Final report

Improving political communication

Results of a field experiment in Pakistan

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Final Report
"Improving Political Communication: Results of a Pilot Field Experiment in Pakistan"*

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Abstract: Elections are a blunt tool for accountability. Can enhanced politician-voter communication in the periods between elections improve democratic outcomes? We partner with a politician in Pakistan and design an experiment with Interactive Voice Response (IVR) — a technology that enables him to robocall a large number of voters in his own voice to ask them questions and receive feedback. We randomize whether respondents receive a call soliciting preferences about upcoming decisions the politician must make. A follow-up call randomizes how responsive the politician is to voters' preferences. Results show that respondents receiving information-seeking calls from the MPA experience improved perceptions of government competence and place greater emphasis on incumbent performance in their electoral calculus. Point estimates indicate a positive but imprecisely estimated effect of the intervention on respondents' perceptions of the incumbent. We also find evidence that the above effects are driven largely by contact that specifically addresses the demands of constituents rather than by the mere fact of increased communication with the MPA, and that some types of content are more valued than others by voters. The main takeaway of the pre-test and pilot is that politician communication using IVR can encourage voters to engage more effectively with the democratic process in some ways.

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1 Introduction

A common refrain among developing country voters is that politicians make promises and bring gifts when elections approach but they disappear between them. Furthermore, developing countries are characterized by relatively weak political party structures, elected representatives without robust organizational resources, and high opportunity costs to voters for collectively sanctioning poor political performance. These characteristics contribute to rendering elections blunt instruments for voters to hold politicians to account for their behavior. One result is that politicians frequently lose office but better candidates rarely get elected. And even if politicians want to perform well, the years between elections and lack of information during those periods makes it difficult for them to do so. All told, the accountability mechanism promised by democratic competition appears largely ineffective in these settings.

Lieberman, Posner and Tsai (2014) outline the conditions under which voters can effectively exercise electoral accountability. They need information on politician performance, benchmarks to judge this performance against, the means to hold politicians accountable, the will to do so, and the belief that other voters will do so as well. Usually more than one of these essential ingredients is absent. Past work that has sought to improve politician accountability by providing information about politicians to voters has generated mixed results (Adida et al., 2016; Banerjee et al., 2010; Bidwell, Casey and Glennerster, 2015; Chong et al., 2015; Dunning et al., 2015; Grossman and Michelitch, 2016; Lieberman, Posner and Tsai, 2014; Bidwell, Casey and Glennerster, 2015). One reason for this is informational interventions place considerable expectations on voters.

Our study departs from this line of work by establishing regular *political communication* between politicians and voters rather than merely handing voters more or new information. We use political communication to provide new information to *politicians* rather than voters — information about voter preferences. We theorize that the availability of high-frequency,

actionable information about voter preferences to politicians will nudge them to become more responsive to voter preferences. In the normal course of events, politicians receive information about the political effectiveness of their policy choices and decisions only when an election occurs. The politician learns whether his performance has been satisfactory to enough voters long after the fact, offering him no way to alter his behavior between elections. Providing frequent two-way communication between politicians and voters allows the politician to learn from voters while on the job.

Our study also builds on a small literature that shows that political messaging regarding the effort and competence of an incumbent can be effective in changing voter evaluations of elected officials (Kendall, Nannicini and Trebbi, 2015). Prior work specifically on credit-claiming by politicians demonstrates that the *quantity* of messages shifts opinion on elected representatives more than the exact content of the messages (Grimmer, Messing and Westwood, 2012). Based on these studies, we hypothesize that voters take "information-seeking" behavior — meaning efforts by the incumbent to learn about the preferences of voters — as a signal of effort and good will, and as a sign that the politician seeks to improve his competence.

The experiment springboards political communication by making use of an interactive voice response (IVR) technology. This technology enables politicians to contact a large number of voters via robocalls recorded in their own voice. Voters in turn can respond to specific questions by pushing keys on their phone. We proceed in two main stages. Stage One of the experiment randomizes whether or not households receive a call from their legislator. Stage Two of the experiment randomizes how responsive the legislator is to the voters' preferences that were collected in the first stage. In Stage Two, some households receive a second phone call specifying what the MPA intends to do with the feedback he received in Stage One while others receive a generic follow-up that does not mention Stage One or the suggestions voters made using IVR. The second stage closes the communication loop, in that

voters receive feedback that tells them they have been heard.

This document reports results of a pre-test and pilot intervention conducted between February and June 2017 in the province of Khyber Pakhtunkhwa (KP), Pakistan, in partnership with one member of the provincial assembly. The objectives of the pre-test were to test and optimize the technology involved as finalize the instruments for the pilot. We also wanted to check if the low rates of take-up by voters that have been highlighted in the literature on the use of information technology in less developed countries could be improved.(Humphreys and Weinstein, 2012; Grossman and Michelitch, 2016; Blair, Littman and Paluck, 2017; Open Parliament, N.d.) We conducted our pre-test using 224 households in a Village Council area we exclude from our pilot, assigning 150 people to get the Stage One *contact* treatment. The remainder (25) were controls or (49) received a simplified version of the treatment. The pilot results revealed that 70.3 percent of respondents answered their phones after one or two attempts to reach them and around 30 percent of respondents who picked up the phone and were asked a question completed the call. Therefore, overall compliance was 21 percent. While this is better than participation in SMS based interventions, likely because we are placing calls rather than relying on responses, we built on this pre-test by changing both the time we placed calls and the number of times that we placed calls to boost compliance rates.

From the pilot, we estimate the effects of "information-seeking" contacts and responsive feedback by the incumbent on voter evaluations. By providing voters the opportunity to offer feedback to the politician and hear a responsive follow-up to their feedback, our study focuses on the impact of the active engagement of the voter in the process of communication. Specifically, we ask three classes of questions. Does this communication loop improve voters' views of the incumbents who is actively engaging with them? Do efforts to reach out change evaluations of government competence more broadly? Finally, does communicating with voters about service delivery foster conditions under which electoral accountability is more

likely to occur in a future election?

In the pilot, we find that within this legislator's constituency, voter support for the incumbent declined during the period we study. Constituents who received robocalls that elicited their preferences evaluate the incumbent more favorably and evaluate government competence more positively than constituents who did not receive robocalls, however many of these effects are impreceisely estimated. Treated voters are also more likely to state that they intend to make voting decisions based on how they view incumbent performance rather than using other criteria. Most of these effects are driven by treated individuals who receive follow-up calls that specifically address their demands rather than by simple repeated robocall contact with the MPA. However, even though treated individuals warm to the incumbent, we find no evidence that they will engage in party switching at the next election. Furthermore, increased attentiveness to performance in the voting calculus is not paired with an increased sense of efficacy. Lastly, respondents were more likely to provide feedback to the MPA when he asked them a question about the use of constituency development funds than when he asked them about his own time-use, indicating that successfully engaging voters in political communication is contingent on the perceived value of that communication.

2 Context and Experimental Design

We conducted this experiment in partnership with one Member of the Provincial Assembly (MPA) in Khyber Pakhtunkhwa — a province of 30 million in Pakistan. This provides a highly appropriate research setting for our purposes. First, politicians in KP face stiff competition in retaining their seats from one election to the next, and are therefore eager to identify and utilize new channels of political communication with voters. Second, Pakistan is in the process of consolidating its democratic institutions. The first democratically elected government to finish its complete term left office in 2013. The current incumbents

are the first in the country's history to follow a democratically elected government. Enhanced communication between politicians and voters constitutes an important step in the democratization of the country.

