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# Can SMS Technology Improve Low Take-up of Social Benefits?<sup>1</sup>

**Abstract:** Low take-up of stigma-free social benefits is often blamed on information asymmetries or administrative barriers. There is limited evidence on which of these potential channels is more salient in which contexts. We designed and implemented a randomized controlled trial to assess the extent to which informational barriers are responsible for the prevalent low take-up of government benefits among Colombian conflict-driven internal refugees. We provide timely information on benefits eligibility via SMS to a random half of the displaced household that migrated to Bogotá over a 6-month period. We show that improving information increases benefits' take-up. However, the effect is small and only true for certain type of benefits. Hence, consistent with previous experimental literature, the availability of timely information explains only part of the low take-up rates and the role of administrative barriers and bureaucratic processes should be tackled to increase the well-being of internal refugees in Colombia.

**Keywords:** Information asymmetries, take-up rate, SMS, RCT

**JEL Codes:** D82, C93

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<sup>1</sup>This paper is part of a larger project of improving the access to government benefits of the Colombian internally displaced households, generously financed by the IDB. A companion paper, that is being considered for publication as a contributive chapter in a policy volume, uses the same randomized experiment described here to analyze the impact of text messages on the *awareness* of displaced households about their entitlement to different types of benefits. Rather, the outcome studied here is the actual benefits *take-up*. Some excerpts such as the description of the experimental design are unavoidably repeated in the two papers. We thank Fernando Barberi, Patricia Justino, Pablo Querubín and Clara Ramírez for useful comments and discussion. We also thank seminar participants at the Impact Evaluation 2011 Conference in Cuernavaca, Mexico. For the data collection we are grateful to Yezid Botiva, Alejandro Garnica and Yamile Palacios from SEI.

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# 1 Introduction

The low take-up of social benefits is a puzzling empirical regularity both in developing and in developed countries. To name just a few examples, Miguel and Kremer (2004) provide evidence that just above half (57%) of a sample of Kenyan households picked up for their school-age kids free deworming pills, which were proved to improve childrens health with little side-effects. Take-up rates of a training program proved to significantly increase consumption are even lower (36%) in India, as reported by Banerjee et al. (2011). Similarly, and also for Kenya, Duflo, Kremer, and Robinson (2010) show that maize farmers do not use fertilizers even when these are available at very low prices. In the case of developed countries, Currie (2004) finds relatively low take-up rates of social programs in both in the US and the UK.

In Colombia violence-driven internal migration is, according to the UN “the biggest humanitarian crisis in Western Hemisphere” (UN 2004). *Acción Social* (AS hereafter), the government agency in charge of social policy when we carried out our experiment, estimates that by the end of 2009 approximately 3.5 million people had been forcibly displaced due to the conflict.<sup>1,2</sup> Internally displaced people (IDPs) are legally entitled to obtain from the government a large set of benefits. Nevertheless, AS estimates that approximately 70% of the eligible IDPs do not take-up any of these benefits. This is puzzling as IDPs are among the most vulnerable people in Colombia: 98% of displaced households live below the poverty line and face unemployment rates much higher than the rest of the population (Ibáñez and Moya 2010).

We designed and implemented a randomized controlled trial (RCT) to assess the extent to which informational barriers are responsible for the prevalent low take-up of government benefits among Colombian internal refugees. Using text messages (SMS), we communicated to a random half of the entitled households that they were eligible for government benefits. At the end of the implementation we conducted personal surveys with treated and control household heads to find out about their eligibility awareness as well as their actual take-up.

Our paper is part of a growing experimental literature that investigates the salience of different channels explaining the puzzle of low take-up of benefits.

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<sup>1</sup> This is almost 9% of Colombia’s population and roughly 8% of all internal refugees worldwide. Indeed, the UN High Commissioner for Refugees (UNHCR 2009) asserts that Colombia is one of the countries with the highest numbers of IDPs worldwide, together with Iraq and Sudan.

