When Digital Nudges Nag: Evidence on Program Take-up from a WhatsApp Intervention*

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Abstract

We study a randomized controlled trial in which undocumented Venezuelan migrants in Colombia were assigned to receive informational videos via WhatsApp encouraging registration in a regularization program. The intervention backfired. Receiving a video reduced take-up by 8 percentage points, a 15 percent decline relative to a control mean of 54 percent, with similar effects across video types. We document two channels through which this effect operates. Among the 72 percent of treated individuals who watched the video, informational exposure reduced take-up by 9.5 percentage points, consistent with an increase in perceived complexity. Among individuals who received but did not engage with the message, assignment also reduced take-up, consistent with friction from unsolicited contact. Stratifying individuals by their predicted likelihood of registration, we find that negative effects are concentrated among those at the margin of the take-up decision, while those with low or high baseline propensities are largely unaffected. These findings suggest that the line between a helpful nudge and a counterproductive nag is thin and that providing more digital information can reduce program participation when it raises perceived costs or triggers disengagement among vulnerable populations.

Keywords: Migrants, Regularization, Program Take-up
JEL Classification: D72, F02, F22, O15, R23

*The order in which the authors' names appear has been randomized using the AEA Author Randomization Tool (#lzrenF4fqQRW), denoted by ⓡ. We thank participants of the World Bank Half Baked Seminar, World Bank Human Development seminar, Economics seminar at the University of Virginia, Hilton Foundation seminar, and ESOC conference at Universidad del Rosario, as well as Erin Kelley, Aart Kraay, Carlos Scartascini, David McKenzie, and Berk Özler for useful comments. We thank IPA Colombia, our partners in this study, particularly Kyle Holloway, Alejandra Rivera, Laura Vargas, Nicole Lesmes, Jacobo Morales, and Ana María Rojas for their support in data collection and intervention implementation. We are also thankful to Ana María Ibáñez, who contributed in the early stages of this project, and to Kike Yra Fonton and Andrés Barinas, who provided excellent research support for the project. This project was approved by IPA IRB protocol 3911. It was also preregistered at the AEA RCT Registry (AEARCTR-0008672). We thank the Hilton Foundation and the RSB Funds of the World Bank for providing financial support for this project. The authors have no conflicts of interest to report. The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors. They do not necessarily represent the views of the World Bank and its affiliated organizations, or those of the Executive Directors of the World Bank or the governments they represent.

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I INTRODUCTION

Individuals from vulnerable populations often display low enrollment in public services that could substantially enhance their welfare (Currie 2006). Access to public programs is rarely automatic, as applicants must navigate stringent eligibility criteria and administrative requirements that impose disproportionate informational and compliance costs on those least equipped to bear them. Social media platforms such as WhatsApp offer a potentially cost-effective channel to reduce these frictions by disseminating timely, targeted information about available programs, and a growing body of development practice has embraced this approach. Yet, whether such information nudges actually raise take-up among vulnerable populations remains an open empirical question, and the conditions under which they may backfire are poorly understood.

Informational videos sent via mobile phones can reach large numbers of people at low cost, including those who lack access to in-person outreach or harbor distrust toward government institutions. This feature makes WhatsApp a particularly attractive delivery channel for public programs targeting highly mobile and underserved populations. We study how informational videos distributed via WhatsApp affect take-up of a regularization program among Venezuelan migrants in Colombia, a population that is both highly mobile and difficult to reach through conventional outreach channels.

This study focuses on Colombia's Estatuto Temporal de Proteccion para Migrantes Venezolanos (ETPV), a 10-year regularization program for undocumented Venezuelan migrants (herein "migrants"). The ETPV grants legal status and benefits to migrants who arrived in Colombia before January 2021, issuing a temporary protection permit (PPT) that serves as an identification document and provides access to healthcare, financial services, and a formal work permit. Previous studies of an earlier version of this program documented large improvements in income and consumption among beneficiaries, but

also persistently low take-up rates (Ibáñez, Moya, Ortega, Rozo and Urbina 2025).

To understand the sources of low take-up, our research team conducted a qualitative study prior to the PPT registration period, which ran from May 2021 to June 2022.[1] We identified three barriers that recur throughout the literature on public program enrollment. First, many migrants were unaware of the program (Finkelstein and Notowidigdo 2019; Bhargava and Manoli 2015). Second, distrust of government institutions created reluctance to engage with the regularization process (Watson 2014). Third, limited knowledge of registration procedures, combined with psychological aversion to program complexity and administrative hassle (Bhargava and Manoli 2015; Kleven and Kopczuk 2011), deterred enrollment even among migrants who were aware of and interested in the program. These findings motivated the design of three informational videos, each targeting one of the identified barriers to enrollment.

We employed a randomized controlled trial to investigate the impact of three video treatments delivered via WhatsApp on PPT take-up rates among 1,375 undocumented Venezuelan migrants. Participants were randomly assigned to one of three treatment arms or a control group, with each video targeting one of the three barriers identified in our qualitative study. In designing the videos, we drew on the EAST framework developed by the Behavioral Insights Team (BIT 2014), which guided our choices about message framing, content sequencing, and delivery timing.

Video 1, which we call the *awareness* video, aimed to increase program awareness. It described the principal benefits of the PPT, including its three-step application process, its accessibility, and its zero cost, and was narrated by an actor presenting as a Colombian official. Video 2, the *trust* video, presented the same information but was narrated by a Venezuelan migrant who had already registered. The narrator offered a personal ac-

[1]The qualitative study consisted of 42 in-depth interviews with Venezuelan migrants residing in Colombian cities with a high density of migrants. Roughly half of the migrants were undocumented and the other half were documented.

count of her experience, emphasizing the program's legitimacy and safety. Video 3, the *step-by-step* video, was narrated by the same Venezuelan migrant and provided detailed guidance on registration procedures, aiming to reduce confusion about requirements and next steps. It was also the longest of the three videos. None of the treatments offered an interactive component; each video directed participants to public offices for additional information and assistance. The awareness and trust videos were each approximately 3.5 minutes long, while the step-by-step video ran 5 minutes and 28 seconds.

We recruited undocumented Venezuelan migrants in person in the departments of Magdalena and Atlántico on the Caribbean Coast of Colombia, regions with high concentrations of highly vulnerable migrants and low regularization rates according to the 2018 Colombian population census. To ensure representativeness, we sampled from the largest urban center, Santa Marta, and surrounding areas, advertising the experiment in locations frequented by Venezuelan migrants and partnering with local community leaders to establish registration points in marginalized communities. Eligible participants were Venezuelan-born, aged 18 or older, had internet access and WhatsApp, and had not yet scheduled a biometric data appointment, the final administrative step before the PPT is issued. We focused on this threshold because it identifies individuals who had not yet committed to completing the registration process. We verified that our sample captured particularly vulnerable migrants by comparing their baseline characteristics to those from comparable national surveys.

Treated participants received their designated video via WhatsApp on four occasions, beginning two months after initial recruitment and repeated at one-week intervals. Each transmission included the video and a short digital survey. Control group participants received only the survey. Participants who reported having scheduled their biometric data appointment were not contacted further. We evaluate treatment effects on three outcomes reported by participants: intention to register, initiation of the registration process,

and requesting or attending the biometric data appointment, which we use as our primary measure of PPT take-up.[2] The experiment had internal validity with treatment and control groups showing balanced across nearly all baseline variables, with statistically significant differences appearing in only 2 of 56 balance tests. Moreover, attrition across waves was not systematically related to treatment assignment.

Surprisingly, we find negative effects of the video intervention on all three outcomes. Receiving a video reduced the intention to register by 12.2 percentage points, the probability of initiating the registration process by 7.7 percentage points, and the likelihood of requesting the PPT by 8 percentage points, representing a reduction of roughly 15 percent relative to the control group mean. Decomposing the ITT following Abadie (2002), we find that both effects are negative for both compliers and defiers. Compliers are individuals who watched the video when assigned to treatment and would not have watched it otherwise; among them, engagement with the informational content discouraged registration. Defiers are individuals who did not watch the video despite being assigned to treatment; among them, assignment itself generated a negative response regardless of engagement with the content.

To examine who is most affected by the intervention, we implement an endogenous stratification approach following Chernozhukov et al. (2018), which characterizes how treatment effects vary across individuals with different baseline probabilities of registering, while avoiding the endogeneity concerns that arise when conditioning on post-treatment variables such as video engagement. The results show that negative treatment effects are not uniform across the population and are largest among individuals with intermediate predicted baseline likelihoods of registering voluntarily. This is inconsistent with a digital exclusion story, in which effects would be concentrated among the least likely

[2]IRB restrictions prevented us from collecting identification information that would have allowed us to match participants to administrative records. When we attempted such matching using names, homonyms were sufficiently common to preclude reliable linkage for the majority of the sample. Our outcome measures therefore rely on self-reported registration status.

to register. Instead, it suggests that the intervention was most disruptive for individuals at the margin of the participation decision, consistent with a nagging effect among those whose registration decision was most sensitive to external contact.

We consider two channels through which informational nudges reduced program take-up. The first is information backfire among individuals who engaged with the video content. Separate ITT estimates by video type show that more detailed content generates larger negative effects, consistent with the interpretation that procedural information increased perceived complexity and hassle costs. This interpretation is supported by the LATE estimates, which show that among individuals whose engagement was affected by treatment assignment, watching the video reduced take-up.

The second channel is contact frictions among non-engagers. The ITT decomposition implies an average effect of 4.3 percentage points among the 28.2 percent of treated individuals who did not watch the video. Follow-up qualitative interviews with 30 treated participants provide suggestive evidence on the nature of these frictions. Non-engagers reported frustration with repeated contact attempts, difficulties accessing video content, and technological barriers including limited internet access and unfamiliarity with digital tools. While this qualitative evidence is not causal, it is consistent with a contact friction mechanism operating independently of video content.

Overall, our findings challenge a common assumption suggesting that well-designed informational nudges are either effective or neutral. Our results show that nudges can generate meaningful negative effects even when carefully designed with behavioral principles in mind, because individuals react negatively to both the content and the mode of delivery. The costs of unsolicited contact can outweigh the benefits of information, particularly for those at the margin of the participation decision.

Our work contributes to the large body of work on public program take-up and its determinants. Prior research has identified information asymmetries, high learning costs, and

administrative complexity as the principal barriers to enrollment (Currie 2006, Kleven and Kopczuk 2011). A growing literature examines whether informational interventions can reduce these frictions, with mixed results. While some studies find that providing clear program information increases take-up (Daponte, Sanders and Taylor 1999, Manoli and Turner 2014,Bhargava and Manoli 2015), others document limited or negligible effects, particularly in settings characterized by complexity or low attention (Bettinger et al. 2012, Hainmueller et al. 2018). We extend this literature by showing that informational interventions can generate meaningful negative effects even when carefully designed with behavioral principles in mind. Our results indicate that the sign of the treatment effect depends critically on baseline motivation and engagement, dimensions that prior work has not examined jointly.

We also contribute to the literature on behavioral nudges and their unintended consequences. The nudging literature has largely focused on whether nudges are effective or neutral, with less attention paid to the conditions under which they backfire (DellaVigna and Linos 2022). We provide evidence of a nagging effect in which repeated unsolicited contact reduces take-up among individuals already inclined to act voluntarily. This mechanism is distinct from inattention or information avoidance and operates through two channels: increased perceived complexity among engagers and contact frictions among non-engagers. Our findings suggest that the line between a helpful nudge and a counterproductive nag can be extremely thin for the most vulnerable populations.

Finally, we contribute to a nascent literature on digital outreach to vulnerable and hard-to-reach populations. An emerging body of evidence suggests that mobile technologies can reduce information asymmetries and improve welfare, but that effects depend critically on individuals' technological literacy and connectivity (Aker and Mbiti 2010, Hjort and Poulsen 2019). Social media platforms can be cost-effective data collection and outreach tools when recipients have sufficient digital access (Beam 2023), but their reach among

the most vulnerable populations remains limited. We add to this evidence by showing that WhatsApp-based interventions targeting undocumented migrants can backfire, inducing frustration and disengagement rather than participation. Our setting, combining government distrust, administrative complexity, and limited digital literacy, represents an important and understudied context for evaluating the reach and limits of digital behavioral interventions.

II REGULARIZATION PROGRAMS IN COLOMBIA

II.A The PEP Program

The Venezuelan exodus is one of the most severe forced migration crises in recent history, with approximately 8 million migrants displaced abroad as of 2025. Colombia has been the principal destination country and has responded with a series of regularization programs designed to formalize migrants' legal status. The first major initiative, the Permiso Especial de Permanencia (PEP), launched in 2018 and enabled nearly half a million irregular Venezuelan migrants to regularize their documents, obtain work permits, access safety nets including health and education services, and validate their educational credentials. Benefits were granted for a period of two years, with a pathway to permanent residency. The program generated substantial welfare gains for participants, with positive effects on labor income, access to public programs, bank account ownership, and health outcomes (Ibáñez, Moya, Ortega, Rozo and Urbina 2025,Urbina et al. 2023).[3]

Despite these benefits, only approximately 60 percent of eligible migrants registered and received the PEP document. Ibáñez et al. (2020) document three principal barriers to registration: limited awareness of eligibility requirements and registration procedures, distrust of the Colombian government, and administrative bottlenecks in the application process. These patterns of incomplete take-up and the barriers underlying them directly

[3]Research on PEP's effects on hosting communities documents negligible impacts on labor market outcomes (Bahar, Ibáñez and Rozo 2021); an overall reduction in crime (Ibáñez, Rozo, Bahar and Urbina 2025); the creation of new small household enterprises (Bahar, Cowgill and Guzman 2023); and no observable changes in host voting behaviors for Colombians (Rozo, Quintana and Urbina 2025).

motivated the design of the informational intervention we study in this paper.