2.1 Sampling

We use multistage cluster sampling in the partner politician's electoral constituency. Within the constituency, we sample Village Councils (VC), which are local government bodies; within Village Council-demarcated areas, we sample settlements, which are clusters of households ranging from 70 to 500 households; within settlements we sample individual households and enroll the male head of household. We sample 11 of the 25 Village Council areas within our partner MPA's constituency and we sample 4 of between 6 and 12 settlements within each Village Council area. Within each settlement, we sample 25 to 28 households. There are a total of 1,218 male heads of household included in the study.

Detailed gazettes enumerating all settlements within the Village Council areas are not available. The list of names of the Village Councils that we have references only one to three of the largest villages. There are unknown numbers of additional smaller settlements within each Village Council. To generate a more complete list of settlements, we sent teams of enumerators to each of our sampled Village Councils to map all settlements that they were sure fell within the boundaries of the Village Council. From this more complete list of settlements, we sample four.

We use a random walk to select households within settlements. Within each sampled settlement, enumerators went to the center of the settlement, and fanned out in two directions (if there were only two directions) or four directions (if there were more), surveying every other household on the right hand side. In each sampled household, we survey the most senior male with a cell phone.[1] Enumerators conduct a baseline survey to collect phone

[1] Most adult males in the area possess cell phones.

numbers and information on pre-treatment covariates, as well as to administer informed consent.

Because we use simple random sampling at each stage of the design, more populous Village Councils and more populous settlements have the same probability of being sampled as less populous Village Councils and less populous settlements. Therefore, as a robustness check, we weight our analysis so that our effects are representative of all households in the constituency who respond to door-to-door sampling. Further details are provided in Appendix A.

2.2 Main Treatment Arms

Our project has three treatment arms, as represented by the household numbers that appear in Table 1. Stage One assigns households into *contact* and *control* conditions. Households that complied with the *contact* treatment in Stage One (i.e. answered the phone) were then randomized into either a *generic* or a *responsive* treatment in Stage Two. Stage Two occurs in time after the completion of Stage One.

Table 1: Number of Households Assigned to Main Experimental Conditions

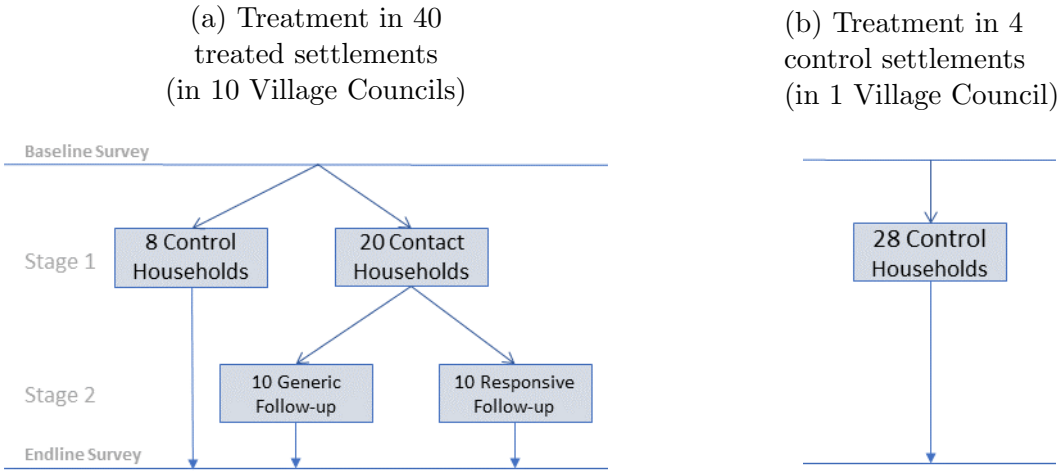
Stage One	Control	Contact	
	426	792	
Stage Two		Generic Follow-Up	Responsive Follow-up
		340	345

Note: The number of households in Stage Two does not equal the number of treated households in Stage One because the 107 Contact households that did not comply with the Stage One treatment were not contacted in Stage Two.

Of the 11 Village Council areas in our study, we randomly place one in a pure control condition. We assigned all sampled households in the single pure control Village Council to the control condition, as shown in Figure 1b. We call these households *pure control* households. Within the ten treated Village Council areas, we block on settlement. We assigned Households in the ten treated Village Council areas to either treatment or control,

as the numbers reported in Figure 1a show.

Figure 1: Main experimental stages and treatement conditions by settlement



Note: This represents the ideal treatment assignment in 37 of our 44 settlements. In the other 7, we sampled 25, 26, or 27 households instead of 20 and the randomization schemes are slightly adjusted to account for this. Furthermore, we randomize Stage Two only among compliers from Stage One rather than all treated households.

Most settlements had 28 households sampled from that settlement. Within that prototypical settlement, eight are sorted into the *control* condition while the remaining 28 households receive a *contact* treatment in Stage One, as depicted in Figure 1a. (We refer interchangeably to these as households and as respondents.) In total, we have 317 control households within the 10 treated Village Council areas. There are an additional 109 control households in the one pure control Village Council area. In the 10 Village Council areas that are assigned to the treatment condition, there are 792 treated households. These numbers do not directly map onto Figure 1a because in 7 of the 44 settlements in the sample, we sampled between 25 and 27 households rather than 28.

We randomize the Stage Two treatment among households who were assigned to Contact in Stage One and who answered the phone when called. Of the 20 treated units per settlement, we hoped to call 10 with a *generic* follow-up call while the other 10 were slotted to receive a *responsive* follow-up call. Because we only randomized among compliers, these groups instead were around eight or nine each rather than 10. In total, 340 households

received the *generic* follow-up call and 345 households received the *responsive* follow-up call. The latter directly addresses the demands of voters in their Village Council area, acknowledging their input, whereas the generic follow-up simply acknowledges prior communication. Drafts of the scripts for the follow-up calls appear in Section 2.3.2.

2.3 Treatment Content

Within the Stage One *contact* condition, we implemented our design to explore whether it is more effective for politicians to communicate with voters rather than not to communicate with them, and if they do communicate, whether two questions are preferred by voters to a single question. We also study whether the nature of the question matters to voters, by having the MPA record two separate questions: one that asks how the MPA should allocate constituency development funds and a second that asks how the MPA should allocate time between legislative debate and other (i.e. constituency) services. We hypothesize that spending questions will engender larger treatment effects because we believe that voters put greater priority on the local development that spending produces than on their representative's parliamentary activities. We randomize which question the MPA asks in the robocall to assess whether this is true. The questions that we use were determined working with our partner MPA, and reflect his informational priorities.

2.3.1 Stage One Treatment (Contact) and Secondary Arms (Questions)

Our Stage One treatment is a recorded call by our partner MPA that asks a question about upcoming decisions and also gives him the opportunity to credit-claim for other activities. The main treatment of theoretical interest is whether a voter receives a call asking a question (*contact*) compared to no direct contact by phone from the MPA (*control*). We also randomized whether respondents received a development question, a time-use question, or both.

