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Building a shield together: Addressing low vaccine uptake against cancer through social norms

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Abstract

We present the results of a large-scale field experiment designed to measure the effect of social norms on parents' decisions to vaccinate their daughters against the human papillomavirus (HPV) in Bogota, Colombia. Because low rates of HPV vaccine adoption are an issue in developed and underdeveloped countries alike, the use of standard social norm marketing strategies to foster vaccination can have the undesirable effect of reinforcing the status quo. In our experiment, parents were exposed to text messages that incorporated variations of static and dynamic social norms. We demonstrate that dynamic social norms and injunctive norms increased the vaccination rate by 23%. Interestingly, we also find that a version of static social norms that uses a loss frame is also effective in fostering vaccination, implying that policy-makers can also benefit from them. Against a common view among academics and practitioners, we found no evidence that static norms reinforce the status quo. Our results highlight the importance of crafting social norms interventions using dynamic and injunctive elements to foster vaccination in settings where the majority has not yet adopted the desired behavior.

JLE classifications: C93, D91, I12, I18, O12.

Keywords: Social norms, vaccines, human papillomavirus, field experiments.

1. Introduction

The adoption of cost-effective life-saving technologies, such as vaccines, continues to present a challenge in the developing world. Similar to various other preventive health investments, vaccines offer substantial economic and social advantages.[1] Despite these benefits, vaccination rates often remain low. According to a collaborative report by the World Health Organization (WHO), the United Nations Children's Fund (UNICEF), and the World Bank, an additional 2 million lives could be saved if 90% of the global population under the age of five were to receive existing vaccines (WHO et al., 2009).

Vaccination against the human papillomavirus (HPV) serves as a prominent illustration of this issue. HPV stands as the most prevalent sexually transmitted infection and is causally associated with nearly all instances of cervical cancer (Bruni et al., 2010). Cervical cancer represents a significant public health concern in the developing world, where approximately 90% of cases arise, and the incidence rate is threefold higher compared to the developed world (Hull et al., 2020). Despite the availability of HPV vaccines since 2006, vaccination coverage remains below optimal levels. Merely 15% of girls within the targeted age group for HPV vaccination receive complete protection (Bruni et al., 2021). This stands in stark contrast to the coverage rates of most vaccines endorsed by the WHO, which typically hover around 80%.[2] This presents a concerning scenario, particularly considering that HPV vaccine availability is no longer a limiting factor in most countries.

Willingness to engage in preventive health behaviors may depend on beliefs about others' engagement (Brewer et al., 2017). Research in behavioral sciences, which has gained wide notice in economic and policy circles since Thaler & Sunstein (2021), has systematically documented that social norms have a powerful effect on behavior change (Nolan et al., 2021; Bicchieri 2017). Social norms refer to beliefs concerning both the actions and approvals of others within a specific reference group. These beliefs encompass what individuals perceive others to do and endorse, as well as what they believe others expect them to do and approve of. These norms are upheld through the mechanisms of social approval or disapproval.[3] Many studies have shown that social norms

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[1] For instance, it is estimated that vaccines prevent approximately 2.5 million deaths among children annually. See WHO et al., (2009) for details.

[2] One exception is the yellow fever vaccine. This vaccine only reached a coverage level of 48% in 2022.

[3] There are many definitions of social norms in fields like philosophy, sociology, psychology, anthropology, legal studies, political science, and others. For a recent discussion of this interdisciplinary literature, see (Legros et al., 2020).

are a useful tool to boost vaccination rates (Moehring et al., 2023). Social norm interventions provide social information describing the prevalence of behavior, i.e., descriptive norms, and its degree of social approval, i.e., injunctive norms (Cialdini et al., 1990). However, when a social norm intervention informs that a behavior is only adopted by a minority, it can have the unintended outcome of entrenching the status quo (Bicchieri & Dimant 2022, Schultz et al., 2007). This is the so-called “boomerang effect” of social norm interventions, which has been documented in different fields.[4] Little is known about the effectiveness of social norms interventions to promote vaccination in settings where conformity with the relevant norm is low, as in the case of the HPV vaccine adoption.

Recent scholarship in social psychology has shown that dynamic social norms can be a potential solution to induce behavioral change in settings with minority norms (Sparkman 2021, Sparkman et al., 2017; Mortensen et al., 2019).[5] According to this scholarship, human behavior is sensitive to information about the change in collective behavior. Providing information about the change sends a signal that the behavior is important for many and that people are making an effort to comply with the new norm scenario. Moreover, the information will also lead people to pre-conform, as they expect the norm to be different in the future (Sparkman 2021). Thus, whether dynamic social norms can increase the adoption of HPV vaccines in settings where take-up rates are low is a relevant question from an academic and policy perspective.[6]

This paper analyzes the impact of a large-scale pre-specified field experiment to increase HPV vaccine uptake in Bogota, Colombia. We partnered with the Health Secretariat of Bogota (HSB) to design and implement a text message campaign that leveraged recent developments in social psychology about the role of dynamic social norms to motivate parents to vaccinate their daughters against HPV. The target population was parents with daughters between 9 and 17 years

[4] Schultz et al., (2007) is the first paper to document this issue, although their results were contested because are potentially confounded with regression to the mean (Verkooijen et al., 2015). Ritcher et al., (2018) document the presence of the “boomerang effect” in the context of sustainable consumption in Germany and Norway. Ozaki et al., (2020) find that descriptive social norms can backfire in the context of disaster prevention.

[5] Based on Sparkman & Walton (2017), we refer to a norm that communicates social information about one point in time as a static norm and one that uses more than one data point as a dynamic norm.

[6] Mortensen et al., (2017) and Sparkman & Walton (2017), the seminal papers on dynamic norms, found experimental subjects’ behavior sensitive to information about the upward change in collective behavior. They found dynamic norms impactful despite informing subjects about the descriptive norm of minority adoption.

who needed the first dose of the HPV vaccine. The HPV vaccination rate for this age group in the city was 30%, way below the 80% required to reach herd immunity (Brisson et al., 2016).[7]

Parents were randomly assigned to one of five social norm interventions, including variations of static norms (descriptive and injunctive) and two forms of dynamic norms. In this manner, we can examine two important empirical issues in the applied social norms literature. Firstly, we can assess whether static norms (either descriptive or injunctive) effectively produce the unintended effect of reinforcing the status quo in settings where the adoption of desired behavior is low. Secondly, we can investigate whether designing messages with dynamic norms outperforms interventions with static designs.