II.B The ETPV Program

Before PEP expired in 2020, the Colombian government launched a substantially larger regularization initiative, the Estatuto Temporal de Protección para Migrantes Venezolanos (ETPV), offering a ten-year regularization program to Venezuelan migrants who had arrived in Colombia prior to January 2021 (see Figure A.1 for a timeline). The application process involved several sequential steps. Migrants first registered in an online census, the Registro Único de Migrantes Venezolanos (RUMV), and uploaded supporting documents establishing proof of arrival in Colombia before January 31, 2021, along with a Venezuelan identity document and a photograph. They then scheduled an in-person biometric appointment, after which a temporary protection permit, the Permiso por Protección Temporal (PPT), was issued virtually and delivered physically three months later. The registration window ran from May 2021 to June 2022. The complete process is described in Figure A.2.

The PPT functions as both an identification and regularization document, granting holders legal status and a work permit, access to public health services including Covid-19 vaccinations, the pension system, education, childcare, and the financial sector, and the ability to validate Venezuelan educational credentials. It also permits unrestricted international travel and serves as proof of residence for purposes of applying for a permanent residency visa. By October 2022, nearly 2.5 million Venezuelan migrants had completed the RUMV census, representing one of the largest regularization efforts in Latin American history (see Figure A.3 for the geographical distribution of registrants).

III THE INTERVENTION

III.A Rationale

Previous work shows results from our qualitative investigation into why Venezuelan migrants did not register for the PEP program.[4] We identified three key barriers that impeded participation, namely: (i) inadequate awareness of the program; (ii) lack of trust in the government due to possible legal complications during the regularization process; and (iii) confusion and insufficient information regarding registration procedures as well as reluctance related to the complex and difficult process. Our WhatsApp intervention targeted these barriers and tried to increase participation in the next regularization program (the PPT) by providing information through cost-effective platforms that could make this intervention scalable.

III.B Design

We randomly assigned a total of 1,375 eligible individuals into four groups of equal size that consisted of three treatment arms and one control group.[5] The research team interviewed individuals selected for the intervention in person at registration to collect basic sociodemographic characteristics. The three treatment arms involved the dissemination

[4]The study included 42 in-depth interviews with Venezuelan migrants living in cities with a high population of Venezuelans. All interviewees had resided in Colombia since 2018, and were: (i) beneficiaries of the Special Permit of Permanence granted by the Administrative Registry of Venezuelan Migrants (PEP-RAMV) plus (ii) migrants who could have benefited from this permit but had not registered in RAMV or (iii) were unable to access PEP despite having participated in RAMV. Participants comprised generally young people (mostly women) with difficulties generating income despite being of working age, who lived in large households (25 women and 17 men took part in the study). The main finding was that the desire to obtain official documentation was a primary motivation for Venezuelan migration to Colombia. Nevertheless, the process was hampered chiefly due to lack of awareness of the program. Migrant networks were essential to publicize both registration in RAMV and the subsequent PEP-RAMV process. Interviews revealed that without physical or virtual contact through social networks with church and migrant group leaders, migrants would have missed the opportunity to regularize their status. There was also fear sparked by rumors that RAMV would be used by the Colombian government to deport undocumented migrants. Finally, bottlenecks in the registration process occurred as a result of: (i) lack of access to information and weak social networks; (ii) lack of money for transportation to registration points; (iii) the cost of losing part of the working day to an activity that did not generate income; and (iv) lack of incentives to register.

[5]Our initial plan was to recruit 4,180 eligible Venezuelan migrants in Colombia. However, in the field, we could only identify and include 1,375 individuals who were undocumented and wanted to participate in the experiment. We revised our pre-analysis plan to reflect the new sample size and estimation strategy.

of a different video through WhatsApp, each addressing a specific barrier to registration for the PPT such as lack of program awareness, distrust of the government, and details regarding registration that were intended to reduce procedural difficulties.

We based the intervention design on the EAST methodology developed by the Behavioral Insights Team (BIT 2014), which emphasizes the principles of making information easy to understand, attractive, social, and timely. We designed the videos to simplify information about benefits, eligibility criteria, and the registration process. The use of graphic design, pop-ups, and images made the videos more attractive, and a financial incentive encouraged viewership.[6] To incorporate the "social" principle, the videos informed migrants that others in their community had successfully registered for the regularization program.[7] We applied the "timely" rule by strategically timing the messages to reach individuals when they were most receptive, based on insights from previous research with this population.

Video 1 featured a Colombian actor who portrayed an official and provided clear and concise information on program eligibility, costs, and the registration process (awareness video). Video 2, narrated by a Venezuelan forced migrant and mother of two children who had registered in the program, provided the same information as Video 1 but added anecdotal evidence about her experience with the program in order to build trust and empathy (trust video). Finally, Video 3, which had the same Venezuelan narrator as Video 2, offered more detail on the registration process with a step-by-step guide on how to register online (step-by-step video). All the video scripts are provided in the Appendix C and design of the intervention is illustrated in Figure 1.

[6]The value of the incentive was 10,000 Colombian pesos (COP).

[7]Social norms insights have proven successful in changing individual behaviors (Allcott 2011, Hassett, Grolleau and Ibanez 2017, Donna, Roberts and Sweeney 2007).

III.C Recruitment and eligibility

In partnership with Innovations for Poverty (IPA) Colombia, we recruited participants in Magdalena and Atlántico as they are home to many highly vulnerable migrants, according to the Colombian population census of 2018. The sample was collected to be representative of both Santa Marta, one of the largest urban centers, and rural areas that included Ciénaga, Sabana Larga, Fundación and Baranóa (Figure 1).[8]

Based on guidance from migrant organizations, public officials, and members of the community, we advertised the program in areas Venezuelan migrants were known to frequent. We mapped these areas and subsequently contacted local community leaders there to elicit support for opening registration points in marginalized communities with many undocumented migrants. Local leaders helped us build trust by offering information about the IPA and the researchers involved in the project. We made several modifications to our data collection process to increase trust and boost response rates. Three important ones were (i) the distribution of related research on regularization programs to local leaders, (ii) discussion to learn how to support Venezuelan migrants more effectively, and (iii) collaborations with Venezuelan enumerators to increase trust.

The study's eligibility criteria were carefully defined to ensure that individuals were both eligible for the program and vulnerable. Specifically, we recruited Venezuelan migrants who were of legal age (18 years or older), resident in any of the selected municipalities, undocumented, and who arrived in Colombia before January 1, 2021. Additionally, participants had to have access to a phone with WhatsApp and internet. The final sample numbered 1,375 individuals who met these criteria and agreed to participate in the study.

Table 1 presents descriptive statistics and provides important insights into the participants. The sample was predominantly composed of young people with an average age of 33.4 years. The majority of participants were female, accounting for approximately 67

[8]We did not recruit individuals in Barranquilla due to implementation costs.

percent of the sample. Furthermore, individuals in our sample also had low income, with an average monthly income of 250,000 COP, which represents 20 percent of the minimum wage in Colombia. The majority reported having access to the internet for at least four hours a day, which initially suggested that mobile-based interventions could be viable for this population. Notably, the descriptive statistics also seem to suggest that trust in the Colombian government was not particularly low. Yet, self-reported measures are probably biased and we did not have comparable values for Colombian natives that permitted us to make any strong conclusions.

We compared our sample of Venezuelan migrants with those surveyed in similar recent surveys including the 2021 Labor Force Survey (Gran Encuesta Integrada de Hogares, GEIH) and the 2020 Venezuelan Refugee Panel Survey (VenRePS) of migrants without a migratory permit. The GEIH is a comprehensive survey that regularly samples households in Colombia to collect data on the labor force and individual demographic characteristics, including those of Venezuelan migrants. The VenRePS, on the other hand, is a representative sample of documented and undocumented migrants who arrived in Colombia between January 2017 and December 2018.

Our analysis in Table A.1 indicates that, as intended, migrants in our intervention were the most vulnerable of all according to measures of income, education, unemployment, and access to health services. This aligns with our reasons for choosing Magdalena and Atlántico as locations where we could find migrants who might lack information on the regularization program or might face other challenges in registering for it.

III.D Conceptual Framework

This subsection provides a framework for interpreting how an informational intervention can reduce program take-up, depending on whether individuals engage with the content or not.

Setup. Let $Z_i \in \{0, 1\}$ denote assignment to treatment (receiving informational videos via WhatsApp), $T_i \in \{0, 1\}$ denote engagement with the informational content (e.g., watching the video and obtaining accurate program information), and $Y_i \in \{0, 1\}$ denote take-up (e.g., initiating or completing registration). Our baseline estimates capture the intention-to-treat (ITT) effect:

$$ITT = \mathbb{E}[Y_i \mid Z_i = 1] - \mathbb{E}[Y_i \mid Z_i = 0].$$

Heterogeneous engagement and behavioral responses. We allow treatment assignment to affect both engagement and take-up, but we do not assume that assignment always increases engagement. Some individuals may actually be less likely to engage with informational content when assigned to treatment than they would have been otherwise, for instance because unsolicited contact generates avoidance or disengagement. Individuals who respond to treatment in the opposite direction from its intent are called defiers, and their presence is central to understanding why the intervention reduced take-up on average.

Two channels. We consider two channels through which the intervention may reduce take-up. The first operates among individuals who engage with the informational content. For these individuals, exposure to the videos may update beliefs about the complexity, time requirements, or administrative burden of the registration process. When information increases perceived costs in this way, the intervention reduces take-up among those who engaged with it most directly. The second channel operates among individuals who receive but do not engage with the content. For this group, the mere receipt of unsolicited messages may generate friction, induce annoyance, reduce trust, or trigger avoidance, lowering take-up even without any information acquisition.

Link to the ITT. Individuals in the sample differ in how their engagement responds to treatment assignment. Compliers engage with the content when assigned to treatment

but would not have otherwise. Defiers do the opposite, disengaging precisely because they were contacted. When both types are present, the overall ITT reflects a combination of their responses, and a negative average effect can arise even when both compliers and defiers experience negative effects. To characterize these patterns, we complement the analysis with bounds on the share and effects of defier-type responses following Kowalski (2023) and an endogenous stratification approach that allows treatment effects to vary with baseline take-up propensity (Abadie, 2002).

Testable predictions. The framework yields two core predictions. If informational content increases perceived costs, treatment effects should be negative among engaged individuals. If unsolicited contact generates frictions, the negative ITT should be driven by individuals who receive but do not engage with the content.

IV DATA

After in-person recruitment (which included a short sociodemographic baseline survey) and randomization, participants were contacted via WhatsApp up to four times to deliver the intervention and collect outcome data. Individuals assigned to treatment received a video link followed by a short survey approximately one hour later, while the control group received only the survey. Participants were recontacted only if they had not responded in previous attempts and had not yet requested the biometric data appointment, which we use as the primary proxy for program take-up.

We define take-up as requesting or attending the biometric appointment. Attendance is required to complete the process, but focusing on appointment requests allows us to capture behavioral responses targeted by the intervention. Due to administrative delays, some appointments occurred after the final contact. The timing and structure of contacts are summarized in Figure A.4.

Survey structure. The WhatsApp surveys included up to five questions corresponding to sequential stages of the registration process: (i) receiving the PPT, (ii) attending the

biometric appointment, (iii) requesting the appointment, (iv) starting the RUMV registration, and (v) intention to register. Questions were asked in reverse order, and respondents exited the survey once they reported completion of a given stage, as earlier steps could be inferred (Figure A.5).

Engagement with the intervention. A key feature of the data is substantial heterogeneity in engagement with the informational content. Table A.2 shows that approximately 15 percent of treated individuals did not watch the video at all, while engagement among those who did watch is highly dispersed, with a bimodal distribution between very low and full completion rates. Engagement also declines sharply across contact attempts. Figure A.6 shows that more than 70 percent of participants engaged with the video at the first contact, but this share decreases markedly in subsequent contacts. As a result, exposure to the informational content is concentrated at early stages of the intervention. Because assignment to treatment does not guarantee exposure to the informational content, the ITT combines responses from individuals who engage with the content and those who do not (i.e., compliers and defiers), a distinction that is central to the empirical strategy in the following section.

V Empirical Strategy

This section describes the empirical approach used to estimate the effects of the intervention and to examine how those effects vary across individuals with different levels of engagement and baseline propensities to register.

V.A Intention-to-Treat effects

We estimate the effects of the intervention on three outcomes: (i) intention to register for the PPT, (ii) initiation of the registration process (defined as starting the RUMV registration), and (iii) program take-up (defined as requesting or attending the biometric data appointment, or receiving the PPT). Outcome information corresponds to the last WhatsApp contact with the participant.

