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Pre-Survey Messaging to Improve Response: Appeals to Authority, Self-Interest, and Salience

Andrew Dillon
Northwestern University

Steven Glazerman
Innovations for Poverty Action

Dean Karlan
Northwestern University and IPR

Christopher Udry
Northwestern University and IPR

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Abstract

Contacting respondents via text messaging before survey administration is potentially a low-cost way to increase contact and response rates. In high-income countries, pre-survey messaging is often used to improve survey response via postcards, letters, advertisements, or gifts. For low-income countries, these strategies do not apply. The researchers conducted two experiments on pre-survey messaging. In the first, they randomized cases from Random Digit Dial (RDD) surveys in four countries (Colombia, Mexico, Philippines, and Rwanda) to receive messages that tested whether respondents better responded to surveys organized by researchers or government. In the second experiment, the researchers randomized pre-survey message content for second-round surveys of 7,000 respondents originally identified through RDD surveys in five countries: Burkina Faso, Colombia, Côte d'Ivoire, Rwanda, and Zambia. The content variations included information about survey participation compensation, key statistics from the survey's previous round about food access and household finance, and general encouragements about survey participation. While pre-survey messages do increase response rates by 2 percentage points on average, they find no impact of message content on rates of contact, survey completion, composition of sample of respondents, or estimated study outcomes from the survey.

1 Introduction

Survey methodologists have long experimented with different ways to improve contact and response rates. Pre-survey messages are designed to encourage participation by informing in advance potential respondents about an upcoming survey. Existing research focuses on postcards, letters, endorsements, and prepaid incentives or gifts (Rao et al., 2010) (Cantor et al., 2007). For low- and middle-income countries (LMICs), however, the insights from this research may have little relevance. Mailing gifts or letters is rarely feasible and the social and economic context for survey participation behavior differs from that of high-income countries. For example, differences between high-income countries (HICs) and LMICs in cell phone ownership and rural cellular coverage may limit the effectiveness of pre-contact messages or increase bias. Respondents in LMICs may systematically differ from high-income country respondents in literacy status, access to information, or confidence in national statistical institutions that may be communicating with them. Such differences across countries may influence the effectiveness of pre-survey messages on survey contact and completion for telephone surveys. Messaging could change sample composition if pre-survey messages effectively screen certain respondent types or introduce response bias if respondents are primed for certain topics in advance of their interview.

We conducted two experiments to disentangle the effectiveness of pre-survey messaging on contact and response rates as well as which types of messages are most effective (AAPOR, 2023). The first experiment was conducted in conjunction with random digit dial (RDD) surveys in Colombia, Mexico, Philippines, and Rwanda. We randomly assign cases to one of two message types where respondents are informed about whether the government or researchers are conducting the survey. The design also includes a no-message control group. We estimate the effects of pre-survey messaging on survey contact and completion to better understand with which types of statistical authorities respondents might cooperate. Whether the relatively low cost of pre-survey messages improves survey contact, completion, and sample composition is ultimately an empirical question, but there is relatively

little evidence on message types. Several behavioral models might support alternative protocol designs. For example, does the message need to be detailed, or general? To address this, we test "general" information, which mentions the survey topic versus "specific" information which cites a statistic that was derived from the first round of the survey. The second experiment was embedded in the protocols for followup surveys from samples recruited through RDD, so the respondents had already been interviewed once and given consent to be re-interviewed. In this second experiment, we tested 10 variations of message content that focused on communicating potential survey participation incentives or about the importance of information. We consider salience of the survey content (food security or household finances), efficacy of one's voice in shaping policy, intrinsic motivation to help oneself or extrinsic motivation to help others, and lastly a reminder that survey responses were incentivized.

2 Existing theory and evidence

2.1 Respondent motivations

A small, but important literature from HICs esimates the effect of different types of pre-survey messages. Christian et al. compare cooperative respondents (those who completed the survey in wave 1 and received a text message reminder) to all other respondents (those who did not complete the survey in wave 1, or those who completed but did not receive a text reminder). They find that the "other" respondents are more likely to complete a smartphone survey when receiving a text message reminder in wave 2 of the study. The significance of the text message reminder does not carry over to other survey modes such as web, paper, or phone. The study was conducted in the United States, where phone ownership is near ubiquitous. Other studies conducted in HICs find that pre-survey text message invitations and reminders did not have a significant effect on response rates (Cabrera-Álvarez and Lynn, 2024) (DuBray, 2013) (Keding et al., 2015) (McGeeney and Yan, 2016).

To identify the pathways by which pre-survey messages might improve survey respondent cooperation, we describe respondent motivations according to "leverage-salience theory". A respondents' propensity to cooperate depends on survey attributes that have different degrees of importance to each individual and a weight or salience in the request made by researchers (Groves et al., 2000). The concern that arises from this model is that appealing to different attributes may change the composition of individuals who cooperate with the survey and therefore who ends up in the analysis sample. It is used to explain why some survey strategies succeed in one setting and fail in another: the same survey protocols may provide different leverage depending on the subgroup and different salience because of variations in study design.

(Singer and Ye, 2013) suggest three behavioral pathways that may explain why individuals respond to surveys: altruism, egoism, and survey characteristics. Altruism might reflect a respondents interest in contributing survey responses that could potentially help others, while egoism might motivate a respondent to contribute survey responses in their own self-interest. Finally, respondents might have a specific interest in a survey topic which makes it interesting to discuss, independent of the value of the data to others or themselves.