The social norms interventions tested in this paper are built based on recent advances in social psychology but designed in a way that minimizes problems of comparability across treatment arms.[8] Parents exposed to the **positive descriptive social norm treatment** (T1) receive a message with information about the number of parents in their locality that already vaccinated their daughters against HPV. This is the standard approach in a large number of social norm interventions and provides a useful benchmark to compare the differential effects of the social norm treatment analyzed in this study. The rest of the treatments are just minor variations of this basic message, which allows for a clear comparison across treatment arms.

Parents in the **negative descriptive social norm treatment** (T2) receive a message with the same information and message structure as in T1 but using a loss frame (e.g. the number of parents who lost the opportunity to vaccinate their daughters).[9] Parents in the **injunctive social norm treatment** (T3) receive a message as in T2, but the message contains an emoticon with a sad face. Parents in the **dynamic social norm treatment** (T4) receive a message with the same content as in T1 with a minor modification: a reference to a specific point in time in which

[7] Using simulations, the authors find that HPV vaccination coverage of 80% for girls and boys can eliminate all forms of vaccine-targeted HPV types. See Brisson et al., (2016) for details.

[8] Under minority behavior scenarios, there are several strategies to increase adoption using social norms. Cialdini's focus theory of normative conduct suggests making salient the injunctive norm to counter the descriptive norm of minority adoption (Cialdini et al. 1990; Schultz et al. 2007). Bicchieri and Dimant (2022) suggest highlighting small-scale examples of adoption. Furthermore, recent research suggests highlighting an upward trend in adoption (Cheng et al. 2022; Mortensen et al. 2017; Sparkman and Walton 2017; Milkman et al. 2022). This paper follows this literature to construct a variety of social norm interventions.

[9] This intervention relates to the literature that tests the effect of gain and loss framing to change health behaviors. The gain and loss framing applications have derived from prospect theory since Kahneman and Tversky (1979). However, research on the effect of these frames on the adoption of health behaviors has shown mixed results depending on whether the behavior one is trying to increase prevention or detection (Salovey and Williams-Piehota 2004). The design of our interventions is informed by this literature.

compliance with the norm is assessed. Finally, parents in the **trending social norm treatment** (T5) received the same message as in T4 with the addition of the rate of increase in HPV vaccination since the specific time reference used in T4. Because all the text messages were crafted as variations of T1, we can isolate the role of any contextual factor that may potentially interact with social norms.

We test these treatments against three control groups. These control groups are constructed in a way that allows us to distinguish the effect of the message content from the effect of receiving a message. We first consider a **pure control group** (C1) that receives no messages. Any difference between a social norm treatment and this control group does not isolate the role of getting a message from its content. Hence, we also consider an **experimental control group** (C2) that received a placebo message, which allows us to control the effect of receiving a message. Finally, the **policy control group** (C3) receives the "business as usual" message that the HSB had used in previous public health campaigns. This helps us to determine whether a design based on behavioral science is more effective than the standard communication strategies used by governments to motivate vaccination as well as to test the effectiveness of the status quo policy.

We complement our social norm intervention with a planning tool message within a factorial experimental design. More specifically, all social norm treatments were cross-randomized with a link with information about the closest vaccination point. Hence, half of the sample in each of the social norm treatment arms received a second message. We are interested in learning whether receiving both a social norm message and a second message with a link to plan a visit to the nearest vaccination point increases the likelihood of HPV vaccination.

The scale of the intervention and use of administrative data for measurement result in precise impact estimates, allowing us to test for differential impacts from alternative social norm interventions. Compared to previous studies that only measure the intention to vaccinate, the administrative data from the HSB has the advantage of allowing us to test the effect of social norms on actual levels of HPV vaccination. All the central aspects of our research design were pre-specified in a pre-analysis plan (PAP) and registered with the AEA's registry for field experiments (Maldonado et al., 2022).[10]

[10] The PAP was registered on the AEA RCT registry on February 2, 2022. It is available at the following link: https://www.socialscienceregistry.org/trials/8543. It is also available in the Online Appendix (Appendix B).

Our results indicate that dynamic social norms are very effective tools to increase HPV vaccine uptake. The estimated effect is 1.28 percentage points, strongly significant at the standard confidence levels. This implies that, compared to the control group, parents exposed to dynamic social norm treatments are 23% more likely to vaccinate their daughters against HPV. These are relatively large effects in comparison with similar studies on the topic and provide strong evidence about the power of dynamic norms to enhance vaccination. However, the injunctive norm holds equal potential to elicit similar effects on vaccination. Additionally, even the negative descriptive norm exhibits a moderate effect on HPV vaccinations, resulting in a 12% increase compared to the control group. While standard descriptive norms do not impact HPV vaccination rates, they also do not display a negative effect, suggesting that concerns about the "boomerang effect" in norm-minority settings may be overstated. If anything, descriptive norms appear to be harmless in the worst-case scenario.[11] It is important to acknowledge that the absence of evidence in our study does not invalidate the potential practical relevance of these concerns in other settings. Rather, we advocate for further exploration of these issues within specific settings.

We also find that there are no complementaries between the social norm interventions and the planning tool, although the estimated coefficients are relatively large. As it is well known, sample sizes needed to estimate interactions are prohibitively large, so we cannot rule out this possibility. Interestingly, all the coefficients associated with these interactions were negative, indicating that receiving a second message might have a counterproductive effect. We also do not find evidence of heterogeneity responses in our setting. All of our results remain robust after implementing a battery of robustness checks. These checks include accounting for a low number of clusters, incorporating covariates through ad-hoc methods or a double-LASSO (Least Absolute Shrinkage and Selection Operator) algorithm, addressing contamination bias, and correcting for multiple hypothesis testing.

