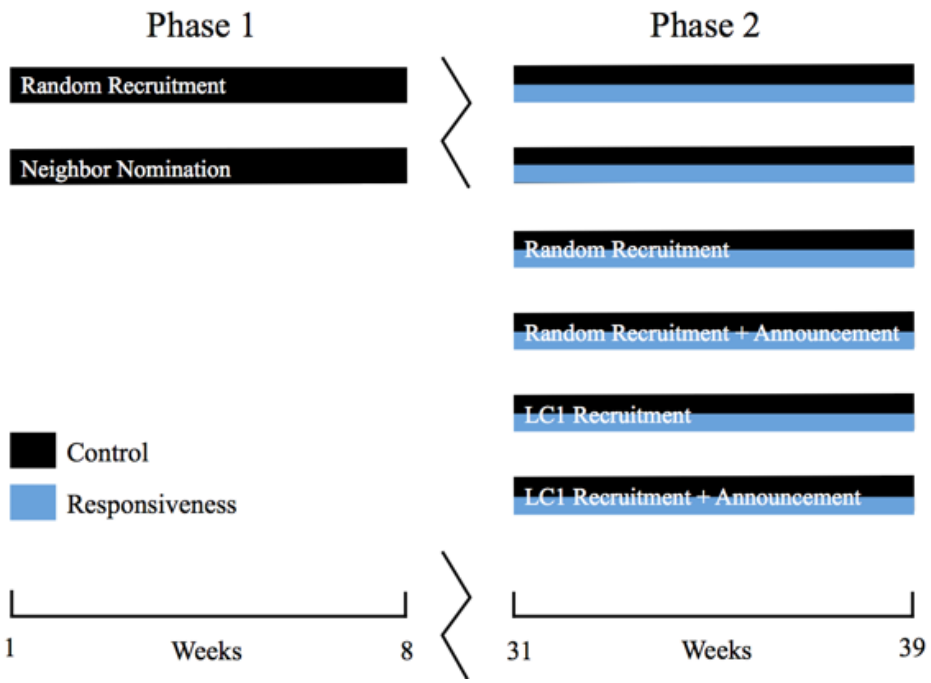

Figure 3. Diagram of experimental design for both Phases 1 and 2

As pre-registered, we measure reporting as follows: (1) The total number of active reporters (i.e., those submitting at least one report) during the reporting period; (2) The total number of reports submitted by each reporter during the reporting period; (3) The total number of reports submitted by each reporter during the last two weeks of the reporting period; and (4) The total number of open-ended reports (e.g. descriptions of location of piles) submitted by each reporter during the reporting period.

3.2 Descriptive Data on Reporters

The reporters in our study are likely to be fairly representative of Kampala residents, since many of the recruitment conditions began with random walks in randomly selected zones around the city. It may be the case that the nomination process produced reporters of a different type on observable characteristics, but we do not find strong evidence for this possibility (Table 1). The only notable exception is that LC1 nomination produced reporters with longer average periods of residence in the zone than did any of the other recruitment conditions. In order to avoid Hawthorne effects, the reporters were asked only to provide brief, non-sensitive information for intake into the KCCA reporting system, rather than a full survey of demographic and attitudinal responses that would have

required a different informed consent process for research subjects. All reporters were fully informed that the platform was being operated and tested with the KCCA.

Table 1. Characteristics of reporters in both Phases

Phase 1	Random Recruitment	Neighbor Nomination
Years in zone (mean)	9.24	9.15
Female (proportion)	0.39	0.45
Age (mean)	30.2	30.8
Satisfied with waste services (proportion)	0.28	0.32
Phase 2	Random Recruitment	LC1 Nomination
Years in zone (mean)	11.0	15.2
Female (proportion)	0.62	0.65
Age (mean)	32.4	36.0
Satisfied with waste services (proportion)	0.36	0.36

4. Analytical Methods

As we pre-registered, we performed hypothesis tests via randomization inference for difference in means between experimental conditions. We assume the sharp null hypothesis (no unit-level treatment effects) such that $Y_i(1) = Y_i(0)$ for all zones or reporters where $Y_i(1)$ is the potential outcome if assigned to nomination and $Y_i(0)$ is the potential outcome if assigned to random recruitment. We then generate 5,000 iterations of our exact clustered randomization procedure and capture the sampling distribution of treatment effects observed under the sharp null. We compare the observed difference in the value of interest between treatment conditions and compare that value to the sampling distribution to compute a *p*-value of how often such a difference would be observed by random chance. For the Phase 2 analysis, because of the ease of reporting on multiple crossed treatment arms, we estimate the effects of treatment at the reporter-level via OLS regression. We have confirmed that

the substantive and statistical significance of all effects are robust to the pre-registered difference-in-means specifications. Appendix F contains the same Phase 2 results with analysis performed at the zone level. We also observed significant non-compliance with the *LC1 Announcement* treatment in Phase 2, prompting us to estimate complier average causal effects as a robustness check on the intent-to-treat results reported below (see SI Appendix G). In no case does this change the substantive or statistical significance of the main results. We also consider, but do not find, evidence for spatial spillover of the Responsiveness treatment across zones (see SI Appendix J).

5. Findings: Phase 1

In the first experiment, we find marginal evidence that nomination boosts rates of report (Figure 4). In total, we received 493 SMS reports that were on-topic and contained information relevant to solid waste management. We see that reporters assigned to nomination submitted more reports over the entire study period as a point estimate, but this value is not highly inconsistent with random chance (Panel A; *te*=0.027, p=0.14). If we instead compare the mean number of responses per reporter by assigned recruitment condition, we find that nominated reporters submitted an average of 0.536 reports, while randomly recruited reporters submitted an average of 0.420 reports, which is again not highly inconsistent with random chance (Panel B; *te* = 0.115, p=0.14). Finally, if we consider how many times reporters responded to open-ended prompts for the locations of trash piles, potentially the most costly type of reporting in terms of effort, we see higher rates of reporting, but not so high that the rate is inconsistent with random chance (Panel C; *te*=0.020, p=0.13). Together, these results are suggestive, but not conclusive of the potential impact of nomination. Thus, we attempt to strengthen the nomination treatment and increase the sample size in the Phase 2 design.

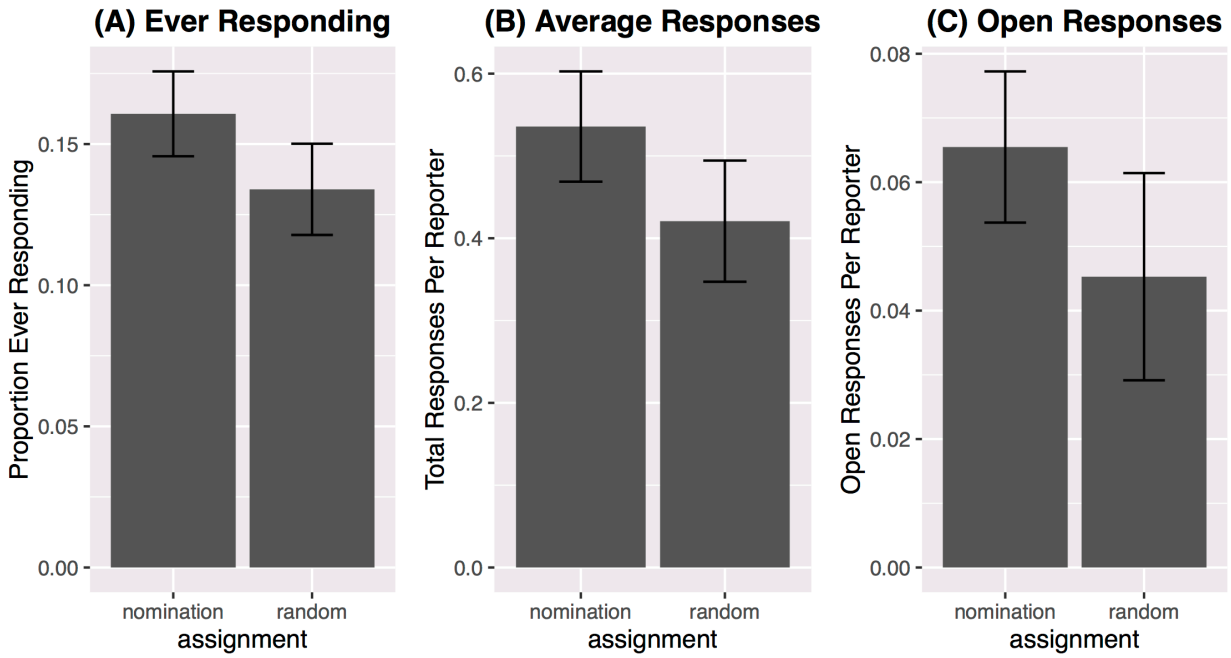

Figure 4. Reporting by recruitment condition during Phase 1. (A) Proportion of reporters who submitted at least one report by recruitment condition; (B) Average number of total reports per reporter by recruitment condition; (C) Average number of open-end reports per reporter on the location of waste piles by recruitment condition. No significant differences in reporting between recruitment conditions identified. All panels display one standard error bars.

6. Findings: Phase 2

In the second experiment, we examine the same three outcomes as a function of the three recruitment and treatment conditions. During Phase 2, we received 6,166 SMS reports that were on-topic and contained information relevant to solid waste management. We report results both for the pooled group of subjects recruited during Phase 1 and 2, as well as the results split by the recruitment phase. Considering first the number of reporters during Phase 2 who submitted at least one, on-topic report about solid waste management during the eight-week period, only the *Responsiveness* condition boosts participation (Table 2). Reporters recruited during Phase 1 from a zone assigned to the responsiveness condition are 50% more likely to be active during Phase 2 than reporters in control zones. Reporters recruited during Phase 2 from a zone assigned to the *Responsiveness* condition are 14% more likely to be active than reporters in control zones. This result indicates that hearing about what the government is doing with the reports can help initiate and sustain engagement in citizen reporting. In contrast, we do not observe any differences in the number of active reporters when

recruiting by either neighbor or LC1 nomination, or when reporters expected the LC1 chairperson to make an announcement about reporters' names at a community meeting. Thus, the evidence suggests that nominations and announcements are not effective at activating reporting on public services in this context, which is good news for policymakers who do not want to needlessly spend extra resources on recruitment and social motivation.