Qualitative and experimental research indicates some behavioral rationales that can motivate individuals to cooperate with surveyors. Altruistic rationales include the importance of the research itself, wanting to be helpful to an interviewer, or civic duty, where survey completion is seen as a public service. Egoistic rationales include enjoyment of the survey process itself, monetary benefits either from respondent gifts/payments or from a belief that participating in a study will lead to some other monetary benefit such as eligibility for a cash transfer program. Following Groves and Cialdini, we expect that pre-survey messages should be reinforced with persuasive messages in introductory scripts.

Other qualitative work on web-based self-administered surveys, (Couper et al., 2008) identified why people do not take surveys. The authors found that the most common reason was privacy (47%), followed by survey characteristics (26%) and negative opinions about

the topic (14%). Groves et al. note that response can be improved by invoking norms of authority, scarcity and social validation. Lynn demonstrated that messaging can increase respondent motivation by increasing the saliency of the study, promoting efficacy of participation, and dispel concerns about survey burden (Mowen and Cialdini, 1980) (Porter and Whitcomb, 2003). All of this literature provides guidance for a set of factors to vary in the messaging strategies that we test, described later in the paper.

2.2 Pre-survey contacts in LMICs

Most of this research literature has been based on self-administered surveys from OECD countries, but it is useful to consider papers that attempt to replicate these results in LMICs. One example (Stecklov et al., 2017) sought tested monetary incentives in Ghana. They found partial replication of response format and amount (promised versus prepaid incentives) mattered in Ghanaian university students taking a self-administered survey, speculating that cultural norms supporting reciprocity in Ghana may underlie the effectiveness of incentives.

Leo and Morello tested the role of combining pre-survey SMS messages with various levels of monetary incentive in phone surveys in Ghana and Tanzania. SMS messages were sent the day before the survey call. They found impacts on completion rates of between 4 and 8 percentage points in Ghana and between 9 and 13 percentage points in Tanzania, depending on the size of the incentive payment. SMS effects were larger for larger incentives in Tanzania but the reverse in Ghana. In both Ghana and Tanzania, they found that SMS alone had a significant effect of 8 and 9 percentage points respectively on completion rates. The addition of monetary incentives yielded mixed results. In Tanzania, the effect on completion rates increased to between 11 and 13 percentage points when monetary incentives were added, while the completion rates in Ghana fell to between 4 and 7 percentage points and were no longer significant. They conclude that using no incentives appears to be just as cost effective for gathering completed responses as using SMS messages alone. More re-

search is needed on monetary incentives in combination with SMS messages.

A study of SMS messages sent 24 hours in advance of an interactive voice response (IVR) survey[1] in Ghana, Malawi, and Nigeria (Amaya et al., 2018) with different survey topics and characteristics found small effects as well. Pre-survey SMS messages in Ghana were found to have significant effect of 1.44 points on completion rates, while the effects in Nigeria and Malawi were positive but not statistically significant. The text messages and findings were as follows:

Ghana, 2015, RDD (+1.4pp completion rate, p<0.0001) You've been selected for a survey on electricity in Ghana. Please expect a call tomorrow evening. Add your voice to the national discussion.

Malawi, 2016, List Sample (+2.1pp completion rate, p=.453) Hello, you previously registered for updates from [CLIENT NAME]. Tomorrow we invite you to answer a telephone survey that will help us improve our information services.

Nigeria, 2015, List Sample (+2.4pp completion rate, p=.116) Hello, you previously signed up to help end poverty by supporting [CLIENT NAME]. Tomorrow we invite you to answer a telephone survey to make your voice heard

Kasy and Sautmann found that sending an SMS message one hour before a survey call (versus not sending any SMS message, or sending the message 24 hours before a live phone interview) increased the survey completion rate, with the difference between a one-hour advance SMS message and no SMS message being about 3 percentage points. These results come from calling a list of screened numbers of small-holder farmers during an agricultural season in Odisha, India.

[1]Interactive Voice Response (IVR) enables automated communications with survey respondents over the phone. In the context of this study, the system places an out-bound dial. When the individual answers, he/she hears a recorded greeting and invitation to begin the survey.

3 Methods

3.1 Experiment 1

We use data from four RDD surveys conducted by [name removed for blinding] between April and September 2020 in Colombia, Mexico, the Philippines, and Rwanda, resulting in 5,719 complete surveys from 30,744 attempted respondents (dataset Innovations for Poverty Action, 2021a) (dataset Innovations for Poverty Action, 2021f) (dataset Innovations for Poverty Action, 2021c) (dataset Innovations for Poverty Action, 2021e). While we refer to them as RDD, they are actually random samples drawn from mobile phone number operator subscriber lists. These data are intended to be representative of the mobile-phone using population in each country, except in Mexico where the survey is limited only to Mexico City area codes.

In each of these sites, [name removed for blinding] randomly assigned respondents to receive a particular SMS message or no message one day before or on the day of the first call attempt. All messages were sent in the language with the highest rates of literacy in the country (see Table 3.1). The SMS did not come from the same number from which interviewers would eventually place their calls.