This paper contributes to a large scholarship documenting the effects of interventions motivated by behavioral science on socioeconomic outcomes (Thaler & Sunstein 2021). In particular, this study belongs to the growing literature on the effect of social norms on a variety of

[11] In the full factorial design, we even observed a substantial 16% effect for the positive social norm treatment among the subgroup of participants who did not receive the second message. Taken together, the evidence presented here calls into question the empirical validity of concerns regarding the "boomerang effect" in our setting.

economic and social outcomes (Nolan et al., 2021).[12] Most closely related to this paper is the use of dynamic norms to increase the adoption of a minority behavior (Sparkman 2021; Mortensen et al., 2017; Sparkman & Walton 2017). This paper contributes to the growing literature on the applications of dynamic norms to increase minority behaviors in different fields. Perhaps the closest antecedent to our study is Milkman et al., (2022), who evaluate the impact of dynamic norms on vaccinations with a field experiment. However, unlike our study which focused on different forms of social norms to isolate the role of their static and dynamic components, Milkman et al., (2022) only tested two dynamic norms among many other non-norm interventions on influenza vaccinations. This paper contributes to previous research to address whether social norms can increase the adoption of HPV vaccination, a behavior that a majority has not yet adopted. It also leverages an upward adoption trend in HPV vaccination to test the effect of dynamic norms. Finally, it tests whether social norms have differential effects on vaccination based on various frames.

This paper also contributes to the emerging scholarship within economics and related social sciences concerning the utilization of behavioral science principles to facilitate the uptake of vaccinations (Betsch et al., 2015; Brewer et al., 2017). Previous scholarly endeavors have effectively demonstrated the efficacy of various strategies such as leveraging identity (Alsan et al., 2024), employing celebrity endorsements (Alatas et al., 2024), utilizing social networks (Athey et al., 2023; Bodine-Baron et al., 2013; Banerjee et al., 2021), implementing micro-incentives (Banerjee et al., 2021), employing reminders (Milkman et al., 2021; Banerjee et al., 2021), and employing implementation intentions (Milkman et al., 2011). Our contribution to this body of literature lies in the utilization of dynamic norms within a context characterized by low compliance with established norms about vaccination.[13]

This study is also motivated by current practices in the applied behavioral science literature concerning vaccination. While there is a substantial body of literature leveraging behavioral

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[12] Scholars have studied how information about compliance with the relevant norm elicits or changes social expectations, ultimately resulting in an increase or decrease in the adoption of behavior in a variety of scenarios such as energy conservation (Allcott 2011; Allcott & Rogers 2014), education choices (Cheng et al. 2022), donations (Alpizar et al. 2008; Smith et al. 2015), civil servants' performance (Dustan et al., 2023), sustainable food consumption (Bergquist & Nilsson, 2018), female labor supply (Bursztyn et al. 2020), littering (Cialdini et al. 1990), climate change (Constantino et al., 2022), tax compliance (Coleman 2007), voting (Gerber & Rogers 2009), vaccination (Moehring et al., 2023; Milkman et al., 2022), and health preventing behaviors (Zhang et al., 2022).

[13] Rabb et al., (2022) also provide evidence of the null effects of behavioral interventions on COVID-19 vaccination uptake.

insights to promote vaccination across psychology, public health, and medical sciences (Reñosa et al., 2021; Brewer et al., 2017), it's notable that most studies fail to measure the effects on actual vaccination rates. Instead, they tend to focus on intermediate outcomes such as intentions or beliefs. Moreover, few studies are based on credible randomized controlled trials, and those that are often utilize very small sample sizes.[14] Recently, there has been growing interest among scholars in "megastudies" as a means to address some of these issues (Milkman et al., 2021a). This is a very important and welcome development to enhance rigor and maximize policy impact. Well-implemented "megastudies" help to minimize publication bias and foster the discovery of novel approaches to solving pressing policy issues. However, these studies typically involve a large number of independent teams testing a vast array of interventions, often lacking a clear theoretical foundation. While they typically use large sample sizes, the sheer volume of interventions means that each treatment condition is allocated a relatively modest sample size (typically below 2,000-2,500 participants). Additionally, most of these studies are conducted exclusively in developed countries and with non-representative samples, raising concerns about external validity.[15]

Our study aims to overcome several of these limitations. We focus primarily on actual vaccination rates, employing a robust experimental design with a large sample. In this regard, our study aligns with the recent trend of "megastudies." However, we diverge from this scholarship by testing a smaller number of interventions rooted in a well-defined theoretical framework, in a large and representative sample drawn from the eligible population of a major city in a developing country.[16] Each treatment condition in our study benefits from a relatively large sample size, enhancing the statistical power and reliability of our findings. In this sense, our study is closely

[14] Not surprisingly, a recent systematic review lamented that the "…current small number of studies and the high variability with regard to quality, methods, measurement of vaccine hesitancy, and outcomes across studies do not allow for a meaningful meta-analysis to ascertain its primary effects on vaccination uptake" (Reñosa et al., 2021).

[15] For instance, a very influential study by Milkman et al., (2021a) in the US tests the effects of 54 different interventions to promote exercise with a sample of 61,293 members of an American fitness chain. The sample size for each intervention was 1,334 on average. Another equally influential study by Milkman et al., (2021b) tests the effect of 19 nudges on vaccination with 47,306 patients of two large health systems in the US. The sample size for each intervention was 2,489 patients on average. These relatively low sample sizes can affect our ability to learn from these experiments. For instance, in the latter paper, the authors claim that the "…best-performing intervention in our study reminded patients twice to get their flu shot at their upcoming doctor's appointment and indicated it was reserved for them." However, they also report that "…we cannot reject the null hypothesis that all 19 effects have the same true value." Whereas the authors use some post-hoc analysis to reach their main finding, the lack of power to distinguish across different treatment arms is a relevant issue.

[16] In a very complete metanalysis of behavioral interventions for vaccination, Malik et al., (2023) identify only 9 out of 155 studies about the use of reminders in developing countries. They also identify only 3 out of 98 studies about the use of message framing.

related to a recent wave of vaccination experiments in economics (Alatas et al., 2024; Banerjee et al., 2021; Ho et al., 2023).