Our baseline estimates capture the intention-to-treat (ITT) effect of assignment to receive informational videos (Z_i) on take-up outcomes (Y_i). These estimates reflect the effect of being assigned to treatment regardless of whether individuals engage with the content (T_i). We estimate the following specification:

$$Y_i = \alpha + \beta \, \textit{Any Video}_i + \varepsilon_i \tag{1}$$

where Y_i denotes the outcome of interest for individual i, and $\textit{Any Video}_i$ is an indicator equal to one if the individual was assigned to receive any informational video. Standard errors are robust to heteroskedasticity. We also estimate specifications that disaggregate effects by video type to assess whether responses differ across the awareness, trust, and step-by-step videos.

V.B Heterogeneous Effects

To examine treatment effect heterogeneity, we implement an endogenous stratification approach following Abadie (2002) and Chernozhukov et al. (2018). This approach characterizes how treatment effects vary across individuals with different baseline likelihoods of registering, while avoiding the endogeneity concerns that arise when conditioning on post-treatment variables such as video engagement.

We proceed in three steps. First, we use the control group to estimate a model predicting take-up as a function of baseline covariates, implementing leave-one-out cross-validation to avoid overfitting. Second, we use the predicted values to partition the full sample into terciles based on predicted take-up probabilities. Third, we estimate equation (1) separately within each tercile.

This design choice is motivated by a specific identification concern. Conditioning on engagement with the video would introduce endogeneity because engagement is itself affected by treatment assignment. By relying exclusively on baseline characteristics that are

fixed prior to treatment assignment, endogenous stratification provides a clean characterization of how treatment effects vary across the take-up distribution and, in particular, allows us to isolate individuals at the margin of the registration decision.

V.C Decomposition of Treatment Effects

To understand where the negative average effect comes from, we decompose the ITT into contributions from two groups: individuals who watched the video and those who did not.

The key observation is that no one in the control group watched a video by construction. This means the control group consists entirely of non-watchers, which allows us to estimate the effect of watching the video directly by comparing treated individuals who watched with the full control group. Following Abadie (2002), combining this estimate with the overall ITT allows us to recover the implied contribution of non-watchers to the average effect arithmetically.

We stress that this decomposition is descriptive rather than causal. We cannot identify a causal effect for non-watchers because we do not observe what would have happened had they been induced to watch. What the decomposition does tell us is whether the negative average effect is concentrated among those who engaged with the content, those who did not, or both. This diagnostic is central to distinguishing between the two channels described in the conceptual framework.

VI INTERNAL VALIDITY

VI.A Successful balance across groups

We examine the internal validity of our experiment in Table 2, which reports balance tests for 14 baseline covariates across treatment and control groups. Of the 56 pairwise comparisons evaluated, only two are statistically significant at the 5 percent level, fewer than the 2.8 rejections expected by chance under the null hypothesis of successful ran-

domization (0.05×56). These results support the validity of random assignment and the interpretation of our estimates as causal effects of the intervention on program take-up.

VI.B Attrition analysis

Table C.1 presents regressions of attrition on baseline sociodemographic characteristics, where attrition is defined as an indicator equal to one if the individual did not respond to any of the four contact attempts. Of the 14 baseline covariates, two predict attrition at conventional levels, having a personal WhatsApp account (relative to a shared account) and being male. Variables such as age, income, and time spent working are uncorrelated with attrition, suggesting that non-response is not systematically related to socioeconomic vulnerability.

Importantly, treatment assignment itself does not predict attrition. Table C.2 reports the correlation between attrition and baseline covariates, their interactions with treatment status, and treatment status alone, for three alternative definitions of attrition. Of the 42 interactions tested, only two are statistically significant, and treatment status by itself does not correlate with attrition under any definition. Table C.3 reports attrition prevalence across these definitions. For the full sample, 22.5 percent of individuals never responded to any of the four WhatsApp contacts, and 27.6 percent responded to at least one follow-up but had not completed the PPT registration process.

Figure C.1 examines attrition by contact attempt. Nearly half of the sample did not respond at the first contact, and response rates declined with each subsequent attempt (Panel A). This pattern also holds in the subsample of 789 individuals who were contacted all four times (Panel B).

VII MAIN RESULTS

This section presents the main empirical findings. We first document the average effects of the intervention and decompose them by engagement. We then examine treatment effect heterogeneity using an endogenous stratification approach.

VII.A Average Effects and the Role of Engagement

Table 3 presents the main estimates of the intervention's effects on program take-up. The estimation sample includes 1,130 individuals who responded to at least one contact attempt. Panel A reports the first-stage relationship between assignment to receive a video and engagement with the informational content. Assignment increases the probability of watching the video by 71.8 percentage points, indicating that while a large majority of treated individuals comply with assignment, roughly 30 percent do not engage with the video content.

Panel B reports intention-to-treat estimates. Assignment to receive any video reduces take-up across all three outcomes. The probability of requesting the PPT decreases by 8 percentage points relative to a control mean of 54 percent, a 15 percent decline. Starting the registration process decreases by 7.7 percentage points relative to a control mean of 59 percent, a 13 percent decline. Intention to register decreases by 12.2 percentage points relative to a control mean of 83 percent, a 15 percent decline. These are sizable reductions, indicating that the intervention meaningfully discouraged participation at every stage of the registration process.

Panels C and D report instrumental variables estimates (i.e., LATE, Local Average Treatment Effects) that isolate the effect of actually watching the video, as opposed to merely being assigned to receive it. Panel C uses a single instrument (assignment to any video) and Panel D uses the three video-type assignments as separate instruments. The LATE estimates are stable across both specifications. Among individuals whose watching behavior was changed by assignment, viewing the video reduces the probability of requesting the PPT by 9.5 percentage points (Panel C) and 8.7 percentage points (Panel D), representing a 16 to 18 percent decline relative to the control mean. Effects on registration initiation are similar in magnitude, while effects on intention to register are larger, ranging from 13.6 to 14.4 percentage points. That these estimates are negative indicates that

the backfire effect is not driven entirely by non-watchers reacting to unsolicited contact. The video itself discouraged take-up among those who watched it.

Panel E decomposes the ITT into the contributions of two groups, following Abadie (2002). The decomposition exploits the fact that no one in the control group was sent a video, so the entire control group consists of individuals who did not watch. This makes it possible to separate the overall effect into a complier component and a defier component. Compliers, who constitute 71.8 percent of the treated sample ($\pi_C = 0.718$), are individuals who watched the video because they were assigned to receive it. Their effect on take-up is captured by the LATE reported in Panel C (τ_C). For PPT request, this effect is -9.5 percentage points. Defiers, who constitute the remaining 28.2 percent ($\pi_D = 0.282$), are individuals whose engagement was not increased, and may have been reduced, by assignment. Their implied effect (τ_D) is recovered residually from the accounting identity $ITT = \pi_C \cdot \tau_C + \pi_D \cdot \tau_D$. For PPT request, this yields an implied defier effect of -4.3 percentage points, with a similar pattern for registration initiation (-4.2 percentage points) and intention to register (-6.6 percentage points).

Two results from this decomposition are important. First, both the complier and defier effects are negative, indicating that the intervention reduced take-up through both channels described in the conceptual framework. Among compliers, exposure to the informational content discouraged registration. Among defiers, assignment itself generated a negative response even without engagement. Second, while the per-individual effect is larger among compliers, the defier channel accounts for a meaningful share of the overall ITT because defiers represent nearly 30 percent of the treated sample.

VII.B Who Is Affected? Evidence from Endogenous Stratification

To further characterize the distribution of treatment effects, Table 4 presents estimates by predicted take-up terciles using the endogenous stratification approach described earlier. For behavioral outcomes, the negative effects on PPT requests and registration initiation

are concentrated among individuals in the medium predicted take-up tercile, with an average predicted probability of 0.534 and a control mean of 56 percent for PPT request. For this group, assignment to treatment reduces the probability of requesting the PPT by 11.9 percentage points, a 21 percent decline relative to the control mean, and registration initiation by 11 percentage points. In contrast, effects for individuals in the low and high predicted take-up terciles are small and not statistically significant for these two outcomes.

The results for intention to register are statistically significant for more groups. Significant negative effects appear in both the low tercile (14 percentage points relative to a control mean of 86 percent) and the medium tercile (15.6 percentage points relative to a control mean of 84 percent), while the high tercile shows a small and insignificant effect. This suggests that the intervention discourages self-reported intentions to register across a wider range of individuals than it affects actual registration behavior.

Overall, the intervention primarily affects individuals at the margin of the take-up decision. This is inconsistent with a digital exclusion story, in which effects would be concentrated among the least likely to take up the program. Instead, it points to a reduction on take-up operating among individuals whose decisions are most responsive to changes in perceived costs and benefits.

VII.C Robustness and additional evidence

What happened at first contact? Appendix Table D.1 reports estimates using only responses from the first contact attempt. Negative effects emerge immediately. Assignment to receive any video reduces the probability of requesting the PPT by 9.4 percentage points relative to a control mean of 27.4 percent, a 34 percent decline. Starting the registration process decreases by 10.7 percentage points and intention to register by 17.8 percentage points. These magnitudes are comparable to, and in some cases larger than, the baseline ITT estimates in Table 3, both in absolute terms and as a share of the respective

control means. This indicates that the negative effects occur at initial exposure and are not an induced by repeated contact.

VIII EXPLORING POTENTIAL CHANNELS FOR THE TAKE-UP REDUCTIONS

This section examines further why the intervention reduces take-up. Guided by the conceptual framework, we consider two channels: (i) information backfire among individuals who engage with the informational content, and (ii) contact frictions among individuals who do not engage.

VIII.A Information Backfire Among Engagers

More detailed content generates larger negative effects. Appendix Table D.2 disaggregates the reduced-form effects by video type. All three videos reduce take-up, but the magnitudes vary systematically with content complexity. The step-by-step video, which provides the most detailed procedural guidance, generates the largest effects across all outcomes, reducing PPT requests by 11.7 percentage points, registration initiation by 10.8 percentage points, and intention to register by 16.8 percentage points. The awareness and trust videos produce smaller and in some cases insignificant effects on behavioral outcomes, though both significantly reduce intention to register. This result across video types is consistent with the interpretation that more detailed procedural information increases perceived complexity or hassle costs, amplifying the backfire effect among those who engage with the content.

These findings are consistent with the LATE estimates reported in Table 3, which show that, among individuals whose engagement is affected by treatment assignment, watching the video reduces take-up. Together, the evidence suggests that exposure to detailed information may increase perceived complexity or hassle costs, leading individuals to opt out of the registration process.

VIII.B Contact Frictions Among Non-Engagers

Non-engagers drive large negative effects. Appendix Table E.1 separates treatment effects by whether the individual watched the video. Among treated individuals who did not watch (N = 427), assignment reduces the probability of requesting the PPT by 25.7 percentage points, starting registration by 26.5 percentage points, and intention to register by 34.2 percentage points, all relative to the same control means. Among those who watched (N = 1,002), effects on behavioral outcomes are small and statistically insignificant, with a reduction of 4.8 percentage points for PPT request and 4.3 percentage points for registration initiation. These patterns are descriptive rather than causal, as individuals who chose not to watch the video when assigned to treatment are not a random subset of the treated sample, and the comparison between watchers and non-watchers reflects selection into engagement as much as the effect of content exposure. Nonetheless, the concentration of negative effects among non-engagers is consistent with the contact friction channel, in which unsolicited outreach generates avoidance or disengagement before any informational content is encountered. The decomposition in Panel E of Table **??** provides a cleaner quantitative bound on this channel, implying an average effect of -4.3 percentage points among the 28.2 percent of treated individuals who did not engage. Overall, the evidence points to contact frictions among non-engagers as a quantitatively important driver of the overall take-up reduction.

What did non-engagers say? We carry out approximately 30 qualitative interviews with participants who were assigned to treatment to inquire about the reasons behind the negative effects. The results suggest that non-engagers report frustration associated with repeated contact attempts, difficulties accessing video content, and technological barriers such as limited internet access or lack of familiarity with digital tools. Some participants also report that receiving messages without engaging with the content leads to confusion or disengagement. While this qualitative evidence is not causal, it provides suggestive

support for a contact friction mechanism among non-engagers.

Overall, the evidence supports two channels through which the intervention reduces take-up. Among engagers, exposure to detailed procedural information reduces take-up, though this channel is smaller in magnitude. Among non-engagers, who account for the larger share of the overall negative effect, assignment itself introduces frictions that discourage participation. Both channels operate most strongly among individuals at the margin of the participation decision, as documented in the endogenous stratification results. These findings indicate that informational interventions can fail not because individuals ignore information, but because they react negatively to both the content and the mode of delivery.

IX DISCUSSION

Providing informational videos via WhatsApp to undocumented Venezuelan migrants reduced program take-up by 8 percentage points, a 15 percent decline relative to the control mean. This result is inconsistent with the assumption that informational interventions are at worst neutral. The negative effect reflects two channels operating across different subgroups. Among individuals who watched the video, exposure to detailed procedural content increased perceived complexity, discouraging registration. Among those who received but did not engage with the message, unsolicited contact generated friction that similarly reduced take-up. The concentration of effects among individuals at intermediate levels of predicted take-up, combined with the immediate emergence of negative responses at first contact, indicates that information backfire rather than contact fatigue is the primary driver of the overall effect.