Table 1: Experimental conditions for SMS content, with percentage of cases assigned to each

Country	SMS language	SMS content				
		None	Basic	Intrinsic motivator		Extrinsic motivator
				Research	Government	Incentives[a]
Colombia	Spanish	50%		25%	25%	
Mexico City	Spanish	50%		25%	25%	
Philippines	English	33%	33%			33%
Rwanda	Kinyarwanda	50%	50%			

All SMS included information on IPA branding as well as information on which day calls for the survey would begin. [a] Incentive text included survey information. This treatment arm can be interpreted as a comparison between including extra information on an incentive versus an SMS with just information on IPA and the survey

These SMS messages contained multiple components: (1) notification of survey day, (2)

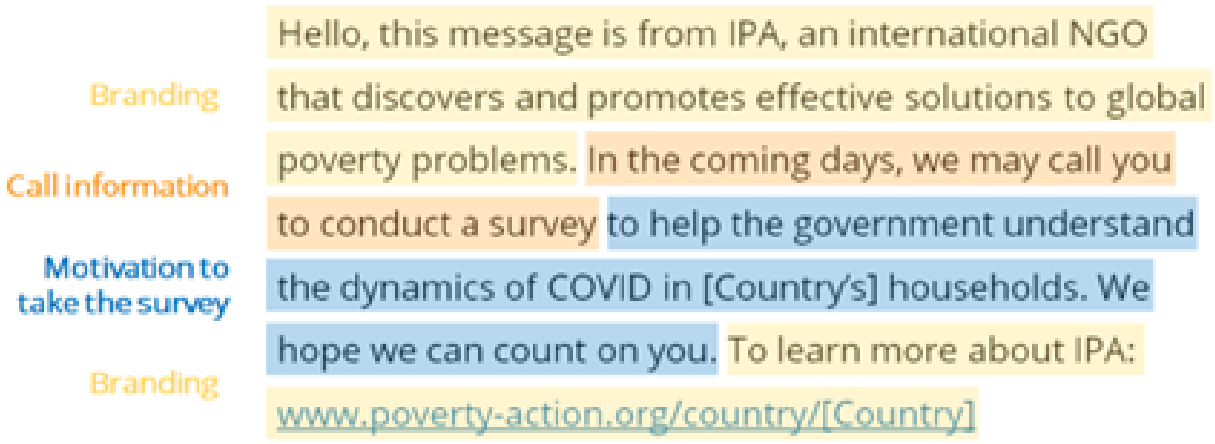

Figure 1: Example of pre-survey SMS message sent to respondents in Experiment #1

branding on the survey firm, and (3) a reason to take the survey. An example format, translated from Spanish, is shown in Figure 1.

Although we cannot isolate the effect of branding or call information, branding and informational components are necessary to ensure that the SMS messaging was viable and consistent with principles of informed consent required for protection of human subjects of research. Therefore, we only modified motivations to take the survey, the text highlighted in blue and we focus on appeals either "to help reseachers understand the dynamics of COVID" or "to help the government understand the dynamics of COVID."

Table 3.1 displays the experimental variations in each site and the proportion of respondents that were sent an SMS. Survey branding and timing were adapted slightly to each country context based on feedback by project teams to ensure that messages were colloquial. These variations included two types of motivations to complete the survey: an intrinsic motivation, that the survey would help either "researchers" or the "government", and extrinsic motivation, that respondents would receive an incentive (a small mobile money deposit upon survey completion). One treatment arm did not receive any SMS messages. This is the control group.

3.2 Experiment 2

In the second experiment, the study teams used the respondents to the random digit dial phone surveys conducted in Experiment 1, collected early in the pandemic in a two-month period between May and June 2020, to form follow-up samples of 1,300 to 1,500 individuals per country that consented to a follow-up. In the next two to five months, study teams implemented follow-up surveys in two-week periods in Burkina Faso, Colombia, Côte d'Ivoire, Rwanda, and Zambia (dataset Innovations for Poverty Action, 2021b) (dataset Innovations for Poverty Action, 2021d).

After enumerators were hired, survey teams randomly assigned respondents to one of 10 intervention arms, stratified on enumerator and prior SMS receipt. Where sampling strata do not have even multiples of 10, we conduct a secondary randomization to determine which treatment arms have one extra sample member.

The research team then worked with each country team to determine language and timing for the SMS messages. In all cases, the survey teams suggested using the language spoken by the majority of respondents for all respondents as the SMS would appear more legitimate. SMS were sent by vendors before the first day of surveying began. All messages were sent as two-part SMS texts with between 160 and 320 characters of text. SMS were sent at a time determined by survey teams to allow time for respondents to receive the SMS or have a literate person read the SMS to them in low literacy contexts. We collected SMS delivery information to measure SMS receipt and compliance in SMS sending time in all sites from the SMS marketing vendors used.

The survey scripts used preloaded introductory text that matched the randomly assigned SMS content. Enumerators received training on treatment variations and these variations were marked in the survey script.

This experiment was embedded in five phone surveys conducted in 2020. The sample sizes, language, and dates are shown in Table 3. The messages used in each of the ten treatment arms for Experiment 2 are shown in Table 4. Treatment Group A1 is the simplest mes-

sage, meant to serve as a default message with no appeals. We refer to this as a placebo. The sample allocation for Experiment 2 can also be interpreted as a 5-by-2 factorial design with four subtypes of "Learning" and a no-treatment control level of the learning factor, as in Table 5.

Balance tests (shown in the Appendix), show that randomization produced treatment groups that had similar background characteristics, as expected, within each country for Experiment 2, the one for which we had background data on all sample members.

3.3 Analysis

Random assignment ensures that there are no systematic differences between treatment arms other than the treatment itself. Therefore, we estimate impacts from a simple regression of outcomes on treatment indicators and some control variables. The only covariates available for Experiment 1 were indicators for country, since the survey was RDD and no background data were available. For Experiment 2, we had access to more detailed demographic data, collected in the first round.