Finally, this paper also contributes to a growing scholarship that uses technological solutions to improve the adoption of health technologies including vaccines. Scholars have explored the potential of telemedicine, mobile health, electronic health records, and artificial intelligence on this matter. Some examples include the use of mobile applications (Atkinson et al., 2016, Fadda et al., 2017), interactive web-based tools (Betsch et al., 2012), or social media interventions (Athey et al., 2023; Alatas et al., 2024). This paper also adds to a nascent literature about experimentation at scale (Muralidharan & Niehaus, 2017; Davis et al., 2017).

The remainder of this paper is structured as follows: Section 2 furnishes details concerning HPV and the context in which the experiment was conducted. Section 3 outlines the research design, describes the data used, and assesses the quality of the experimental design. Section 4 delves into the econometric specification and the statistical analyses conducted to test the main and auxiliary hypotheses of this study. Section 5 presents the primary results, along with robustness checks and supplementary analyses. Section 6 offers a cost-effectiveness analysis. Finally, Section 7 provides concluding remarks.

2. Background

2.1. Cervical cancer and HPV

Cervical cancer is the fourth most common cancer in women worldwide, and it is one of the three most frequent cancers in women younger than 45 (D'Oria et al., 2022). In Colombia, new cervical cancer cases represented 7.9 percent of all cancer cases in 2020, equivalent to 4,742 cases in that year (Cordoba-Sanchez et al., 2022). According to the Ministry of Health in Colombia, cervical cancer is the leading cause of death from cancer in Colombia's women aged 30 to 59.

Almost all cervical cancers are caused by HPV (Walboomers et al., 1999). In addition to cervical cancer, HPV is associated with oropharyngeal, anus, genitals, head, and neck cancer. Estimates show that 75 percent of women and men who are sexually active will acquire HPV in their lifetime (Mavundza et al., 2021). Fortunately, the risk of HPV infection and the development of cervical cancer can be greatly reduced through an HPV vaccine (WHO 2017).

The economic consequences of cervical cancer are important. Cervical cancer is associated with higher healthcare expenses and income losses, for the patient and her family. For governments, it is linked with productivity losses and increased healthcare costs. In the United

States (Shah et al., 2020), cervical cancer is estimated to cause a 100% increase in average health expenditures (USD 10,031 for a cervical cancer patient versus USD 4,913 for non-patients). In Colombia, Tapiero (2007) approximated that the diagnosis and treatment of cervical cancer incurred expenses amounting to USD 9,053 in 2004, which was 3.2 times the country's GDP per capita for that year.[17]

2.2. Setting

Colombia is a high-middle-income country with almost universal health coverage. Life expectancy at birth (78.2) is higher than the average of Latin American (LAC) countries (77.4) and health spending per capita is USD 960, slightly below the regional mean (USD 1,026). In terms of women´s empowerment, Colombia ranks 22 out of 153 countries in terms of the global gender gap index, an indicator designed by the World Economic Forum to measure progress towards the gender equality goal. This good performance is driven by significant progress in terms of educational attainment and health conditions where gender gaps have disappeared (Maldonado 2024). However, important gaps remain in terms of health outcomes. Colombia falls short of the LAC countries' average concerning measures of quality care. Specifically, the five-year survival rates for breast (72), cervical (49), and colon (35) cancers trail below the LAC regional averages (OECD/The World Bank, 2020). Thus, although significant progress has been made in expanding coverage, there is room for improvement on quality issues.

The Colombian healthcare system is widely recognized as one of the best in Latin America. According to a WHO report, Colombia ranked 22nd among 191 countries in terms of health system performance in 2000 (Tandon et al., 2000). This is partly attributed to the enactment of Law 100 in 1993, which brought about a fundamental shift in the healthcare system.[18] Within just a few years of the reform, healthcare coverage increased from 21% in 1993 to 65% in 2003. By 2023,

[17] Direct costs associated with cervical cancer cover expenses for consultations, laboratory tests, biopsy, cystoscopy, chemotherapy, and drugs. Indirect costs include costs associated with morbidity and premature death.

[18] One of the pivotal aspects of Law 100 was the introduction of private health providers, known as Entidades Promotoras de Salud (EPS), into the healthcare market. Another significant aspect of the health reform was the establishment of a subsidized regime to cover the poorest population. Consequently, Colombia has developed a robust health system capable of addressing many of the challenges faced by health systems globally. For instance, Colombia's health system demonstrated excellent performance in handling the COVID-19 pandemic. This is evident in the Global Health Security Index, which assesses countries' preparedness for pandemics and epidemics. Colombia ranks 38th among 195 countries in this index, placing it behind only Mexico, Chile, Argentina, and Peru in the region (Bell & Nuzzo, 2021).

Colombia had achieved almost universal coverage, with 95% of its population covered by a core set of services.[19]

Colombia was among the first countries in South America to implement HPV vaccination. Since 2012, the Colombian government has administered the HPV vaccine through the Expanded Program on Immunization (PAI). This vaccine is targeted and free for girls between 9 and 17. Although individuals can be affiliated with a private insurer or covered under the subsidized regime, the country’s health system allows citizens to be vaccinated at any vaccination point regardless of their health provider.

In 2012, Colombia was one of the leaders in HPV vaccination coverage in Latin America (Cordoba-Sanchez et al., 2022). However, the country’s vaccination program's success came to a halt after an outbreak of unknown etiology in the municipality of Carmen de Bolivar. Similar incidents have occurred in Denmark, Japan, and Australia (Simas et al., 2019). Although safety studies found no association between the HPV vaccine and Carmen de Bolivar’s events, vaccine coverage rates began to decline steadily, reaching their lowest point in 2016 (Cordoba-Sanchez et al., 2022). Coverage levels of HPV vaccination have been recovering over the past years (Figure A.1 in the Online Appendix). However, they are still far from the pre-Carmen de Bolivar levels, representing a challenge for the vaccination policy in Colombia.