Table 2. Total number of active reporters during Phase 2

	DV: At Least One Report During Phase 2		
	(Pooled)	(P1 Reporters)	(P2 Reporters)
Responsiveness	0.065***	0.096***	0.048**
	(0.017)	(0.027)	(0.023)
Neighbor Nomination	-0.002	-0.002	
	(0.029)	(0.027)	
LC1 Nomination	-0.007		-0.007
	(0.022)		(0.023)
LC1 Announcement	0.028		0.027
	(0.022)		(0.023)
Phase 2	0.133***		
	(0.028)		
Intercept	0.075	0.192***	0.351***
	(0.046)	(0.023)	(0.023)
Observations	2,866	1,021	1,845
F Statistic	16.001***	6.470***	1.955
Note: one-tailed tests		*p<0.1; **p<0.05; ***p<0.01	

Turning to the total number of reports made by each reporter during the 8-week Phase 2 reporting period, we find very similar results, with only the responsiveness treatment driving more reports (Table 3). Pooling zones across recruiting periods, we find that the *Responsiveness* treatment increased the average number of reports per reporter by approximately 0.4 over eight weeks. This result is largely driven by the significant effect that the *Responsiveness* treatment had on treated Phase 1 reporters, among whom the *Responsiveness* treatment increased the number of total reports per

reporter by 83%. In contrast, the *Responsiveness* treatment did not increase the total number of reports by Phase 2 reporters in ways that are highly inconsistent with random chance (for P2 Reporters model, p=0.12). Like the results for active reporters, we do not observe any differences in the number of reports per reporter when recruiting was done by either neighbor or LC1 nomination, or when reporters expected the LC1 to make an announcement about the platform and reporters' names at a community meeting.

Table 3. Total number of reports submitted by each reporter during Phase 2

	DV: Total Number of Reports During Phase 2		
	(Pooled)	(P1 Reporters)	(P2 Reporters)
Responsiveness	0.431***	0.789***	0.232
	(0.144)	(0.200)	(0.194)
Neighbor Nomination	0.002	0.005	
	(0.241)	(0.200)	
LC1 Nomination	0.136		0.129
	(0.179)		(0.194)
LC1 Announcement	0.076		0.073
	(0.179)		(0.194)
Phase 2	1.027***		
	(0.228)		
Intercept	0.107	0.952***	2.272***
	(0.378)	(0.174)	(0.197)
Observations	2,866	1,021	1,845
F Statistic	13.526***	7.773***	0.654
Note: one-tailed tests		*p<0.1; **p<0.05; ***p<0.01	

Finally, we consider the total number of reports by each reporter during the last two weeks of the 8-week reporting period. As pre-registered, we are interested not only in the total effects of the *Responsiveness* treatment and the recruitment conditions, but also whether social motivation or government responsiveness can drive longer-term engagement in the collaborative management of public services. Like previous estimations, we fail to reject the null that any recruitment condition or

that the announcement about reporting by local leadership significantly increased reporting during the last two weeks of Phase 2. We do find, however, that responsiveness from government has a significant and positive effect. The *Responsiveness* treatment boosted reporting by Phase 1 reporters 123% and boosted reporting by Phase 2 reporters 32%. This result highlights how responsiveness is necessary to sustain engagement, even if it is not a predictor of initial engagement. Indeed, our theory predicts greater treatment effects as time elapses, as the beliefs of treated and control subjects diverge.

Table 4. Total number of reports submitted by each reporter during the last two weeks of Phase 2

	DV: Total Number of Reports During Last Two Weeks of Phase 2		
	(Pooled)	(P1 Reporters)	(P2 Reporters)
Responsiveness	0.091^{***}	0.119^{***}	0.075^{**}
	(0.026)	(0.035)	(0.036)
Neighbor Nomination	-0.005	-0.004	
	(0.044)	(0.035)	
LC1 Nomination	0.012		0.011
	(0.033)		(0.036)
LC1 Announcement	-0.007		-0.007
	(0.033)		(0.036)
Phase 2	0.113^{***}		
	(0.042)		
Intercept	-0.002	0.097^{***}	0.233^{***}
	(0.069)	(0.031)	(0.036)
Observations	2,866	1,021	1,845
F Statistic	6.273^{***}	5.661^{***}	1.503
Note: one-tailed tests			*p<0.1; **p<0.05; ***p<0.01

To aid the interpretation of this key finding, Figure 3 shows the proportion of reporters who submitted solid waste reports in response to each of the 15 prompts during the Phase 2 reporting period. The effect of the responsiveness treatment is most pronounced at the end of the reporting period when pooling all reporters. For reporters recruited during Phase 1, responsiveness to reports was critical for

boosting reporting throughout the reporting period. In SI Appendix E, we show that responsiveness boosted engagement both for reporters who were active during both halves of the Phase 1 reporting period and for reporters who became deactivated, indicating that responsiveness can both keep and bring citizens out of the valley of disengagement. For reporters recruited during Phase 2, responsiveness to reports only boosted reporting for the second half of the reporting period, which is consistent with our theoretical predictions. Future research might fruitfully disentangle which components of the omnibus treatment contribute most to the treatment effects that we estimate.

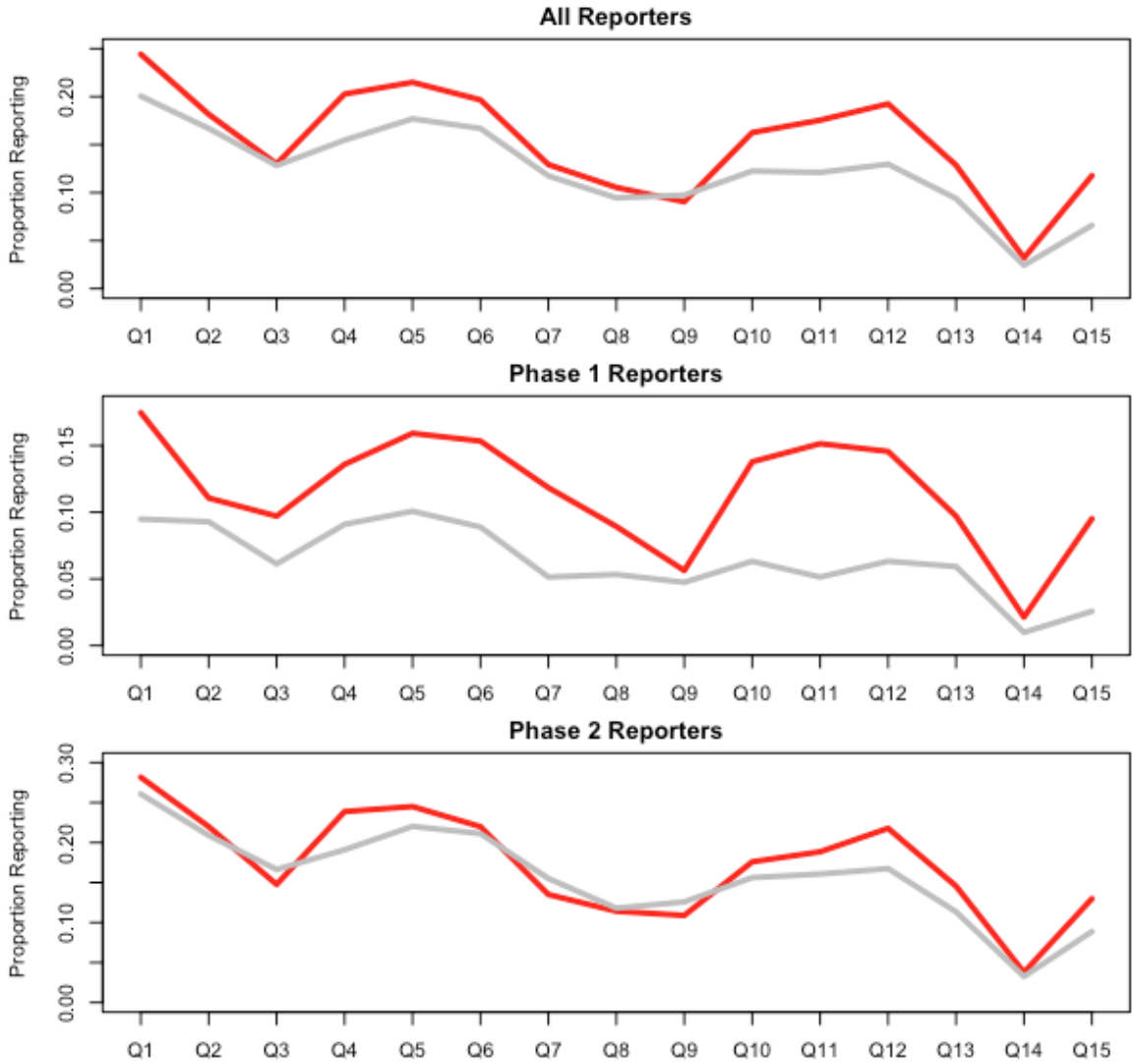

Figure 3. Proportion of reporters responding to each prompt during Phase 2 broken out by phase of recruitment. *Legend:* **red** is reporters assigned to the responsiveness condition, **grey** is reporters assigned to the control condition for responsiveness.