We consider four possible outcomes in Table 2:

Table 2: Experiment 2 outcomes of interest

Contact	Whether the respondent answered the phone
Completion	Whether the survey was completed
Survey response	Response value to a particular question
Respondent characteristics	To assess the effect on sample composition

Both experiments considered the first two outcomes, contact and completion rates. The second experiment considered survey response and respondent characteristics (sample composition). The variable used to assess impacts on survey responses themselves was a food security question that had a dichotomous response.

The regression specification can be written as follows:

$$Y_i = \beta_0 + \beta T + \delta X + \eta E_i + \alpha S_i + \epsilon_i$$

Y_i is the outcome for individual i. T is a vector of treatment variables for each of the arms (with the no-SMS control omitted in Experiment 1 and the placebo treatment omitted in Experiment 2). X is a vector of covariates. E is a set of enumerator fixed effects. S is a set of study/country fixed effects, with unobserved outcome determinants in the error term ϵ assumed to be independent and identically distributed and uncorrelated with treatment. To identify the effect of treatment assignment on sample composition, we interact the treatment indicators with an indicator variable for completed a round 2 survey.

Table 3: Characteristics of RECOVR Followup Surveys in Experiment 2 Sites

Country	SMS language	Start date[a]	End date[a]	Sample size
Burkina Faso	French	Nov. 16	Nov. 27	1,327
Colombia	Spanish	Aug. 13	Aug. 21	1,455
Côte d'Ivoire	French	Oct. 5	Oct. 17	1,287
Rwanda	Kinyarwanda	Aug. 26	Sep. 4	1,486
Zambia	English	Nov. 27	Dec. 21	1,271
N				6,826

[a] All surveys started and ended in 2020.

In Experiment 2, the four hypotheses of interest are tested, in accordance with the pre-analysis plan[2], by constructing linear tests of the β coefficients, using the following combinations of individual treatment arm indicators:

[2]See AEA RCT Trial Registry AEARCTR-xxxxxx [registration number removed for blinding]

Table 4: Treatment arm descriptions and sample allocations, Experiment 2

Treatment Arm	Sample Allocation	Text Content (variations in red and green)
No self-interest appeal		
A1 Placebo/short message	16.70%	Hello from IPA. Thank you for completing our survey last month! As we agreed, we are now calling again for another brief survey. Your continued participation is appreciated.
A2 General Learning (Food access)	8.30%	Hello from IPA. Thank you for completing our survey last month! As we agreed, we are now calling again for another brief survey. From the last survey, we learned about important trends in food availability in Colombia. Your continued participation is appreciated.
A3 General Learning (Household finances)	8.30%	Hello from IPA. Thank you for completing our survey last month! As we agreed, we are now calling again for another brief survey. From the last survey, we learned about important trends in household finances in Colombia. Your continued participation is appreciated.
A4 Specific Learning (Food access)	8.30%	Hello from IPA. Thank you for completing our survey last month! As we agreed, we are now calling again for another brief survey. From the last survey, we learned that 39% of Colombians had difficulty in buying food due to COVID. Your continued participation is appreciated.
A5 Specific Learning (Household finances)	8.30%	Hello from IPA. Thank you for completing our survey last month! As we agreed, we are now calling again for another brief survey. From the last survey, we learned that 34% of Colombians depleted their savings to meet their needs due to COVID. Your continued participation is appreciated.
Self-interest appeal		
B1 Self Interest	16.70%	Hello from IPA. Thank you for completing our survey last month! As we agreed, we are now calling again for another brief survey in which you will earn 5000 pesos. Your continued participation is appreciated.
B2 General Learning (Food access) + Self interest	8.30%	Hello from IPA. Thank you for completing our survey last month! As we agreed, we are now calling again for another brief survey in which you will earn 5000 pesos. From the last survey, we learned about important trends in food availability in Colombia. Your continued participation is appreciated.
B3 General Learning (Household finances) + Self interest	8.30%	Hello from IPA. Thank you for completing our survey last month! As we agreed, we are now calling again for another brief survey in which you will earn 5000 pesos. From the last survey, we learned about important trends in household finances in Colombia. Your continued participation is appreciated.
B4 Specific Learning (Food access) + Self interest	8.30%	Hello from IPA. Thank you for completing our survey last month! As we agreed, we are now calling again for another brief survey in which you will earn 5000 pesos. From the last survey, we learned that 39% of Colombians had difficulty in buying food due to COVID. Your continued participation is appreciated.
B5 Specific Learning (Household finances) + Self interest	8.30%	Hello from IPA. Thank you for completing our survey last month! As we agreed, we are now calling again for another brief survey in which you will earn 5000 pesos. From the last survey, we learned that 34% of Colombians depleted their savings to meet their needs due to COVID. Your continued participation is appreciated.

Note: Colombia used for illustration. The country name, statistics, and incentive amounts and units were all adapted by country.