<sup>2</sup> AS was replaced in 2011 with the Administrative Department for Social Prosperity.

One such channel suggested by the literature is stigma. In a pioneer paper, Moffit (1983) argued that the stigma (of being identified as needy or disadvantaged) enters negatively in the cost-benefit analysis of people who are eligible for benefits. Hence, non-participation in social programs is a rational decision. Other explanations that are generally offered in the literature have to do with information asymmetries and transaction costs. On the one hand, people may just ignore that they are eligible for certain benefits, or are uncertain about the actual gain from participating in a particular program. On the other, transaction costs arise when obtaining the benefit is costly, for instance because it involves cumbersome paperwork or other bureaucratic obstacles.

These reasons are not mutually exclusive. Stigma can be thought as another cost that adds to the potential transaction costs. In turn, getting information might not be worth if applying to benefits is very costly, both in terms of transaction costs and stigma. For instance, eligible people often cite in surveys lack of information as the main reason for not taking-up benefits (Coe 1983). However, Currie (2004) argues that because surveys are usually not anonymous, people can be influenced by stigma while citing a different reason for a certain behavior.

Moreover, disentangling which of these causes is empirically more salient is very challenging. In her thorough review of the determinants of low take-up rates Currie (2004) concludes that after years of research little is known about the precise types of mechanisms that explain the puzzle of low take-ups. However, although testing in an objective way the saliency of the stigma hypothesis *vis-a-vis* the transaction costs is almost impossible as stigma is a subjective feeling that adds to objective costs, there is a large amount of suggestive evidence against the relevance of stigma. For instance, Currie (2004) notices that the take-up of social programs both in the US and the UK is low in both means tested programs or non-means tested programs. This is important as stigma is usually identified as a potential explanation of low take-up in means tested programs where it becomes apparent what are the material shortages of potential recipients.

This suggest that stigma is a rather negligible explanation of the low take-up leaving information asymmetries and transactions costs as the two remaining candidate explanations. We believe this is also the case in our particular set up. First, the program that we study, that distributes benefits to IDPs in Colombia, is not means tested. Second, the target population are victims of the long-lasting Colombian conflict, most of whom are forced to flee from their lands leaving everything behind. Thus, the existence of an objective condition that makes these households needy of social assistance is likely to reduce the scope for stigma.

RCTs have recently been conducted to experimentally manipulate factors thought to influence low take-up in a controlled environment.<sup>3</sup> A few papers, for example, have concluded that information is empirically not such an important reason for the low take-up of benefits either. For example, Duflo, Kremer, and Robinson (2010) show that the take-up of fertilizer in Kenya is low despite farmers knowing both how to apply it and that doing so would be highly profitable. In turn, Jensen (2010) documents that students in Dominican Republic that are taught about the wage premium of completing secondary education *vis-a-vis* dropping out and staying with just a primary diploma, are not more likely to complete high school than control students. Other papers do find a significant role of information asymmetries. Daponte, Sanders, and Taylor (1999) conduct an RCT in the context of the Food Stamp Program in the US, to provide direct evidence on the importance of information. They find that 35% of the households that were given information about their eligibility to the program applied to obtain it. This effect is however small, as it implies that the majority of the informed households (65%) did not apply. This is consistent with the findings of Duflo and Saez (2003), who study the effects of information on the take-up of a retirement plan by employees. Employees were randomly encouraged (through payments) to attend a meeting providing information about the plan. The authors find a statistically significant (yet small) information spillover effect on the plan take-up of the co-workers of informed individuals. Our results are consistent with these findings. We show that providing timely information about program eligibility increases the take-up of only certain benefits, and in relatively low rates.