Stage One Scripts

Assalam U Alaikum. I am [MPA NAME], your elected MPA. I am calling you as your elected representative in the provincial assembly. This phone call is a part of a new effort to reach out to my constituents to get their opinions and inform them about my activities. This phone call will not take long.

Contact development question: As you know, many development works in the constituency have been completed and some are underway. Recently, I have approved some more funds for the constituency. I would like to know your opinion on how to spend this money. I am going to read a list of options to you. Please press the number that corresponds to the option you would like to recommend. Please wait until I have finished the list before choosing.

- If you prefer roads, press 1
- If you want to have a funeral services area, press 2
- If you prefer electricity infrastructure like transformers and electric poles, press 3
- If you want to have paved streets, press 4

Contact legislation question: As your MPA, I have participated actively in the Khyber Pakhtunkhwa assembly's discussions. Besides other successes, I have passed a bill to prohibit interest on loans. Secondly, I have also passed a bill to do away with the housing tax in the rural areas of the province. Do you want me to continue spending time in the assembly discussions or to concentrate more on your other issues and concerns?

- If you prefer I spend more time participating in the assembly discussions, press 1
- If you prefer I spend more time looking at other issues, press 2

Thank you for your feedback. In the end I would like to bring to your notice that in terms of education, I have opened two degree colleges in our constituency, one for boys and another for girls; different schools have been upgraded; and new schools are being constructed. In terms of health care, we are going to build a large hospital in the constituency, and many BHUs [Basic Health Units] have been upgraded to RHCs [Rehabilitation Hospital Units]. In terms of electricity infrastructure we have done a record number of works; installed transformers and electric poles and overhauled the old transformers. In terms of roads, the construction of Takht Bhais main road, from Razaro to Saro Shah, has been approved and the work is already underway. For the farmers of this constituency, we are building watercourses. To prevent Razaro and Uthmanzai from flooding, we are strengthening the banks of River Jindi. Similarly, we are making progress in other areas such as paving the streets and sewers. As you know I am the only MPA in this constituency who is trying to bring the general public and MPA closer. I have started different ways of communicating with my constituents. I promise you that in coming days I will do everything for the prosperity and development of my constituency. I look forward to your support. Good Bye.

Embedded SMS Experiment We also embed two smaller SMS experiments prior to our Stage One contact. These are aimed at improving compliance in the experiment. We build on the evidence gathered in SMS mobilization campaigns encouraging electoral turnout that indicates positive effects of mobilization messages in developed contexts in both high and low salience elections (Dale and Strauss, 2009; Malhotra et al., 2011) as well as in developing countries (Aker, Collier and Vicente, 2016).[2] We craft SMS messages to study whether some types of messages are more likely to encourage citizens to spend a small amount of time responding to their MPA's question. The results are reported in Section 3.1 and generally show that, if anything, simple encouragements are more effective than SMS encouragements that attempt to induce peer pressure or focus on particularistic rather than communal benefits.

2.3.2 Stage Two Treatment (Follow-up Contact)

After we collected the results from the first treatment and aggregated them at the village level for anonymity and clarity, we shared them with the MPA and his responses were crafted. His responses were given two different formats: a follow-up phone call that provided *responsive* feedback or one that provided *generic* feedback. The two follow-up messages were written working with the MPA.

Stage Two Scripts

Hello. I am [MPA NAME] your elected MPA. I am calling you, my constituents, to let you know about my programs for the coming months and my last year [as an MPA].

[***Responsive component:*** You got a call on my behalf a while ago in which I asked for your opinion on the development works in the constituency and about my performance. In that regard, I want to get in touch with you again. I have received the results of the feedback that you gave me on the phone, and I have reviewed it very carefully. Now I know that what sort of development works you prefer to be done in the constituency. Since most of you wanted more

[2]However, there is also evidence that SMS messages may be insufficient to prompt citizens in developing countries to report public service deficiencies or corruption (Blair, Littman and Paluck, 2017; Grossman, Michelitch and Santamaria, 2016).

work to be done paving streets, constructing roads, and having better electricity infrastructure like installing new transformers and electricity lines, I want to assure you that in my last year [as an MPA] I will get down to it. Moreover, as most of you liked and appreciated my performance in the provincial assembly, I will put more effort into representing you in an effective manner in the assembly.

I want to apprise you of a few more things.]

This year, after a great struggle, I will inaugurate two degree colleges, one for boys and another for girls, in the constituency. Similarly, the hospital that has been completed will start its operations very soon. I will also create job opportunities for you. Let's work together for the development and prosperity of this constituency. Thank you.

3 Data

We conducted baseline and endline surveys of 1,218 households in the partner MPA's constituency. We asked a short battery of demographic and political knowledge questions in the baseline while also collecting phone numbers, and we asked a series of questions about political preferences and attitudes in both the baseline and endline surveys. We group our outcomes into three domains: incumbent evaluation, government performance, and prospects for accountability.

In the incumbent evaluation domain, we ask respondents a feeling thermometer question (ranging from 1 to 10) about their MPA ("MPA thermometer") and about his party ("inc party thermometer") as well as asking them about their preferred party ("prefers MPA party"). In the government performance domain, we ask people whether they agree that the provincial government is competent at providing goods ("prov govt competent provider"), whether having democratically elected officials is important to them ("elections important"), and whether they agree that the state ("state looks after"), the incumbent's party ("party looks after"), and the MPA ("MPA looks after") look after them. In the prospects for accountability domain, we ask whether the MPA is likely to follow through on his promises ("MPA likely to follow through") and whether they can affect change in politics ("political

efficacy"). In this domain, we also ask them to rank six inputs into their voting decision in order of importance; one of these inputs is MPA performance ("MPA performance important in voting decision"), while the others are the party of the incumbent, the promises the incumbent makes, the family's political preferences, and the state of the Pakistani economy.

For each of our three outcome domains, we construct an index out of the relevant questions, using a method described in Kling, Liebman and Katz (2007). This method creates an index as a sum of standardized variables, where we construct each standardized variable by subtracting the mean of the control group and dividing by the standard deviation within the control group. Following Kling, Liebman and Katz (2007), we impute all missing values using the mean of the treatment group. For the incumbent evaluation index and the government performance index, we collected all of our measures at both baseline and endline. For these two indices we use difference-in-differences estimation, constructing the indices by standardizing measures to the post-treatment control mean and standard deviation and imputing missing data by mean of the treatment group *and* time period. We construct the prospects for accountability index using answers to questions asked exclusively in the endline survey, and thus results compare treated and control respondents.

In Panel A of Table 2, we present summary statistics of data collected in the baseline survey and analyzed in what follows. Throughout Table 2, we split the data into three treatment conditions: the main contact condition, control units in the same villages as those in the contact condition, and control units in the pure control condition. We split responses this way because we hypothesize that results for respondents from the pure control village council will be different than those for the rest of the sample.[3] The reason is that we expect there may be some spillover within settlements from treated to control respondents. Panel A1 shows the breakdown of basic covariates by treatment status and Panels A2 and A3

[3]While we include these pure control units in the main analysis as control units, they have no influence on the effect estimates because settlement-level fixed effects absorb all of the pure control units. Furthermore, the pure control households are not perfectly balanced because they were cluster randomized into that condition at the Village Council area. Again the settlement-level fixed effects will absorb these units and render the imbalance inconsequential.

show summary statistics for two sets of outcomes from the baseline survey. Comparing the treatment and control units (after dropping the pure control units), evaluations of provincial government competence in providing goods is the only pre-treatment covariate with a statistically significant difference at the 0.1 level.[4] For outcomes we collect in the baseline, we prefer a difference-in-differences estimation as outlined in Section 4.1.