Table 5: Experiment 2 treatment arms represented in factorial design format

	Control	Learning			
		General		Specific	
		Food Access	Finance	Food access	Finance
	(1)	(2)	(3)	(4)	(5)
Placebo (A)	A1 16.70%	A2 8.30%	A3 8.30%	A4 8.30%	A5 8.30%
Incentive (B)	B1 16.70%	B2 8.30%	B3 8.30%	B4 8.30%	B5 8.30%

Table 6: Experiment 2 hypotheses of interest

Any information vs none	A1 & B1	vs.	A2-A5 & B2-B5
General vs specific	A2-3 & B2-3	vs.	A4-5 & B4-5
Food access vs. finance	A1, A4 & B1, B4	vs.	A2, A5 & B2, B5
Placebo vs. incentive reminder	A1-A5	vs.	B1-B5

4 Results

4.1 Experiment 1

In the first experiment, we find that sending a pre-survey SMS message reduces contact rates. Specifically, assignment to the treatment group reduced the fraction of respondents who answered the phone by 1.0 percentage points (standard error of 0.6 percentage points, abbreviated hereafter as "pp SE") compared to an average of 55.7 percent contact rate (respondents who answer the phone) for the no-SMS control group. This is a statistically significant change at the $p < 0.10$ level. This is in agreement with (Amaya et al., 2018), where all three countries in the study also experienced a lower, though not statistically significant, contact rate for the pre-survey SMS message group. These effects are shown in Panel 1 of Table 7. The reduction is largest in the Philippines, where pickup rates for the treatment group are 3.7 percentage points lower (1.5 pp SE). In other sites the effect on pickup rates was not statistically significant. One way to explain the negative impact is that receiving the message may lead respondents to screen their calls, and thus not an-

swer. There is precedent for this. An experiment using pre-visit flyers during door-to-door solicitations of charitable giving found similar effects on answering the door (DellaVigna et al., 2012).

Despite the lower contact rate, the pre-survey SMS message results in a statistically significant increase in survey completion. Averaging across all sites, the impact was 1.1 percentage points (0.4 pp SE). These effects are shown in Panel 2 of Table 7. This effect is driven by increases in three sites: 2.4 percentage points (1.1 pp SE) in Colombia, 1.2 percentage points (0.4 pp SE) in Mexico City, and 3.8 percentage points (1.8 pp SE) in Rwanda. In the Philippines, the negative effect on answering the phone leads to a negative, albeit not statistically significant, effect on survey completion.

While sending a pre-survey message increases completion rates, the different content of the messages we tested did not make a difference in pickup or completion rates. Findings in Table 8, specifically the p-values associated with comparisons of different SMS message types, suggest that the message content does not affect response behavior. Averaging across sites, the SMS effects (relative to no SMS) were within 1 percentage point for each pair of message types (e.g. researchers vs. government, incentive reminder versus information only) and those differences were not statistically significant. It should be noted that these treatment contrasts were subtle, changing just one word in the message for the researcher versus government comparison.

Table 9 shows that the messaging strategies produced respondent samples that in most cases had similar characteristics. We compared self-reported age, gender, educational attainment (more versus less than secondary), employment status, or poverty probability. Of 24 tests comparing a specific messaging treatment arm to the no SMS condition, only 2 are statistically significant. For example, across all sites, respondents from the group assigned to receive a message with a reminder of the incentive are 1.3 years younger (0.7 years SE) and 2 percentage points lower (0.9 pp SE) on the poverty probability index, on average, than respondents in the group assigned to receive no message. Of 12 tests

comparing different message strategies to each other, only the incentive message verus information-only message is statistically significant. The significant results are from the Philippines only. This finding is consistent with Leo and Morello (2016), which does not find statistically significant results when a monetary incentive treatment is added to an SMS message treatment.

Table 7: Impact of Receiving an SMS Text on Answering Phone or Completing Survey

	Colombia		Mexico City		Rwanda		Philippines		All sites	
Panel 1: Answered the phone										
Control: No SMS (fraction answered)	0.627		0.549		0.582		0.479		0.557	
Treatment: Any SMS (difference)	-0.010		-0.008		0.009		-0.037	***	-0.010	*
Panel 2: Completed the survey										
Control: No SMS (fraction complete)	0.238		0.057		0.422		0.186		0.172	
Treatment: Any SMS (difference)	0.024	**	0.012	***	0.038	**	-0.014		0.011	***
N	6,018		13,392		3,339		7,995		30,744	

Each 'difference' is calculated from an OLS regression of 'answered' or 'complete' on treatment status, with enumerator fixed effects and covariates. Covariates include day of week and time of day of the first attempt. Results in the 'All sites' column includes country fixed effects. SMS effects are intent-to-treat and do not account for SMS messages that were not delivered. Robust standard errors were used to calculate statistical significance. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

4.2 Experiment 2

In the second experiment, all study subjects received a pre-survey text message, but the content of those messages varied in more ways. Also, the study population consisted of sample members who were contacted through an RDD survey but had already completed the first-round interview and agreed to be re-contacted. We present findings for this population separately.

4.2.1 Main results

Pre-survey message content did not have a meaningful statistically significant impact on survey contact or completion rates. The differences in these rates, shown as regression-adjusted impacts in the top of Table 10, did not vary across treatment arms by more than 2 percentage points.

To test the study's main hypotheses, we look beyond individual treatment arms and examine effects on respondent contact and survey completion for linear combinations of treatment conditions that correspond to pre-specified contrasts. There we do see evidence (with marginal significance, meaning $0.05 < p < 0.10$) that messages with any information reduced the contact rate by one percentage point. This result is counter-intuitive, since we hypothesized that providing information in the text message would make the survey content more salient and therefore increase the propensity to answer the phone. This could mean that the survey content itself was not interesting to respondents, leading them to purposely avoid answering the phone.

The content type did have a marginally significant effect on survey completion. Specifically, information on food access increased the completion rate 2 percentage points relative to messages with information on household finances. These marginally significant effects could still be consistent with sampling error. A total of 12 hypotheses were tested (four questions about three outcomes), so it was likely that at least one or two test statistics would have fallen into the rejection region by chance. There are no impacts of message type on the response variable, access to food, measured at followup.