While the evidence for or against the information channel is mixed. Currie (2004) suggests that, at least in the case of the US and the UK, the main reason explaining low take-up is transaction costs. She concludes that lowering administrative barriers and making program enrollment automatic (that is eliminating bureaucratic obstacles) is key for increasing take-up. Because governments are largely unsuccessful in increasing take-up rates, Currie suggests that boosting participation rates can be done, for instance, by giving private institutions a stake in assisting individuals getting enrolled.<sup>4</sup> Our findings second these policy prescriptions in that lack of a transparent and accessible information strategy for the IDPs in Colombia only explains part of the low take-up of benefits, and the transaction costs involved in claiming these should be addressed to alleviate the

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<sup>3</sup> While there is also non-experimental evidence, here we focus on the experimental papers to contextualize our own RCT.

<sup>4</sup> Administrative costs are also an important factor in Warlick (1982), Dorsett and Christopher (1991), Konig and Ridder (1997), Currie (2000) and Bitler, Currie, and Scholz (2003).

situation of the country's conflict driven refugees. Providing timely information about benefits is of little use if the bureaucratic obstacles that prevent people from accessing them are still in place.

Our paper is not the first that uses SMS for the provision of information. The flow of timely information associated with the use of cell phones has been increasingly proved cost-effective to achieve desirable outcomes in both developing and developed countries. Karlan et al. (2011) implemented field experiments in Bolivia, Peru and the Philippines to evaluate the impact of saving reminders (sent by either text messages or letters) on the amount saved and the likelihood of reaching one's savings goal. Reminders that focused on specific future expenditures were especially effective. Aker (2008) exploits the expansion of cell-phone coverage in Niger as a quasi-experiment to estimate the impact of information on the performance of the grain market. She finds that the introduction of cell phones reduced price dispersion across markets, thus increasing consumption and welfare. Jensen (2007) uses a similar identification strategy to analyze the impact of the introduction of information on the fishing industry of Kerala, India. Again, he finds a large reduction in price dispersion following the introduction of cell phones. Allison and Strauss (2009) implement a field experiment whereby SMS are sent to encourage people to vote in the 2006 US elections. They conclude that text messages are a very cost-effective voters' mobilization strategy. Similarly, Suarez (2005) argues that the victory of the PSOE party over rival PP in the 2005 Spanish elections can be explained by the mobilization of new voters (youngsters and absentee) that was facilitated by the use of SMS after the terrorist attacks of March 11, 2005, right before the elections. She argued that SMS raised consciousness on the PP responsibility in the attacks after having supported the 2003 Iraq invasion while in office.

In this paper we find that information received through SMS increase the take-up of benefits by Colombian IDPs. However, consistent with some of the literature cited above, the impact is relatively small.

The rest of the paper is organized as follows: Section 2 briefly provides some context about the Colombian conflict and the IDPs. Section 3 explains the experimental design and Section 4 the results. We conclude in Section 5.

## 2 Context

Colombia has experienced a low-intensity civil strife for about five decades. The country's most salient illegal armed group is the FARC (from the Spanish acronym of Revolutionary Armed Forces of Colombia). In addition to the guerril-

las the conflict features another illegal armed group since the early 1980s—the paramilitary forces, 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 2009).

Land disputes are the main driver of forced displacement: up to 4 million acres of land have been abandoned by the original owners. 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% 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 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. Eighty percent of IDPs never return to their households (NVS-II 2008). IDPs are further hurt 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% of displaced households are female-led (this is 50% more than the national average), and one fifth of household heads are illiterate. In addition, children and adolescents are at greater risk after displacement than are 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. There is no doubt that IDPs are among the most vulnerable people in Colombia.

