Text Messages as Social Policy Instrument:
Evidence from a Randomized Controlled Trial with
Internal Refugees in Colombia

Abstract

We carried out a randomized controlled trial in Bogotá, home to the majority of Colombia's internally displaced persons (IDPs), to assess whether the use of SMS technology effectively informed this population of its eligibility to receive social benefits. By estimating the Local Average Treatment Effect of the text message on the awareness of available benefits, we find that on average treated households are more aware of their rights, but this varies across benefit type. Hence the analysis suggests that the intervention was successful in empowering IDPs and indicates that the use of SMS as a policy instrument should be expanded.

1. Introduction

Like other Latin American countries, Colombia witnessed the rise of guerrilla movements in the 1960s and 1970s. However, unlike the rest of the region, armed opposition is still active to date and it is largely represented by the *Revolutionary Armed Forces of Colombia* (hereafter FARC from its acronym in Spanish).[1] In addition to the guerillas the conflict features another illegal armed group since the early 1980s—the paramilitary forces. These right-wing militias were originally formed by local elites, landowners, and drug lords to counteract guerrilla extortion and ransom. The Colombian conflict has been especially harmful for the civilian population. Both guerrillas and paramilitaries have specialized in victimizing civilians which includes the forced displacement of a large share of the population (Vargas, 2016). In fact, violence-driven internal migration has become the most dramatic social consequence of the conflict, affecting up to 90 percent of the country's municipalities.[2] Many municipalities that receive internally displaced people (IDPs) lack the capacity to handle the inflow of refugees. Moreover, 98 percent of displaced households live below the poverty line and face unemployment rates much higher than the rest of the population (Ibáñez and Moya, 2010). IDPs are among the most vulnerable populations in Colombia. For this reason the United Nations classified this situation as "the biggest humanitarian crisis in Western Hemisphere" (UN, 2004). According to UNHCR (2016), by the end of 2015 6.9 million people had been forcibly displaced from their homes (about 14% of the total population), and that year Colombia had the world's largest IDP population (pg. 30).

Land disputes are the main driver of forced displacement and Colombia has today the most unequal land distribution of Latin America (Ibáñez, 2009). The illegal expropriation of large amounts of acreage is also attributed to the need for arable land for the cultivation of coca, the main element used in the production of cocaine. Estimates show that Colombia exports approximately 70 percent of the world's supply (Mejía and Restrepo, 2010). Other key causes of displacement are the extortion of businesses, landowners, and farmers by armed groups; the

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[1] A peace agreement between the government and FARC was reached in September 2016 but was not endorsed by the citizens in a referendum that took place in October 2 of that year. At the time of submitting this article the faith of the agreement, and hence that of FARC as an armed group was largely uncertain.

[2] Ibáñez (2009) and Ibáñez and Velásquez (2009) report a list of displacement episodes that have caused the majority of a town's population to flee. For instance, 95 percent of inhabitants left Bojayá, in the department of Chocó. Cocorná, in Antioquia, saw 94 percent of its inhabitants flee, and in El Tarra and Peque 82 percent and 78 percent of the population, respectively, were forcibly displaced.

forced recruitment of soldiers, especially child soldiers; and the intimidation of social and community leaders, which greatly hinders civil resistance and the ability to engage in collective action (Ibáñez, 2009).

The costs of forced displacement are pervasive, as evidenced by the extensive loss of assets and dissolution of family and community networks: 80 percent of IDPs never return to their households (NVS-II, 2008). IDPs are further put at risk by limited access to formal and informal risk-sharing mechanisms, which consequently exposes them to more acute shocks to their personal income and consumption (Ibáñez and Moya, 2010). According to NVS-II (2008), 43 percent of displaced households are female-led (this is 50 percent more than the national average), and one fifth of household heads are illiterate. In addition, children and adolescents are at greater risk through a displacement than adults. This group makes up approximately two-thirds of IDPs, and they are heavily economically dependent upon their parents and child labor practices for survival.