Table 8: Impact of SMS by Content Type

	Colombia		Mexico City		Rwanda		Philippines		All sites	
Panel 1: Answered the phone										
Control mean: No SMS	0.627		0.549		0.582		0.479		0.557	
[1] SMS (Researchers)	-0.012		-0.002						-0.005	
[2] SMS (Government)	-0.008		-0.013						-0.011	
[3] SMS (Information only)					0.009		-0.044	***	-0.015	
[4] SMS (Incentive)							-0.028	*	-0.010	
p-value: [1] - [2]	0.837		0.327						0.544	
p-value: [3] - [4]							0.243		0.668	
Panel 2: Completed the survey										
Control Mean: No SMS	0.238		0.057		0.422		0.186		0.173	
[1] SMS (Researchers)	0.026	*	0.014	***					0.017	***
[2] SMS (Government)	0.022		0.01	*					0.014	**
[3] SMS (Information only)					0.038	**	-0.022	*	0.001	
[4] SMS (Incentive)							-0.006		0.003	
p-value: [1] - [2]	0.827		0.557						0.561	
p-value: [3] - [4]							0.126		0.814	
SMS delivery rate	76.9%		36.8%				80.8%		63.3%	
N	6,018		13,392		3,339		7,995		30,744	

See Table 2 for explanatory notes. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

4.2.2 Heterogeneity

We examined heterogeneity by country, gender, education level, and estimated household poverty. In each case we conducted a homogeneity test to determine whether the treatment effect interactions with the variable in question were jointly significant. Then we estimated and reported those interaction terms, noting whether each was significantly different from zero. We repeated this for each of the study's four main hypotheses. The results are shown in Appendix Tables A.1-A.3 and summarized in Table 11, which shows the p-values for each homogeneity test.

Variation across country appears to be consistent with sampling variance in most cases. Joint significance tests are only rejected for the effect of "any information" on contact and

completion rates. In those two cases we see significant positive impact on both rates for Colombia and significant negative impact on both rates for Zambia.

Variation across country appears to be inconsistent with sampling variance, with joint significance tests being rejected for the effect of "incentive" and "any information" on contact rates, completion rates, and food security.

There were no significant differences in effects by gender, but the overall effects masked differences by education and poverty levels. While there was some evidence of heterogeneous impacts on contact rates based on education level, this did not translate into heterogeneous impacts on completion rates.

Table 9: Sample Composition by Treatment Arm

	Age (Yrs)		Female	Secondary Education	Household Size	Employed	Poverty Probability	
Panel 1: Predicted Sample Mean by Treatment Arm								
No SMS	33.6		53%	70%	4.6	41%	21%	
Any SMS	33.7		53%	71%	4.5	42%	20%	**
[1] SMS (Researchers)	33.4		52%	69%	4.4	40%	20%	
[2] SMS (Government)	34.5		55%	72%	4.5	39%	20%	
[3] SMS (Information only)	33.8		52%	71%	4.7	44%	20%	
[4] SMS (Incentive)	32.3	**	56%	72%	4.5	41%	19%	**
Panel 2: P-value for test:								
p-value: [1] - [2]	0.337		0.396	0.398	0.676	0.878	0.807	
p-value: [3] - [4]	0.02	**	0.172	0.763	0.286	0.421	0.353	
N	4,762		5,218	5,210	5,211	5,220	4,890	

The sample is restricted to complete surveys. Each column presents results from a single OLS regression of the dependent variable described in the column heading on the SMS treatment variables with enumerator and project fixed effects and covariates for day of week and time of day. SMS effects are intent-to-treat and do not account for SMS messages that were not delivered. Robust standard errors were used to calculate statistical significance. Poverty probability is the predicted probability from the PPI, estimating that the respondent is below each country's national poverty line. Employed indicates that the respondent worked for one or more hours in the 7 days prior to the survey. Tests in the upper panel compare each SMS treatment group to 'No SMS'. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table 10: Impact of Pre-Survey Message Type on Survey Response

	Contact[a]	Completion[b]	Food Security (R2)[c]
Sample Mean (Percentage) Individual Treatment Arms			
[A2] General info, food access	-0.013	0.013	0.008
	-0.020	-0.020	-0.020
[A3] Specific info, food access	-0.025	-0.003	-0.032
	-0.020	-0.020	-0.030
[A4] General, household finance	-0.025	0.024	0.000
	-0.020	-0.020	-0.020
[A5] Specific, household finance	-0.019	-0.003	-0.001
	-0.020	-0.020	-0.030
[B1] Incentive	-0.002	-0.003	0.009
	-0.020	-0.020	-0.020
[B2] Incentive * General food access	-0.003	0.014	-0.009
	-0.020	-0.020	-0.020
[B3] Incentive * Specific, food access	-0.022	-0.018	-0.017
	-0.020	-0.020	-0.030
[B4] Incentive * General, HH finance	-0.025	0.003	-0.007
	-0.020	-0.020	-0.030
[B5] Incentive * Specific, HH finance	-0.019	-0.012	-0.007
	-0.020	-0.020	-0.030
Hypothesis Tests			
Incentive vs. Placebo reminder A1-A5 vs. B1-B5	-0.004	-0.001	0.000
p-value:	0.627	0.866	0.987
Any information vs. None A1 & B1 vs. A2-A5 & B2-B5	-0.014	-0.016	-0.012
p-value:	0.084 *	0.093 *	0.331
Specific vs. General A2, A4 & B2, B4 vs. A3, A5 & B3, B5	0.005	0.002	-0.009
p-value:	0.580	0.882	0.537
Food access vs. HH Finance A2-3 & B2-3 vs. A4-5 & B4-5	0.006	0.009	0.012
p-value:	0.476	0.356	0.396
N	6812	6812	5136