In recent years the Colombian Constitutional Court has specifically targeted the needs of IDPs, ruling that the government must put an end to their situation. The government has made efforts to comply with this mandate, but with relatively little success. Law 387 of 1997 created the Unique Registry of Displaced Population (hereafter RUPD from its acronym in Spanish). The RUPD, managed by AS up to 2011, constitutes the official account of displaced households nationally with the objective of identifying the IDPs that are eligible for benefits. Indeed, before receiving any type of aid, displaced households have to apply for 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 forced migration. To avoid misallocation of benefits, AS investigates

whether or not everyone who claims having been forcibly displaced is truthful. Only those whose condition is confirmed are included in the RUPD. Inclusion in the registry is a necessary yet not a sufficient condition for receiving benefits. These are largely demand-driven, and often entail additional requirements.<sup>5</sup>

This system has several major limitations. First, in order to receive updates about their status in the inclusion process an applicant must visit an Attention and Orientation Unit (UAO from its Spanish acronym). This entails large transactions costs, including transportation expenses, long waiting lines, and the forgone daily income of the household head, who must assist in person. Applicants usually have to repeat the process several times, when they are told that their inclusion in the RUPD is still pending.<sup>6</sup> But transaction costs is not the only (a priori) explanation of the low take-up of benefits. Another contributing factor is the lack of information about what benefits can be claimed once inclusion in the registry is secured. In all, as mentioned in the introduction, AS estimates that some 70% of the eligible households do not claim any benefit.

### 3 Experimental design

We sent a text message to the registered cell phones of a random half the RUPD-included IDPs that arrived to Bogotá in the last quarter of 2009 and first of 2010. The SMS was as concise as possible, so as to fit in just one message and being readable in one screenshot. The message reads: “ACCIÓN SOCIAL informs that you have been included in the RUPD. Please go to the closest UAO for more information.”

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5 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, say, books or uniforms.

6 During the survey stage of our intervention we collected direct accounts of the experiences of our subjects going to the UAO to find out about their application status. Some common experiences were the following. In order to obtain the tickets authorizing attention at the UAO, 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 Bogotá, an 8-million people city). Some 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 status is still unknown. Sometimes personnel at UAOs simply are not aware of the information requested on how to access certain benefit—or are just unwilling to provide it.

We implemented this information strategy because it was virtually costless. Thus, any gain in terms of take-up following our treatment can easily be interpreted as cost-beneficial.<sup>7</sup> A key issue of the strategy is whether all potential recipients have cell phones where to receive the SMS. As it turns out, recent estimates suggest that over 98% 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, due to their migrant condition, they move around often. The World Bank estimates that over three quarters of the world's population have access to cell phones, with the number of users increasing six-fold between 2000 and 2012, from 1 billion to about 6 billion. Interestingly, most users (about 5 billion) are in developing countries.<sup>8</sup>

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.

### 3.1 Implementation

The experiment was implemented from September 2009 through to February 2010 in coordination with AS. The role of AS was twofold. First, the agency provided us with real time data on the IDPs that were included in the RUPD. These data were then used to allocate households randomly into treatment and control groups. Second, after we assigned households into treatment and control groups, the agency transmitted the SMS to the treated subjects.

In the absence of reliable and comprehensive official records on the take-up of benefits by eligible IDPs, we carried out a follow-up survey to gather such information. The survey was conducted in April and May 2010. In addition to questions about the take-up of benefits by members of the household, we asked a large set of questions about demographic characteristics of the household and pre-treatment outcomes. We use these both to check the balance of the treated and control samples after the randomization and as controls in the empirical

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<sup>7</sup> In fact, by the time this paper went to press, and following the results from the described experiment, the government of Colombia scaled-up the SMS strategy to communicate inclusion to the RUPD in the whole country.

<sup>8</sup> See World Bank's report: *Information and Communications for Development 2012: Maximizing Mobile*. Available at: <http://siteresources.worldbank.org/EXTINFORMATIONANDCOMMUNICATIONANDTECHNOLOGIES/Resources/IC4D-2012-Report.pdf>. Last accessed 12/16/13.



analysis (Section 4). We also asked whether they knew they were included in the RUPD.<sup>9</sup>

### 3.2 Selection and attrition

Given previous victimization of IDPs and the possibility that they may still be targeted by illegal armed group, the office of the 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 in Bogotá, receiving applications by IDPs to be included in the RUPD, with a package of consent forms.<sup>10</sup> The consent form was to be attached to the application form and returned to AS. After the agency validated the declared facts and decided upon the inclusion of applicants in the registry, it forwarded to us the consent forms of the included households. We used these to filter out of our sample all the IDPs who did not sign the consent form.