In recent years the Colombian Constitutional Court has specifically targeted the needs of IDPs. In decree T-025 of 2004, the Court established that IDPs are more vulnerable than the rest of the population and that their basic rights are "largely and systematically violated" and thus require special care and benefits from the government. Despite the government's attempts to comply with the Court's mandate by targeting benefits, by the end of 2007 the Court had ruled that the rights of IDPs still had not yet fully been reinstated. In this paper we identify a particular mechanism through which a relatively low-cost intervention may boost the efficiency of public policies directed at IDPs: the use of SMS technology.

Law 387 of 1997 created the Unique Registry of Displaced Population (hereafter RUPD from its acronym in Spanish). The RUPD (today called Unique Registry of Victims—RUV-) constitutes the official account of displaced and other victimized households nationally and its mission is to assist the government in identifying the recipients of welfare benefits. Law 387 established that displaced households have the right to apply for its inclusion in the RUPD. Most applications are submitted upon IDPs' arrival at their new destination. The application requires a detailed account of the facts that precipitated the IDPs' flight, and this information thus enables

the government to assess whether the household can be included in the RUPD, which ensures The IDP and her dependents access to a range of benefits.[3]

Nonetheless, this system has several major limitations. First, in order to receive updates about their status in the inclusion process an applicant must visit a designated office created for aiding IDPs known as Attention and Orientation Units (hereafter UAO from its Spanish acronym) Second, the procedures involved are very costly, including transportation costs, long waiting lines, and the implicit loss of income due to consecutive appointments because of information delays. This loss is significant for this population, whose income depends upon labor in the informal sector. Another contributing factor to the low benefit demands of displaced households is the lack of readily available information about what benefits can be claimed.[4]

By the time our intervention started in 2009, the government's agency in charge of managing the RUPD and assisting IDPs, *Acción Social* (since 2011 replaced by the Victim's Unit) estimated that approximately 70 percent of households included in the RUPD were unaware of their ability to receive benefits. To address this problem we carried out a randomized-controlled trial to assess whether a government's communication strategy involving the use of SMS technology could raise awareness about the IDPs RUPD inclusion, along with the knowledge of what benefits are people entitled to. Hence, the objective of this experiment was to corroborate the hypothesis that information and communication technologies (ICTs), such as SMS could empower Colombia's IDP population to better emerge from their precarious situation.

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[3] Note that inclusion in the RUPD is a necessary condition for receiving benefits. These benefits are demand-driven, and often households have to fulfill additional requirements. For instance, public schools are mandated to offer a place to school-aged children from displaced households. However, this does not always guarantee that the child is actually enrolled in school. Sometimes this is because of a family choice (perhaps a working child is more useful for the household) or because the household does not have enough resources to buy books or a uniform.

[4] During the survey stage of our intervention we collected direct accounts of the experiences of our subjects going to the UAO to obtain information about their status and benefits. One commonality that surfaced was that, in order to be able to obtain the tickets authorizing attention at the UAO customer service windows, IDPs have to arrive the night before and maintain their place in line overnight. Some IDPs cannot afford transportation costs to UAOs (of which there are only five in Bogota). Yet another group cannot leave their job to go to the UAO or have no one who can take care of their children in the meanwhile. Among those who can make it to the UAO, once they finally reach the service window they are often told to come back some other day because their information is not yet loaded into the system. Sometimes personnel at UAOs simply are not aware of the information requested on how to access certain benefit—or are simply unwilling to provide it.

The paper is organized in the following manner. Section 2 describes the experimental design, and Section 3 reports the main results of the intervention. In this section we provide evidence that the intervention was successful in improving IDPs' awareness of their inclusion in the RUPD and the benefits that accompany inclusion. Section 4 summarizes our investigations and details future work and policy recommendations.