Panel B shows summary statistics for our three categories of outcomes on the endline. The most important detail to emerge from the summary statistics is the overall downward trend in evaluations of the incumbent, government, and prospects for accountability. This contextualizes the positive findings we detect. Our treatment did not generate absolute increases in perceptions of the MPA, his party, or government performance, but instead reduced the drop in perceptions of these individuals and institutions.

3.1 Compliance

Overall, 86.5 percent of households answered the Stage One contact call, and 35.6 percent of those that answered responded to at least one question the MPA asked. Attrition was just below 1 percent and is not predicted by treatment. This means about 30.8 percent of households in our sample not only answered the call but answered some of the MPA's questions. This is a high rate of compliance. We define "Stage One partial compliance" as answering the phone whereas we define "Stage One full compliance" as listening long enough to answer at least one IVR question. We carried out Stage Two among only the 86.5 percent of households that answered the Stage One contact call. Of these households, 88.2 percent of them answered the Stage Two phone call, and 38.9 percent of those that answered listened all the way to the end of the call. This means that about 34.5 percent of the households assigned to receive the follow-up call listened all the way to the end. This is also a high rate of compliance.

[4]Kolmogorov-Smirov tests also fail to reject the null that the treatment and control groups have different distributions for all non-binary pre-treatment covariates and baseline outcomes.

Table 2: **Summary Statistics**

	Whole Sample		Contact Treatment			Control			Pure Control		
Panel A: Baseline Data	Min	Max	Obs.	Mean	SD	Obs.	Mean	SD	Obs.	Mean	SD
Panel A1: Covariates											
Prefers MPAs Party	0	1	792	0.201	0.401	316	0.174	0.38	109	0.138	0.346
Owns Motorized Vehicle	0	1	792	0.433	0.496	317	0.432	0.496	109	0.523	0.502
Knows PML-N out of KP Govt.	0	1	789	0.492	0.5	316	0.462	0.499	109	0.44	0.499
Knows Who President Is	0	1	782	0.469	0.499	317	0.495	0.501	108	0.657	0.477
Panel A2: Evaluation Outcomes											
Incumbent evaluation index	−0.81	2.254	792	0.093	0.913	317	0.052	0.89	109	−0.152	0.751
MPA thermometer (1-10)	1	10	792	3.554	2.793	317	3.423	2.644	109	2.743	2.234
Inc. party thermometer (1-10)	1	10	792	4.537	3.067	317	4.536	3.134	109	3.789	2.579
Prefers MPAs party (0/1)	0	1	792	0.201	0.401	316	0.174	0.38	109	0.138	0.346
Panel A3: Performance Outcomes											
Government performance index	−1.254	2.222	792	0.008	0.608	317	0.01	0.581	109	−0.03	0.479
Prov. govt. competent provider (1-4)	1	4	651	2.647	0.769	267	2.753	0.735	102	2.725	0.966
Elections important (1-4)	1	4	761	3.106	1.103	304	3.145	1.099	106	3.415	0.904
State looks after (1-5)	1	5	791	1.53	0.879	316	1.579	0.941	109	1.541	0.845
Party looks after (1-5)	1	5	789	1.976	1.327	314	1.857	1.244	108	1.648	1.008
MPA looks after (1-5)	1	5	789	2.129	1.422	317	1.987	1.355	109	1.725	1.088
	Whole Sample		Contact Treatment			Control			Pure Control		
Panel B: Endline Data	Min	Max	Obs.	Mean	SD	Obs.	Mean	SD	Obs.	Mean	SD
Panel B1: Evaluation Outcomes											
Incumbent evaluation index	−0.81	2.254	792	0.008	0.97	317	−0.072	0.891	109	−0.296	0.785
MPA thermometer (1-10)	1	10	785	3.134	2.895	314	2.981	2.616	108	2.241	2.174
Inc. party thermometer (1-10)	1	10	785	4.186	3.314	314	3.933	3.196	108	3.046	2.522
Prefers MPAs party (0/1)	0	1	785	0.21	0.408	314	0.175	0.381	108	0.139	0.347
Panel B2: Performance Outcomes											
Government performance index	−1.254	2.222	792	−0.051	0.526	317	−0.094	0.495	109	−0.231	0.422
Prov. govt. competent provider (1-4)	1	4	722	2.53	0.819	289	2.502	0.821	107	2.206	1.007
Elections important (1-4)	1	4	743	3.207	1.001	302	3.248	1.012	104	3.394	0.897
State looks after (1-5)	1	5	780	1.533	0.858	312	1.612	0.949	108	1.509	0.717
Party looks after (1-5)	1	5	783	1.731	1.149	311	1.585	0.966	108	1.333	0.723
MPA looks after (1-5)	1	5	782	2.073	1.382	314	1.85	1.281	108	1.667	0.986
Panel B3: Accountability Outcomes											
Accountability prospects index	−1.3	1.813	792	0.063	0.561	317	−0.002	0.537	109	0.005	0.571
MPA likely to follow through (1-4)	1	4	677	2.455	0.769	267	2.434	0.76	93	2.419	0.771
Political efficacy (1-4)	1	5	778	2.35	1.369	311	2.315	1.383	108	2.398	1.427
MPA performance important in voting decision (1-6)	0	5	697	2.395	1.898	288	2.115	1.939	86	2.058	2.111

Beyond large and significant variations across settlements in average response rates, the best predictor of answering at least one question asked by the MPA is whether the first question the respondent was asked was about development or about MPA time-use. As seen in Table 3, controlling for settlement fixed effects and several baseline covariates, being asked a question about development priorities rather than about MPA time-use ("Asked dev. q. first") leads to a 10.9 percentage point increase in the probability of answering the first question you hear. This represents an over 100 percent increase in the response rate. This demonstrates the strong effects of the importance of the question being asked on the willingness for voters to engage with their MPA.

Table 3: **Question Type and Compliance**

	Answered First IVR Question
Intercept	0.099***
	[0.033]
Asked dev. q. first (0/1)	0.109***
	[0.032]
Prefers MPAs party (0/1)	0.032
	[0.038]
Owns motorized vehicle (0/1)	0.048
	[0.042]
Knows PML-N out of KP govt. (0/1)	−0.014
	[0.031]
Knows Who President Is (0/1)	0.075**
	[0.038]
Settlement FEs	X
N	779

Notes: *p<0.1; **p<0.05; ***p<0.01. OLS estimates; standard errors accounting for clustering at the settlement level in square brackets.

3.1.1 SMS encouragement experiment

We also conducted an SMS encouragement experiment before the first round of calls in order to boost compliance. We assigned households that were going to receive an IVR call to one of six SMS treatment conditions. The SMS message was sent one hour before the IVR calls were placed. The various treatment groups can be found in Table 4. The SMS experiment took the shape of a two by three experiment where: (1) respondents either received a generic participation encouragement (*placebo*), an encouragement focusing on the *community* benefits to participating, or an encouragement focusing on the *personal* benefits to participating; and (2) respondents either had no mention of peer participation (*no peer*) or were reminded at the end of the encouragement message that their *peers* were also participating. The full texts of the SMS treatments can be found in Appendix C.