Of course, the households that did not consent (and hence that we were not allowed to contact by SMS) were not part of the control group either. Indeed, we eventually needed to interact with all participating households in the survey stage, whether treated or control, and for this we needed consent anyway.

Thus, the mandate of the General Prosecutor and the subsequent pre-intervention consent stage created a situation of a substantial self-selection of IDPs into our experiment. Table 1 shows that out of the entire population of RUPD-included IDP households that arrived to Bogotá in the 6 months of our intervention (1433) only 43% signed the consent form. The complex logistics involved in the distribution of consent forms throughout the city and the training of officials receiving RUPD applications factored into this low rate. Indeed, officials taking the application 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 personal burden that had no associated compensation.

This could potentially be a threat to the external validity of our results. If the households that agree to be contacted by phone happen to differ systematically

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<sup>9</sup> While there was no subject in our sample that was not included in the registry, there is no guarantee that text messages sent to the registered cell phones of treated households will actually be received and read by the targeted recipient. Thus, with the answer to this question we are able to measure treatment *compliance* and then apply instrumental variables methods to compute the causal effect of being assigned to treatment on take-up of benefits.

<sup>10</sup> 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 such office.

**Table 1:** Selection and attrition.

	N	%
Included	1433	100
Consent	607	43
Interviewed	218	15 (36)

from those who did not sign the form, according to characteristics that may be correlated with the take-up of benefits, then our estimates would be biased.

In addition to selection, our experiment faced massive attrition. In the survey stage we had to track down every single subject through Bogotá, including in many dangerous and recondite places. In spite of the great effort by our enumerators, only 36% of the households were found (Table 1). A high attrition rate was expected for 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 identify themselves as such in the first location they reach following displacement. However, 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, by communicating with the subjects through cell phones we 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 form of narratives. Among other interesting insights, these illustrate the most common reasons for attrition as the following:

1. *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.
2. *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.
3. *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 take-up of benefits, between households actually surveyed and those who were not found.

We now test whether the problems of selection and attrition summarized by Table 1, inherent to our experiment because of the target population and the institutional constraints, are likely to bias the results. The reason we can run such test is because we have data, albeit on only few observable characteristics, on the 1433 RUPD-included IDP households that constitute our universe of interest. These data were obtained from AS 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 application, the *cause of the displacement* and the *region of origin*. From the 1433 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 according to the characteristics listed, and hence whether selection or attrition are likely to cause any bias to our estimates of the impact of the SMS reception on the take-up of benefits.

The t-tests of mean differences are summarized in Table 2. Here we focus, for expositional reasons, on the first two variables (*gender* of the household head in Panel A, and *number of IDP beneficiaries* claimed in Panel B).<sup>11</sup> Table 2 compares the mean differences of the two variables across households according to: i) whether they signed the consent form or not (first comparison within each panel) and, ii) whether they participated in the survey or were not found (second comparison). The first comparison 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. The second comparison tests whether there is a likely bias due to attrition. We report the standard errors and thus it becomes apparent that the differences are not significant in any of the cases (both across comparison and across variables—including those reported in the Appendix). This suggests that the sample of households that participated in

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<sup>11</sup> Equivalent tables dealing with the *cause of displacement* and the *region of origin* are reported in the Appendix (Tables A.1 through A.4). These comparisons use a non-parametric chi-squared test instead of the t-test. This because 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.”