2.   Experimental Design

The experiment was composed of the transmission of simple text messages directed to the registered cell phones of a random half the displaced households upon inclusion to the RUPD.[5,6,7] IDP households assigned to the control group had to follow the regular procedure (i.e., arrive at UAO center and wait to be assisted) in order to be informed of the status of their application. Lastly, a post-intervention survey, described in Section 2.3, was conducted in order to evaluate the impact of the SMS strategy on IDP households' awareness of their inclusion in the RUPD and their entitlement to benefits.

2.1   Implementation

The entire intervention was executed during six months in coordination with *Acción Social*. It occurred between September 2009 and February 2010, and a survey was subsequently carried out between March and May 2010. *Acción Social's* involvement in this experiment was twofold and took place both at the beginning and the end of the implementation phase. First, the agency provided us with the data on the newly included IDPs in the RUPD. These data were then used to allocate households randomly into treatment and control groups. Second, after we assigned

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[5] It is estimated that over 98 percent of displaced households have cell phones (NVS-II, 2008). This is because penetration of mobile telecommunications is very large in developing countries, especially in the case of our target population because they move around often because of their displaced condition.

[6] The SMS sent to treated households was the following: *ACCIÓN SOCIAL informs that you have been included in the RUPD. Please go to the closest UAO for more information.*

[7] A second SMS reminder that RUPD inclusion entitled them to official benefits was also sent to half of those receiving the baseline treatment. However due to the reduced sample of subjects taking part in the follow-up survey who actually received this second message, we have no power to analyze the impact of this additional piece of information. Section 2.2 gives discusses the issues of selection and attrition and assesses whether these are likely to generate any bias.

households intro treatment and control groups, the agency transmitted the SMS to the treated subjects.

Because we found no reliable or comprehensive official information system with which to monitor awareness and subsequent appropriation of social benefits by included IDPs, we conducted our own follow-up survey to identify whether the use of SMS to communicate inclusion in the RUPD had a significant impact on benefit awareness and knowledge. We executed a survey pilot run March 12 and 13 2010, that allowed us to adjust the original questionnaire and assess the length of the interview. Then the fieldwork took place between April 7th and May 19th 2010.

In order to measure the impact of the SMS intervention on the awareness of benefits our survey includes questions regarding: i) whether households knew they were included in the RUPD,[8] and ii) whether they knew some or any of the benefits to which they were entitled. In addition, we included a complete set of questions that allowed us to build a demographic profile of the displaced household, which we use as controls in our empirical analysis (Section 3).

2.2   Sample Issues and Threats to Inference

Selection

Given IDPs' vulnerable status and the fact that they have been victimized by armed groups, the office of the Colombia's General Prosecutor mandated that any person or institution wishing to contact displaced individuals by cell phone or any other means must first have their written consent. As a result, we staged a pre-intervention phase whereby we provided every office receiving RUPD-inclusion applications in Bogotá with a package of consent forms.[9] The consent form (which is provided in the online Appendix), was to be attached to the displacement declaration form and returned to *Acción Social*, which in turn would forward it to our research team in order to select from the RUPD only those who had consented. On the other hand, we were not permitted to contact by SMS the households that who did not consent. Of course the

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[8] For reasons that we will explain in section 2.2, we cannot guarantee that text messages sent to the registered cell phones of treated households will actually be received and read by the target party. Thus with the answer to this question we are able to measure *compliance* and then apply instrumental variable methods to compute the *causal effect* of being assigned to treatment on awareness and take-up of benefits.

[9] To ensure that every IDP was given the choice to sign one such form, the number of forms provided was based on estimated flow of IDPs to each office.

households that did not consent were not part of the control group either, because we eventually needed to interact with all participating households in the survey stage and for this we needed consent. Thus the mandate of the General Prosecutor and the subsequent pre-intervention consent stage made so that there is substantial self-selection of IDPs into the experiment. Out of the entire population of IDP households that arrive to Bogotá during our intervention (1,433 households) only 43 percent (607) signed the consent form.[10]

This could potentially constitute a threat to our results. If the households that agree to be contacted by phone happen to be systematically different than those who did not sign the form according to characteristics that may be correlated with the awareness of the social benefits then our estimates would be biased. For instance, if signing the consent form is correlated with households' own assessment of how likely it is to reach them via cell-phone, then by excluding those who did not consent we are filtering out several potential non-compliers, which in turn would lead us to overestimate the intention to treat effect. In general, if self-selection into the intervention is correlated with either treatment compliance or the potential outcomes, then by relying on a non-random share of the population of IDPs, our results would be biased.