These findings carry direct implications for the design of digital outreach programs targeting vulnerable populations. The most important is that expanding informational content does not reliably increase participation and may actively reduce it when the content raises perceived complexity. Programs seeking to increase take-up in settings involving

complex administrative procedures should prioritize simplifying and shortening informational content over expanding its scope. A related implication concerns targeting. Individuals at the margin of the participation decision are the most sensitive to changes in perceived costs, which means that interventions increasing perceived complexity may disproportionately reduce participation among those who are closest to acting voluntarily. Finally, digital delivery through platforms such as WhatsApp introduces additional constraints beyond information design, including technological literacy and user experience, that can generate disengagement even before informational content is encountered.

More broadly, these results challenge the assumption that accurate and accessible information is at worst neutral in settings targeting vulnerable populations. In contexts involving complex administrative decisions, informational interventions can reduce participation by raising perceived costs among individuals who are already close to acting voluntarily. The barriers to take-up in such settings are therefore not purely informational, and interventions targeting those barriers must account for how information interacts with perceived complexity and individual heterogeneity in baseline motivation. Digital outreach at scale may be most effective when it reduces procedural burdens rather than expanding informational content.

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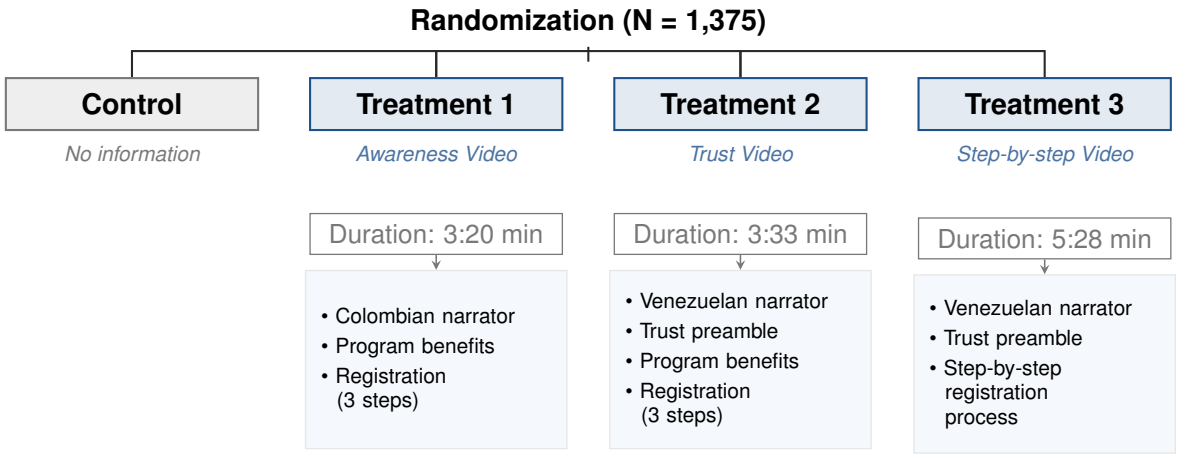
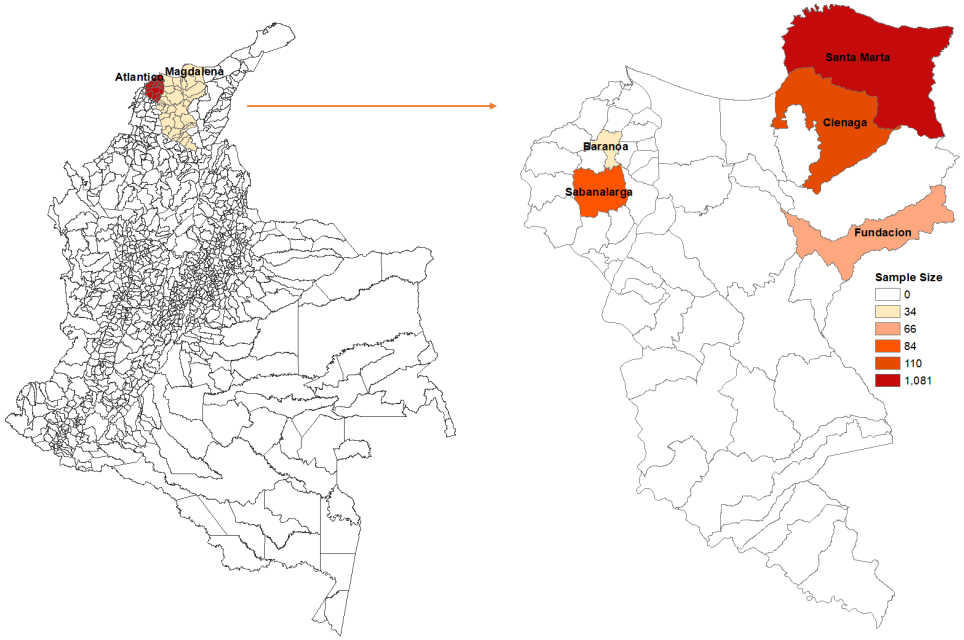
X Figures and Tables

Figure 1. The Intervention



Panel A. Intervention Design



Panel B. Geographic Location

Notes: The map on the left depicts the departments where the experiment took place, and the map on the right displays the cities and the sample size of the intervention.

Table 1. Sample Characterization

	Average	SD	Min	Max	N
Age	33.44	11.29	18.00	75.00	1,375
Male [=1]	0.33	0.47	0.00	1.00	1,375
Ed. Level: Primary or Less [=1]	0.19	0.39	0.00	1.00	1,375
Ed. Level: General or diversified school [=1]	0.59	0.49	0.00	1.00	1,375
Number of household members	4.68	1.74	0.00	14.00	1,375
Personal Income (COP)	249,472	217,353	0.00	1,700,000	1,375
Activity spent the most time: Working [=1]	0.42	0.49	0.00	1.00	1,375
Internet Access more than 4 hours per day [=1]	0.67	0.47	0.00	1.00	1,375
Trust in Colombian Government (1-5 scale)	4.27	1.02	1.00	5.00	1,375
Personal use of WhatsApp [=1]	0.77	0.42	0.00	1.00	1,375
Facebook or Instagram account [=1]	0.53	0.50	0.00	1.00	1,375
Twitter account [=1]	0.12	0.32	0.00	1.00	1,375
E-mail account [=1]	0.17	0.38	0.00	1.00	1,375
Social desirability index (1-4 scale)	2.28	1.40	0.00	4.00	1,375

Notes: Definition dependent variables: (i) Trust in Colombian Government is the answer to the question "Do you trust the Colombian Government?" on a five-point scale from 1-strongly disagree to 5-strongly agree; (ii) social desirability index is constructed using four questions from the Marlowe-Crowne social desirability scale (see Crowne and Marlowe 1960 for details). The four questions are: "It is sometimes hard for me to go on with my work if I am not encouraged (false corresponds to higher social desirability)"; "There have been times when I was quite jealous of the good fortune of others (false corresponds to higher social desirability)"; "I am always willing to admit when I make a mistake (true corresponds to higher social desirability)"; and "I am always courteous, even to people who are disagreeable (true is associated with higher social desirability)." Each statement gets a score of zero or one (assigned to higher social desirability answers). The total level of social desirability bias is calculated by summing the scores of all questions. Higher values indicate more social desirability bias.

Table 2. Covariate Balance Across Treatment Arms

	Control	Information Video	Trust Video	Step-by-Step Video	Any Video	P-value			
	(1)	(2)	(3)	(4)	(5)	(1)-(2)	(1)-(3)	(1)-(4)	(1)-(5)
Age	33.130	32.607	32.797	35.251	33.551	0.533	0.691	0.017	0.548
	(11.151)	(10.847)	(10.840)	(12.113)	(11.335)				
Male [=1]	0.305	0.334	0.313	0.348	0.332	0.418	0.830	0.235	0.367
	(0.461)	(0.472)	(0.464)	(0.477)	(0.471)				
Ed. Level: Primary or Less [=1]	0.210	0.199	0.165	0.187	0.184	0.722	0.129	0.445	0.277
	(0.408)	(0.400)	(0.372)	(0.391)	(0.388)				
Ed. Level: General or diversified school [=1]	0.625	0.560	0.620	0.535	0.572	0.082	0.891	0.016	0.081
	(0.485)	(0.497)	(0.486)	(0.499)	(0.495)				
Number of household members	4.593	4.613	4.751	4.770	4.711	0.880	0.225	0.193	0.273
	(1.733)	(1.708)	(1.677)	(1.822)	(1.737)				
Personal Income (Sin*)	9.886	9.583	9.413	9.856	9.616	0.429	0.217	0.935	0.391
	(4.841)	(5.232)	(5.234)	(4.958)	(5.142)				
Activity spent the most time: Working [=1]	0.452	0.460	0.461	0.512	0.478	0.834	0.824	0.120	0.417
	(0.498)	(0.499)	(0.499)	(0.501)	(0.500)				
Trust in Colombian Government (SD)	0.067	0.003	-0.059	-0.012	-0.023	0.380	0.082	0.281	0.139
	(0.912)	(0.989)	(0.994)	(1.008)	(0.997)				
Internet Access more than 4 hours per day [=1]	0.277	0.217	0.261	0.225	0.234	0.070	0.640	0.119	0.114
	(0.448)	(0.413)	(0.440)	(0.418)	(0.424)				
Personal use of Whatsapp [=1]	0.795	0.754	0.774	0.740	0.756	0.191	0.493	0.084	0.133
	(0.404)	(0.432)	(0.419)	(0.439)	(0.430)				
Facebook or Instagram account [=1]	0.516	0.537	0.557	0.523	0.539	0.585	0.284	0.843	0.457
	(0.500)	(0.499)	(0.498)	(0.500)	(0.499)				
Twitter account [=1]	0.110	0.097	0.101	0.146	0.115	0.584	0.730	0.150	0.789
	(0.313)	(0.296)	(0.302)	(0.354)	(0.319)				
E-mail account [=1]	0.167	0.167	0.159	0.213	0.180	1.00	0.784	0.122	0.589
	(0.374)	(0.374)	(0.367)	(0.410)	(0.384)				
Social desirability index (SD)	0.009	0.017	0.045	-0.072	-0.003	0.917	0.631	0.302	0.848
	(1.016)	(0.979)	(0.974)	(1.030)	(0.995)				
Observations	347	341	345	342	1,028	688	692	689	1,375

Notes: Columns (1)–(5) present the mean and standard deviation for the control, the three treatments, and any of the treatment samples. Columns (6)–(9) depict the p-value of the t-test. Definition-dependent variables: (i) Personal income was transformed using the inverse hyperbolic sine transformation (see Burbidge, Magee and Robb 1988 and MacKinnon and Magee 1990 for details). (ii) Trust in Colombian Government is the standardized answer to the question "Do you trust the Colombian Government?" on a five-point scale from 1-strongly disagree to 5-strongly agree. (iii) Social Desirability Index is constructed using four questions from the Marlowe-Crowne social desirability scale (see Crowne and Marlowe 1960 for details).