7. Attitudinal and Behavioral Outcomes on Trust in Government

To test the proposition that responsiveness from government increases reporter beliefs that government is responsive to their concerns, we fielded a post-reporting survey to measure reporters' trust in government and their behavioral willingness to help the KCCA manage services apart from solid waste. This survey (instrument available in SI Appendix I), administered five weeks after the Phase 2 reporting period ended, was intended to measure behavioral spillover from experiencing responsiveness from government in other areas of citizen engagement and to assess whether the treatment changed broader attitudes.[7] While responsiveness strongly influenced week-to-week reporting, it appears from the survey data that this effect quickly wears off and does not have long-term implications for attitudes about government and willingness to volunteer time to help government test and create processes for citizen engagement (Figure 4). In no case did volunteers randomly assigned to the *Responsiveness* treatment hold significantly more favorable attitudes about public services or government, measured by stated satisfaction with solid waste services, perceptions of KCCA responsiveness, and trust in government. Likewise, when reporters were asked to volunteer their time to help the KCCA develop and test a more general reporting platform for citizen monitoring across a range of public services, treated reporters were no more likely to volunteer either before or after an SMS reminder. These results intimate that deeper attitudes related to trust in government and willingness to assist future efforts are either difficult to move or the effects of interventions are short-lived.

[7] We find no evidence of differential attrition in the endline survey by the Responsiveness treatment condition (Chi-Squared test, p=0.55).

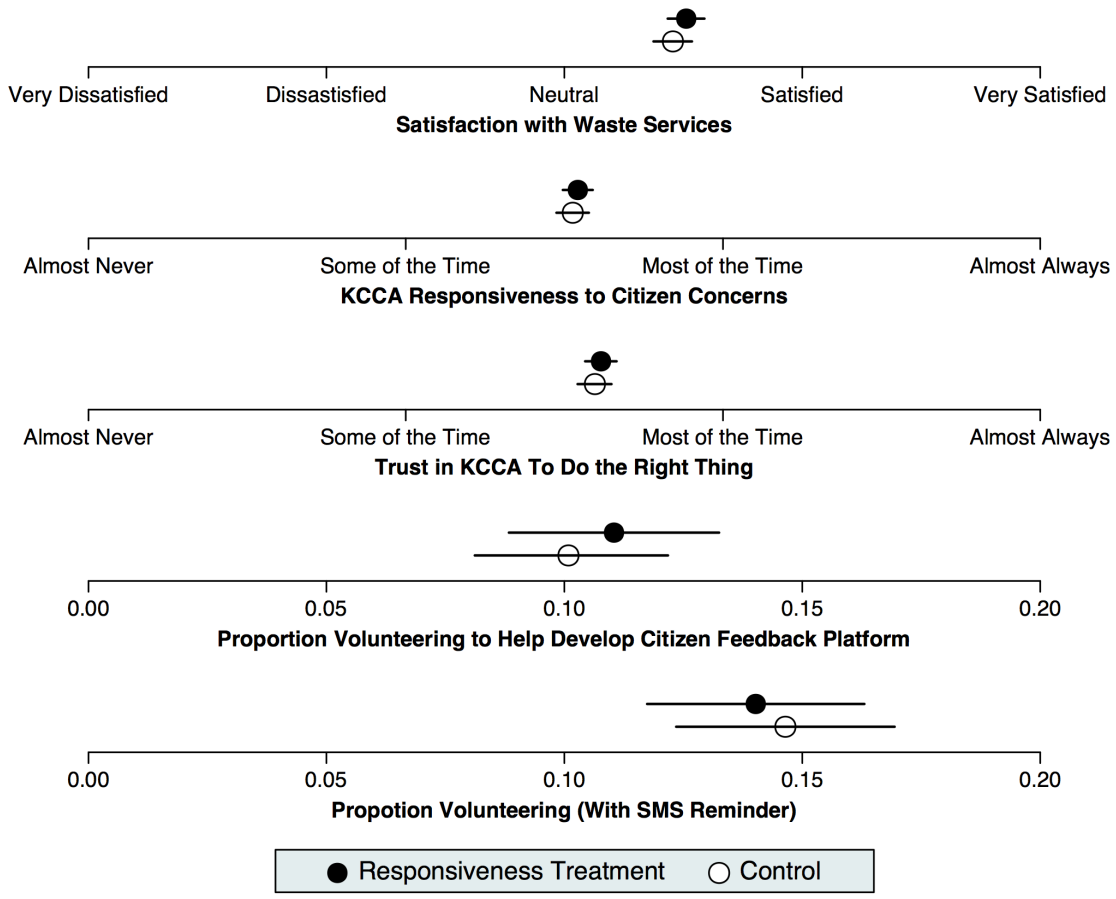

Figure 4. Attitudinal and behavioral responses to the Responsiveness treatment, with 95% CIs.

8. Discussion and Conclusions

Engaging citizens in monitoring public services may be critical to governance, and the text-messaging platform at the core of our field experiments exemplifies the tools that governments now commonly use to engage citizens worldwide (Weerakkody et al. 2015). Yet despite significant effort and investment to engage citizens in governance, many of these efforts nonetheless fail. Many of these failures do not appear in the scientific literature (McGee and Carlitz 2013; Dahlander and Piezunka 2014), making it difficult to understand how new technologies are or are not ushering in good governance.

We theorized that citizens fall into a *valley of disengagement* when they do not believe government is responsive to their concerns. Without input on failing or deficient services, governments struggle to

target services where they are in highest demand and ensure that frontline providers are not shirking. Substandard service delivery leads to further distrust and disengagement of citizens in a negative, self-reinforcing cycle. We theorized that governments might break out of this cycle by recruiting citizens with prosocial attributes, by heightening the social value of public goods and services, and by demonstrating responsiveness to citizen concerns.

To test these hypotheses, we created a partnership with the Kampala Capital City Authority in Uganda and modified an SMS platform to prompt and process thousands of spatially-explicit citizen reports about solid waste services. We find that citizens nominated by neighbors and local leaders – an effort to recruit citizens that value collective goods more highly – did not report significantly more frequently. Likewise, local leaders' announcements of citizen participation also did not increase reporting. From a policy perspective this is good news, suggesting that governments do not need to invest in costly, intensive screening methods to recruit monitors. In contrast, we find strong evidence that reporters who experienced a responsive government, effected through weekly personalized messages sharing real government plans emanating from reports, were significantly more likely to engage over several months.

Our findings are some of the first suggesting the limited effectiveness of attempting to activate community networks through nominations and recognition. These results are inconsistent with other findings on the significance of social networks for driving engagement of citizens in public affairs in Uganda (Blaschke et al. 2013). More broadly, our results offer some caution about the promise of initiating and sustaining collaborative forms of governance by relying on pre-existing social networks for the selection and motivation of citizen monitors, especially where trust in government is low (Olsson et al. 2006; Tkacheva and Bauhoff 2015; Avdeenko and Gilligan 2015).

Alternatively, this study produced strong evidence that government responsiveness can sustain citizen reporting on public services over time. A lack of responsiveness to citizens' reporting efforts might explain the relatively low rates of participation found in many e-governance platforms. However, the evidence also suggests that government responsiveness, while it can raise citizen engagement, does not appear to shift citizens' trust in government nor change their willingness to participate in future governance efforts. Other research suggests that attitudes about government change slowly, and that

important factors influencing citizens' trust in government include perceptions of efficacy (Parent et al. 2005), government responsiveness to citizens (Tolbert et al. 2006; Welch et al. 2005), and political-cultural variables like general satisfaction with democracy (Christensen et al. 2014). Future research might fruitfully parse which components of responsiveness are most effective at sustaining engagement and boosting trust over time. Additionally, subsequent research should investigate how improvements to the public service itself drive citizen engagement in governance.

New technologies hold the potential to make responsiveness more timely and targeted and thus enhance engagement by citizens. In organizing the responsiveness treatment that was part of this study, we were able to take advantage of information systems that sent targeted information to individuals based on their location. The magnitude of responsiveness increased dramatically at costs within reach for a municipal government in a developing country. In light of the widely available tools, the potential to increase citizen engagement in governance is high – provided governments are responsive.

Research has generated mixed evidence about citizen engagement in governance, with recent reviews highlighting the need to better understand feedback loops and the strategic nature of public engagement (Fox 2015). We show that responsive governments cannot depend on ICT without also credibly signaling their commitment to act on the information provided by citizens. By doing so, however, governments may be able to break out of the valley of disengagement, potentially overcoming the self-reinforcing cycle of low citizen involvement and the substandard provision of public goods.

References

Ansell, Chris, and Alison Gash. 2008. Collaborative Governance in Theory and Practice. *Journal of Public Administration Research and Theory* 18:4 (October): 543-571.

Ashraf, Nava, Oriana Bandiera, and B. Kelsey Jack. 2014. No margin, no mission? A field experiment on incentives for public service delivery. *Journal of Public Economics* 120 (December): 1-17.

Avdeenko, Alexandra, and Michael J. Gilligan. 2015. International interventions to build social capital: evidence from a field experiment in Sudan. *American Political Science Review* 109:3 (August): 427-449.

Baldassarri, Delia, and Guy Grossman. 2013. The effect of group attachment and social position on prosocial behavior. Evidence from lab-in-the-field experiments. *PLOS One* 8:3 (March): e58750.

Banerjee, Abhijit, Angus Deaton, and Esther Duflo. 2004. Health, Health Care, and Economic Development: Wealth, Health, and Health Services in Rural Rajasthan. *The American economic review* 94:2 (May): 326-330.

Banerjee, Abhijit, and Esther Duflo. 2006. Addressing Absence. *The Journal of Economic Perspectives,* 20:1 (December): 117-132.

Beaman, Lori, and Jeremy Magruder. 2012. Who gets the job referral? Evidence from a social networks experiment. *The American Economic Review* 102:7 (December): 3574-3593.

Berardo, Ramiro, and John T. Scholz. 2010. Self-Organizing Policy Networks: Risk, Partner Selection, and Cooperation in Estuaries. *American Journal of Political Science* 54:3 (July): 632-649.