Attrition

Moreover, the attrition rate was rather high compared to other randomized experiments. In the survey stage of the experiment, in spite of the great effort by the enumerators, only 36% of the 607 households who signed the consent form were found. A high attrition rate was expected due to two reasons. First, IDP households migrate at a higher rate than other populations, which makes them difficult for survey teams to locate. For example, IDPs tend to declare their status in the first location they reach following displacement. This can be misleading, however, as many IDPs do not stay long-term at this first location: The spontaneous nature of forced displacement usually causes this population to flee to the nearest safe area (usually urban centers), which is not necessarily that where the household will settle on a permanent basis. Second, the

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[10] The complex logistics involved in the distribution of consent forms throughout the city and the training of officials receiving declarations factored into this low rate. Indeed, officials taking the statement of the newly arrived IDPs were not obliged to hand them the consent form and many of them actually saw this extra step in the process as a burden to avoid.

communication mean used during the experiment, cell phones, increased the likelihood of observing a high attrition rate. When trying to locate households through their registered cell phone to arrange a meeting for the survey to take place, we found that IDPs' cell phones were often shared among family and neighbors, and often lost or stolen. During the survey stage we collected qualitative evidence in the forms of narratives. These illustrate the most common reasons for attrition as the following:

Outdated contact details included in the consent form. In several instances the registered cell phone was not in service or calls were forwarded automatically to the mailbox, or the cell phone was registered under an unrelated name. Upon visiting registered addresses, the enumerators often discovered that the sample subjects either had left or had never resided there.

Mobility of IDPs. Due to budget constraints and the narrow scope of the trial in Bogotá the enumerators were not able to track subjects that had moved to other locations.

Appointment defectors. The enumerators encountered numerous IDPs who reneged on their interview appointments. The two contributing factors were IDPs' inability to leave work and the provision of false addresses. The former is a direct result of IDPs' employment instability and the informal job sectors in which they work. The latter stems from distrust and their suspicion based on their prior victimization.

As with selection, the attrition problem is also likely to generate bias estimates if there are systematic differences, according to characteristics that are associated with the awareness of benefits, between households actually surveyed and those who were not found.

We now test whether the problems of selection and attrition, inherent to our experiment because of the target population and the institutional constraints, are likely to bias the results relative to the treatment effect that we would get if we were to scale the intervention to the entire IDP population. The reason we can run such test is because we have data, albeit on only few observable characteristics, on the 1,433 IDP households that constitute our universe of interest. These data were obtained from *Acción Social* and include information on the *gender* of the household head (or the representative that signs the RUPD-application on behalf of the entire

household), the *number of beneficiaries* or household members attached to a single declaration[11], the *cause of the displacement* and the *region of origin*. From the 1,433 households we can identify those that signed the consent form and moreover, conditional of having signed it, we can identify those that were surveyed. This information allows us to test whether there are systematic differences across such samples and hence whether selection or attrition are likely to cause any bias to our estimates of the impact of the SMS reception on benefits' awareness.

In the case of the potential differences between households who signed the consent form and those who did not, results from a t-test of mean differences are summarized in Table 1. For expositional reasons, in the main text we focus on the first two variables (*gender of the household head* in Panel A, and *number of IDP beneficiaries claimed* in Panel B).[12] This tests whether self-selection into the experiment (by filling the consent forms) is correlated systematically with the available observable characteristics and thus is likely to cause any obvious bias. Mean differences are not significant for any of the observable characteristics (standard errors are in parentheses). This suggests that the sample of households that participated in the experiment is presumably not different from the sample that did not.