Each column is from a separate regression that includes country fixed effects, age, gender, education, household size, and poverty (PPI score) [a] Contact = respondent answered the phone [b] Completion = respondent completed survey [c] Food Security R2 = respondent able to buy usual amount of food (yes/no), asked at follow-up. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table 11: Tests of homogeneity of impacts (p-values)

Outcome and Contrast Tested	Country		Gender		Education		PPI Quartile
Contact[a]							
Incentive v. Placebo reminder	0.384		0.596		0.001	***	0.360
Any info vs. none	0.003	***	0.791		0.559		0.185
Specific vs. General	0.737		0.945		0.614		0.502
Food access v. HH Finance	0.929		0.888		0.560		0.455
Completion[b]							
Incentive v. Placebo reminder	0.780		0.933		0.689		0.712
Any info vs. none	0.002	***	0.406		0.730		0.255
Specific vs. General	0.455		0.455		0.929		0.714
Food access v. HH Finance	0.686		0.717		0.356		0.383
Food Security[c]							
Incentive v. Placebo reminder	0.530		0.065	*	0.004	***	0.163
Any info vs. none	0.376		0.961		0.000	***	0.541
Specific vs. General	0.809		0.973		0.313		0.582
Food access v. HH Finance	0.583		0.068	*	0.187		0.788

N= 6,812. PPI is the Poverty Probability Index. [a] Contact = respondent answered the phone [b] Completion = respondent completed survey [c] Food Security R2 = respondent able to buy usual amount of food (yes/no), asked at follow-up. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

The incentive messaging had the largest impact on contact rates for the most educated of the sample, those with more than secondary education, and the messaging that provided any type of information about the prior survey had a negative impact on contact rates for this same subgroup, and no detectable impact on the rest of the sample. We did not detect any significant differences by poverty quartile, except for the any-info messaging, which improved contact rates the most for higher poverty households. This effect was statistically significant for improving completion rates as well, but that result depended on how we defined poverty quartiles. Using absolute levels of poverty probability did not result in a significant impact on completion rates, but using quartiles defined relative to the sample within each country (shown in Table 10) the variation in impacts by PPI was statistically significant at the 0.05 level. Absolute impact estimates (shown in Appendix Table X) were 3 or 2 percentage points for the top two quartiles respectively and 0 and -2

points for the bottom two quartiles respectively. There was no evidence of heterogeneous impacts by PPI on the outcome measure (food insecurity at followup).

4.2.3 Robustness

The results are robust to model specification. The outcomes of interest here – contact, completion, and food security—are all dichotomous. Nevertheless, we estimate impacts using a linear probability model for convenience of interpreting coefficients (presented in Table 9) as marginal effects. When we re-estimated the impacts using logit models, the p-values for the study's four pre-specified hypothesis tests (and three main outcomes) are nearly identical to those in Table 10. This is not surprising given that the means of the outcome variables are not close to 0 or 1. (Table 7 or appendix table. Same as Table 7 but logit)

Attrition is not a concern for this study. For survival outcomes, there is no study attrition by definition because completion at follow-up is the outcome of interest. For survey outcomes (in this case, food insecurity), any effects of nonresponse, including differential nonresponse, at follow-up are part of the treatment effect on purpose. In other words, impacts on the composition of the sample are part of the effect we are trying to measure. Therefore, attrition is a non-issue.

Non-compliance is also not a concern. Since the treatment – sending an SMS message to the survey respondent – is highly controlled by the experimenter and does not rely on respondents to "take up", there is trivial non-compliance. In three of the five countries we were able to confirm that SMS messages were sent as intended. In those countries less than 3 percent of respondents were not sent the message as intended. Those few cases were distributed evenly across treatment arms.

5 Discussion

We find that sending a pre-survey message helps by improving response rates, though this effect is small, costs for pre-survey messaging are also small. The content of the message does not make respondents more likely to answer the phone or complete the survey. Message content also did not result in a different average response to a typical survey question.

5.1 Null findings for impacts of message content were unexpected

The null findings from the study were not far from expert beliefs, but closer to zero than experts predicted. We submitted a survey to the Social Science Prediction Platform where we described the experiment and elicited expert guesses on the likely impact estimates for each hypothesis and outcomes. We obtained 51 expert opinions and compare the distributions of those guesses to the study estimates for completion rates (Figure 2). The corresponding results for contact rates were qualitatively the same. From Figure 2, it is apparent that the null effects we find here are below expectations on average by about 2 to 6 percentage points, depending on the hypothesis. Still, for the two weaker contrasts, general versus specific information and food access versus household finance, the modal expert response was zero or very close to zero.