Table 3. Assignment, Engagement, and Take-Up: IV Estimates and Defier Decomposition

Panel A. First Stage. Dependent variable: Watched Video (T)	
	(1)
Assignment to Any Video (Z)	0.718^{***}
	(0.014)
R^2	0.391
Observations	1,375

	PPT Request (1)	Start Registration (2)	Intend to Register (3)
Panel B. Reduced Form (ITT): Effect of Assignment (Z) on Outcomes			
Assignment to Any Video (Z)	-0.080^{**}	-0.077^{**}	-0.122^{***}
	(0.034)	(0.033)	(0.027)
Panel C. LATE (2SLS): Single-Instrument Specification			
Watched Video (T)	-0.095^{**}	-0.092^{**}	-0.144^{***}
	(0.040)	(0.040)	(0.032)
Control mean	0.538	0.585	0.826
Panel D. LATE (2SLS): Multi-Instrument Specification			
Watched Video (T)	-0.087^{**}	-0.085^{**}	-0.136^{***}
	(0.040)	(0.039)	(0.032)
Control mean	0.538	0.585	0.826
Panel E. Decomposition of ITT: Complier and Defier Effects			
Complier share (π_C)	0.718	0.718	0.718
Defier share (π_D)	0.282	0.282	0.282
ITT	-0.080	-0.077	-0.122
Complier effect (τ_C)	-0.095	-0.092	-0.144
Defier effect (τ_D)	-0.043	-0.042	-0.066
Observations (Panels B–E)	1,130	1,130	1,130

Notes: The table reports estimates from an instrumental variables framework examining the effect of informational videos on program take-up. Panel A reports the first-stage relationship between assignment to receive any video (Z) and video watching (T). Panel B reports reduced-form (ITT) estimates of assignment on outcomes. Panel C reports two-stage least squares (2SLS) estimates using assignment as an instrument for video watching. Panel D reports 2SLS estimates using multiple instruments corresponding to the three video types. Panel E presents a decomposition of the ITT following Angrist, Imbens, and Rubin (1996), where the ITT is expressed as a weighted average of treatment effects for compliers and defiers: $ITT = \pi_C\tau_C + \pi_D\tau_D$. The complier share is defined as $\mathbb{E}[T|Z = 1] - \mathbb{E}[T|Z = 0]$, and the defier share as the fraction of individuals with $Z = 1$ and $T = 0$. The implied defier effect is computed residually from this decomposition. Dependent variables are: (1) Request PPT, an indicator equal to one if the individual reported requesting the PPT or attending the biometric appointment in the last survey; (2) Start Registration Process, an indicator equal to one if the individual reported starting the RUMV census; and (3) Intention to Register, an indicator equal to one if the individual reported intending to register. The sample excludes 245 individuals who did not respond to any WhatsApp follow-ups. All panels use the same estimation sample (N = 1,130), ensuring consistency across estimation stages. Robust standard errors are reported in parentheses. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

Table 4. Heterogeneous Effects by Predicted Take-Up: Evidence from Endogenous Stratification

	Endogenous Strata			
	Low	Medium	High	Full Sample
Panel A. PPT Request				
Assignment to Any Video	−0.019	−0.119**	−0.091	−0.080**
	(0.060)	(0.056)	(0.062)	(0.034)
R^2	0.000	0.009	0.007	0.005
Control mean	0.500	0.560	0.556	0.538
Panel B. Start Registration				
Assignment to Any Video	−0.034	−0.110**	−0.081	−0.077**
	(0.059)	(0.056)	(0.062)	(0.034)
R^2	0.001	0.008	0.006	0.005
Control mean	0.590	0.600	0.566	0.585
Panel C. Intend to Register				
Assignment to Any Video	−0.140***	−0.156***	−0.053	−0.122***
	(0.050)	(0.050)	(0.054)	(0.030)
R^2	0.022	0.019	0.003	0.015
Control mean	0.860	0.840	0.778	0.826
Mean predicted take-up	0.372	0.534	0.752	
Observations	343	492	295	1,130

Notes: The table reports heterogeneous treatment effects by predicted take-up terciles using an endogenous stratification approach. The sample is partitioned into terciles based on predicted take-up estimated using baseline covariates in the control group. Predicted values are obtained using leave-one-out cross-validation to avoid overfitting, following **?** and Chernozhukov et al. (2018). Each column reports ITT estimates of assignment to any video (Z) on the outcome of interest within each tercile. Dependent variables: (1) Request PPT is an indicator equal to one if the individual reported having requested the PPT or attended the biometric appointment in the last survey contact; (2) Start Registration Process is an indicator equal to one if the individual reported starting the RUMV census in the last survey contact; (3) Intention to Register is an indicator equal to one if the individual reported the intention to start the RUMV census in the last survey contact. The sample excludes 245 individuals who did not respond to any WhatsApp follow-ups. Robust standard errors are reported in parentheses. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

Contents

A Details on the ETPV and Intervention

Figure A.1. ETPV Registry and Program Rollout

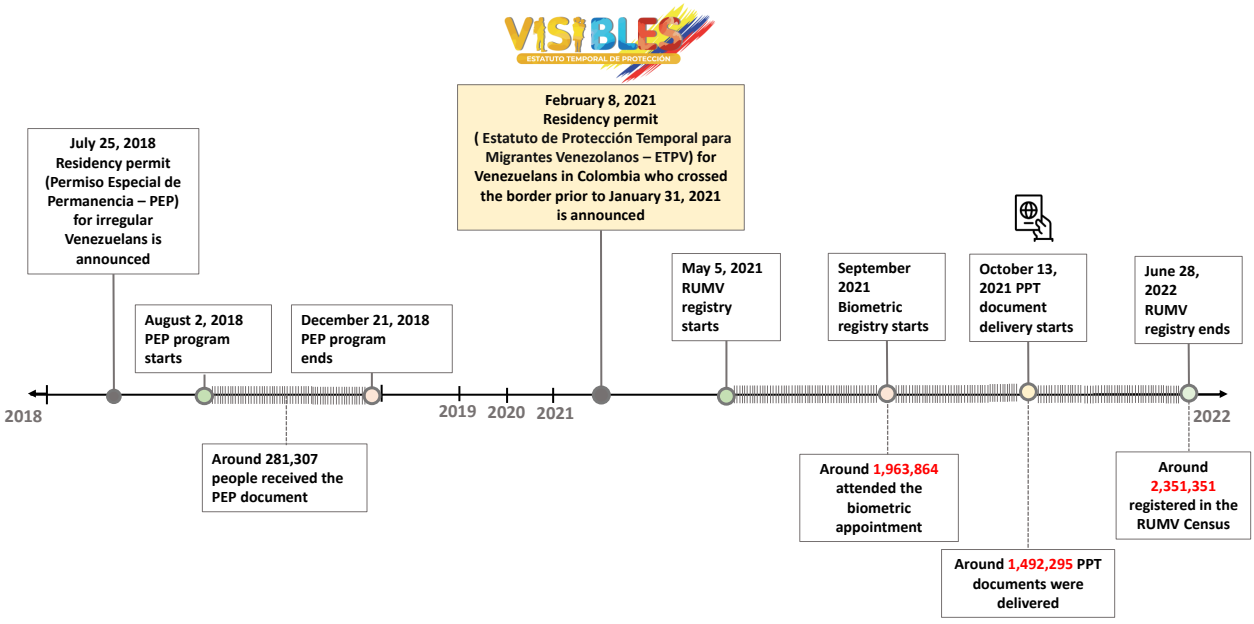


Figure A.2. ETPV Application Process

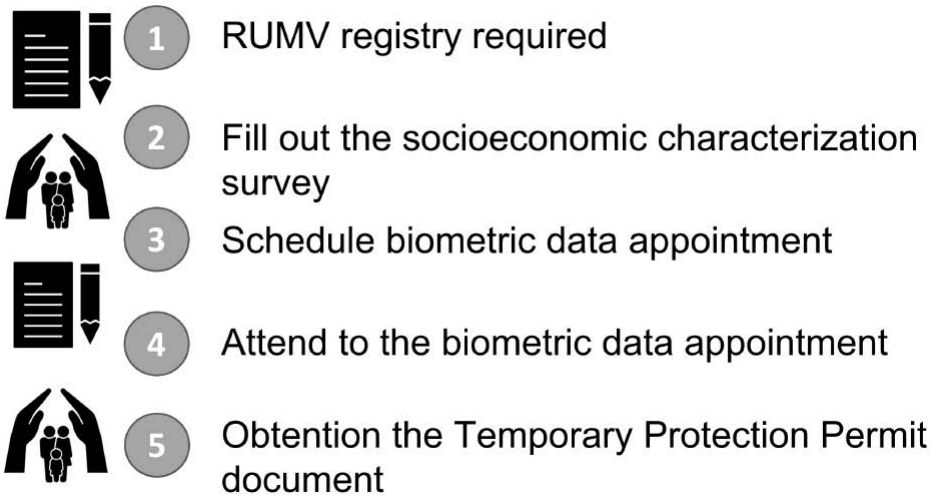


Figure A.3. Geographical Distribution of Venezuelans Registered in the RUMV Census