In terms of actual figures Panel A of Table 1 shows that over half of the household representatives are women, which is consistent with the aforementioned fact that women largely head IDP households. The difference in the proportion of women between those who signed the consent form and those who did not is a non-significant 0.3 percentage points. In addition, households who signed the consent form include on average 0.1 additional beneficiaries relative to those who did not sign. This difference is not only small but also not statistically significant.

Importantly, within the sample of households who signed the form and thus participated in the experiment, comparing those that were eventually interviewed versus those affected by attrition does not yield statistically significant differences either. T-tests of mean differences for this are summarized in Table 2. According to Panel A, the difference in the proportion of households headed by a woman who participated in the follow-up survey and those who were not located for an interview is somewhat larger (6.7 percentage points), but it is not significant.

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[11] Every declarant includes in his or her declaration a certain number of beneficiaries comprised of the number of household members who fled together and for whom the facts reported in the declaration apply.

[12] Equivalent tables dealing with the *cause of displacement* and *the region of origin* are reported on the online Appendix. These comparisons use a non-parametric chi-squared test instead of the "t", as there are multiple categories in each variable. In this case the null hypothesis is that the samples come from the same distribution of "causes" or "regions of origin".

Moreover, as in the case of selection, households who participated in the follow-up survey include on average 0.1 additional beneficiaries relative to those who were not found.

We then claim that neither selection nor attrition constitute big threats to our results. But we have only suggestive evidence to back such claim as unfortunately there is a limited number of observable characteristics available for the entire population and we ignore how the samples differ across other dimensions. However, of the 218 households (902 people) surveyed, exactly half were treated and half belonged to the control group. Since we did not fix the number of survey respondents but rather this was determined by the high attrition rate, the fact that the originally assigned proportions of units treated and controlled (50/50) remained unchanged post attrition is a remarkable coincidence that further supports the idea that attrition rates are not systematically related to the treatment status. Therefore, we firmly assert that our results are not likely to be biased.

Table 1: Assessment of potential bias due to self-selection into experimental sample

	Consent	No consent	Difference
Panel A: Sample differences in declarant's gender (= 1 if female)			
	0.563	0.559	0.003
	(0.020)	(0.017)	(0.027)
Panel B: Sample differences in number of beneficiaries attached to declaration			
	2.163	2.043	0.120
	(0.088)	(0.099)	(0.132)

Table 2: Assessment of potential bias due to attrition

	Interviewed	Not found	Difference
Panel A: *Sample differences in declarant's gender (= 1 if female)*			
	0.606	0.539	0.067
	(0.033)	(0.025)	(0.042)
Panel B: *Sample differences in number of beneficiaries attached to declaration*			
	2.235	2.123	0.111
	(0.154)	(0.107)	(0.184)

Nonetheless selection and attrition did hurt the experiment by reducing the expected sample importantly. But the fact that we do find significant results in spite of our large standard errors is again suggestive of the important role that SMS can play as social policy tools.

3   Results

The first substantive question is whether the randomization was successful in generating comparable households in the treatment and the control groups. Table 3 shows that this is the case as there are no significant differences between treated and controlled households in terms of a large set of variables. We divide these into four categories: i) cause of displacement, ii) perpetrator, iii) household characteristics, and iv) individual (declarant) characteristics. Since the t-tests reveal no significant difference between treated and control units in over 90 percent of the observable pre-treatment characteristics (the only two exceptions are whether the declarant was sick the week before the survey or whether she new how to *send* SMS), then the sample appears to be largely balanced and hence we believe that the effect of the SMS treatment on the post-treatment outcome variables is likely to be *causal*.