Berkes, Fikret. 2009. Evolution of co-management: Role of knowledge generation, bridging organizations and social learning. *Journal of Environmental Management* 90:5 (April): 1692-1702.

Bhuiyan, Shahjahan H. 2010. A Crisis in Governance: Urban Solid Waste Management in Bangladesh. *Habitat International,* 34:1 (January): 125-133.

Björkman, Martina, and Jakob Svensson. 2009. Power to the People: Evidence from a Randomized Field Experiment on Community-Based Monitoring in Uganda. *Quarterly Journal of Economics,* 124:2 (May): 735-769.

Blaschke, Sean M., Peter P. Carroll, Daniela Rojas Chaves, Michael G. Findley, Madeleine C. Gleave, Robert N. Morello, and Daniel L. Nielson. 2013. Extrinsic, Intrinsic, and Social Incentives for

Crowdsourcing Development Information in Uganda: A Field Experiment. 6th Annual Conference on the Political Economy of International Organizations, Universities of Mannheim and Heidelberg, Germany.

Brabham, Daren C. 2009. Crowdsourcing the Public Participation Process for Planning Projects. *Planning Theory* 8:3 (August): 242-262.

Brady, Henry E., Kay Lehman Schlozman, and Sidney Verba. 1999. Prospecting for participants: Rational expectations and the recruitment of political activists. *American Political Science Review* 93:1 (March): 153-168.

Brogden, Mike, and Nijhar Preeti. 2005. Community Policing. National and international models and approaches. New York, NY: Routledge.

Budhathoki, Nama R., and Caroline Haythornthwaite. 2013. Motivation for Open Collaboration Crowd and Community Models and the Case of OpenStreetMap. *American Behavioral Scientist* 57:5 (May): 548-575.

Caseley, Jonathan. 2006. Multiple accountability relationships and improved service delivery performance in Hyderabad City, Southern India. *International Review of Administrative Sciences* 72:4 (December): 531-546.

Chandler, Dana, and Adam Kapelner. 2013. Breaking monotony with meaning: Motivation in crowdsourcing markets. *Journal of Economic Behavior & Organization* 90 (June): 123-133.

Christensen, Tom, and Per Lægreid. 2014. Trust in Government: The Relative Importance of Service Satisfaction, Political Factors and Demography. *Public Performance & Management Review* 28:4 (June): 478-511.

Dahlander, Linus, and Henning Piezunka. Open to suggestions: How organizations elicit suggestions through proactive and reactive attention. *Research Policy* 43:5 (June): 812-827.

Duflo, Esther, Pascaline Dupas, and Michael Kremer. 2015. School Governance, teacher Incentives, and pupil-teacher ratios: Experimental evidence from Kenyan primary Schools. *Journal of Public Economics* 123 (March): 92-110.

Evans, Angela M., and Adriana Campos. 2013. Open Government Initiatives: Challenges of Citizen Participation. *Journal of Policy Analysis and Management* 32:1 (Winter): 172-185.

Fafchamps, Marcel, and Alexander Moradi. 2015. Referral and Job Performance: Evidence from the Ghana Colonial Army. *Economic Development and Cultural Change* 63:4 (July): 715-751.

Fox, Jonathan A. 2015. Social Accountability: What Does the Evidence Really Say? *World Development*

72 (August): 346-361.

Grossman, Guy, Macartan Humphreys, and Gabriella Sacramone-Lutz. 2014. "I wld like u WMP to extent electricity 2 our village": On Information Technology and Interest Articulation. *American Political Science Review*, 108:3 (August): 688-705.

Grossman, G., Macartan Humphreys, and Gabriella Sacramone-Lutz. 2015. Information Technology and Political Engagement: Mixed Evidence from Uganda. Working paper. Draft Version 1.1.: October 6, 2015. http://www.columbia.edu/~mh2245/papers1/GHS_Scale.pdf.

Grossman, Guy, Kristin Michelitch, Marta Santamaria. 2016. Texting Complaints to Politicians: Name Personalization and Politicians' Encouragement in Citizen Mobilization. *Comparative Political Studies* (September).

Joshi, Anuradha. 2013. Do they work? Assessing the Impact of Transparency and Accountability Initiatives in Service Delivery. *Development Policy Review* 31:s1 (July): 29-48.

Karlan, Dean, and Margaret A. McConnell. 2014. Hey look at me: The effect of giving circles on giving. *Journal of Economic Behavior & Organization* 106 (October): 402-412.

Katusiimeh, Mesharch W., Arthur P. J. Mol, and Kees Burger. 2012. The Operations and Effectiveness of Public and Private Provision of Solid Waste Collection Services in Kampala. *Habitat International* 36:2 (April): 247-252.

Kiewiet, D. Roderick, and Mathew D. McCubbins. 1991. *The Logic of Delegation*. University of Chicago Press.

Kim, David A., Alison R. Hwong, Derek Stafford, D. Alex Hughes, A. James O'Malley, James H. Fowler, and Nicholas A. Christakis. 2015. Social network targeting to maximise population behaviour change: a cluster randomised controlled trial. *The Lancet* 386:9989 (July): 145-153.

Kinobe, Joel R., Charles B. Niwagaba, Girma Gebresenbet, Allan J. Komakech, and Björn Vinnerås. 2015. Mapping out the solid waste generation and collection models: The case of Kampala City. *Journal of the Air & Waste Management Association* 65:2 (January), 197-205.

Leifeld, Philip, Volker Schneider. 2012. Information Exchange in Policy Networks. *American Journal of Political Science* 56:3 (July): 731-744.

Linders, Dennis. 2012. From e-government to we-government: Defining a typology for citizen coproduction in the age of social media. *Government Information Quarterly* 29:4 (October), 446-454.

Linders, Dennis. 2013. Towards open development: Leveraging open data to improve the planning

and coordination of international aid. *Government Information Quarterly* 30:4 (October), 426-434.

Mansuri, Ghazala, and Vijayendra Rao. 2013. Can participation be induced? Some evidence from developing countries. *Critical Review of International Social and Political Philosophy* 16:2: 284-304.

McCubbins, Mathew D., and Schwartz, Thomas. 1984. Congressional Oversight Overlooked: Police Patrols versus Fire Alarms. *American Journal of Political Science* 28:1 (February): 165-179.

McGee, Rosemary, and Ruth Carlitz. 2013. Learning Study On "The Users" In Technology for Transparency And Accountability Initiatives: Assumptions and Realities. *Knowledge Programme*. http://www.ruthcarlitz.com/pdf/IDS-UserLearningStudyonT4T&AIs.pdf

McGuire, M. 2006. Collaborative Public Management: Assessing What We Know and How We Know It. *Public Administration Review* 66:s1 (December): 33-43.

Oates, Briony J. 2003. The potential contribution of ICTs to the political process. *Electronic Journal of e-Government* 1:1: 31–39.

Okot-Okumu, James, and Richard Nyenje. 2011. Municipal Solid Waste Management Under Decentralization in Uganda. *Habitat International* 35:4 (October): 537-543.

Olken, Benjamin. 2007. Monitoring Corruption: Evidence from a Field Experiment in Indonesia. *Journal of Political Economy* 115:2: 200-249

Olsson, Per, Lance Gunderson, Stephen Carpenter, Paul Ryan, Louis Lebel, Carl Folke, and C.S. Holling. 2006. Shooting the Rapids: Navigating Transitions to Adaptive Governance of Social-Ecological Systems. *Ecology and Society* 11:1 (June).

Ostrom, Elinor. 1996. Crossing the great divide: coproduction, synergy, and development. *World Development* 24:6 (June): 1073-1087.

Oteng-Ababio, Martin. 2010. Private Sector Involvement in Solid Waste Management in the Greater Accra Metropolitan Area in Ghana. *Waste Management & Research* 28:4 (April): 322-29.

Parent, Michael, Christine A. Vandebeek, and Andrew C. Gemino. 2005. Building Citizen Trust Through E-government. *Government Information Quarterly 22*(4): 720-736.

Parks, Roger B., Paula C. Baker, Larry Kiser, Ronald Oakerson, Elinor Ostrom, Vincent Ostrom, Stephen L. Percy, Martha B. Vandivort, Gordon P. Whitaker, and Rick Wilson. 1981. Consumers as Coproducers of Public Services: Some Economic and Institutional Considerations. *Policy Studies Journal* 9:7 (June): 1001-1011.

Rotberg, Robert I., and Jenny C. Aker. 2013. Mobile Phones: Uplifting Weak and Failed States. *The Washington Quarterly* 36:1 (Winter): 111-125.

Sandström, Annica, Beatrice Crona, and Örjan Bodin. 2014. Legitimacy in Co-Management: The Impact of Preexisting Structures, Social Networks and Governance Strategies. *Environmental Policy and Governance 24*:1 (January/February): 60-76.

Schultz, P. Wesley, Jessica M. Nolan, Robert B. Cialdini, Noah J. Goldstein, and Vladas Griskevicius. 2007. The Constructive, Destructive, and Reconstructive Power of Social Norms. *Psychological science* 18:5 (May): 429-434.

Seidel, Claudius E., Basanta E. Thapa, Ralf Plattfaut, and Björn Niehaves. 2013. Selective crowdsourcing for open process innovation in the public sector: are expert citizens really willing to participate?. In *Proceedings of the 7th International Conference on Theory and Practice of Electronic Governance*, 64-72.

Smith, Matthew L., Katherine M. Reilly, Yochai Benkler. 2013. *Open Development: Networked Innovations in International Development*. Cambridge: The MIT Press.

Speer, Johanna. 2012. Participatory Governance Reform: A Good Strategy for Increasing Government Responsiveness and Improving Public Services? *World Development* 40:12 (December): 2379-2398.