Table 4: Number of Individuals by SMS Treatment Arm

	Placebo	Community	Personal
No Peer	120	120	120
Peer	120	156	156

First, in Table 5 we display the raw proportions and standard errors of households who did or did not answer the IVR call by SMS encouragement condition. None of the comparisons between these proportions are statistically significant except for the comparison between the *peer* message and non-peer message among those told to think about the encouragement as personally beneficial. While this may indicate that peer effects are largest when people consider the personal stakes of political participation, we caution against over-interpreting this effect in a small sample.

Table 5: Proportion of Respondents Answering the Phone by SMS Treatment

	Placebo	Community	Personal
No Peer	0.875	0.85	0.825
	(0.03)	(0.033)	(0.035)
Peer	0.875	0.859	0.897
	(0.03)	(0.028)	(0.024)

Second, in Table 6 we display the raw proportions and standard errors of whether individuals answered a question in the IVR call. This represents a higher level of engagement with the call. As we can see, the stand-out group both in size and statistical significance, is the pure placebo group, indicating that longer messages may have hurt participation in this intervention.

Table 6: Proportion of Respondents Answering Any Question by SMS Treatment

	Placebo	Community	Personal
No Peer	0.425	0.3	0.292
	(0.045)	(0.042)	(0.042)
Peer	0.258	0.263	0.321
	(0.04)	(0.035)	(0.037)

4 Analysis

4.1 Estimation

We employ two different specifications to estimate treatment effects of our intervention on the main outcomes of interest. For both specifications, we present intent-to-treat (ITT) effects and local average treatment effects (LATEs) among compliers using instrumental variables.

Our main specification uses some baseline covariate information as well as settlement fixed effects to improve the power of our tests. For outcomes where we do not have a baseline measure, our preferred specification uses simple pre-treatment covariate adjustments and block (settlement) fixed effects to increase the power of our tests:

$$Y_{ij} = \tau D_i + \beta \mathrm{X}_i + \theta_j + \epsilon_{ij}, \tag{1}$$

where Y_{ij} is the endline value of some outcome for household i in settlement j, where X_i is a vector of pre-treatment variables, and where θ_j is a settlement fixed effect. Note that when we analyze spillover effects, we remove the fixed effects because the pure control was randomly selected at the village level. We estimate the ITT effects using OLS for all outcomes of interest and use 2SLS for the LATEs.

For outcomes where we have a baseline measure, our preferred specification uses a difference-in-differences approach, continuing to adjust for pre-treatment covariates:

$$Y_{ijt} = \alpha_0 D_i + \alpha_1 T_t + \tau D_i T_t + \beta \mathrm{X}_i + \theta_j + \epsilon_{ijt}, \tag{2}$$

where Y_{ijt} is the outcome at time $t = 0$, the baseline, or time $t = 1$, the endline, and T_t is a dummy for that is 1 at the endline and 0 at the baseline. Again we estimate coefficients using OLS for the ITT effects and 2SLS for the LATEs.

In all specifications, our control variables X_i include baseline measures of whether the respondent supports the incumbent's party, whether the household has a motorized vehicle, and a measure of the political knowledge of the respondent (based on answers to questions about whether the respondent knows that the party in control of the national assembly is not in the KP provincial assembly government and whether he knows the identity of the president). For some outcomes, we are able to use a difference-in-differences specification, whereas for others we use pre-treatment covariates in addition to the above covariates to boost our power. The additional pre-treatment covariates we use follow those specified in our pre-analysis plan. We report both heteroskedasticity consistent standard errors as well as cluster-robust standard errors at the settlement level.

Although we originally intended to fit all of our analyses using sampling weights, many of our analyses are on subsets of our full sample. Sampling weights do not make sense applied to those regressions. Therefore, we report all analyses without any weights. See Appendix A for results of the analysis of the Stage One treatment effects with sampling weights. The results are qualitatively similar.

4.2 Stage One effects of any contact

We present the overall effects of the intervention — the Stage One *contact* treatment — in Table 7. Overall, respondents warmed to the MPA and his party, evaluated the government as more competent, and prospects for accountability improved, although many of these estimates are imprecisely estimated. All the indices are expressed in standard deviation units whereas the effects on constituent outcomes are in their natural units. Summary statistics including standard deviations can be found in Section 3. The first column presents the ITT effects, while columns two and three present LATE effects on partial and full compliers, respectively. We define partial compliers as households that answered the Stage One *contact* call and full compliers as households that listened to the call long enough to answer at least

one of the IVR questions.

4.2.1 Evaluation of the incumbent

In the first panel covering incumbent evaluations, none of the effects are statistically significant, although all point estimates are positive. More specifically, receiving any *contact* from the MPA increases evaluations of the incumbent and his party by 0.05 standard deviation units, although the effect is not statistically significant. We also see a larger effect of the intervention on support for the incumbent party than for the MPA, but these effects are also not statistically significant. Lastly, there little evidence that respondents intend to party switch to the MPA's party following the intervention, although again the signs are positive.

There is more precise evidence of an overall treatment effect on support for the MPA when analyzing an embedded endorsement experiment. In our endline survey, we asked about the level of support for two bills, randomizing whether the MPA's name appeared next to the bill or not. We assigned two settlements per Village Council to receive the endorsed version of the questions and the remaining two settlements to receive the control version.[5] The English translations of the questions are given below; the italicized phrase was delivered to respondents only in the treated settlements.

- A recent bill [*sponsored and led by [MPA NAME]*] did away with housing taxes in rural areas. Some argue that this is fair as it relieves a burden on rural citizens, but others argue it hurts the ability of the KP government to provide services. Do you agree with this bill?
- A recent bill [*sponsored and led by [MPA NAME]*] restricted lenders from charging interest. While this may make it harder to receive loans, it also prevents immoral lending practices. Do you agree with this bill?

We first conduct a simple analysis of the endorsement experiment, interacting the main

[5]We randomized the experiment at the settlement level rather than the household level to ensure that enumeration teams would have no difficulty administering the correct version. As a result, our analysis includes village rather than settlement fixed effects.