**Table 2:** Assessment of potential bias due to selection and attrition.

Panel A: <i>Sample differences in declarant's gender</i> <sup>a</sup>		
	Consent vs. no consent	
Consent	No consent	Difference
0.563	0.559	0.003
(0.020)	(0.017)	(0.027)
	<u>Interviewed vs. not found</u>	
Interviewed	Not found	Difference
0.606	0.539	0.067
(0.033)	(0.025)	(0.042)
Panel B: <i>Sample differences in number of beneficiaries</i>		
	<u>Consent vs. no consent</u>	
Consent	No consent	Difference
2.163	2.043	0.120
(0.088)	(0.099)	(0.132)
	<u>Interviewed vs. not found</u>	
Interviewed	Not found	Difference
2.235	2.123	0.111
(0.154)	(0.107)	(0.184)

Notes: Standard errors in parentheses. <sup>a</sup>share of females. \*\*\* is significance at the 1%; \*\* is significance at the 5%; \* is significance at the 10%.

the experiment is not different from the sample that did not and, within the participants, the sample of households interviewed a posteriori is not different from the sample that was subject to attrition. Of course, this statement is only true for the few observable characteristics for which these comparisons can be made.

Panel A of Table 2 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. The difference in the proportion of women as household representatives who participated in the follow-up survey and those who were not located for an interview is somewhat larger (6.7 percentage points) but still not significant. Panel B repeats the exercise but focuses on sample differences across the average number of beneficiaries per declaration. None of these differences is either large or significant at conventional levels.

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.

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 of information in the take-up of benefits.

## 4 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 full battery 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 any of the observable pre-treatment characteristics (except whether the declarant was sick the week before the survey or whether she new to send SMS), the *ignorability* assumption of the Rubin Causal Model (Rubin 1974) holds. Moreover, given that every unit had an equal chance of receiving treatment a priori, we believe that the effect of the SMS treatment on the post-treatment outcome variables is indeed *causal*.

Next we look at the impact of the informational SMS on the take-up of benefits. It is worth noting, however that a simple regression of the take-up rate on the SMS reception is likely to produce biased results. This is because even if the allocation of households to receive or not the information about eligibility through text messages is random, the actual reception is arguably endogenous. 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 non-compliance are closely related with the large attrition rate: many cell phones were not currently in use or were uncharged, lost or stolen. Alternatively, the message was perhaps accidentally deleted.

To the extent that the actual SMS reception is correlated with unobserved characteristics that affect the likelihood of take-up, regressing this outcome on SMS reception produces biased results. However, the actual treatment

**Table 3:** Descriptive statistics.

	Treated (N=109)	Control (N=109)	Difference
<i>Panel A: Cause of displacement</i>			
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
<i>Panel B: Perpetrator</i>			
Paramilitaries	0.266	0.33	-0.064
Guerrillas	0.624	0.642	-0.037
Not known	0.128	0.073	0.055
<i>Panel C: Household characteristics</i>			
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*
<i>Panel D: Declarant's characteristics</i>			
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

\*\*\* is significance at the 1%; \*\* is significance at the 5%; \* is significance at the 10%.

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 take-up is instrumental variables (IV). That is, the actual message reception should be instrumented with the (exogenous) treatment status.<sup>12</sup>

<sup>12</sup> In cases like this, in which the instrument is binary (i.e., the allocation to treatment or control), the IV estimator is called the *Wald estimator*. Moreover, the effect estimated with this strategy is a Local Average Treatment Effect (LATE), and it provides information only on the impact of the SMS reception on the IDP households affected by the instrument (i.e., on the compliers).

Table 4 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% level.

This is the equivalent of the first stage of the IV estimation of the effect of SMS reception on the take-up of benefits. The second stage is in turn presented in Table 5. It uses the specification of column 5 of Table 4, that includes the entire set of controls.<sup>13</sup> In turn, each column reports the results on a different outcome (benefit). 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). These are: i) Free medical attention, diagnosis and medicines; ii) Access to temporary housing and rent subsidy; iii) Access to supplies for cleaning, cooking, mattresses and clothing; and iv) A basket with non-perishable food (like a bag of rice).