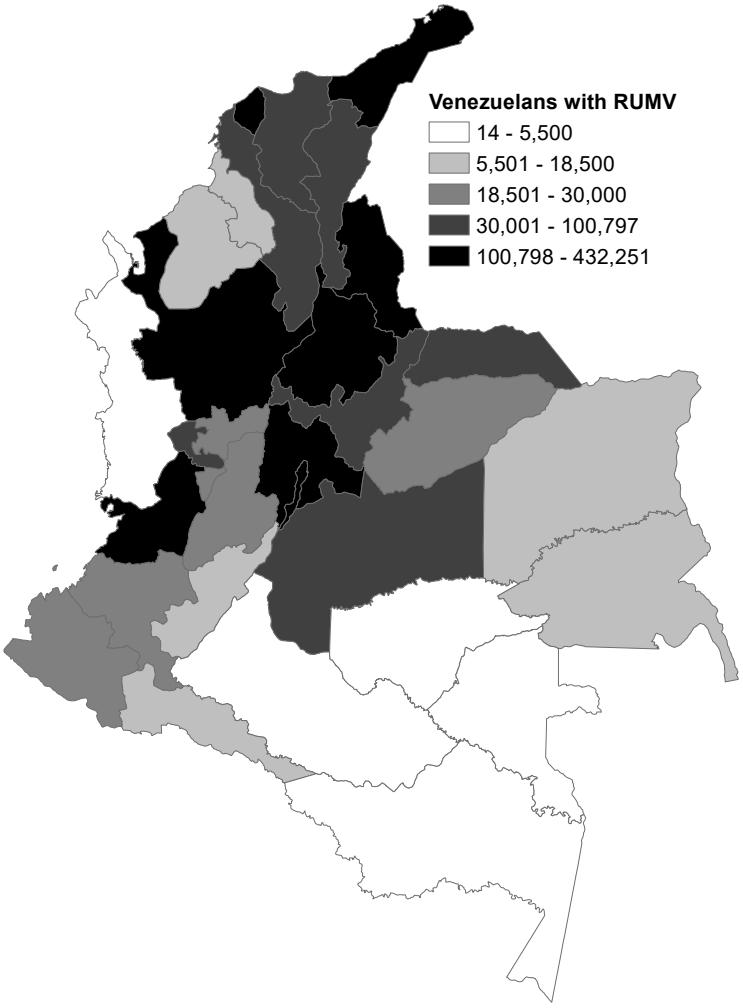


Figure A.4. Iterative WhatsApp Survey Collection

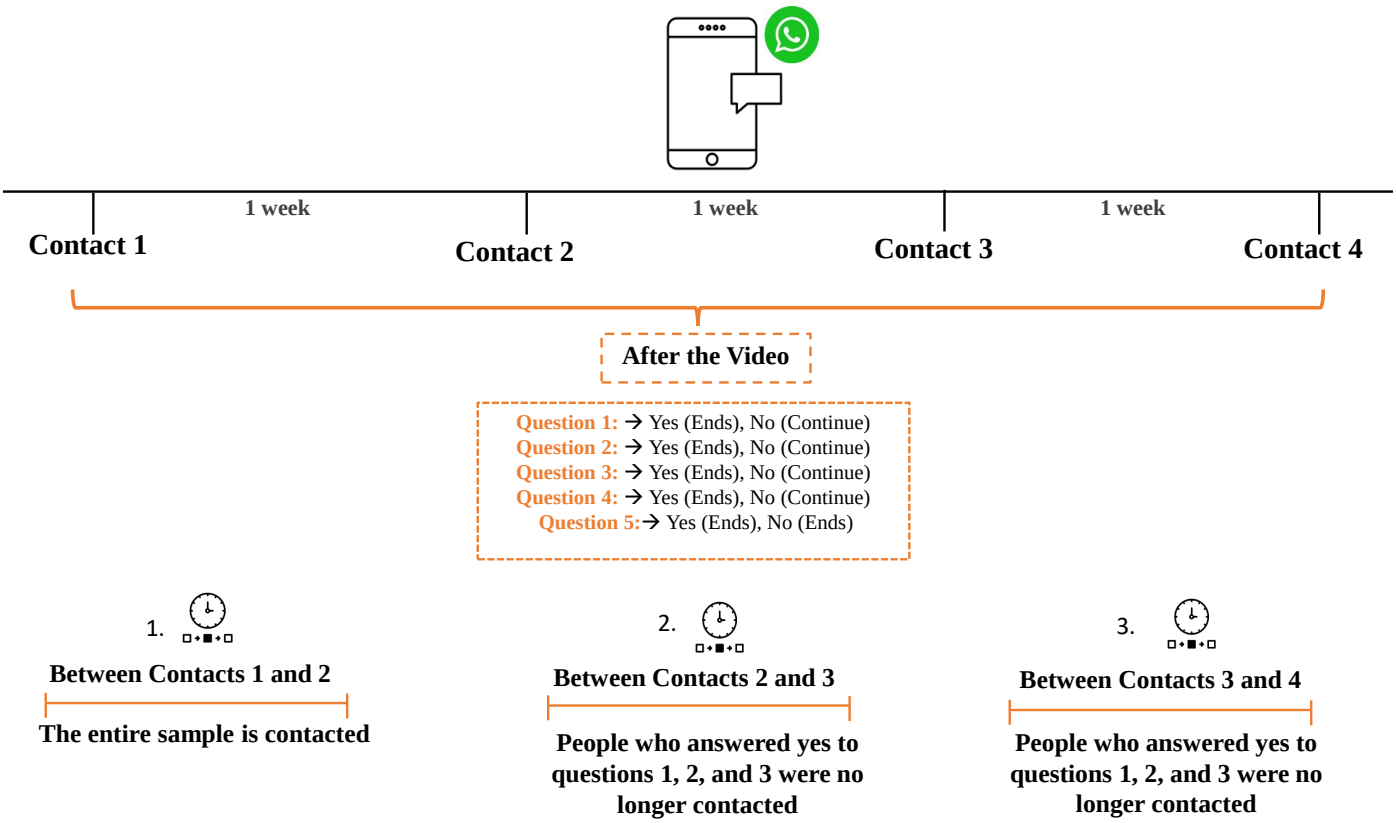


Figure A.5. Iterative WhatsApp Survey Structure

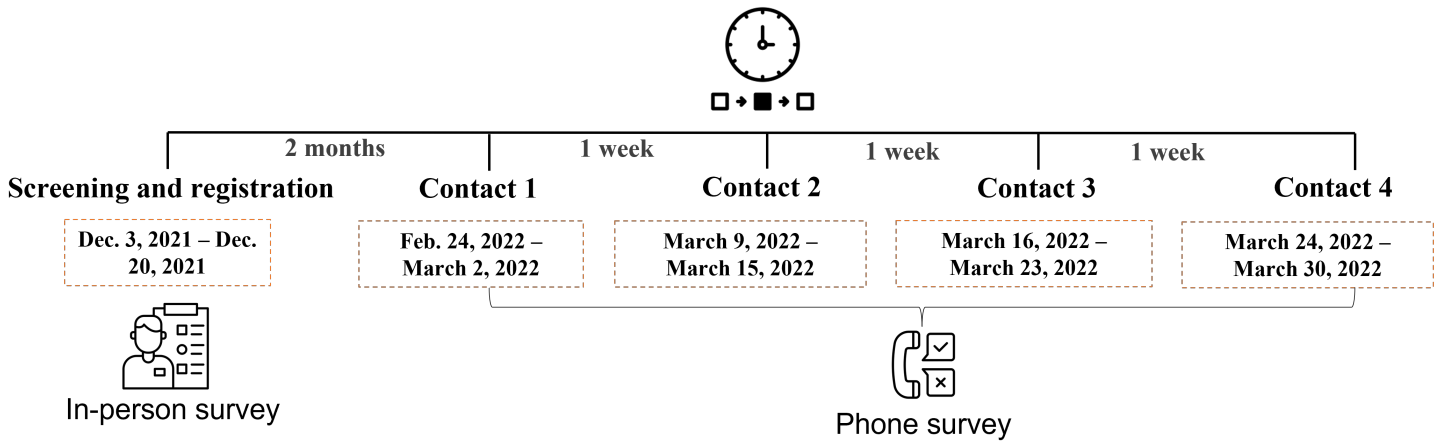


Table A.1. Sample Comparability
(Intervention vs. GEIH vs. VenRePs Samples)

				Mean Difference P-value	
	Intervention	GEIH	VenRePS	ETPV vs. GEIH	ETPV vs. VenRePS
	(1)	(2)	(3)	(4)	(5)
Age	33.444	33.25	32.836	0.640	0.105
	(11.286)	(11.810)	(10.882)		
Male [=1]	0.325	0.441	0.4	0.000	0.000
	(0.469)	(0.497)	(0.490)		
Years of education	4.518	7.814	13.043	0.000	0.000
	(2.051)	(4.028)	(2.862)		
Total income (Log)	0.209	0.542	0.354	0.000	0.000
	(0.161)	(0.268)	(0.213)		
Health regime: Subsidized [=1]	0.149	0.195	0.016	0.001	0.000
	(0.356)	(0.397)	(0.127)		
Health regime: Contributory [=1]	0.107	0.097	0.009	0.337	0.000
	(0.309)	(0.295)	(0.095)		
Health regime: None [=1]	0.847	0.708	0.974	0.000	0.000
	(0.360)	(0.455)	(0.158)		
Unemployed [=1]	0.226	0.083	0.304	0.000	0.000
	(0.419)	(0.276)	(0.460)		
Observations	1,375	1,792	2,317	3,167	3,692

Notes: The first column presents the mean and standard deviation for the sample of this study. Column (2) shows the mean and standard deviation for the Venezuelans who responded to the Colombian Labor Force Survey of December of 2021, known as Gran Encuesta Integrada de Hogares (GEIH). Column (3) depicts the mean and standard deviation for the undocumented Venezuelans surveyed in the Venezuelan Refugee Panel Survey (VenRePS) of 2020. Columns (4) and (5) present the p-value for the mean difference between samples.

Table A.2. Video Play Time Percentage Distribution

Play Time (%)	Observations	%
0%	128	15.40
1-10%	14	1.68
10%-20%	8	0.96
20%-30%	11	1.32
30%-40%	7	0.84
40%-50%	14	1.68
50%-60%	8	0.96
60%-70%	15	1.81
70%-80%	11	1.32
80%-90%	18	2.17
90%-99%	327	39.35
100%	270	32.49
Total	831	100

Notes: This table shows the distribution of maximum video play-time for treated individuals.

Table A.3. Distribution of Possible Contact Combinations
Individuals Contacted Four Times

Panel A: Possible Contact Cases

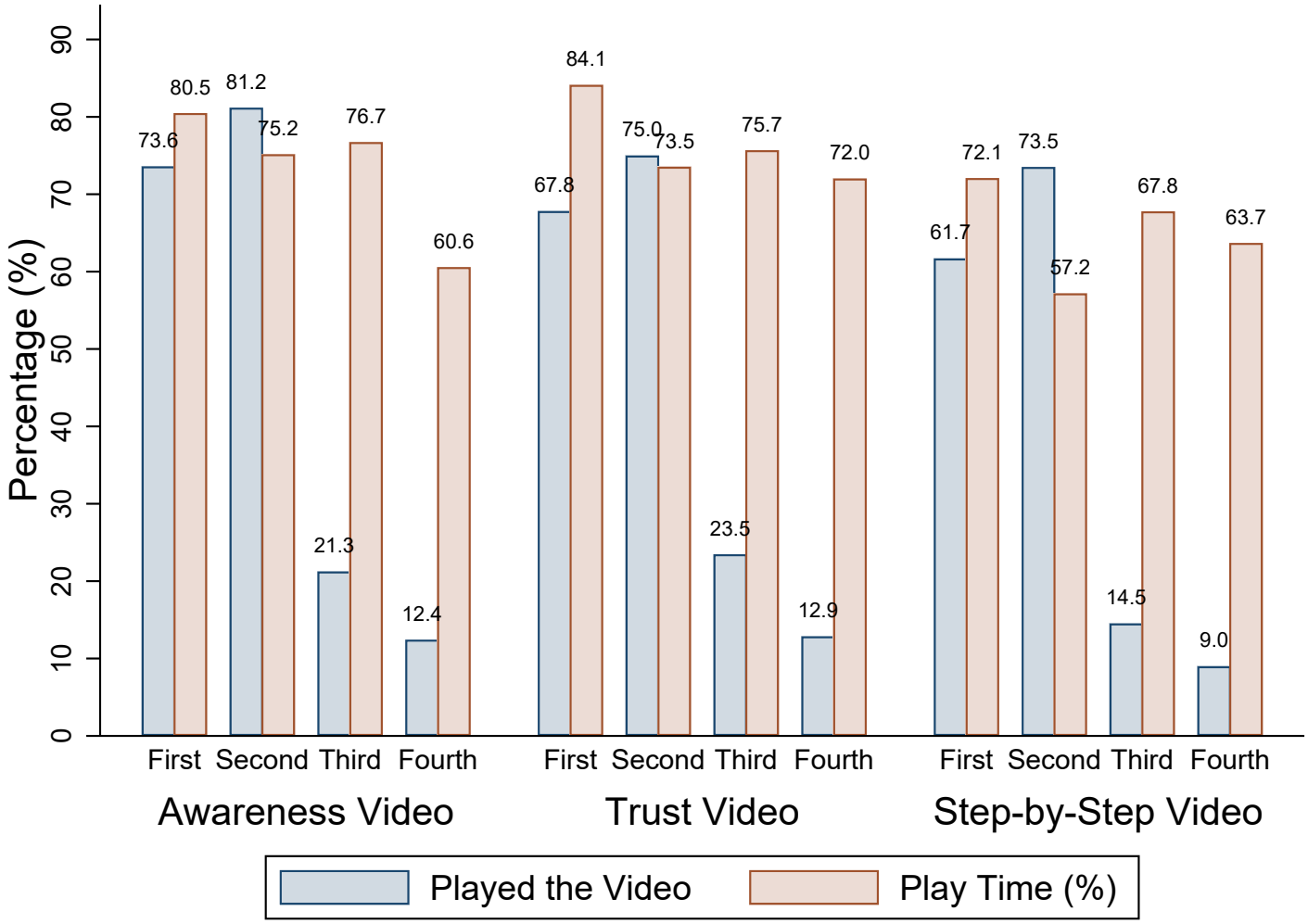
Possible Contact Cases	Number of Individuals	(% of Total)
0 0 0 0	310	39.29
0 0 0 1	22	2.79
0 0 1 0	15	1.90
0 0 1 1	22	2.79
0 1 0 0	30	3.80
0 1 0 1	24	3.04
0 1 1 0	20	2.53
0 1 1 1	41	5.20
1 0 0 0	50	6.34
1 0 0 1	15	1.90
1 0 1 0	8	1.01
1 0 1 1	23	2.92
1 1 0 0	15	1.90
1 1 0 1	13	1.65
1 1 1 0	29	3.68
1 1 1 1	152	19.26
Total	789	100

Panel B: Successfully Reached Combinations

Successfully Reached Possible Combinations	Number of Individuals	(%)
Never	310	39.29
One Time	117	14.82
Two Consecutive Times	57	7.22
Two Non-Consecutive Times	47	5.95
Three Consecutive Times	70	8.87
Three Non-Consecutive Times	36	4.56
Always	152	19.26
Total	789	

Notes: In the table on the left "0" corresponds to the individuals who were contacted but did not answer the survey and "1" to the individuals who were contacted and completed the survey.

Figure A.6. Video Play Rates by Treatment Arm



Notes: The percentage is calculated over the treated sample contacted in each of the treatment arms. The treated sample for the first contact corresponded to 750 individuals, for the second contact to 257 individuals, for the third contact to 176 individuals, and for the fourth contact to 105 individuals.

B Treatment Scripts

B.A Treatment 1 Script: Information Video

[A Colombian actor resembling an official provides the information.]

Good morning, I am going to tell you what the Temporary Statute for Venezuelan Migrants is, better known as ETPV. The ETPV is a measure created for the regularization of Venezuelans for 10 years in Colombia. It will allow you to apply for the Temporary Protection Permit, known as PPT, which will give you access to the following benefits:

- Get vaccinated against Covid-19
- Full access to health services for you and your family
- Access to government subsidies through SISBEN
- Access to any job with an employment contract in Colombia
- Apply for a resident visa to be permanently legal in Colombia
- Validate professional degrees
- Open a bank account and apply for credits
- Enter and leave the country without restriction
- Access to the retirement system

You are eligible to apply to the PPT and it's free. In addition, 1,434,975 Venezuelans have already registered. I am going to explain how to apply, everything is done online and you just have to follow the following three steps:

1. Enter the page https://www.migracioncolombia.gov.co/visibles to register in the Unique Registry of Venezuelan Migrants, more known as RUMV

2. After registering for the RUMV, you schedule the appointment for the collection of biometric data on the page: https://agendamigracoletp.emtelco.co/#/. You must confirm the appointment in your email and attend the biometric data collection in person on the assigned date
3. You will receive the PPT virtually and three months later they will deliver it to you physically.

I will tell you what you need to register in the RUMV:

1. Computer with internet
2. Active email
3. Have the following three documents scanned:
    - Identity Document: the passport, the Venezuelan ID or the Special Permit of Permanence are valid.
    - Photography with a white background. Remember that you can take it from your cell phone.
    - "Prueba Sumaria": this is a document that proves that you arrived in Colombia before January 31, 2021. It could be a certificate of medical attention, the certificate of your child's grades, the certification of your work, or any similar document that certifies that you were in Colombia before the stipulated date.

Remember that all persons of legal age in your household must register separately. However, when you make the RUMV registration, you will have the option of adding the minors in your charge, the system will schedule the appointment for taking biometric data for children between seven and 18 years old. Children under seven do not need an appointment because they have access to benefits with your PPT.

I WILL SUMMARIZE THE STEPS FOR YOU:

- REGISTER IN THE RUMV
- APPOINTMENT FOR THE BIOMETRIC DATA
- OBTAINING THE PPT

DON'T FORGET TO SCAN:

- YOUR PHOTOGRAPH
- YOUR IDENTITY DOCUMENT
- YOUR "PRUEBA SUMARIA"

SAVE YOUR EMAIL AND PASSWORD, YOU WILL RECEIVE YOUR DOCUMENT THERE
Do you need more information? Enter the website of https://www.migracioncolombia.gov.co/visibles

B.B Treatment 2 Script: Information Video Leveraging In-group Trust

[A Venezuelan woman with children provides the information; the goal is for the vulnerable migrant to identify with the person providing the message.]

