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).

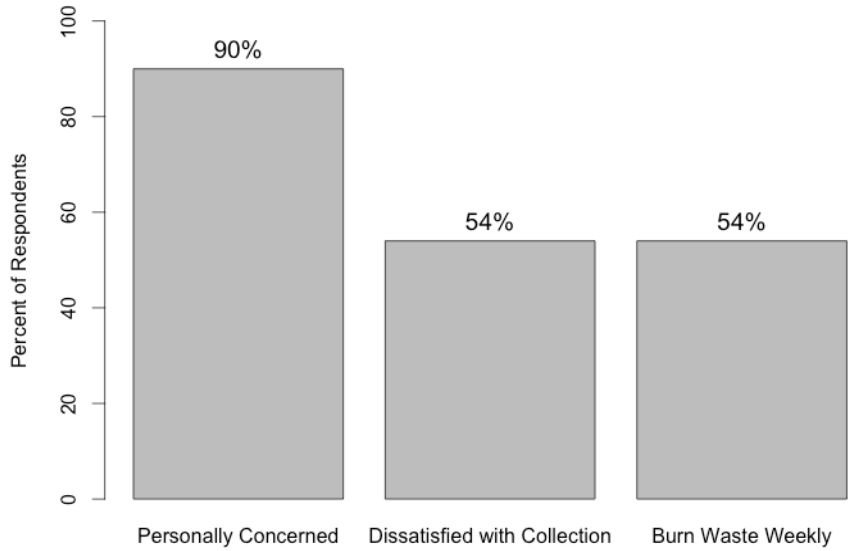

Figure A1. Resident perceptions of solid waste services and conditions in preliminary survey.

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.

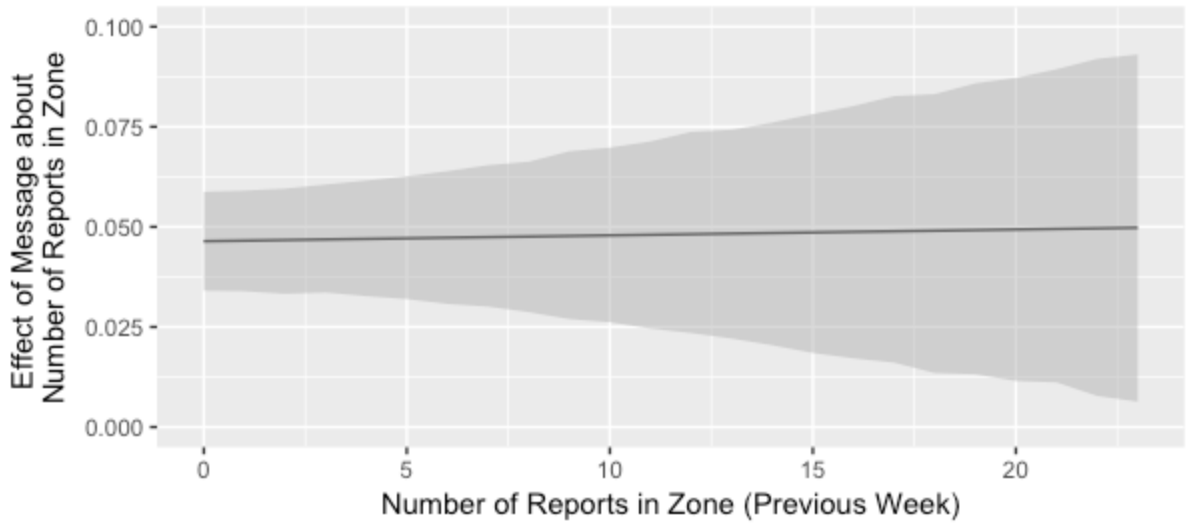

Figure H1. Marginal effects plot of conditional effect of message about zone-wise reports by the number of reports communicated in the message.

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		