Table 7: **Treatment Effects of Stage One Contact**

	ITT	Partial Compliers LATE	Full Compliers LATE	N
A. Incumbent evaluation index	0.046	0.054	0.152	1203
	(0.064)	(0.074)	(0.209)	
	[0.048]	[0.055]	[0.155]	
MPA thermometer (1-10)	0.078	0.09	0.256	1191
	(0.183)	(0.211)	(0.597)	
	[0.15]	[0.174]	[0.491]	
Inc. party thermometer (1-10)	0.331	0.382	1.084	1191
	(0.206)	(0.237)	(0.674)	
	[0.203]	[0.235]	[0.648]*	
Prefers MPAs party (0/1)	0.013	0.015	0.042	1191
	(0.028)	(0.032)	(0.092)	
	[0.016]	[0.019]	[0.053]	
B. Government performance index	0.07	0.081	0.229	1202
	(0.04)*	(0.046)*	(0.13)*	
	[0.036]*	[0.042]*	[0.12]*	
Prov. govt. competent provider (1-4)	0.155	0.179	0.487	959
	(0.075)**	(0.087)**	(0.237)**	
	[0.079]*	[0.093]*	[0.254]*	
Elections important (1-4)	0.033	0.038	0.108	1102
	(0.089)	(0.102)	(0.288)	
	[0.041]	[0.047]	[0.133]	
State looks after (1-5)	−0.002	−0.002	−0.005	1182
	(0.072)	(0.084)	(0.236)	
	[0.056]	[0.065]	[0.183]	
Party looks after (1-5)	0.051	0.059	0.166	1180
	(0.088)	(0.102)	(0.289)	
	[0.096]	[0.112]	[0.316]	
MPA looks after (1-5)	0.065	0.075	0.211	1186
	(0.096)	(0.11)	(0.313)	
	[0.054]	[0.063]	[0.178]	
C. Accountability prospects index	0.065	0.075	0.211	1174
	(0.035)*	(0.04)*	(0.113)*	
	[0.045]	[0.053]	[0.149]	
MPA likely to follow through (1-4)	0.044	0.051	0.148	1002
	(0.055)	(0.063)	(0.183)	
	[0.062]	[0.073]	[0.213]	
Political efficacy (1-4)	0.021	0.024	0.068	1181
	(0.087)	(0.1)	(0.282)	
	[0.089]	[0.105]	[0.295]	
MPA performance important in voting decision (1-6)	0.221	0.254	0.74	1029
	(0.137)	(0.158)	(0.461)	
	[0.135]	[0.159]	[0.464]	

Notes: *p<0.1; **p<0.05; ***p<0.01. Each cell represents a model fit with either OLS or 2SLS. For the LATEs, *contact* instruments for partial compliance — answering the phone — in the second column and full compliance — answering an IVR question — in the third column. We present two standard errors: (HC2 robust standard errors) and [cluster robust standard errors] at the settlement level. Estimates for domains A and B are specified using the difference-in-differences specification in Equation 2 while estimates in panel C are fit using the simple specification reported in Equation 1. All estimates include settlement FEs. Most models include "supports MPA party," "owns motorized vehicle," "knows PML-N out of KP govt," and "knows who President is" as covariates. Exceptions as specified in our pre-analysis plan are made for the following outcomes: when "supports MPA party" is the outcome, we exclude the baseline variable "supports MPA party" as a covariate; for all outcomes in domain C except for "political efficacy," we add baseline values of "political efficacy" as a covariate; when "MPA likely to follow through" is the outcome, we include baseline values of "MPA thermometer" as a covariate. We build the index for each of our outcome domains using the method described in Kling, Liebman and Katz (2007). This method creates an index as a sum of the constituent outcomes, standardized by the mean and standard deviation of that outcome for the control group in the baseline time period (except for domain C where we just use the control group). All missing values are imputed using the mean of the respective treatment and time group.

contact treatment with the endorsement treatment. Table 8 contains the results of an OLS regression of the level of support for the bill, ranging from one to four, regressed on a dummy variable for whether the respondent received the endorsed version of the questions, a dummy variable for whether the household was in the main contact treatment, and the interaction of the two dummy variables. In the control group, the endorsement had a clear negative effect on overall support for the bill. However, the effect of the endorsement in the contact group is significantly more positive, changing the effect of the endorsement to around 0. The intervention thus appears to have improved support for the incumbent. One possible explanation for the precision of these results in contrast with the results from the straightforward feeling thermometers is that this endorsement experiment mitigated desirability bias in the control group, whose members might have been more likely to rate the MPA more highly when asked directly about him. Of course, it is also possible that the difference in results is due to different specifications and different questions being asked. Nonetheless, the sign of the effect in the endorsement experiment and in the main experiment is always positive and consistent with a shift towards the MPA in the *contact* group.

4.2.2 Government performance

In the results reported in the second panel of Table 7, covering government performance, we again see all positive signs except for a very small negative effect on "state looks after." Rather than simple evaluations of the incumbent, these questions seek to grasp perceptions of the competence of several governing institutions and individuals. The treatment effect on the index of these outcomes is 0.07 and statistically significant at the 0.1 level. The local average treatment effect of this index among full compliers is 0.23 standard deviations. This positive effect is largely driven by the positive and statistically significant effect of the intervention on perceptions of provincial government competence. Given that in his recording, the MPA discusses several projects that he has already implemented, it is possible that when asked about competence in providing services, constituents are pleased by news of these projects.

Table 8: **Endorsement Experiment Effects**

	Support Interest Bill (1)	Support Tax Bill (2)
Intercept	3.294	3.013
	[0.105]***	[0.194]***
Endorse	−0.265	−0.209
	[0.128]**	[0.147]
Contact	−0.144	−0.102
	[0.076]*	[0.098]
Endorse × Contact	0.252	0.375
	[0.111]**	[0.15]**
Village Fixed Effects	X	X
Covariates	X	X

Notes: *p<0.1; **p<0.05; ***p<0.01. Both columns represent a distinct OLS model. Both outcomes range from 1 to 5, where 5 represents a higher level of support for the bill. Standard errors accounting for clustering at the settlement level are presented in brackets. Both models include Village Council area fixed effects (since the endorsement was randomized at the settlement level). Both models also adjust for baseline values of co-partisanship with the incumbent ("supports MPA party"), baseline assets ("owns motorized vehicle"), and baseline political knowledge ("knows PML-N out of KP govt" and "knows who President is").

In standard deviations, the ITT effect of our intervention on perceptions of government performance is about 0.2 standard deviations, although there is significant missingness on this outcome variable.[6] The other ITT effects are all smaller than 0.05 standard deviations and are not statistically significant.

4.2.3 Prospects for accountability

We also see positive effects of a similar magnitude on the prospect for accountability index, reported in the third panel of Table 7, although results are statistically significant only using heteroskedasticity-robust standard errors rather than errors clustered at the settlement level. The point estimate for the index indicates a positive effect of 0.06 standard deviations

[6]However, missingness on this outcome is not predicted by treatment status.

while the LATE among full compliers indicates a positive effect of 0.21 standard deviations. Although none of the ITT effects on the constituent outcomes — beliefs about the ability for the MPA to follow through, political efficacy, or the importance of MPA performance in the voting decision — are statistically significant, they are all positive and represent increases of about 0.1 standard deviations. Overall, it appears that the intervention encourages conditions where electoral accountability is more likely to occur.

Although weak and noisy, results indicate that respondents appreciate being asked about their preferences, are likely to view the government as more competent when hearing about their representative's activities and goals, and are thinking more about performance and policy when deciding who to vote for in the future.

4.3 Stage Two effects of responsive follow-up calls

It is unclear from the analysis of Stage One results whether the content of the phone call or the simple fact of receiving the phone call drives these effects. In order to parse this, we study whether the content of a follow-up call matters for evaluations of the incumbent, evaluations of government performance, and prospects for accountability. In the Stage Two analysis, we compare respondents who received a *responsive* follow-up call from the MPA to those who simply got a second call from the MPA, allowing us to control for the number of contacts and focus instead on the level of engagement engendered by the political communication.