Bogotá, the capital city of Colombia and the setting for this study, exhibits a poor performance in terms of HPV vaccination coverage. According to official statistics, Bogotá's HPV vaccine coverage was 25.7% before the intervention.[20] These averages conceal significant heterogeneity across the city, with eligible girls from impoverished neighborhoods having lower vaccination rates. Therefore, our intervention is targeted towards improving access to HPV vaccines in areas where it can yield substantial long-term well-being benefits

3. Research design

3.1. The intervention

We partnered with the HSB, La Liga Colombiana Contra el Cancer, the Behavioral Government Lab, the Inter-American Development Bank, and the American Cancer Society to

[19] According to the OECD, Colombia also has a very low outpocket expenditure, 13.7% of healthcare expenditure. Its coverage for DTP and measles vaccines is high for Latin American standards. For more details, see the OECD Health at Glance country note for Colombia in the following link: https://www.oecd.org/colombia/health-at-a-glance-Colombia-EN.pdf

[20] To provide context, Antioquia—the wealthiest department in the country—achieves an HPV vaccine coverage rate of 47.8%. Bogota is also slightly below the national level of HPV vaccine coverage (33.1%).

implement a field experiment to offer solutions to the aforementioned challenges. As part of this project, we conducted qualitative work to understand the drivers and barriers behind HPV vaccination in Bogota, Colombia. With those insights and a careful reading of the psychological and medical on this matter, we designed a large text message campaign that tested the effectiveness of alternative social norm framings.

The selection of the text message campaign as our experiment delivery was informed by the technological structure of the HSB and its vaccination efforts. The HSB had in the past run text message campaigns to increase vaccinations, but not HPV vaccinations. Due to the current institutional framework in Colombia, health providers report data to the HSB about all eligible individuals for vaccination. These include information about their progress in terms of recommended vaccinations. This centralized information system was instrumental in evaluating the effectiveness of our interventions.

Our field experiment exploits alternative ways to communicate social norms through text messages to increase HPV vaccinations. The challenging context of this social norms experiment is the minority adoption nature of HPV vaccinations in Bogota, i.e., only 30% of the population vaccinated their daughters against HPV in 2020. However, there has been a 128% increase in vaccination rates in Bogota since 2016. We leverage those statistics to design the content of our social norm messages.

Figure 1 presents the treatment arms and their respective sample sizes. The experimental sample is composed of 34,506 parents and was randomly selected from a population of eligible parents. Section 3.3 describes the sample design in detail. The experiment consists of sending weekly messages to the target population's parents over eight weeks through an SMS online platform between October 21 – December 14, 2021. The content of the message remains constant throughout the weeks. We discuss the content of these messages below. The timeline and exact day of text message delivery during the intervention are reported in Figure 2.

Table 1 presents the messages delivered as part of this intervention. Recall that our main goal is to test whether dynamic norms are an effective tool for promoting HPV vaccination in settings with low levels of compliance with the desired behavior. Hence, we consider a variety of static norm designs for comparison. Following a large literature on social norms, we consider two variations of static norms: descriptive and injunctive. The descriptive norm design is very popular among researchers and provides information about the level of compliance of the desired behavior in the reference group.[21]

Parents exposed to the **positive descriptive norm treatment** (T1) receive a text message with a norm of the following form: "Hello [Name of parent]. 3 of every 10 parents in your neighborhood vaccinated their daughters against HPV and protected them against cancer. Secretariat of Health". Notice that this variation uses a *gain frame* to communicate the benefits of protection against cancer derived from HPV vaccination. According to some scholars (Salovey & Williams-Piehota 2004), one can expect a gain-framing message to be a better tool for increasing HPV vaccinations. However, because HPV vaccination is adopted only by a minority in our setting, this may not be empirically true.

We also tested a variation of the previous message where the information was provided in terms of the failure of compliance with the relevant norm. In this treatment arm, parents exposed to the **negative descriptive norm treatment** (T2) receive a text message with a norm of the following form: "Hello [Name of parent]. 7 of every 10 parents in your neighborhood lost the opportunity to vaccinate their daughters against HPV and protected them against cancer. Secretariat of Health". This variation takes advantage of findings from the prospect theory. Kahneman & Tversky (1979) postulate that the expected negative utility is greater when losing a given amount than the positive expected utility from gaining the same amount.[22] In consequence, we use this variation to test whether a *loss frame* is more effective than the *gain frame* used in T1.

Finally, we also consider an injunctive norm. Previous scholarship has shown that injunctive norms can overcome the shortcomings of descriptive norms on minority behaviors (Allcott 2011; Schultz et al., 2007). Jacobson et al., (2024) suggest that injunctive norms trigger self-reflection and effortful self-regulation that might compensate for the automatic perception of descriptive norms. A common way in the literature to insert injunctive norms into norm-based messages is by adding an emoticon to transmit an accepted behavior.[23] Thus, parents exposed to

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[21] Starting from the foundational work by Cialdini et al., (1990), a large scholarship has documented the importance of descriptive social norms for behavioral change in topics as diverse as environmental conservation, health behaviors, energy conservation, and anti-smoking campaigns. Cialdini (2021) offers an overview of this literature.

[22] Previous research has found mixed evidence on the impact of framing on health behaviors (Salovey & Williams-Piehota 2004). Based on prospect theory, Salovey & Williams-Piehota (2004) show that if the health behavior is illness-detecting, a loss frame message would be more impactful at increasing that behavior. On the contrary, a gain-frame message would be more impactful when the health behavior is illness-preventing.

[23] For instance, Schultz et al., (2007) and Allcott (2011) use emoticons to dissuade clients from consuming more energy when learning that their neighbors consume more energy than them. Bhanot (2021) experimentally finds that emoticons increase the impact of norm-based messages in water conservation due to their injunctive norm message.

the **injunctive norm treatment** (T3) receive a text message with the exact content as in T2 with the inclusion of the emoticon as shown in Table 1.