Good morning, my name is María González, I am a Venezuelan immigrant, I arrived in Colombia irregularly with my children in July 2020, and I am going to tell you what is the Temporary Statute for Venezuelan Migrants, better known as ETPV. The ETPV is a measure created for the regularization of Venezuelans for 10 years in Colombia. It will allow you to apply for the Temporary Protection Permit, known as PPT, which will give you access to the following benefits:

- Get vaccinated against Covid-19
- Full access to health services for you and your family
- Access to government subsidies through SISBEN
- Access to any job with an employment contract in Colombia
- Apply for a resident visa to be permanently legal in Colombia
- Validate professional degrees
- Open a bank account and apply for credits
- Enter and leave the country without restriction
- Access to the retirement system

You are eligible to apply to the PPT and it's free. In addition, 1,434,975 Venezuelans have already registered. I am going to explain how to apply, everything is done online and you just have to follow the following three steps:

1. Enter the page https://www.migracioncolombia.gov.co/visibles to register in the Unique Registry of Venezuelan Migrants, better known as RUMV
2. After registering for the RUMV, you schedule the appointment for the collection of biometric data on the page https://agendamigracoletp.emtelco.co/#/. You must confirm the appointment in your email and attend the biometric data collection in person on the assigned date
3. You will receive the PPT virtually and three months later they will deliver it to you physically.

I will tell you what you need to register in the RUMV:

1. Computer with internet
2. Active email
3. Have the following 3 documents scanned:

- Identity Document: the passport, the Venezuelan ID or the Special Permit of Permanence are valid.
- Photography with a white background. Remember that you can take it from your cell phone.
- "Prueba Sumaria": this is a document that proves that you arrived in Colombia before January 31, 2021. It could be a certificate of medical attention, the certificate of your child's grades, the certification of your work, or any similar document that certifies that you were in Colombia before the stipulated date.

Remember that all persons of legal age in your household must register separately. However, when you make the RUMV registration, you will have the option of adding the minors in your charge, the system will schedule the appointment for taking biometric data for children between seven and 18 years old. Children under seven do not need an appointment because they have access to benefits with your PPT.

I WILL SUMMARIZE THE STEPS FOR YOU:

- REGISTRATION IN THE RUMV
- APPOINTMENT FOR THE BIOMETRIC DATA
- OBTAINING THE PPT

DON'T FORGET TO SCAN:

- YOUR PHOTOGRAPH
- YOUR IDENTITY DOCUMENT
- YOUR "PRUEBA SUMARIA"

SAVE YOUR EMAIL AND PASSWORD, YOU WILL RECEIVE YOUR DOCUMENT THERE
Do you need more information? Enter the website at https://www.migracioncolombia.gov.co/visibles

B.C Treatment 3 Script: Registration Process Video Leveraging In-group Trust

[A Venezuelan woman with children provides the information; the goal is for the vulnerable migrant to identify with the person providing the message.]

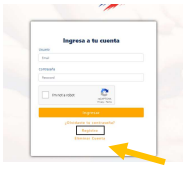
Good morning, my name is María González, I am a Venezuelan immigrant, I arrived in Colombia irregularly with my children in July 2020, and I will explain to you step-by-step how I applied for the Temporary Protection Permit, better known as the PPT.

Figure B.1. Registration Process Video Step-by-Step

1. I entered the Migración Colombia page: https://www.migracioncolombia.gov.co/
2. Click on the button **"MAKE THE REGISTRATION IN THE RUMV HERE"**

3. Click on the button **"MAKE THE REGISTRATION IN THE RUMV HERE"**

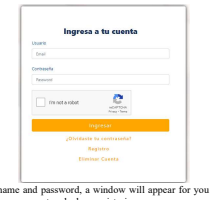
4. I entered my account and username, if you do not have it, follow the following procedure, select the option **REGISTER**



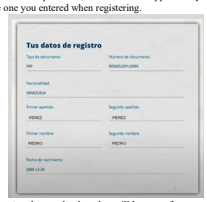
5. In the register option, fill in the corresponding information. Remember that you must have an active email and you must have the number of one of the following types of documents: Passport, PEP, identity card, or birth certificate.

6. After filling in the information and selecting to register, you will receive an account activation email to your email, select the activation link and with this you will have your registration done.

7. With your registration done, you will have an active username and password. The username corresponds to the email and password you used to register. Now, you must enter them in the window of the home page, it will appear just as soon as you finish the registration.



8. Once you have entered the username and password, a window will appear for you to review your information and verify that this is the same as the one you entered when registering.

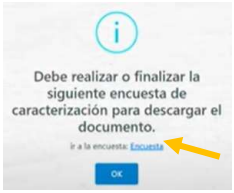


9. Review the information, select **next** and enter the data that will be part of your resumé. In the Operation Type box, select the only option that appears, the rest you can easily fill out.

10. Select next and enter your requested address and contact information.
11. Select next and register your family group information. Here you can add minors in your charge. If you are only going to make your registration, you do not need to fill out this information. Remember that all persons of legal age must register, it is not a registration per household, but per individual person.
12. Select the following and attach the required documents:
    - Upload a document type photograph on a white background
    - Upload a photo of the identification document
    - Upload the "prueba sumaria". Remember that this is a document that proves that you arrived in Colombia before January 31, 2021. It could be a certificate of medical attention, the certificate of your child's grades, the certification of your work by the employer, or any similar document that certifies that you were in Colombia before the stipulated date. This document is only for people who were irregularly in Colombia before January 31, 2021. If not, you should not upload it

After uploading all the documents, select **UPLOAD DOCUMENTS**

13. When uploading the documents, you will get an ad with the indication to read and accept the terms, select the **I agree box** and then the button **FINISH**
14. Once the registration is finished, a window will appear with the announcement that will refer you to the completion of the Characterization Survey: Select go to the survey



15. **When you go to the survey, you will have to fill out the following information (show list on screen):**
    - Questions of recognition and permanence
    - Questions about your documentation, ethnicity and identity
    - Questions about your family group
    - Questions about living condition
    - Questions about occupation and study
    - Questions about social security
    - Questions about health
    - Question about reasons for migration
    - Questions about perception of integration
    - Questions about vulnerability ad
16. Once the survey is finished, you will be ready to schedule your face-to-face appointment for biometric registration.

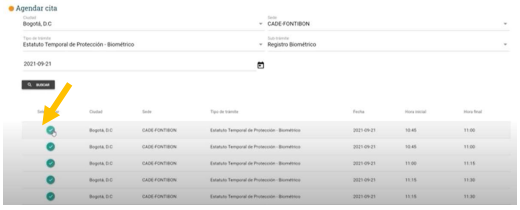
Figure B.2. Registration Process Video Step-by-Step

17. After completing the survey, you will be directed to the window that allows you to schedule your appointment. There you must select the button, schedule appointment

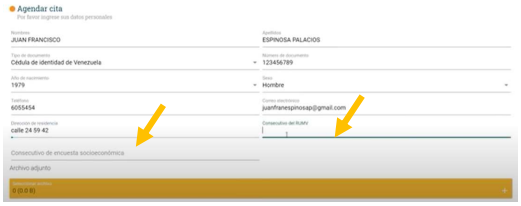
18. You will get an informative notice, after reading it, click accept

19. Fill in the data for the appointment scheduling, you must fill out the following information:
- **City**: The city in which you are going to carry out the procedure
- **Headquarters**: The closest office to the place where you live
- **Type of Procedure:** Temporary Protection Status - Biometric
- **Sub-procedure**: Biometric Registration
- Date available to attend the appointment

The system will show you the dates and times available to carry out your procedure, select the date and venue that is of interest to you, by clicking the green button to the left of the appointment.

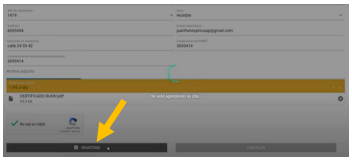


20. When you select the button, a window will appear for you to fill in your personal data.

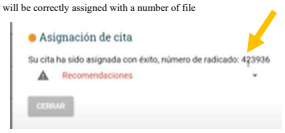


You must bear in mind that in the consecutive field of the RUMV and in the consecutive field of the socioeconomic survey, you must enter the document number generated in the pre-registration. Which I point out to you in the following image.

21. In the same window, you must upload the certificate of your registration.
22. Click I'm not a robot and follow the safety instructions. Finally, click on register and with this your appointment will be assigned.



23. Your appointment will be correctly assigned with a number of file

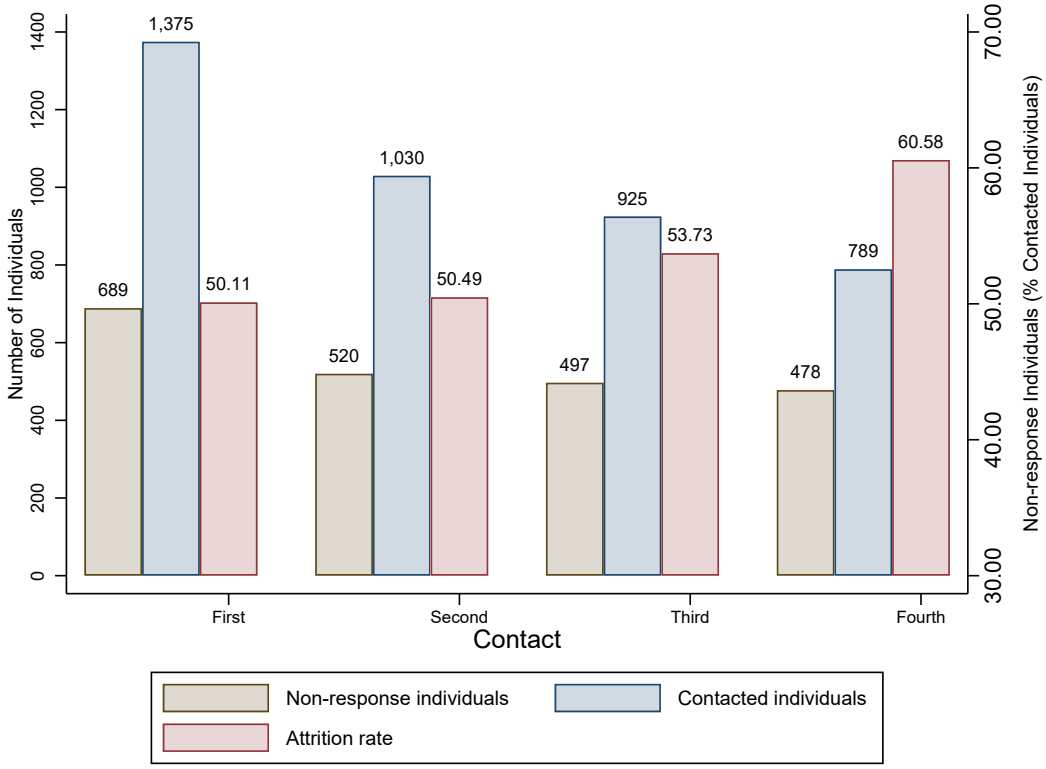


24. Finally, you must check your mail both in received and in unwanted messages, or in spam to see if an email with the confirmation has arrived of the appointment. There you must confirm whether or not you will attend the appointment.

C Attrition Analysis

Figure C.1. Attrition Rate by Type of Treatment

Panel A: Full Sample



Panel B: Contacted Four Times

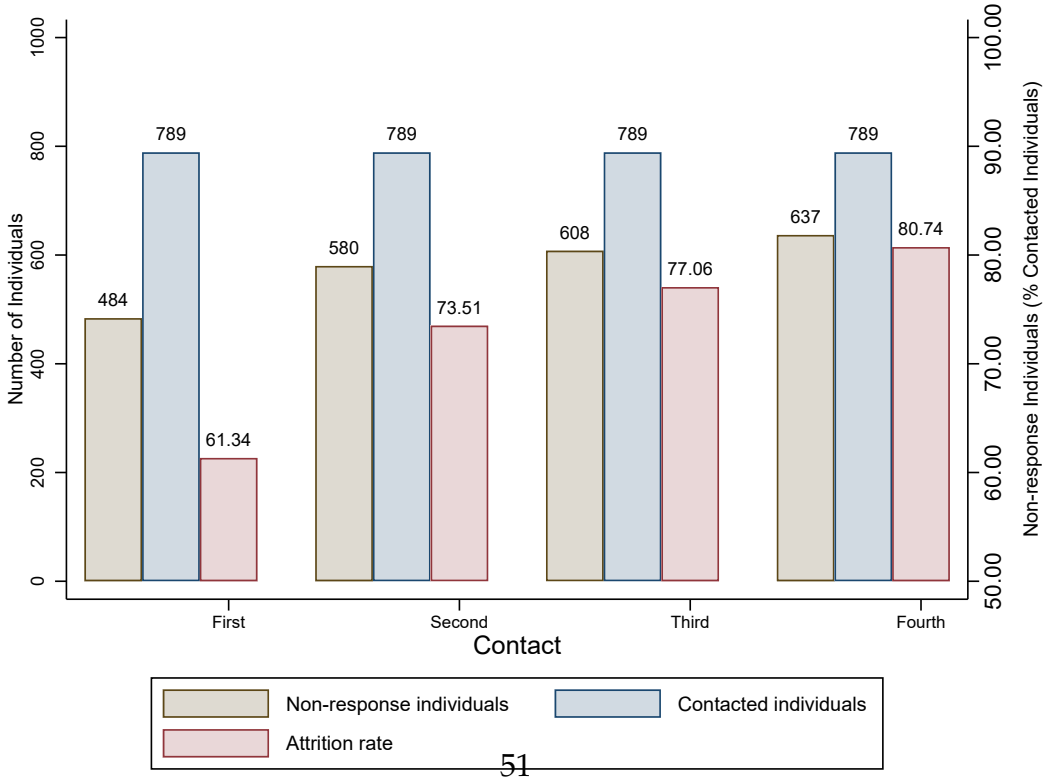


Table C.1. Characterizing Attrition
Attrition [=1 if individual never responded to IWS]

	Attrited Individuals (Never responded)
Age	-0.000
	(0.001)
Male [=1]	0.051*
	(0.026)
Ed. Level: Primary or Less [=1]	0.047
	(0.037)
Ed. Level: General or diversified school [=1]	-0.043
	(0.031)
Number of household members	-0.003
	(0.007)
Personal Income (Sin*)	0.001
	(0.003)
Activity spent the most time: Working [=1]	-0.016
	(0.029)
Trust in Colombian Government (SD)	0.009
	(0.012)
Internet Access more than 4 hour per day [=1]	-0.006
	(0.025)
Personal use Whatsapp [=1]	-0.060**
	(0.027)
Facebook or Instagram account [=1]	0.020
	(0.027)
Twitter account [=1]	-0.052
	(0.044)
E-mail account [=1]	-0.050
	(0.033)
Social desirability index (SD)	0.014
	(0.016)
R-squared	0.024
Mean Dependent Variable	0.225
Observations	1,375

Notes: Attrited Individuals is an indicator [=1] for the people who did not answer the survey and could not be contacted through WhatsApp. *** significant at the 1%, ** significant at the 5%, and * significant at the 10%.