Table 3: Descriptive Statistics

	Treated (N=109)	Control (N=109)	Difference
Panel A: *Cause of displacement*			
Threats	0.798	0.734	0.064
Killings	0.092	0.11	-0.018
Attack to town	0.009	0.018	-0.009
Forced recruitment	0.083	0.083	0.000
Other	0.018	0.055	-0.018
Panel B: *Perpetrator*			
Paramilitaries	0.266	0.33	-0.064
Guerrillas	0.624	0.642	-0.037
Not known	0.128	0.073	0.055
Panel C: *Household characteristics*			
Size at displacement	3.624	3.991	-0.367
Current size	4.165	4.110	0.055
No. displacement episodes	1.202	1.239	-0.037
Assets prior to disp	0.633	0.679	-0.046
Ethnicity: Afro-Colombian	0.064	0.092	-0.028
Ethnicity: Indigenous	0.046	0.046	0.000
Ethnicity: Other	0.89	0.862	0.028
SMS literacy	0.881	0.872	0.018*
Panel D: *Declarant's characteristics*			
Sex (1=woman)	0.633	0.606	0.028
Age	36.972	36.11	0.862
Education	2.358	2.367	-0.009
Sick last week	0.486	0.450	0.037**
Looked for job last week	0.459	0.468	0.009
Community network	0.037	0.055	-0.018

Given that the randomization was successful, the next question is whether treated households benefited from the experiment. Table 4 reports the incidence of benefit awareness in the treatment and the control group (first two columns) and the mean difference (column 3). We first look at the aggregated set of benefits in the top row. Benefits are then disaggregated in subsequent rows as follows: i) *Housing* aggregates temporary housing and rent subsidy; ii) *Supplies* aggregates kitchen and cleaning supplies, beds and mattresses and clothing; iii) *Medical care* aggregates the right to medical assessment, psychological assessment, emergency medical care, and medicines; iv) *Food* aggregates the right to receive food supplies. [13]

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[13] Because our survey was carried out shortly after the treatment took place, we refrain from looking at its effect on benefits that are not usually claimed in the first few months of the displacement episode. In particular, we focus on the benefits that constitute the so-called Emergency Humanitarian Help (Decree 2569 of 2000).

Table 4: Awareness of benefits - *Intention to treat*

	Treated	Control	Difference
All benefits	0.336	0.314	0.023
	(0.014)	(0.013)	(0.019)
Housing	0.436	0.385	0.050
	(0.034)	(0.033)	(0.047)
Supplies	0.087	0.094	-0.007
	(0.014)	(0.014)	(0.019)
Med. care	0.489	0.433	0.055*
	(0.024)	(0.024)	(0.034)
Food	0.523	0.569	-0.046
	(0.048)	(0.048)	(0.068)

Notes: * Significant at 10%, ** significant at 5%, *** significant at 1%.

As it turns out, the only significant difference (at the 10 percent level) is that of *Medical Care*. Treated households are six percentage points more aware than the control of their right to request benefits related to medical care. We argue that the reason that the difference is not significant for the rest of benefit indicators may be due to non-compliance. Indeed, a non-negligible share of households that were assigned to treatment reported in the survey not to have received the text message. The reasons behind the noncompliance are closely related with the large attrition rate: many cell phones were not currently in use or were uncharged, lost or stolen. Or the message was perhaps accidentally deleted.

The presence of non-compliers suggests that the SMS reception is likely to be endogenous because it may be correlated with characteristics that we are not controlling for and that may affect the benefits awareness. However, the actual treatment assignment, which was fully under our control, is random. This suggests that the most sensible empirical strategy to identify the causal effect of the SMS reception on the IDP-benefits awareness is an Instrumental Variables (IV) one, where the actual message reception is instrumented with the (exogenous) treatment status.

The IV estimate of the causal effect is in essence the ratio of the mean difference of the rate of benefit knowledge to the mean difference of SMS reception, with both differences computed across treatment status. This IV estimator is called the *Wald* estimator, and it holds in

cases like ours, when the instrument is *binary*. The causal effect computed this way is called Local Average Treatment Effect (LATE), and it provides information only on the impact of the treatment on the IDP households affected by the instrument.