We contrast these static versions of social norms with dynamic versions that incorporate information about the change of the desired behavior in a reference period. Dynamic norms highlight an increasing or decreasing change in the adoption of a behavior over time (Nolan et al., 2021; Sparkman 2021).[24] Norm-based interventions that contain dynamic norms have shown promising results in increasing minority (Milkman et al., 2022; Mortensen et al., 2017; and Sparkman & Walton 2017) and even majority behaviors (Dustan et al., 2023).[25]

We study a variation between two dynamic normative interventions, herein referred to as **dynamic norm treatment** (T4) and **trending norm treatment** (T5). Both interventions incorporate a temporal reference point, specifically set at 2016. The difference between these two norms resides in the inclusion of a percentage change in the adoption of minority behavior. To be more specific, the dynamic norm was framed in the following way: "Since 2016, 3 of every 10 parents in your town began vaccinating their daughter against HPV, protecting them from cancer." On the other hand, the trending norm was framed with the following structure: "3 of every 10 parents in your town vaccinated their daughter to protect them from cancer, an increase of 128% since 2016." The differences are minor with respect to the standard descriptive approach, yet recent scholarship suggests that these changes can have powerful effects on behavior.[26]

Drawing on the methodologies proposed by Mortensen et al., (2017) and Sparkman & Walton (2017), our dynamic norms are designed to incorporate the levels of minority adoption of HPV vaccination. Notice that our design explicitly integrates the descriptive norm, ensuring that any observed differences between the descriptive and dynamic normative frameworks can be attributed solely to the inclusion of the temporal reference point and the percentage change.[27]

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[24] The recent literature uses various terms to call the same type of changing social information. Mortensen et al., (2017) coined them *trending norms*, while Sparkman & Walton (2017) named them *dynamic norms*. More recently, Milkman et al., (2022) call it a *growing norm*. In this paper, we will refer to the umbrella of these norms as *dynamic norms*, following Nolan et al., (2021).

[25] Dustan et al., (2023) use a qualitative dynamic norm to boost civil servants' performance in Peru in a majority setting. Among several behavioral interventions, the dynamic norm was identified as one of the most effective tools.

[26] There is growing literature on field experiments testing dynamic norms, mostly outside economics. These include papers on sustainability (Mortensen et al., 2017; Sparkman & Walton 2017), education (Cheng et al., 2022), political economy (Dustan et al., 2023), and vaccinations (Milkman et al., 2022).

[27] Because there are few papers in the dynamic norms literature, scholars like Cheng et al., (2022), Dustan et al., (2023), and Milkman et al., (2022) have tested different ways to incorporate dynamic elements in their norm-based interventions. We focus on these two elements following the discussion of the literature provided by Nolan et al., (2021).

Disentangling the effect of receiving a message from the effect of being exposed to a behavioral principle through a message is a key challenge that scholars face when designing social norm interventions. To make clear the distinction between these two effects, we first consider a **pure control group** (C1) that receives no messages. Comparing a social norm treatment with the pure control group does not allow us to isolate the role of receiving a message from the message content. This is important because one concern is that the norm intervention can serve just as a reminder regardless of the message content. For this reason, we also include an **experimental control group** (C2) that includes some behavioral elements (e.g. message personalization and an identification of the sender) that are similar to all the other social norm treatments described above. In this way, we can isolate the role of all the other message components from the norm component and rule out the possibility that the messages serve only as reminders.

Finally, behaviorally motivated interventions do not occur in a vacuum. They usually take place within environments where governments have previously attempted partial solutions to the mentioned policy issues, often without rigorous research designs. Policymakers might find it beneficial to assess the effectiveness of these prior solutions and determine if the alternative approaches proposed by scholars yield better results than what has already been attempted. Hence, we also consider a **policy control group** (C3) that replicates the type of messages that the government used to motivate vaccination in the target population in the past. This helps us to determine whether a design based on behavioral science is more effective than the standard communication strategies used by governments to motivate vaccination as well as to test the effectiveness of the status quo policy.

Measuring the impact of social norms interventions through text messages poses challenges, as researchers may unintentionally introduce additional elements into their treatments that could unexpectedly influence behavior. Research in fields such as psychology and communication studies has shown that factors like word choice, sentence structure, and formatting can impact how a message is received and understood.[28] Therefore, we design our social norm interventions using words carefully chosen to ensure comparability across treatment conditions, as

[28] For instance, Bryan et al., (2011) find that small wording differences in message-based behavioral interventions have large effects on turnout rates. Specifically, the authors study whether wording survey items encourages subjects to think of themselves as voters rather than as voting (verb treatment). They find a large positive effect on voter turnout. More related to our interest in social norms, Orvell et al., (2019) find that that a subtle linguistic cue, the generic usage of the word “you,” affects how people interpret norms.

discussed above. By controlling for the role of other contextual elements in the text message content, we expect to minimize this risk. Furthermore, we constructed all treatment arms based on the **positive descriptive norm treatment** (T1). We have deliberately circumvented the potential risk of introducing elements in the wording of the messages that could hinder a comprehensive comparison across treatment arms regarding the normative content. Therefore, by avoiding the introduction of elements that might cause unexpected behavioral responses and by clearly comparing different normative content, we can isolate the impact of specific elements used in framing the text messages within our interventions.

Notice that all social norm interventions and the placebo message delivered for the experimental control group include two fixed elements found effective in other settings: the name of the recipient and the sender's information, in this case, "Secretariat of Health" (Constantino et al. 2021; Bursztyn et al., 2020). The policy control group receives a message that is not personalized.

One of the caveats of our experimental design is the lack of elicitation of the target population's social expectations. Bicchieri (2017) deems it necessary to measure social expectations and conditional preferences before implementing social norms interventions. As in Schultz et al., (2007) and Allcott (2011), there exists the risk of reinforcing the low levels of HPV vaccination for parents whose beliefs about HPV vaccination prevalence are higher than the descriptive norm communicated. However, measuring social expectations is unfeasible in population-level and large-scale experiments due to cost and logistical reasons. For this reason, we design our experiments in a way that a clear comparison between the descriptive and dynamic elements of the norm-based intervention can be established. In this way, we can isolate the role of each element and evaluate whether this concern is empirically relevant.

These norm-based interventions were cross-randomized with a complementary intervention that provided simple tools to facilitate the process of planning vaccination. A subset of our sample received an additional message with the following content: "Make an appointment to vaccinate your daughter against HPV at the nearest vaccination site: https://bit.ly/ssaludbog. Secretariat of Health." By providing a link with information regarding the nearest vaccination site, we expect to reduce the intention-action gap among parents. Although we pre-specified in our PAP the focus on the "short model" of our factorial design, we will also discuss the results of the "long model" in section 5.[29]

This experiment was implemented within the regular communication policy of the SHB. Participants are not informed that they are part of these experiments. This is standard practice for government interventions, and IRB approved it.