In column 1 we look at the entire set of benefits. Thereafter we disaggregated in medical care-type benefits (column 2), housing (column 3), supplies (column 4) and food (column 5). Importantly, the reception of the eligibility information increases the take-up of the aggregate of all benefits: Having received the information makes the recipient household 12 percentage points more likely to take-up at least one of the Emergency Humanitarian benefits. This is, however, driven by the large and positive impact of the treatment on the take-up of medical care. Having received the

**Table 4:** Effect of treatment on reported SMS reception – Probit regression.

<b>Dependent variable: SMS reception</b>					
<i>T=1</i>	1.064*** (0.028)	1.097*** (0.029)	1.124*** (0.032)	1.184*** (0.000)	1.249*** (0.000)
<i>Controls:</i>					
Cause of disp.		✓	✓	✓	✓
Perpetrator			✓	✓	✓
Households charact.				✓	✓
Declarant charact.					✓

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

<sup>13</sup> Robust standard errors are reported throughout.

**Table 5:** Effect of SMS on Benefit Take-up—Wald estimator.

	Benefits Take-up				
	All benefits	Medical care	Housing	Supplies	Food
$\widehat{SMS}$	0.117* (0.062)	0.152* (0.071)	0.064 (0.114)	-0.010 (0.241)	0.167 (0.111)
All controls	✓	✓	✓	✓	✓

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

Robust standard errors in parentheses.

eligibility information increase the likelihood that the displaced household uses medical services by 15 percentage points. Though positive for the other categories (with the exception of supplies) the effect is not statistically significant.

## 5 Conclusion

The low take-up of social benefits is an empirical puzzle that has been largely studied both theoretically and empirically. There are three potential causes for this behavior: stigma, lack of information and transaction costs. There is growing evidence that stigma is unlikely to be empirically salient. Further evidence, some of which stems from the implementation of RCTs both in developing and developed countries, suggests that information does play a role although it cannot explain the entire puzzle.

This paper adds to this evidence. Motivated by the experience of Colombian conflict-driven internal refugees, we implement an RCT to assess the extent to which poor information mechanisms from the government to benefit-eligible IDPs can explain the low take-up of the benefits that this population is entitled to.

We find evidence supporting the importance of adopting timely and cost-beneficial strategies like the use of SMS to provide information. Moreover, this strategy is favored by the stylized fact that almost all the IDPs uses cell-phones. Our findings suggest that receiving information about benefits eligibility increases the take-up of at least one of the Emergency Humanitarian services by almost 12 percentage points. In turn, this is driven by an increase in the likelihood of accessing medical care of over 15 percentage points. While our sample is relatively small to produce precise estimates, the rest of the benefit packages (with the exception of household supplies) is positively (though not significantly) affected by the reception of the information.

After learning about these findings, in 2011 the government of Colombia decided to adopt this communication strategy and scale it up to the whole



country. Today, IDPs are readily informed about their entitlement to access benefits and subsidies. This is one example of how academic research can help designing better policies.

Nonetheless, our experiment also sheds light on the fact that providing information is not enough to have a substantial effect on take-up if transaction costs, in this case associated with the cumbersome process described in Section 3, are not reduced. This insight is consistent with the review of Currie (2004) on what explains the low take-up of social benefits.