Stevenson, William B., and Danna Greenberg. 2000. Agency and Social Networks: Strategies of Action in a Social Struction of Position, Opposition, and Opportunity. *Administrative Science Quarterly* 45:4 (December): 651-678.

Tkacheva, Olesya, and Sebastian Bauhoff. 2015. Social Capital and Community Monitoring of Healthcare Services in Tajikistan. RAND Working Paper. Prepared for the presentation at the Annual Meeting of the Midwestern Political Science Association, Chicago, IL, April 16-19, 2015. http://www.rand.org/content/dam/rand/pubs/working_papers/WR1000/WR1097/RAND_WR1097.pdf

Tolbert, Caroline J., Karen Mossberger. 2006. The Effects of E-Government on Trust and Confidence in Government. *Public Administration Review* 66:3 (May/June): 354-369.

Vráblíková, Kateřina. 2014. How Context Matters? Mobilization, Political Opportunity Structures, and Nonelectoral Political Participation in Old and New Democracies. *Comparative Political Studies* 47:2 (February): 203-229.

Weerakkody, V., Irani, Z., Lee, H., Osman, I., & Hindi, N. 2015. E-government implementation: A bird's

eye view of issues relating to costs, opportunities, benefits and risks. *Information Systems Frontiers*, *17*(4), 889-915.

Welch, Eric W., Charles C. Hinnant, and M. Jae Moon. 2005. Linking Citizen Satisfaction with E-Government and Trust in Government. *Journal of Public Administration Research and Theory* 15:3 (July): 371-391.

Supplementary Information

Contents

A. Pre-Experimental Scoping Survey

During the summers of 2013 and 2014, we completed preliminary research for the project and established the partnerships necessary to carry out the reported field experiments. To scope out the relevance of our project to field conditions in Kampala, we embedded questions about satisfaction with solid waste services into a broader household survey undertaken for a different project. In total, we received responses from 439 individuals identified through a random walk pattern in randomly selected neighborhoods across Kampala. Initial survey data indicates that Kampala citizens are highly concerned about solid waste services in their communities. For brevity, we provide summary statistics about three questions: (1) personal concern about waste disposal; (2) dissatisfaction with current collection services; and (3) self-reports of burning waste at least one time per week. The vast majority of respondents are personally concerned with the state of solid waste collection and a majority are actively dissatisfied with the current state of solid waste services in their neighborhoods. Additional survey responses reveal that a minority of households are able to take advantage of formal waste collection services, and most households are forced to burn their trash on a weekly basis. Our survey data suggest that 86 percent of Kampala residents own mobile phones, so recruitment for monitoring can occur from the vast majority of residents in all zones of the city (Figure A1).

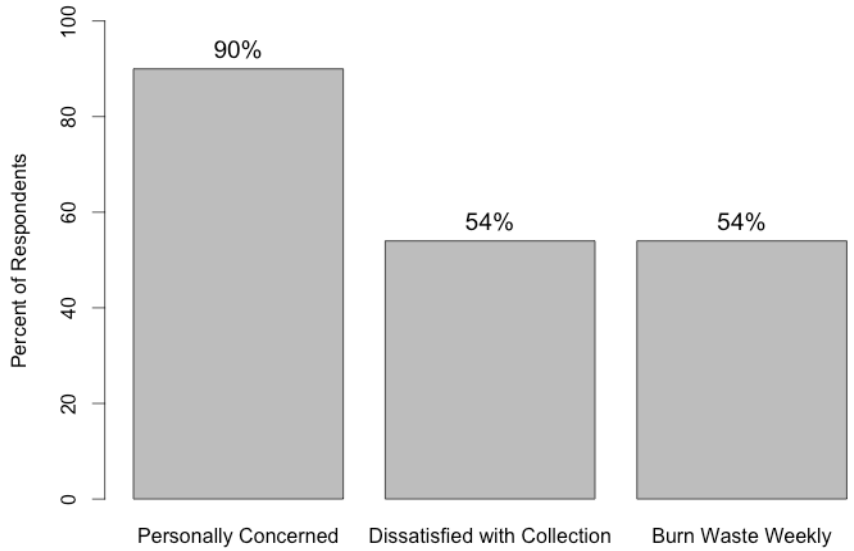

Figure A1. Resident perceptions of solid waste services and conditions in preliminary survey.

B. Reporter Recruitment Protocols

Below are instructions used by the enumeration team to recruit subjects for the study. Recruitment drives were carried out from November 5-26, 2015 for Phase 1, and June 9-16, 2016 for Phase 2. The recruitment team comprised approximately 20 Ugandans.

Setting up in the zones (Phase 1 and 2)

When your team first enters a village, inform the village chairperson of the project and secure their support for any project-related activities, such as an LC1 recruitment or LC1 announcement treatment. Use the information flyer (see *Reporter Recruitment Flier* below) and introduction letter to help gain the support of the LC1. If the LC1 is unavailable, ask him to delegate the responsibility to another local authority figure, such as the Vice-Chairperson, Secretary of Defence, or member of the Local Council Committee.

Next, have the chairperson or a resident of the zone describe the boundaries of the zone. Discuss how to divide up the zone into four cells of roughly similar size, and begin recruitment activities according to the treatment assigned to that zone. Five individuals will be recruited from each of the four cells. In this way, 20 reporters will be recruited in each zone. [In Phase 1, our recruitment team was asked to recruit three individuals from each of the four cells per zone, for a total of 12 reporters per zone]. Eligible subjects must be an adult (over the age of 18), a resident of the zone and the primary user of their own cell phone.

Random Citizen Recruitment (Control treatment for Phases 1 and 2)

For zones that are assigned for random recruitment, the enumeration team will follow a random walk pattern (see the *generating a random walk pattern* section below) to select subjects. First, find your way to the center of one of the four cells in a zone, then follow a random walk pattern for three minutes using a timer on a phone or tablet. Once the timer reaches three minutes, attempt to recruit the nearest adult. If the adult is ineligible or refuses to participate, restart the timer and follow a random walk pattern again for three minutes to select the next potential subject. The same process will be

followed until 5 subjects have been selected in each cell. The work is complete when a total of 20 subjects have been recruited in the zone. [In Phase 1, our recruitment team was asked to recruit three individuals from each of the four cells per zone, for a total of 12 reporters per zone]. Use the following steps to sign up the subjects.

1) Introduce yourself and inform the subject about the citizen monitoring program.
2) If the subject is interested in participating, read the flyer (see the *Reporter Recruitment Flier* below) to the subject in his/her preferred language.
3) Enumerators should not place any pressure on the respondents to participate, including informally with body language.
4) The subject is under no obligation to respond and may terminate the interview at any time without consequence.
5) If the subject agrees to participate, survey the subject using the Kobocollect survey.

Neighbor Nomination (Phase 1 treatment)

Contact the first adult in sight. To be eligible, the person must be an adult resident of the zone. Explain the program, hand them an information slip and answer any questions they raise. Ask them if they'd be willing to nominate a "reliable and trustworthy" person from the zone to become a reporter in the system. Follow the steps to sign up a subject in the previous section. If so, ask the person to make a personal introduction to the nominee either by calling the person or by making a face-to-face introduction. Make sure the nominated individual is an adult resident of the zone. Explain the program to the nominated individual, hand them an information slip and answer any questions they raise. Ask them if they'd be willing to participate as a reporter and remind them they have been nominated by a neighbor. If yes, sign them up using the survey on KoboConnect. Ask the person if they would like to nominate anyone to be a reporter, regardless of whether they have signed up or not. If no, again randomly walk for 2-3 minutes. Repeat the sign-up process.

LC1 Announcement (Phase 2 treatment)

Recruit subjects using the recruitment method assigned to the zone (see Random Citizen Recruitment

or LC1 Recruitment). Additionally, inform the subject that in an upcoming zone meeting, the LC1 will announce them as a citizen monitor selected to represent the zone. After all 20 reporters have been recruited in a zone, provide the LC1 with a list of the names of those selected to be citizen monitors. Secure the LC1s commitment to announcing the program and names of citizen monitors at an upcoming zone meeting. Lastly, complete the *LC1 Announcement* survey on Kobocollect to gather the LC1s contact information. The implementation team will contact LC1s by phone one week following the completion of the recruitment activity to remind the LC1s to make the announcement at a zone-wide meeting.

LC1 Recruitment (Phase 2 treatment)

Subjects in zones assigned to Treatment 3 will be recruited by the LC1. The LC1 will personally introduce the subject to the recruitment team and recommend them as a citizen monitor. Once the recruitment team has been introduced to the subject, follow the instructions below to sign up a subject.

Reporter Recruitment Flier (Phase 1 and 2)

Invitation to Report on Solid Waste Collection in Your Neighborhood!

We are an independent research group launching a project that will allow residents of Kampala to use SMS to report on waste management issues in their neighborhoods. Your input is very valuable and we hope you will participate in making Kampala a cleaner and more livable city. We are asking you to join the platform.

If you sign up to be a reporter, we will send you 2-3 messages per week over 8 weeks asking you to report on the solid waste condition and services in your neighborhood. Each week there will be a lottery to win airtime.

All messages that you send and receive from us will be toll-free and will not reduce your airtime. If you ever have questions, you can send the message "HELP" to 6585. Someone will contact you to answer your questions. You can also send the message "STOP" to 6585 at any time to stop receiving

messages.

Your name or contact information will not be shared with anyone. Your responses will be used to inform the Kampala Capital City Authority about which areas of Kampala require better waste management services. Please contact Jacob Skaggs (0780291311) if you have any questions or concerns about the program.

Generating a random walk pattern

1) Find an intersection in each of the assigned cells. An intersection is the crossing of any road, path, or alley that leads to the entrance of residential dwellings. The starting intersection should be located by walking several minutes into the assigned cell.
2) Assign each direction leading from the intersection a number. Roll the dice and move in the direction selected randomly.
3) Any time you reach another intersection, assign each direction that moves forward from your walk path a number and roll the dice, moving in the direction selected randomly. You should only turn around if you reach a dead end or the edge of the assigned cell.
4) The only reason that the randomly chosen direction should not be an option is if you have already been down a path and you know that it leads to a dead end.