3.2. Experiment

We implemented a randomized controlled trial to test the effectiveness of the norm-based messages described above on HPV vaccination rates. More specifically, we implemented a factorial randomized design where the social norm treatments were cross-randomized with a simple tool for planning vaccination. Each factor of this experiment consists of a stratified trial at the parent level (Imbens & Rubin 2015, and Gerber & Green 2012). One of the girls' parents (typically their mothers) was assigned to one of the treatment or control groups. This decision was driven by the information available in the HSB's records, which typically collects information about mothers. Notice that each participant was assigned to only one of the aforementioned treatments.

We used the rerandomization algorithm developed by Morgan & Rubin (2012). This algorithm avoids the risk of pre-treatment imbalance for a given set of covariates by allowing treatment re-randomization without affecting the design's statistical properties. Stratification was defined by locality (19 out of 20 localities, excluding Sumapaz due to a small number of eligible girls) and vaccinee's age. Because stratification was based on age, we constructed an indicator variable to avoid the "curse of dimensionality," typically associated with using a categorical variable. A dummy variable equal to 1 for girls aged 9 and 10 years old was used for the stratification. On average, these dummy variables split the experimental sample in half.

The main outcome for the analysis is a binary measure of whether a parent's daughter is vaccinated against HPV during the SMS campaign window (8 weeks). Using an actual measure of vaccination is an advantage in comparison with many studies that only measure intentions.[30]

[29] Our focus on the "short model" is justified because we are interested in testing many variations of the same treatment, as we will discuss later. See Cochran & Cox (1957) for details.

[30] In a recent systematic review examining interventions aimed at increasing vaccine uptake for COVID-19, Batteux et al. (2022) found that only 7 out of 39 studies measured actual vaccination rates. Similarly, a review conducted by Reñosa et al. (2022) on the utilization of behavioral science to enhance vaccine uptake in general revealed that only 14 out of 48 studies specifically addressed actual vaccination behavior. Consequently, a mere fraction of interventions aimed at promoting vaccination prioritize assessing the relevant behavioral outcome.

Recent scholarship has raised concerns about how factorial designs are analyzed and implemented in practice (Muralidharan et al., 2023). Our design is subjected to these issues because of the cross-randomization of planning tools in our experimental design. Following Muralidharan and coauthors, we will estimate the "long model", but our main focus is on the "short model" for the main results in the paper.[31] This decision was pre-specified in our PAP (Maldonado et al., 2022). Our interest in our experiment is the main effects, but it is also of secondary interest to evaluate whether there are positive complementarities between norm-based interventions and planning tools. We emphasize here that these results are weighted averages of interactions with other treatments for those specifications based on the "short model."

The sample size by treatment arm is around 4,600 observations. In our power calculations, we assumed an effect size of 3 percentage points change in the vaccination rate for an individual randomized design. This considers a test for differences in proportions (Chi2 test), assuming 90 percent power, and accounts for multiple comparisons using a Bonferroni correction (allowing up to 17 comparisons). A minimum sample size of 13,578 observations is estimated using the above parameters. The sample analyzed in this experiment is 34,506 participants.

It is important to note that non-compliance with treatment status can be an issue given the nature of our intervention. The registered cellphone numbers may not be updated or may be inactive for various reasons. Due to our experimental design, these issues should be evenly distributed across treatment groups to ensure they do not compromise the internal validity of our research. Furthermore, the technology used to deliver text messages does not enable us to confirm whether individuals assigned to treatments actually read the messages. Consequently, we interpret our results as intention-to-treat (ITT) estimates.

3.3. Data and sample

The target population for this intervention consists of parents with unvaccinated daughters ages 9-17 registered with a cellphone number in the administrative records of the HSB. The administrative records are pulled based on girls between 9-17 years who were pending the first

[31] The nature of our interventions justifies our focus on the "short model" for the analysis. As recognized by the technical literature in experimental designs, analyzing the "short model" in factorial designs is appropriate when evaluating the effectiveness of many potential treatments or many variations of the same treatment (Cochran & Cox 1957). In this scenario, researchers are interested in learning what works among many alternatives and further testing the power of promising approaches to affect outcomes. Our experiment is based on the same principle because they are part of a learning process to define which behavioral interventions may be more appropriate to boost vaccination rates in our setting.

HPV vaccine. Based on the administrative records, we determined that the population of unvaccinated girls aged 9-17 in the city was 440,010 in September 2021. Because Colombia has almost universal health coverage and all institutional providers are required by law to provide information to the HSB, we believe administrative records are of good quality in terms of coverage.

We constructed the sampling frame for our experiment from this population. Figure 3 describes the steps in constructing the sampling frame. Our inclusion criteria are Bogota residency, the record of at least one parent, and a valid cellphone number of the parent. Due to our block-randomized design, we need information about the parents, the locality, and the vaccinee's age as well as other pre-treatment covariates. We discarded 216,371 records due to incomplete information about parents. Since this is an intervention based on text messages, we dropped those girls whose parents' phones do not appear on the database. 63,602 observations were discarded in this step. We also dropped all the observations from neighbor localities outside Bogota or records without information regarding their locality. We also eliminated records from Sumapaz, a very small locality in Bogota, with only 41 observations. The final sampling frame for this experiment with unvaccinated girls is 131,124 records. Figure A.3 in the Online Appendix presents a map with the distribution of the sampling frame across localities in Bogota. Most parents are located in poor and populated localities. We selected a random sample of 34,506 observations from this sampling frame.[32]

It is important to acknowledge the limitations of the data. Although the administrative records are of good quality in terms of coverage, as discussed above, they were incomplete in many relevant covariates. As a consequence, we lost almost half of the eligible parent population. The completeness of records is endogenous, which may affect the external validity of our ITT estimates. Because the set of available covariates in the HSB's administrative records is limited, we are unable to fully explore this sample selection issue in detail. Accordingly, we lack the data to evaluate the severity of this issue. Of course, this does not affect the internal validity of our research design.