In order to be able to use this empirical strategy it must be the case that treatment status is a good instrument of SMS reception. That is, the coefficient from a regression of the SMS reception on the actual treatment assignment has to be significantly different from zero. We now show that this is the case. Table 5 reports the Probit estimates of the impact the treatment had on (self-reported) SMS reception. While the estimate presented in column 1 includes no controls, columns 2 through 5 include all the controls described in Table 3 one extra category at a time: Column 2 includes controls regarding the cause of displacement, while column 3 adds perpetrator dummies. Lastly, columns 4 and 5 add household and individual characteristics, respectively. In all cases the estimate of the causal effect is positive and significant at the 1 percent level.

Table 5: Effect of treatment on reported SMS reception – Probit regression

Dependent variable: *SMS reception*

$T = 1$	1.064***	1.097***	1.124***	1.184***	1.249***
	(0.028)	(0.029)	(0.032)	(0.000)	(0.000)
Controls:					
Cause of disp.		✔	✔	✔	✔
Perpetrator			✔	✔	✔
Househols charact.				✔	✔
Declarant charact.					✔

Notes: * Significant at 10%, ** significant at 5%, *** significant at 1%.

Robust standard errors in parentheses

Table 6 reports the results for the five sets of benefits as defined above. All regressions include the full set of controls (as in the last column of Table 5) and report robust standard errors. Importantly, the treatment increases significantly (at the 5 percent level) the awareness of the aggregate of all benefits. This is, however, driven by the large and positive impact of the treatment of the awareness of *Medical care* benefits, and by the impact on the awareness on the

right of claiming housing-related benefits. The treatment does not increase awareness of the right to *Food* or *Supplies*.[14]

Table 6: Effect of SMS on Benefit Awareness– Wald estimator

| | Awareness of Benefits | | | | |
	All benefits	Medical care	Housing	Supplies	Food
$\widehat{SMS}$	0.075**	0.148**	0.150*	-0.014	-0.014
	(0.037)	(0.063)	(0.090)	(0.037)	(0.116)
All controls	✔	✔	✔	✔	✔

Notes: * Significant at 10%, ** significant at 5%, *** significant at 1%.
Robust standard errors in parentheses

3.1 Heterogeneous effects

We now explore potential heterogeneity in treatment effects. To that end we repeat the analysis of Table 6 but split the sample according to characteristics of the household representative in charge of applying for benefits on behalf of all household members. We focus on the aggregate measure of awareness of all benefits.

Recall that, when looking at the entire sample, SMS reception –instrumented with treatment assignment- increases the awareness that the household is entitled to receive benefits by 7.5 percentage points (see column 1 of Table 6). We first explore whether this average effect is differential according to the sex of the declarant. In the first two columns of the top panel of Table 7 we split the sample according to whether the household head is a woman or a man. We find that the average effect, reported on Table 6, is entirely driven by women: while in the subsample of men the effect is very small (1 percentage point increase in awareness of benefits eligibility after SMS reception) and not significant, in the subsample of women it is large and significant at the 5% level. For households with women in charge of the displacement declaration process, receiving the SMS increases awareness in 12 percentage points. This

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[14]We do not analyze the effect of treatment on the *take-up* of the different benefits. The reason for this is the fact that while awareness can be influenced by an improvement on communication, the actual request of benefits is a choice of the IDP. Therefore the relevant variable to observe is awareness.

heterogeneity is important to note as it suggests that the types of communication strategies like the one analyzed here are likely to be more successful in terms of achieving the policy objective if they target women rather than men.

It is also worth looking at potential heterogeneous effects according to other observable characteristics of the applicants. For instance using information and communication technologies for social policy purposes is likely to be more effective if targeted individuals are relatively younger. This is because younger adults use such technologies more than older ones. This hypothesis is confirmed in the third and fourth columns of the top panel of Table 7. There, we split the sample according to the mean age of the applicant (36 years old), and note that while the effect of SMS reception on the awareness of benefits of the relatively older is not significant, that on the relatively younger is large and significant at the 1% level. Receiving the SMS increases the awareness of this sub-population in 14 percentage points.