C. Prompts Sent to Citizen Reporters

1. How many times have you observed waste being picked up and removed from your zone in the last weeks? [REPLY with a number]
2. How many waste heaps have you observed being burned in your zone during the last 24 hours? [REPLY with a number]
3. Please describe the location of any waste heap that needs attention from the KCCA or its contractors. [REPLY with a location description]

(In the Experiment 1, each of the three messages above were sent to all subjects once each week over a period of 8 weeks).

1. Does a rubbish truck come to your neighborhood? 1) no 2) yes 3) don't know
2. When did the rubbish truck last collect your rubbish? A) never B) more than two weeks ago C) last week D) this week
3. What is the most common way for your neighbors to dispose of their rubbish? 1) burn rubbish 2) throw in a rubbish pile 3) throw in a ditch 4) use a rubbish truck 5) don't know
4. How happy are you with rubbish collection services? 1) very unhappy 2) unhappy 3) neither happy nor unhappy 4) happy 5) very happy 6) don't know
5. How often do you see rubbish spilling from rubbish trucks? 1) never 2) rarely 3) two times a month 4) once a week 5) many times a week 6) don't know
6. How much waste is there on the ground in your neighborhood? (1) none (2) some small piles (3) a few larger piles (4) waste in many places 5) don't know
7. On the path you walk in and out of your zone, how many waste piles would you see? [Respond with a number]
8. In a typical week, how many times would you see burning rubbish if you walked in the zone for fifteen minutes per day?
9. How often does the rubbish truck collect rubbish on the chosen day of the week? 1) never 2) not often 3) often 4) very often 5) don't know
10. How happy are you with how often your rubbish is collected? 1) very unhappy 2) unhappy 3) neither happy or unhappy 4) happy 5) very happy 6) don't know
11. How happy are you with the distance from your home to the rubbish truck? 1) very unhappy 2) unhappy 3) neither happy or unhappy 4) happy 5) very happy
12. How well do rubbish collectors treat you? 1) very bad 2) bad 3) neither bad nor good 4) good 5) very good
13. What is the biggest problem with your rubbish collection service? [open response]
14. Are there any other rubbish or sanitation services that you would like? [open response]
15. Please describe how to reach the largest rubbish pile near your home. [open response]

D. Pre-Registered Hypotheses

H1a: More nominated reporters will **respond to at least one prompt** than randomly recruited reporters.

H1b: Nominated reporters will **respond to more prompts** than randomly recruited reporters, measured as a count both over the entire 8-week experiment and within individual weeks.

H1c: Nominated reporters will **respond to more open-ended prompts** than randomly recruited reporters, measured as a count both over the entire 8-week experiment and within individual weeks.

H1a: More reporters assigned to the LC1 recruitment treatment will respond to at least one prompt than randomly recruited reporters.

H1b: More reporters assigned to the LC1 announcement treatment will respond to at least one prompt than reporters in the announcement control condition.

H1c: More reporters assigned to the responsiveness treatment will respond to at least one prompt than reporters in the responsiveness control condition.

H2a: Reporters assigned to the LC1 recruitment treatment will respond to more prompts than randomly recruited reporters, measured as a count both over the entire 8-week experiment and within individual weeks.

H2b: Reporters assigned to the LC1 announcement treatment will respond to more prompts than reporters in the announcement control condition, measured as a count both over the entire 8-week experiment and within individual weeks.

H2c: Reporters assigned to the responsiveness treatment will respond to more prompts than reporters in the responsiveness control condition, measured as a count both over the entire 8-week experiment and within individual weeks.

H3a: Reporters assigned to the LC1 recruitment treatment will respond to more open-ended prompts than randomly recruited reporters, measured as a count both over the entire 8-week experiment, within individual weeks (to measure changes in participation over time), and for the final two weeks (to measure attrition).

H3b: Reporters assigned to the LC1 announcement treatment will respond to more open-ended prompts than reporters in the announcement control condition, measured as a count both over the entire 8-week experiment, within individual weeks (to measure changes in participation over time), and for the final two weeks (to measure attrition).

H3c: Reporters assigned to the responsiveness treatment will respond to more open-ended prompts than reporters in the responsiveness control condition, measured as a count both over the entire 8-week experiment, within individual weeks (to measure changes in participation over time), and for the final two weeks (to measure attrition).

E. Heterogeneous Treatment Effects of Responsiveness for Inactive and Active Phase 1 Reporters

As an extension to our main analysis, we are interested in whether responsiveness can both cause more persistent reporting among activated reporters, as well as activate or re-activate reporters who fell below the engagement threshold that we theorize above. Thus, we divide all Phase 1 reporters who were prompted during Phase 2 for reports into three subgroups: (1) reporters who were active in both the first and second half of the reporting period during Phase 1 (the "activated" sub-group); (2) reporters who were only active in the first half of the reporting period during Phase 1 (the "deactivated" sub-group); and (3) reporters who were never active during Phase 1 (the "inactive" sub-group). Table F1 displays reporter-wise regression results for the number of reports received in total and during the last two weeks of Phase 2, with inactive reporters and the control group as the baseline conditions.

Table E1. Reporting by Phase 1 reporters during Phase 2 by activation status.

	DV: Number of Responses Received:	
	Total	Last two weeks
	(1)	(2)
Responsiveness	0.000	0.000
	(0.172)	(0.037)
Deactivated	4.141***	0.437***
	(0.301)	(0.065)
Activated	4.395***	0.395***
	(0.376)	(0.081)
Responsiveness X Deactivated	1.031***	0.230***
	(0.398)	(0.086)
Responsiveness X Activated	1.330***	0.250**
	(0.495)	(0.107)
Intercept	-0.000	-0.000
	(0.118)	(0.026)
Observations	1,021	1,021
Adjusted R^2	0.473	0.205
F Statistic	183.777***	53.460***
Note:		*p<0.1; **p<0.05; ***p<0.01

The results show that responsiveness does not boost rates of reporting for inactive reporters. For reporters that were inactive for the entirety of Phase 1, the responsiveness condition has no effect on reporting. In contrast, responsiveness further boosts reporting for both activated and deactivated Phase 1 reporters, indicating not only that responsiveness can keep reporters out of the valley of disengagement, but also that it can reactivate those who have fallen below the activation threshold for engagement.

F. Phase 2 Results by Zone

Here we report results both for the pooled group of subjects recruited during both Phase 1 and 2, as well as the results split by the recruitment phase at the zone level. We found that six of the seven zones in Phase 2 that our enumerators recruited 15 or fewer reporters were subsequently assigned to the *Responsiveness* treatment. We thus also examine the subset of zones with 16 or more reporters for the split Phase 2 analysis (recall the target was to recruit 20 reporters per zone) where the number of reporters at the zone-level is balanced by treatment condition. In all tables reported below, the base conditions are *Random Citizen* recruitment, the control condition for the LC1 announcement about reporters, and the control condition for the *Responsiveness* treatment.

Considering first the total number of reporters during Phase 2 who submitted at least one, on-topic report about solid waste management during the eight-week period, only the *Responsiveness* condition boosts the number of active reporters as hypothesized (Table 2). In both the pooled and split models, zones assigned to the responsiveness condition have approximately one extra reporter who is active on average than zones assigned to control (across all zones and experimental conditions, the mean is approximately five active reporters per zone during Phase 2). This translates to a 20 percent increase in the mean number of reporters and indicates substantive as well as statistical significance. This result indicates that hearing about what the government is doing with the reports via targeted outreach can help initiate engagement in citizen reporting. In contrast, we do not observe any differences in the number of active reporters when recruiting by either neighbor or LC1 nomination, or when reporters expected the LC1 to make an announcement about the platform and reporters at a community meeting. Thus, the evidence suggests that social motivations are not effective at activating reporting on public services in this context.

Table F1. Total number of active reporters by zone during Phase 2

	DV: Total Number of Active Reporters Per Zone			
	active.reporters			
	(P1/P2 Pooled)	(P1)	(P2)	(P2 CS)
Responsiveness	0.957***	1.100***	0.863*	0.847*
	(0.307)	(0.320)	(0.528)	(0.563)
Neighbor Nomination	0.059	0.019		
	(0.448)	(0.322)		
LC1 Nomination	-0.122		-0.188	-0.289
	(0.428)		(0.534)	(0.561)
LC1 Announcement	0.483		0.531	0.576
	(0.422)		(0.525)	(0.565)
Reporters (Zone)	0.374***	0.267***	0.384***	0.193
	(0.043)	(0.085)	(0.080)	(0.274)
Phase 2	1.602***			
	(0.541)			
Zones	189	89	97	89
Adjusted R^2	0.604	0.173	0.219	-0.003
F Statistic	48.749***	7.123***	7.728***	0.942
Note: one-tailed tests		*p<0.1; **p<0.05; ***p<0.01		

Turning to the total number of reports made by zone during the 8-week Phase 2 reporting period, we find very similar results, with only the responsiveness treatment driving more reports (Table 3). Pooling zones across recruiting periods, we find that the *Responsiveness* treatment increased the number of reports per zone by approximately 6.6 over eight weeks (across all zones and experimental conditions, the mean is approximately 32 reports per zone during Phase 2). This result is largely driven by the significant effect the *Responsiveness* treatment had on zones where recruitment took place during Phase 1, where the treatment increased the number of total reports by zone by approximately nine (Model P1). In contrast, the *Responsiveness* treatment did not increase the total number of reports among Phase 2 zones in ways that are highly inconsistent with random chance (for P2 and P2 CS models, p=0.10~0.15). Like the results for the total number of active reporters, we do not observe any differences in the number of total reports by zone when recruiting was done by either neighbor or LC1 nomination, or when reporters expected the LC1 to make an announcement about the platform and reporters at a community meeting.