5.2 Implications for practice

The findings from this paper have implications for how pre-survey messaging is used to improve survey response. Considering that the costs of sending pre-survey text messages in many contexts are low and that it is difficult enough to find any ways to improve response rates to phone surveys, the evidence from Experiment 1 would suggest that researchers should generally send such message, regardless of message content. The experimental results suggest further that sending messages could reduce costs by pushing would-

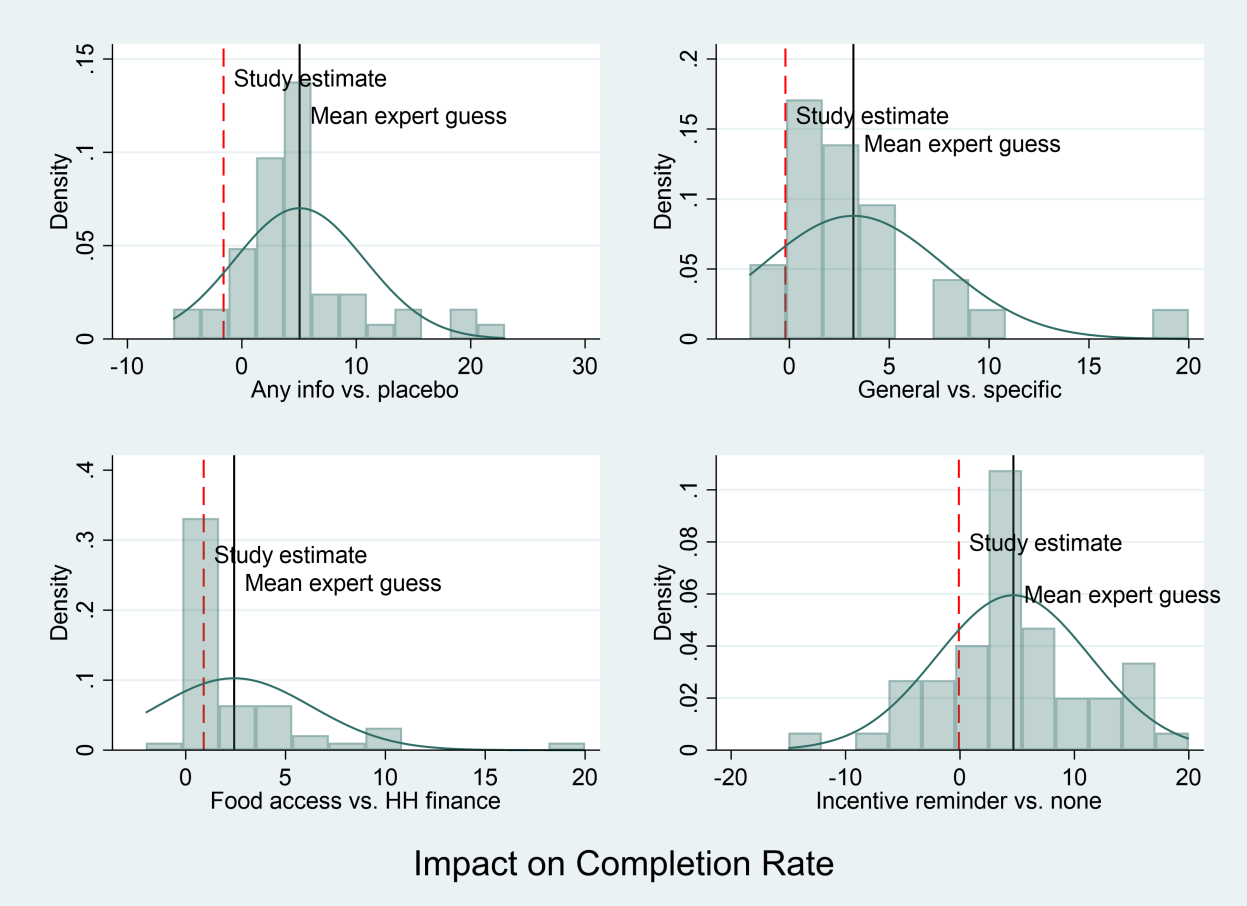

Figure 2: Impacts on Completion Rate: Study Point Estimates vs. Expert Guesses

be non-consenters to screen the call, thus reducing interviewer time. Combined with recent evidence on maximum survey attempts (Bogicevic et al., 2021), this could lead to non-trivial efficiency gains in phone survey operations.

Besides understanding whether pre-survey messaging can increase contact and response rates, discussions about ethics have also emphasized the importance of providing respondents with feedback from surveys in which they participate (Asiedu et al. 2021). Integrating pre-survey messaging into panel surveys might have marginal but low cost effects on participation rates, but also provide a mechanism to acknowledge and inform participants about their previous survey participation.

5.2.1 Limitations

The usual caveat applies with respect to generalizability of the study's findings. Experiment 1 was conducted with an RDD survey and Experiment 2 was conducted with a followup from an RDD (the consenters from round 1). These are very specific situations and may not apply to listed samples when there is already a relationship between the interviewer and respondents, where they might recognize the caller or the caller's institution. For example, pre-survey contacts may not be necessary for high frequency phone surveys or phone surveys that follow in-person contact. Also, this study (both experiments) was conducted with a general population of mobile phone service subscribers, which tends to include higher income and more urban respondents than surveys of the general or vulnerable populations (Glazerman et al., 2023).

5.3 Directions for future research

As phone surveys and other alternatives to in-person interviewing increase in prevalence in research in LMICs, the search will continue for ways to improve response rates, particularly for "cold-call" style surveys like RDD. As mobile phone penetration increases in LMICs, and smartphone availability grows, there will be expanded opportunities to provide respondents with multiple ways to complete surveys, such as SMS, IVR, or dial-in to live interviewer, as well as various push-to-web modes. In each of these cases, researchers will seek to find persuasive messages to gain respondent cooperation. The experimental evidence presented here suggests that the content may be less important than the medium.

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