In Table 9, we see that among compliers in Stage One, receiving the *responsive* follow-up generally leads to an increase in most of our outcomes relative to the group that receives the *generic* follow-up, although almost all estimates are imprecisely estimated. Nonetheless, there is suggestive evidence that responsive linkages are more valuable than simple credit-claiming messages. The first column presents the ITT effects, while columns two and three present LATE effects on partial and full compliers, respectively. We define partial compliers as households that answered the Stage Two *responsive* follow-up call and full compliers as

households that listened to the *responsive* follow-up call to completion.

More precisely, every sign is positive except for one outcome in each domain. The only statistically significant result is the effect on the ranking of MPA performance, indicating that responsive linkages may trigger voters to think more seriously about the promises made by the MPA in the series of phone calls as they are a direct response to their requests in Stage One. This is the only effect, besides the incumbent party thermometer, which has a sizable effect estimate of around 0.15 standard deviations.

When looking at the marginal effect of the *responsive* follow-up over the *generic* follow up, the LATEs on the three indices for full compliers are all around a 0.1 standard deviation increase. Because all of these units are a sub-sample of those in the Stage One *contact* treatment, it appears that most of the positive effects of this intervention flow through those who were assigned to receive the responsive feedback. This highlights the need for not just credit-claiming and information-seeking, but also closed feedback loops where voters know that their elected official is aware of their preferences.

5 Conclusion

Overall, there is weak but consistent evidence that receiving calls seeking information about voter preferences as well as credit claiming in the MPA's voice increases perceptions of government competence and improves prospects for accountability. Furthermore, it appears that it is not the simple acts of contact and credit claiming that drives these effects but rather the responsive follow-up calls that directly address the requests of voters. These calls account for most of the positive effects. Nonetheless, many of the estimates are noisely estimated, likely due to imperfect compliance and some difficulties experienced by voters answering the questions in the phone calls. Furthermore, it is clear that engagement with this form of political communication is greatly increased by the perceived value of the information being

Table 9: **Marginal Effects of Stage 2 Responsive versus Generic Follow-up**

	ITT	Partial Compliers LATE	Full Compliers LATE	N
A. Incumbent evaluation index	0.019	0.022	0.059	674
	(0.085)	(0.097)	(0.256)	
	[0.065]	[0.075]	[0.199]	
MPA thermometer (1-10)	0.124	0.142	0.374	669
	(0.251)	(0.287)	(0.755)	
	[0.16]	[0.186]	[0.494]	
Inc. party thermometer (1-10)	0.268	0.307	0.807	669
	(0.261)	(0.299)	(0.788)	
	[0.25]	[0.29]	[0.759]	
Prefers MPAs party (0/1)	−0.027	−0.031	−0.081	669
	(0.038)	(0.043)	(0.113)	
	[0.03]	[0.035]	[0.092]	
B. Government performance index	0.024	0.028	0.073	674
	(0.052)	(0.06)	(0.157)	
	[0.043]	[0.05]	[0.134]	
Prov. govt. competent provider (1-4)	0.041	0.047	0.127	528
	(0.091)	(0.104)	(0.283)	
	[0.05]	[0.058]	[0.156]	
Elections important (1-4)	0.059	0.067	0.176	612
	(0.117)	(0.132)	(0.351)	
	[0.059]	[0.068]	[0.18]	
State looks after (1-5)	0.015	0.017	0.046	663
	(0.09)	(0.103)	(0.272)	
	[0.033]	[0.038]	[0.103]	
Party looks after (1-5)	−0.068	−0.078	−0.204	664
	(0.12)	(0.137)	(0.361)	
	[0.126]	[0.147]	[0.382]	
MPA looks after (1-5)	0.043	0.049	0.129	664
	(0.132)	(0.151)	(0.399)	
	[0.091]	[0.106]	[0.282]	
C. Accountability prospects index	0.032	0.036	0.096	660
	(0.041)	(0.047)	(0.124)	
	[0.037]	[0.044]	[0.117]	
MPA likely to follow through (1-4)	0.009	0.011	0.031	563
	(0.064)	(0.074)	(0.208)	
	[0.055]	[0.066]	[0.186]	
Political efficacy (1-4)	−0.078	−0.09	−0.24	663
	(0.098)	(0.112)	(0.3)	
	[0.107]	[0.127]	[0.335]	
MPA performance important in voting decision (1-6)	0.275	0.317	0.849	579
	(0.157)*	(0.181)*	(0.492)*	
	[0.157]*	[0.186]*	[0.486]*	

Notes: *p<0.1; **p<0.05; ***p<0.01. Each cell represents a model fit with either OLS or 2SLS. For the LATEs, *contact* instruments for partial compliance — answering the phone — in the second column and full compliance — answering an IVR question — in the third column. We present two standard errors: (HC2 robust standard errors) and [cluster robust standard errors] at the settlement level. Estimates for domains A and B are specified using the difference-in-differences specification in Equation 2 and estimates in panel C are fit using the simple specification reported in Equation 1. All estimates include settlement FEs. Most models include "supports MPAs party," "owns motorized vehicle," "knows PML-N out of KP govt," and "knows who President is" as covariates. Exceptions as specified in our pre-analysis plan are made for the following outcomes: when "supports MPA party" is the outcome when we exclude the baseline variable of "supports MPA party" as a covariate; for all outcomes in domain C except for "political efficacy," we add baseline values of "political efficacy" as a covariate; when "MPA likely to follow through" is the outcome, we include baseline values of "MPA thermometer" as a covariate. We build the index for each of our outcome domains using the method described in Kling, Liebman and Katz (2007). This method creates an index as a sum of the constituent outcomes, standardized by the mean and standard deviation of that outcome for the control group in the baseline time period (except for domain C where we just use the control group). All missing values are imputed using the mean of the respective treatment and time group.

transmitted. Specifically, being asked about development priorities leads to a 100 percent increase in response rates to the IVR question over being asked about MPA time-use. We interpret this as showing that voters evaluate communication by elected officials in terms of the salience of the content to their own lives, and we suspect that prior informational studies may have generated weak or non-existent findings when the information disseminated was remote to voters' everyday concerns.

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A Analysis with survey weights

Here we replicate our main analyses using survey weights. Because we sample 11 of the 25 Village Council areas in our study district with simple random sampling. Furthermore, within sampled Village Council areas, we sample four settlements using simple random sampling and have to adjust for the size of those settlements. Thus for each unit i, we apply the following weights in all analyses:

$$w_{isv} = \frac{n}{N}\frac{m_v}{M_v}\frac{o_s}{O_s} = \frac{11}{25}\frac{4}{M_v}\frac{25}{O_s},$$

where n is sampled Village Council areas (11), N is the total number of Village Council areas (25), m_v is the number of sampled settlements in Village Council v (4), M_v is the number of settlements in Village Council v (varies), o_s is the number of sampled households in settlement s (25), and O_s is the number of households in settlement s (varies).

Table A.1 replicates the main analysis table but with survey weights added. The effects are qualitatively similar.