Table F2. Total number of reports by zone during Phase 2

	DV: Total Number of Citizen Reports by Zone During Phase 2			
	(P1/P2 Pooled)	(P1)	(P2)	(P2 CS)
Responsiveness	6.640***	9.064***	5.217	3.658
	(2.581)	(2.628)	(4.554)	(4.722)
Neighbor Nomination	0.561	0.281		
	(3.760)	(2.643)		
LC1 Nomination	2.790		2.523	1.785
	(3.598)		(4.526)	(4.707)
LC1 Announcement	1.321		1.321	1.216
	(3.545)		(4.460)	(4.741)
Reporters (Zone)	2.246***	1.374**	2.006***	0.111
	(0.361)	(0.699)	(0.763)	(2.297)
Phase 2	13.118***			
	(4.547)			
Zones	189	89	96	89
Adjusted R^2	0.494	0.125	0.071	-0.038
F Statistic	31.636***	5.191***	2.807**	0.194
Note: one-tailed tests				*p<0.1; **p<0.05; ***p<0.01

Finally, we consider the total number of reports per zone by treatment condition for the last two weeks of the 8-week reporting period. As pre-registered, we are interested not only in the total effects of the *Responsiveness* treatment and the experimental recruitment conditions, but also whether social motivation or government responsiveness can drive longer-term engagement in the collaborative management of public services. Like previous estimations, we do not find that any recruitment condition or that the announcement about reporting by local leadership significantly increased reporting during the last two weeks of Phase 2. We do find, however, a strong signal that responsiveness from government to the citizen reports has a significant and positive effect on reporting across all zones in both of the field experiments, actually having a substantively similar effect. This result suggests that responsiveness is necessary to sustain reporting, even if it is not a predictor of initial engagement. Indeed, only the responsiveness treatment has a lasting effect for the entirety of the reporting period for reporters recruited during Phase 1 and for the end of the reporting period for reporters recruited during Phase 2.

Table F3. Total number of reports by zone during last two weeks of Phase 2

	DV: Total Number of Citizen Reports by Zone During Last Two Weeks of Phase 2			
	(P1/P2 Pooled)	(P1)	(P2)	(P2 CS)
Responsiveness	1.413***	1.363***	1.575**	1.519**
	(0.450)	(0.409)	(0.824)	(0.867)
Neighbor Nomination	0.028	-0.008		
	(0.656)	(0.412)		
LC1 Nomination	0.199		0.155	0.178
	(0.628)		(0.819)	(0.865)
LC1 Announcement	-0.152		-0.141	-0.096
	(0.619)		(0.807)	(0.871)
Reporters (Zone)	0.264***	0.182**	0.231**	0.310
	(0.063)	(0.109)	(0.138)	(0.422)
Phase 2	1.449*			
	(0.794)			
Zones	189	89	96	89
Adjusted R^2	0.291	0.109	0.045	-0.005
F Statistic	13.879***	4.605***	2.117*	0.880
Note: one-tailed tests			*p<0.1; **p<0.05; ***p<0.01	

G. Complier Average Causal Effects for *LC1 Announcement* treatment in Phase 2

The recruited reporters in the zones assigned to the *LC1 Announcement* treatment were fully informed that their names and responsibilities would be announced at an upcoming community meeting. We delivered the list of recruited reporters to all zone chairs in this treatment condition and asked them to make such an announcement. We followed-up with a reminder one week after delivering the list of reporters. At the end of the reporting period, we made three attempts to call all 50 LC1 chairs who had been asked to make this announcement at a community meeting based on the zone-wise random assignment. We were able to reach 42 chairs and learned that 16 made the community announcement and 26 did not make the community announcement. Those who did not make the community announcement reported that they were busy, were away from the zone, or did not remember, among other reasons.

In the main results above, we report intent-to-treat estimates that do not take into account the actual delivery of the *LC1 Announcement* treatment. Here we estimate complier average causal effects via 2-stage least squares, where the treatment assignment used as an instrument for the delivery of treatment. Because we did not deliver the names of reporters to LC1 chairs in zones assigned to control, we rule out two-sided non-compliance. We were not able to collect information about compliance for 8 of the 50 zones assigned to treatment, so we estimate the bounds of CACE. Table G1 drops the zones with missing compliance information. Table G2 assumes that all zones with missing compliance data are compliers. Table G3 assumes all zones with missing compliance data are non-compliers. All tables show the second stage estimates. In no case do we find treatment effects that diverge in substantive or statistical significance from the intent-to-treat results reported in the main text.

Table G1. CACE for *LC1 Announcement* Condition with missing compliance data dropped

	Procedure for Missing Compliance Data: Dropped		
	Total Responses	Active Ever	Last 2 Week Responses
	(1)	(2)	(3)
Responsiveness	0.229	0.049**	0.077**
	(0.202)	(0.024)	(0.037)
LC1 Nomination	0.099	-0.010	-0.004
	(0.201)	(0.023)	(0.037)
LC1 Announcement	0.065	0.035	-0.020
	(0.554)	(0.065)	(0.104)
Intercept	2.289***	0.352***	0.240***
	(0.195)	(0.024)	(0.035)
Observations	1,710	1,710	1,710
Note: one-tailed tests			*p<0.1; **p<0.05; ***p<0.01

Table G2. CACE for *LC1 Announcement* Condition with missing compliance data assumed to be in compliance

	Procedure for Missing Compliance Data: Upper Bound		
	Total Responses	Active Ever	Last 2 Week Responses
	(1)	(2)	(3)
Responsiveness	0.239	0.050**	0.075**
	(0.195)	(0.023)	(0.036)
LC1 Nomination	0.123	-0.009	0.012
	(0.195)	(0.023)	(0.036)
LC1 Announcement	0.159	0.060	-0.015
	(0.424)	(0.050)	(0.078)
Intercept	2.271***	0.351***	0.233***
	(0.190)	(0.023)	(0.034)
Observations	1,845	1,845	1,845
Note: one-tailed tests			*p<0.1; **p<0.05; ***p<0.01

Table G3. CACE for *LC1 Announcement* Condition with missing compliance data assumed to be out of compliance

	Procedure for Missing Compliance Data: Lower Bound		
	Total Responses	Active Ever	Last 2 Week Responses
	(1)	(2)	(3)
Responsiveness	0.235	0.049**	0.075**
	(0.194)	(0.023)	(0.035)
LC1 Nomination	0.135	-0.005	0.011
	(0.193)	(0.023)	(0.036)
LC1 Announcement	0.233	0.087	-0.023
	(0.622)	(0.073)	(0.115)
Intercept	2.268***	0.349***	0.234***
	(0.195)	(0.024)	(0.035)
Observations	1,845	1,845	1,845
Note: one-tailed tests			*p<0.1; **p<0.05; ***p<0.01

H. Examining the possibility of a social norm treatment effect in the *Responsiveness* treatment

As part of the *Responsiveness* treatment, we informed all reporters about the total number of reports received from their zone during the previous week during four weeks of the 8-week reporting period by SMS text-message. Although reporters were never informed about the total number of other reporters in their zone, which makes it difficult for the reporters to interpret the raw number of reports as a social norm, it is nonetheless possible that this information introduced a social norm into the treatment. For example, perhaps being informed about a high number of responses induced free-riding behavior or in the opposite direction pressure to comply with a descriptive social norm (e.g., Schultz et al. 2007). Recall that the intention of this component of the treatment was to make salient to reporters that specific reports were being processed and noticed, one necessary part of beliefs about responsiveness.

Nonetheless, to rule out to possibility of a social norms effect from the messages, we examine whether reporter behavior is conditional on the number of messages they were told were received from the zone the previous week. To do so, we form a seven-week panel of all reporters assigned to the Responsiveness condition that contains data on whether they submitted a report in a given week, whether they submitted a report the previous week, the total number of reports from the zone the previous week, and whether they received a message about the total number of reports from the zone the previous week. Note the the zone-wise number of messages received was a noisy signal from week to week. We exploit this noisy signal to estimate the effect of receiving a message about zone-wise reporting conditional on the amount of zone-wise reporting. Also note that the messages informing reporters of this number were only sent prior to reporting weeks 2, 3, 4, and 6. We thus specify a model of the following form:

$$y_{i,t} = \alpha_i + \beta_1 N_{j,t-1} + \beta_2 M_{j,t} + \beta_3 (N_{j,t-1} * M_{j,t}) + \beta_4 y_{i,t-1} + \varepsilon_i \qquad \textbf{(H1)}$$

Where $y_{i,t}$ is a binary indicator of whether a report was submitted by reporter *i* during week *t*, α_i is a reporter-level fixed effect implementing by demeaning, $N_{j,t-1}$ is the number of reports received from the zone during the previous week, $M_{j,t}$ is an indicator of whether a message was sent about the

number of reports received from the zone during the previous week, ($N_{j,t-1}$* $M_{j,t}$) is an interaction term that models whether the effect of an such message is conditional on the number of messages received at the zone level, $y_{i,t-1}$ is an indicator of whether a report was submitted by reporter *i* during the previous week *t-1*, and ε_i is the error term. The key parameter of interest is β_3, which indicates whether the effect of receiving a message about the number of zone-wise reports is conditional on the specific number of reports indicated in the message. Recall that we expect β_2 to be positive if our messages are having the intended effect, but that the effect should be unconditional on the specific number in the message if there is no social norm effect.

The results rule out concerns about a social norms treatment confounding our results. We obtain a precisely estimate zero interaction effect for β_3 (Table H1). Figure H1 shows marginal effects of the message about zone-wise reports.