## Appendix

**Table A.1:** Differences in cause of displacement: Consent vs. no consent.

Cause of displacement	Consent	No consent	Difference
Death threat	77.93	75.29	2.64
Forced recruitment threat	11.17	12.64	-1.47
Disappearance of family member	2.72	2.87	-0.15
Armed combat	1.36	1.44	-0.07
Physical mistreat	0.27	1.15	-0.88
Death of family member	4.36	4.02	0.34
Deny or restrict access to survival goods	0.27	0.86	-0.59
Theft of goods by armed actor	0.27	0.86	-0.59
Kidnap of family member	0.27	0.57	-0.3
Sexual violence	0.82	0	0.82
N/A	0.54	0.29	0.26

**Table A.2:** Differences in cause of displacement: Interviewed vs. not found.

Cause of displacement	Interviewed	Not found	Difference
Death threat	81.06	76.17	4.89
Forced recruitment threat	9.09	12.34	-3.25
Disappearance of family member	2.27	2.98	-0.71
Armed combat	1.52	1.28	0.24
Physical mistreat	0	0.43	-0.43
Death of family member	4.55	4.26	0.29
Deny or restrict access to survival goods	0.76	0	0.76
Theft of goods by armed actor	0	0.43	-0.43
Kidnap of family member	0	0.43	-0.43
Sexual violence	0	1.28	-1.28
N/A	0.76	0.43	0.33

**Table A.3:** Differences in region of origin: Consent vs. no consent.

Region	Consent	No Consent	Difference
Amazonas	0	0.57	-0.57
Antioquia	8.99	7.18	1.81
Arauca	2.72	1.72	1
Atlántico	0.54	1.15	-0.6
Bogotá	0.27	0.57	-0.3
Bolívar	3	1.72	1.27
Boyacá	0.82	1.72	-0.91
Caldas	0.82	1.44	-0.62
Caquetá	4.9	6.03	-1.13
Casanare	0.82	0.29	0.53
Cauca	4.09	2.3	1.79
Cesar	2.18	1.72	0.46
Chocó	3	3.45	-0.45
Cundinamarca	2.45	4.02	-1.57
Córdoba	3.27	4.02	-0.75
Guajira	0.82	0.86	-0.04
Guaviare	1.63	2.59	-0.95
Huila	10.35	10.92	-0.57
Magdalena	1.36	3.45	-2.09
Meta	5.99	4.31	1.68
Nariño	5.72	7.18	-1.46
Norte de Santander	1.36	1.72	-0.36
Putumayo	2.45	2.59	-0.13
Quindío	0.27	0	0.27
Santander	4.36	3.16	1.2
Sucre	0.82	2.01	-1.19
Tolima	19.62	14.66	4.96
Valle del Cauca	6.54	8.05	-1.51
Vaupés	0.27	0	0.27
Vichada	0.27	0.57	-0.3
No Answer	0.27	0	0.27

**Table A.4:** Differences in region of origin: Interviewed vs. not found.

Region	Interviewed	Not found	Difference
Antioquia	9.85	8.51	1.34
Arauca	3.79	2.13	1.66
Atlántico	0.76	0.43	0.33
Bogotá	0.76	0	0.76
Bolívar	0.76	4.26	-3.5
Boyacá	0.76	0.85	-0.09
Caldas	0.76	0.85	-0.09
Caquetá	3.79	5.53	-1.74
Casanare	0	1.28	-1.28
Cauca	6.82	2.55	4.26
Cesar	0.76	2.98	-2.22
Chocó	0.76	4.26	-3.5
Cundinamarca	4.55	1.28	3.27
Córdoba	3.03	3.4	-0.37
Guajira	0	1.28	-1.28
Guaviare	1.52	1.7	-0.19
Huila	13.64	8.51	5.13
Magdalena	0.76	1.7	-0.94
Meta	5.3	6.38	-1.08
Nariño	6.06	5.53	0.53
Norte de Santander	1.52	1.28	0.24
Putumayo	1.52	2.98	-1.46
Quindío	0.76	0	0.76
Santander	5.3	3.83	1.47
Sucre	0.76	0.85	-0.09
Tolima	21.21	18.72	2.49
Valle del Cauca	3.79	8.09	-4.3
Vaupés	0	0.43	-0.43
Vichada	0	0.43	-0.43
No Answer	0.76	0	0.76

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