Table A.1: **Treatment Effects of Stage One Contact Treatment, with sampling weights**

	ITT	Partial Compliers LATE	Full Compliers LATE	N
A. Incumbent evaluation index	0.025	0.029	0.081	1203
	(0.073)	(0.085)	(0.238)	
	[0.043]	[0.05]	[0.138]	
MPA thermometer (1-10)	0.03	0.035	0.096	1191
	(0.201)	(0.234)	(0.647)	
	[0.155]	[0.182]	[0.5]	
Inc. party thermometer (1-10)	0.274	0.319	0.883	1191
	(0.241)	(0.28)	(0.774)	
	[0.17]	[0.199]	[0.535]*	
Prefers MPAs party (0/1)	0.001	0.001	0.004	1191
	(0.034)	(0.039)	(0.109)	
	[0.016]	[0.019]	[0.052]	
B. Government performance index	0.105	0.122	0.339	1202
	(0.046)**	(0.054)**	(0.152)**	
	[0.039]***	[0.046]***	[0.131]***	
Prov. govt. competent provider (1-4)	0.203	0.236	0.622	959
	(0.085)**	(0.098)**	(0.263)**	
	[0.076]***	[0.089]***	[0.234]***	
Elections important (1-4)	0.032	0.038	0.102	1102
	(0.121)	(0.139)	(0.379)	
	[0.043]	[0.051]	[0.138]	
State looks after (1-5)	0.006	0.007	0.018	1182
	(0.072)	(0.084)	(0.233)	
	[0.05]	[0.059]	[0.162]	
Party looks after (1-5)	0.118	0.137	0.378	1180
	(0.109)	(0.127)	(0.352)	
	[0.096]	[0.114]	[0.314]	
MPA looks after (1-5)	0.149	0.174	0.479	1186
	(0.124)	(0.144)	(0.397)	
	[0.087]*	[0.105]*	[0.282]*	
C. Accountability prospects index	0.035	0.041		1174
	(0.044)	(0.051)		
	[0.051]	[0.06]		
MPA likely to follow through (1-4)	−0.01	−0.011		1002
	(0.063)	(0.072)		
	[0.066]	[0.078]		
Political efficacy (1-4)	0.052	0.061		1181
	(0.105)	(0.121)		
	[0.087]	[0.104]		
MPA performance important in voting decision (1-6)	0.105	0.122		1029
	(0.167)	(0.193)		
	[0.227]	[0.268]		

Notes: *p<0.1; **p<0.05; ***p<0.01. Each cell represents a model fit with either OLS or 2SLS. For the LATEs, *contact* instruments for partial compliance — answering the phone — in the second column and full compliance — answering an IVR question — in the third column. We present two standard errors: (HC2 robust standard errors) and [cluster robust standard errors] at the settlement level. Estimates for domains A and B are specified using the difference-in-differences specification in Equation 2 and estimates in panel C are fit using the simple specification reported in Equation 1. All estimates include settlement FEs. Most models include "supports MPAs party," "owns motorized vehicle," "knows PML-N out of KP govt," and "knows who President is" as covariates. Exceptions as specified in our pre-analysis plan are made for the following outcomes: when "supports MPA party" is the outcome when we exclude the baseline variable of "supports MPA party" as a covariate; for all outcomes in domain C except for "political efficacy," we add baseline values of "political efficacy" as a covariate; when "MPA likely to follow through" is the outcome, we include baseline values of "MPA thermometer" as a covariate. We build the index for each of our outcome domains using the method described in Kling, Liebman and Katz (2007). This method creates an index as a sum of the constituent outcomes, standardized by the mean and standard deviation of that outcome for the control group in the baseline time period (except for domain C where we just use the control group). All missing values are imputed using the mean of the respective treatment and time group. All regressions are estimated with sampling weights to account for cluster sampling.

B Spillover effects

Because we set aside one Village Council area as a pure control area, we can compare control households in Village Council areas with treated units to these pure control households. Mostly due to power and the inability to include settlement fixed effects, we are unable to show clear spillover effects. If anything, they are mostly positive and substantively small.

Table B.1: **Spillover Effects of Stage 1 Contact Treatment**

	Control - Pure Control)	N
A. Incumbent evaluation index	0.022 (0.126) [0.071]	424
MPA thermometer (1-10)	0.059 (0.307) [0.3]	419
Inc. party thermometer (1-10)	0.126 (0.355) [0.316]	419
Prefers MPAs party (0/1)	0 (0.057) [0.019]	419
B. Government performance index	0.097 (0.07) [0.073]	423
Prov. govt. competent provider (1-4)	0.312 (0.154)** [0.201]	348
Elections important (1-4)	0.12 (0.151) [0.084]	395
State looks after (1-5)	0.067 (0.129) [0.053]	416
Party looks after (1-5)	0.04 (0.139) [0.199]	413
MPA looks after (1-5)	−0.072 (0.154) [0.069]	419
C. Accountability prospects index	−0.018 (0.061) [0.075]	413
MPA likely to follow through (1-4)	0.032 (0.096) [0.139]	351
Political efficacy (1-4)	−0.153 (0.159) [0.112]	416
MPA performance important in voting decision (1-6)	0.057 (0.262) [0.29]	361

Notes: *p<0.1; **p<0.05; ***p<0.01. Each cell represents results of a different OLS model. Estimates in panels A and B are fit using the difference-in-differences specification in Equation 2 while Estimates in panel C is fit using the simple specification in Equation 1. Most models include the following covariates: co-partisanship with the incumbent ("Supports MPAs party"), baseline assets ("Owns motorized vehicle"), and baseline political knowledge ("Knows PML-N out of KP Govt." and "Knows Who President Is"). Exceptions as specified in our pre-analysis plan are made for the following outcomes: when "Supports MPAs party" is the outcome when we exclude the baseline variable of "Supports MPAs party" as a covariate; for all outcomes in Panel C except for "Political efficacy", we add baseline values of "Political efficacy" as a covariate; lastly, when "MPA likely to follow through" is the outcome we include baseline values of "MPA thermometer" as a covariate. For each of our three outcome domains, the index is built from the constituent outcomes using the method described in Kling, Liebman and Katz (2007). This method creates an index as a sum of standardized variables, where we construct each standardized variable by subtracting the mean of the control group and dividing by the standard deviation within the control group. All missing values are imputed using the mean of the treatment group for the missing data. For the two indices where we have all of the baseline values, we construct the indices by standardizing measures to the post-treatment control mean and standard deviation and imputing missing data by mean of the treatment group *and* time period. We present two standard errors: standard robust HC2 standard errors and cluster robust standard errors at the settlement level.

C Text of SMS encouragement experiment

Table C.1: SMS Treatment Conditions

	Placebo	**Community**	**Personal**
No Peer	Hello (name from survey). In a few hours you will receive a call from your [MPA NAME] from this number. We hope you can take the call.	Hi (name from survey). In a few hours you will receive a call from your [MPA NAME] from this number so that he can talk to you about how to do a good job for everyone in his constituency. We hope you can take the call.	Hi (name from survey). In a few hours you will receive a call from your [MPA NAME] from this number so that he can talk to you about how best to help you and your family. We hope you can take the call.
Peer	Hi (name from survey). In a few hours you will receive a call from your [MPA NAME] from this number. [MPA NAME] has also called a lot of other people and most of them have responded. We hope you can take the call.	Hi (name from survey). In a few hours you will receive a call from your [MPA NAME] from this number so that he can talk to you about how to do a good job for everyone in his constituency. [MPA NAME] has also called a lot of other people and most of them have responded. We hope you can take the call.	Hi (name from survey). In a few hours you will receive a call from your [MPA NAME] from this number so that he can talk to you about how best to help you and your family. [MPA NAME] has also called a lot of other people and most of them have responded. We hope you can take the call.

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