Table H1. Parameter estimates for Model H1

Parameter	Description	Estimate	Std. Error	p
β_1	**Number** of Zone-Wise Reports	0.0025	0.0009	0.005
β_2	**Message** about Zone-Wise Reports	0.0464	0.0063	<0.001
β_3	**Number** X **Message**	0.0001	0.0009	0.87
β_4	Active Previous Week (Reporter)	0.0698	0.0077	<0.001

Notes: Observations: 19,370. Reporters: 1,490.

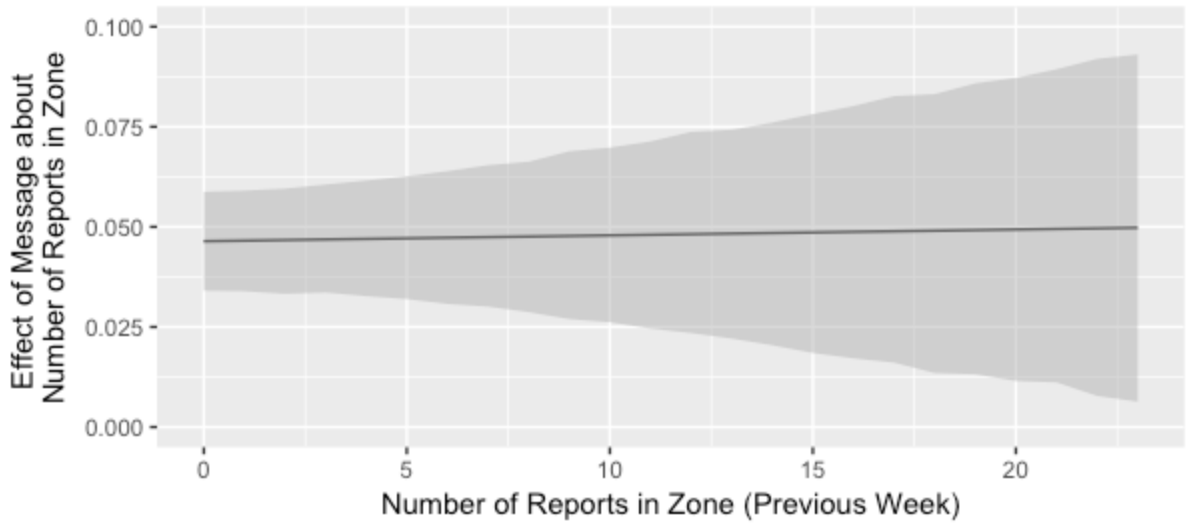

Figure H1. Marginal effects plot of conditional effect of message about zone-wise reports by the number of reports communicated in the message.

I. Post-reporting survey to measure reporters' trust in government

The post-reporting survey below was conducted five weeks following the end of the Phase 2 treatment period and was designed to explore how the responsiveness treatment might influence citizens' trust government and willingness to volunteer on its behalf. One potential challenge of the survey design is that we assume subjects understand that KCCA is the provider of waste management services in their neighborhood. However, we included language in both the recruitment script and introductory text messages reinforcing the idea that KCCA provides waste management services in Kampala, with language such as "[...] waste collection services provided by KCCA", and "Your responses [...] inform the Kampala Capital City Authority about which areas of Kampala require better waste management services."

1. How often do you think is the KCCA responsive to concerns of Kampala residents

    A. Almost never

    B. Only some of the time

    C. Most of the time

    D. Almost always

    E. Refused to answer

2. How much of the time do you think you can trust the KCCA to do what is right?

    A. Almost never

    B. Only some of the time

    C. Most of the time

    D. Almost always

    E. Refused to answer

3. How satisfied are you with rubbish collection services in your zone?

    A. Very dissatisfied

    B. Dissatisfied

    C. Neither satisfied nor dissatisfied

    D. Satisfied

E. Very satisfied

F. Refused to answer

4. “The KCCA is interested in establishing a reporting platform to engage residents in managing all kinds of services, include road quality, sanitation, lighting, and waste management. In the months ahead, the KCCA will need help testing and improving the platform before it launches. Would you be willing to volunteer your time to help the KCCA test and develop the platform, which might involve responding to questions, sending messages, and attending focus group meetings? If so, please text VOLUNTEER to 6585 and we will include your name in a list of people willing to help the KCCA manage services in Kampala.”

J. Spillover between Zones for Responsiveness Treatment

We consider the possibility of spillover effects for the Responsiveness treatment. Since the KCCA formed actual management plans to address solid waste problems based on the reports that they received, including zone-wide clean-ups, it is possible that responsiveness spills into nearby zones. The exact boundaries between zones are not always clear and waste collection truck might plausibly stop in contiguous zones to those targeted for cleanup as part of the Responsiveness treatment. This could increase beliefs about the responsiveness of the KCCA among reporters in nearby zones. Alternatively, reporters in nearby zones might observe the KCCA or its contractors taking action and be more likely to submit their own reports and requests. If this is correct (we believe it is not likely), then reporters in nearby zones might be motivated to report more often based on exposure to a nearby zone in the Responsiveness treatment.

To investigate this possibility, we take the compound exposure to direct and indirect treatment as the randomly assigned treatment variable. In particular, we consider there to be four treatment conditions: [Control, No Indirect]; [Control, Indirect]; [Treated, No Indirect]; [Treated, Indirect]. The treatment assignment corresponds to each zones' treatment assignment and whether a contiguous zone is assigned to the Responsiveness treatment. Some zones share borders with more zones than others, meaning that the probability of exposure to contiguous, indirect treatment is not equal between units. We thus calculate the probability of exposure to each of the four compound exposure conditions and estimate an inverse-weighted regression of treatment effects based on these probabilities. Note that there are a limited number of isolated zones in our sample with are not eligible to receive indirect treatment. For the purpose of investigating spillover, these zones and their reporters are dropped from the following analysis.

For our outcomes of interest – proportion of subject ever-reporting, total number of reports, and total number of reports in the final two weeks – we never find a spillover effect among control subjects who are exposed to indirect treatment by a contiguous zone. Estimates of spillover effects in this group are variable and unstable. If we instead look at the effect of indirect treatment among treated subjects (comparing [Treated, Indirect] to [Treated, No Indirect]), we similarly find that the direction and magnitude of estimated spillover effects are unstable. Only in the case of Phase 1 Reporters' number

of reporters in the final two weeks do we final a significant positive effect (Table J3, P1 Reporters model), but this is in the context of highly variable and unstable estimates across models. Nonetheless, direct treatment effects are highly stable across models (Tables J1-J3). We take this to mean that spillover is not a major concern for the analysis of our data.

Table J1. Total number of active reporters during Phase 2, considering spillover

	DV: At Least One Report During Phase 2		
	(Pooled)	(P1 Reporters)	(P2 Reporters)
Control, Indirect	-0.012	-0.029	-0.005
	(0.029)	(0.046)	(0.038)
Treated, No Indirect	0.120***	0.064*	0.144***
	(0.031)	(0.049)	(0.039)
Treated, Indirect	0.057**	0.105**	0.030
	(0.029)	(0.043)	(0.037)
Neighbor Nomination	0.035	0.048	
	(0.035)	(0.031)	
LC1 Nomination	-0.015		-0.003
	(0.025)		(0.027)
LC1 Announcement	-0.012		-0.017
	(0.024)		(0.025)
Phase 2	0.185***		
	(0.033)		
Intercept	0.002	0.178***	0.370***
	(0.060)	(0.040)	(0.035)
Observations	2,400	811	1,589
F Statistic	12.736***	4.120***	4.779***
Notes: one-tailed tests; weighted by inverse probability of assignment to exposure type; baseline is [control, no indirect] condition		*p<0.1; **p<0.05; ***p<0.01	

Table J2. Total number of reports submitted by each reporter during Phase 2, considering spillover

	DV: Total Number of Reports During Phase 2		
	(Pooled)	(P1 Reporters)	(P2 Reporters)
Control, Indirect	0.225	-0.215	0.432
	(0.242)	(0.352)	(0.317)
Treated, No Indirect	0.926***	0.478	1.132***
	(0.255)	(0.375)	(0.332)
Treated, Indirect	0.636***	0.707**	0.584**
	(0.234)	(0.332)	(0.311)
Neighbor Nomination	0.372	0.414*	
	(0.285)	(0.240)	
LC1 Nomination	0.291		0.347
	(0.203)		(0.223)
LC1 Announcement	-0.286		-0.299
	(0.194)		(0.210)
Phase 2	1.343***		
	(0.271)		
Intercept	-0.582	0.914***	1.993***
	(0.490)	(0.308)	(0.295)
Observations	2,400	811	1,589
F Statistic	9.549***	3.651***	2.817**

Notes: one-tailed tests; weighted by inverse probability of assignment to exposure type; baseline is [control, no indirect] condition

*p<0.1; **p<0.05; ***p<0.01

Table J3. Total number of reports submitted by each reporter during the last two weeks of Phase 2, considering spillover

	DV: Total Number of Reports During Last Two Weeks of Phase 2		
	(Pooled)	(P1 Reporters)	(P2 Reporters)
Control, Indirect	0.030	-0.023	0.057
	(0.045)	(0.057)	(0.060)
Treated, No Indirect	0.114***	-0.054	0.189***
	(0.047)	(0.061)	(0.063)
Treated, Indirect	0.141***	0.134***	0.142***
	(0.043)	(0.054)	(0.059)
Neighbor Nomination	0.011	0.033	
	(0.052)	(0.039)	
LC1 Nomination	0.046		0.067
	(0.037)		(0.042)
LC1 Announcement	-0.065*		-0.072*
	(0.036)		(0.040)
Phase 2	0.160***		
	(0.050)		
Intercept	-0.102	0.095*	0.185***
	(0.090)	(0.050)	(0.056)
Observations	2,400	811	1,589
F Statistic	5.768***	4.283***	3.133***
Notes: one-tailed tests; weighted by inverse probability of assignment to exposure type; baseline is [control, no indirect] condition	*p<0.1; **p<0.05; ***p<0.01		