Contrary to what one would expect the effect if larger for relatively less educated individuals. After dividing the sample according to the mean score of the education level indicator (equivalent to having up to primary education), we find that the effect of the SMS reception is not significant for households whose representative has an education level beyond elementary school (last two columns of the top panel of Table 7).

In the bottom panel of the Table we show that the effect is higher for bigger households (that have over 4 members), and for households that have had a sick member in the last month. This is not surprising as, as suggested by Table 6, results are driven by health care-related benefits and households that have sick members are more likely to pay attention to opportunities to obtain medical assistance.

Finally we find no differential effect according to whether the household head works or not, as evident from the last two columns of the bottom panel of Table 7.

Table 7: Effect of SMS on Benefit Awareness (*All Benefits*)
Heterogeneous Effects

Applicant characteristics:	Sex		Age		Education	
Subsample:	Woman	Man	> 36 yr	≤ 36 yr	> primary	≤ primary
$\widehat{SMS}$	0.115**	0.0117	0.0690	0.137***	0.00329	0.110**
	(0.0532)	(0.0631)	(0.0758)	(0.0466)	(0.0564)	(0.0531)
Applicant characteristics:	H/hold members		Sick		Works	
Subsample:	> 4	≤ 4	Yes	No	Yes	No
$\widehat{SMS}$	0.165**	0.0632	0.153***	-0.0230	0.0179	0.0945
	(0.0702)	(0.0441)	(0.0580)	(0.0593)	(0.0477)	(0.0595)
All controls	✓	✓	✓	✓	✓	✓

Notes: * Significant at 10%, ** significant at 5%, *** significant at 1%. Robust standard errors in parentheses.

In a final exercise we look at the characteristics that make the control group more likely to know about their eligibility. Indeed, it is important to know what are the factors that make displaced household knowledgeable of their rights in the absence of an expedite communication strategy like the one we analyze here.

To explore this issue we regress the dummy that indicates that the household is aware of its entitlement to benefits from the government on the same characteristics used to investigate the heterogeneous effects in Table 7. We focus however on the subsample of control households, dropping the treated from the sample. Consistent with the findings reported in the previous table, Table 8 shows that both bigger households as well as households with a member experiencing sickness during the previous month, are more likely to know of its eligibility. Both factors increase the demand for social services within the household, which may explain why household heads are pushed to find out what type of help they can receive from the government.

Table 8: Determinants of Benefit Awareness (*All Benefits*)
Sample of Non-Treated

Dependent variable:	Benefits awareness
Characteristic of declarant:	
Sex (= 1 if woman)	-0.008
	(0.082)
Age	-0.001
	(0.003)
Education level	0.010
	(0.030)
H/hold members	0.045***
	(0.016)
Sick last month	0.163**
	(0.077)
Worked last week	0.046
	(0.078)

Notes: * Significant at 10%, ** significant at 5%, ***
significant at 1%. Robust standard errors in parentheses.

4   Conclusion

We presented evidence from a randomized-controlled trial that the use of SMS as a channel to improve the communication between the government and beneficiaries of social programs can empower vulnerable populations and substantially increase their welfare. In this respect, we conclude that SMS represents a potentially effective instrument for social policy.

Our findings are specific to the Colombia context, a country that has experienced internal conflict for 40 years and has the world's largest IDP population. We demonstrate that an inexpensive intervention such as SMS directed to vulnerable households increases awareness of their entitlement to social benefits.

The significance of our results, coupled with the low intervention costs, provides a strong argument for the inclusion of this research in policymaking under Colombia's new administration. The present is an opportune time for more experimental pilots to be replicated nationwide. The purpose of this strategic approach would ensure the increase of social benefits

available to IDPs. Undoubtedly, this issue would be high on the government's social policy agenda.

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