Table C.2. Characterizing Attrition

	Attrited Four Contacts (1)	Lost-to-Follow-Up (2)	Lost-to-Follow-Up or Attrited Four Contacts (3)
Age	-0.002	0.001	-0.001
	(0.002)	(0.002)	(0.003)
Male [=1]	0.090*	0.032	0.122*
	(0.054)	(0.058)	(0.064)
Ed. Level: Primary or Less [=1]	0.074	-0.162**	-0.087
	(0.076)	(0.082)	(0.090)
Ed. Level: General or diversified school [=1]	-0.076	-0.021	-0.097
	(0.065)	(0.070)	(0.077)
Number of household members	-0.004	-0.019	-0.023
	(0.013)	(0.014)	(0.016)
Personal Income (Sin*)	-0.004	0.002	-0.002
	(0.005)	(0.006)	(0.006)
Activity spent the most time: Working [=1]	0.003	-0.019	-0.016
	(0.057)	(0.061)	(0.068)
Internet Access more than 4 hour per day [=1]	-0.005	-0.015	-0.020
	(0.048)	(0.052)	(0.058)
Trust in Colombian Government (SD)	0.001	-0.005	-0.003
	(0.026)	(0.028)	(0.031)
Personal use Whatsapp [=1]	-0.116**	-0.023	-0.139**
	(0.058)	(0.062)	(0.069)
Facebook or Instagram account [=1]	0.039	-0.010	0.029
	(0.054)	(0.058)	(0.065)
Twitter account [=1]	-0.060	0.046	-0.014
	(0.088)	(0.095)	(0.105)
E-mail account [=1]	-0.095	0.096	0.002
	(0.067)	(0.072)	(0.080)
Social desirability index (SD)	0.007	0.087***	0.094**
	(0.031)	(0.033)	(0.037)
Age x 1[Any Video Treatment]	0.002	-0.003	-0.001
	(0.002)	(0.003)	(0.003)
Male [=1] x 1[Any Video Treatment]	-0.054	0.012	-0.042
	(0.061)	(0.066)	(0.073)
Ed. Level: Primary or Less [=1] x 1[Any Video Treatment]	-0.022	0.163*	0.141
	(0.087)	(0.094)	(0.104)
Ed. Level: General or diversified school [=1] x 1[Any Video Treatment]	0.059	0.016	0.075
	(0.074)	(0.080)	(0.088)
Number of household members x 1[Any Video Treatment]	-0.000	0.021	0.021
	(0.015)	(0.016)	(0.018)
Personal Income (Sin*) x 1[Any Video Treatment]	0.007	-0.006	0.001
	(0.006)	(0.007)	(0.007)
Activity spent the most time: Working [=1] x 1[Any Video Treatment]	-0.033	0.010	-0.023
	(0.066)	(0.071)	(0.079)
Internet Access more than 4 hour per day [=1] x 1[Any Video Treatment]	-0.001	0.012	0.011
	(0.056)	(0.061)	(0.067)
Trust in Colombian Government (SD) x 1[Any Video Treatment]	0.010	0.008	0.018
	(0.029)	(0.031)	(0.035)
Personal use WhatsApp [=1] x 1[Any Video Treatment]	0.077	0.031	0.108
	(0.066)	(0.071)	(0.078)
Facebook or Instagram account [=1] x 1[Any Video Treatment]	-0.032	0.052	0.020
	(0.062)	(0.067)	(0.074)
Twitter account [=1] x 1[Any Video Treatment]	0.014	-0.032	-0.018
	(0.102)	(0.110)	(0.121)
E-mail account [=1] x 1[Any Video Treatment]	0.057	-0.132	-0.075
	(0.077)	(0.083)	(0.092)
Social desirability index (SD) x 1[Any Video Treatment]	0.008	-0.068*	-0.060
	(0.036)	(0.039)	(0.043)
Any Video Treatment [=1]	-0.105	-0.043	-0.149
	(0.160)	(0.172)	(0.191)
R-squared	0.035	0.024	0.044
Observations	1,375	1,375	1,375
Mean Dependent Variable	0.225	0.276	0.502

Notes: Attrited Four Contacts is an indicator [=1] for the people who did not answer the survey. Lost-to-Follow-Up is a dichotomous variable if the individual responded to at least one of the follow-ups 1 through 3 and responded that he/she had not completed the registration process for the PPT or requested the biometric appointment.

Table C.3. Distribution of Attrition by Treatment Status and Different Definitions of Attrition

	All Sample	Control	Awareness Video	Trust Video	Step-by-step Video	Any Video
	(1)	(2)	(3)	(4)	(5)	(6)
Attrited Four Contacts	0.225	0.167	0.223	0.232	0.281	0.245
Lost-to-Follow-Up	0.276	0.285	0.296	0.287	0.237	0.273
Lost-to-Follow-Up and Attrited Four Contacts	0.501	0.452	0.519	0.519	0.518	0.518
Observations	1,375	347	341	345	342	1,028

Notes: Attrited Four Contacts is an indicator [=1] for the people who did not answer the survey. Lost-to-Follow-Up is a dichotomous variable if the individual responded to at least one of the follow-ups 1 through 3 and responded that he/she had not completed the registration process for the PPT or requested the biometric appointment.

D Robustness Analysis

Table D.1. Intervention Effects on PPT Take-up Rates (Responses from First Contact)

Indicator Variables	Request PPT	Start Registration Process	Intention to Register
	(1)	(2)	(3)
Panel A. ITT - General Effect			
β_1[Any Video]	-0.094***	-0.107***	-0.178***
	(0.027)	(0.030)	(0.032)
FDR q-values	[0.001]	[0.001]	[0.001]
R-squared	0.010	0.011	0.027
Panel B. ITT - Dissaggregated Effect			
β_1[Awareness]	-0.069**	-0.095**	-0.151***
	(0.033)	(0.037)	(0.039)
β_2[Trust]	-0.080**	-0.061	-0.139***
	(0.033)	(0.037)	(0.039)
β_3[Step-by-Step]	-0.135***	-0.169***	-0.248***
	(0.034)	(0.038)	(0.039)
R-squared	0.014	0.018	0.035
Mean Untreated Group	0.274	0.365	0.786
Observations (All Panels)	1,130	1,130	1,130

Notes: Dependent variables: (i) Request PPT is an indicator [=1] if the individual reported having requested the PPT, or requested or attended the biometric appointment in the last survey contact. (ii) Start Registration Process is an indicator [=1] if the individual reported starting the RUMV census in the last survey contact. (iii) Intention to Register is an indicator [=1] if the individual reported the intention to start the RUMV census in the last survey contact. The experiment had 1,375 individuals registered. This table excludes from the sample 245 individuals who did not answer any of the four WhastApp surveys. Standard errors are reported in parentheses and False Discovery Rate (FDR) q-values are reported in brackets. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

Table D.2. Intervention Effects on PPT Take-up Rates by Treatment Arm

Indicator Variables	Request PPT	Start Registration Process	Intention to Register
	(1)	(2)	(3)
β_1[Awareness]	-0.060	-0.057	-0.103***
	(0.041)	(0.041)	(0.036)
β_2[Trust]	-0.065	-0.069*	-0.098***
	(0.041)	(0.041)	(0.036)
β_3[Step-by-Step]	-0.117***	-0.108**	-0.168***
	(0.042)	(0.042)	(0.037)
R-squared	0.007	0.006	0.019
Mean Untreated Group	0.538	0.585	0.826
Observations (All Panels)	1,130	1,130	1,130

Notes: Dependent variables: (i) Request PPT is an indicator [=1] if the individual reported having requested the PPT, or requested or attended the biometric appointment in the last survey contact. (ii) Start Registration Process is an indicator [=1] if the individual reported starting the RUMV census in the last survey contact. (iii) Intention to Register is an indicator [=1] if the individual reported the intention to start the RUMV census in the last survey contact. The experiment had 1,375 individuals registered. This table excludes from the sample 245 individuals who did not answer any of the four WhastApp surveys. Standard errors are reported in parentheses and False Discovery Rate (FDR) q-values are reported in brackets. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

E Heterogeneous Effects

Table E.1. Heterogeneous Effects on PPT Take-up Rates by Video Reproduction

Indicator Variables	Request PPT		Start Registration Process		Intention to Register	
	Didn't Played Video	Played Video	Didn't Played Video	Played Video	Didn't Played Video	Played Video
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. General Effect - ITT						
$\beta_1[AnyVideo]$	-0.257***	-0.048	-0.265***	-0.043	-0.342***	-0.082***
	(0.051)	(0.035)	(0.051)	(0.034)	(0.044)	(0.029)
R-squared	0.056	0.002	0.059	0.002	0.123	0.008
Panel B. Dissaggregated Effect - ITT						
$\beta_1[Awareness]$	-0.228**	-0.040	-0.240**	-0.036	-0.240***	-0.087**
	(0.095)	(0.043)	(0.095)	(0.042)	(0.082)	(0.036)
$\beta_2[Trust]$	-0.295***	-0.026	-0.293***	-0.032	-0.411***	-0.045
	(0.081)	(0.043)	(0.081)	(0.043)	(0.070)	(0.036)
$\beta_3[Step-by-Step]$	-0.245***	-0.082*	-0.258***	-0.066	-0.343***	-0.119***
	(0.070)	(0.045)	(0.070)	(0.045)	(0.060)	(0.038)
R-squared	0.057	0.003	0.059	0.002	0.128	0.011
Mean Untreated Group	0.538	0.538	0.585	0.585	0.826	0.826
Observations (All Panels)	427	1,002	427	1,002	427	1,002

Notes: Dependent variables: (i) Request PPT is an indicator [=1] if the individual reported having requested the PPT, or requested or attended the biometric appointment in the last survey contact. (ii) Start Registration Process is an indicator [=1] if the individual reported starting the RUMV census in the last survey contact. (iii) Intention to Register is an indicator [=1] if the individual reported the intention to start the RUMV census in the last survey contact. The experiment had 1,375 individuals registered. This table excludes from the sample 245 individuals who did not answer any of the four WhastApp surveys. Standard errors are reported in parentheses and False Discovery Rate (FDR) q-values are reported in brackets. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

Table E.2. Heterogeneous Effects on PPT Take-up Rates by Video Reproduction

Indicator Variables	Request PPT	Start Registration Process	Intention to Register
	(1)	(2)	(3)
Panel A. General Effect - ITT			
$\beta_1[AnyVideo]\times$ I(Played the Video)	0.210***	0.222***	0.260***
	(0.048)	(0.047)	(0.041)
$\beta_2[AnyVideo]$	-0.257***	-0.265***	-0.342***
	(0.052)	(0.052)	(0.045)
Diff. Effect= $\beta_1+\beta_2$	-0.048	-0.043	-0.082***
	(0.034)	(0.034)	(0.030)
R-squared	0.022	0.024	0.048
Mean Untreated Group	0.538	0.585	0.826
Observations (All Panels)	1,130	1,130	1,130

Notes: Dependent variables: (i) Request PPT is an indicator [=1] if the individual reported having requested the PPT, or requested or attended the biometric appointment in the last survey contact. (ii) Start Registration Process is an indicator [=1] if the individual reported starting the RUMV census in the last survey contact. (iii) Intention to Register is an indicator [=1] if the individual reported the intention to start the RUMV census in the last survey contact. The experiment had 1,375 individuals registered. This table excludes from the sample 245 individuals who did not answer any of the four WhastApp surveys. Standard errors are reported in parentheses and False Discovery Rate (FDR) q-values are reported in brackets. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.