Another concern is the quality of information regarding cellphone numbers. This is crucial because treatments would be delivered via text message. In Colombia, it is not uncommon for individuals to change their cellphone numbers for various reasons. To address this concern, we

[32] The rest of the observations in the sampling frame were allocated to companion experiments. The PAP describes these companion experiments. See Maldonado et al., (2022) for details.

randomly selected a sample of 100 records and contacted them by phone to verify their accuracy against the information available at the HSB. We confirmed that approximately 40% of the respondents' information matched the data available at the HSB. Additionally, around 31% of respondents did not answer the call, preventing verification of whether this was due to incorrect numbers or simply non-response. We are not aware of previous efforts to measure this issue to have a comparative perspective on the matter. Furthermore, it is plausible that many surveyed individuals are more inclined to check messages than answer calls, as suggested by some evidence in marketing and opinion polls.[33]

3.4. Summary statistics and pre-treatment balance

Table 2 presents the descriptive statistics of the available variables in the database. The average age of vaccine recipients in the sample is 11 years old, which aligns with the SHB's preference for girls aged 9 to 11. Regarding EPS membership, 19.3% of our sample belongs to Famisanar EPS, which is nearly identical to the 19.5% reported for the entire city in December 2021. There are variations among EPSs, with certain ones having a smaller representation in our sample compared to the city's averages (such as Compensar EPS and EPS Sanitas), while others have a larger representation in our sample than in the city's population (e.g., Capital Salud EPS). About 77% of the sample is covered under the contributory scheme. Only 3.8% of the sample is uninsured, consistent with the fact that healthcare coverage is almost universal.

In terms of demographic characteristics, 98.9% of the sample are Colombian, with less than 1% identifying themselves as members of an ethnic group. Only 1.6% reported being displaced by the armed conflict. Utilizing location information provided by the parents, mainly addresses, we reconstructed the socioeconomic stratum, a commonly used proxy for well-being in the city to determine local social policies such as subsidies and transfers. In our sample, over 90% belonged to strata 1 to 3. These strata, eligible for subsidies, are generally considered low-income groups. Ergo, this suggests that our experimental sample is largely composed of low-income people.

Table 3 shows that treatments exhibit balance across observable characteristics within the sample. This table presents the mean and standard deviation for all relevant covariates by treatment arm. As anticipated, the reported means demonstrate similarity across treatment arms for most pre-

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[33] Many opinion polls and newspapers articles have highlighted this issue. See, for instance, https://leadferno.com/blog/survey-texting-is-the-preferred-way-to-communicate

treatment characteristics. In Column 9, we present the p-values associated with a joint orthogonality test for all control groups, revealing no significant imbalances across control groups for any of these covariates. Column 10 further displays the p-values for a joint orthogonality test encompassing all treatment arms (i.e., both control and treatment groups). We find evidence consistent with pre-treatment balance for all covariates, except for the indicator variable denoting membership to EPS Capital Salud. We have reasons to consider the lack of balance as a statistical artifact in this case. Overall, we observe that the reported means and standard deviations are quite similar across treatment arms. Hence, any statistical differences are likely to be explained by the sample size. Consequently, we find no evidence of differences in pre-treatment characteristics, indicating that our design possesses a high degree of internal validity.

Tables A.1 and A.2 in the Online Appendix present the results of all 2 by 2 comparisons across treatment arms using standard t-tests. Among 400 comparisons, 27 differences are statistically significant at the 90 percent confidence level. Nevertheless, these discrepancies are minor and are likely attributable to multiple hypothesis testing. Consequently, the internal validity of our research design is deemed high.

As noted by Imbens (2015), conventional t-tests can be influenced by large sample sizes, leading to an increased risk of detecting false significant results unrelated to actual differences between the empirical distributions of the variables under comparison. Standardized tests offer a remedy to this issue. In our PAP, we specified the use of standardized tests due to the sizable sample size in our study. To streamline our presentation, we aggregate the treatment variables into a single dummy variable, which takes the value of 1 if the parent is exposed to any of the social norm treatments. In Figure 4, we compare this aggregated treatment variable against the pure control group. Our analysis reveals that none of the pre-treatment covariates exhibit imbalances exceeding the 0.3 absolute difference threshold proposed by Imbens (2015). Moreover, these findings remain consistent across alternative thresholds suggested in the statistical literature, such as 0.2 or 0.1. All estimated differences approach zero, providing further evidence of the robust internal validity of our research design.

Figure A.4 in the Online Appendix shows the results of the same exercise using the experimental (a) and the policy control group (b). The evidence is consistent with the results reported here.

4. Empirical model

4.1. Basic model

The impact analysis is based on a standard ITT analysis. Recall that the main outcome variable is a binary measure of whether a parent's daughter is vaccinated against HPV during the text message campaign window. Also, remember that the software we use to send the text messages does not allow us to identify who receives or reads the messages. Thus, a treatment-on-the-treated (TOT) analysis is not possible.

We estimate (pre-specified) models of the following form:

$$(1)\quad Y_{ij} = \sum_{l=0}^{L} \lambda_l C_j^l + \sum_{k=1}^{K} \beta_k Z_j^k + \theta_s + X_{ij}^{'} \delta + \varepsilon_{ij};$$

where Y_{ij} is the main outcome of interest for vaccinee i from parent j measured two months after the end of the SMS campaign. Z_j^k is an indicator variable for a parent being assigned to one of the k social norm treatments. C_j^l is an indicator for a parent being assigned to one of the l control groups. θ_s is a vector of randomization strata dummy variables (locality*age), X_{ij} is a set of pre-treatment covariates at the vaccinee and parent level, and ε_{ij} is the error term for vaccinee i from parent j. β_k captures the ITT effect for each social norm treatment arm, which is the effect of being selected to receive a text message based on a social norm treatment.[34] To facilitate the interpretation of our results, we use the pure control group as a benchmark, so we exclude the indicator variable equal to 1 for those assigned to the pure control group in the main equation to avoid the dummy trap.[35]

We originally planned in our PAP clustering standard errors at the parent level, given that randomization was implemented at this level conditional on locality and age. However, this methodological decision had minor consequences as all parents have only one daughter in the relevant age for HPV vaccination in our sample. Technically there is no need to cluster the standard

[34] The version equation presented here is slightly different than the one proposed in our PAP. These changes are only introduced for clarification.

[35] We proposed to use the experimental control as the reference group in our PAP. We chose to present the main results of our experiment in terms of the pure control group to facilitate interpretation. This decision has no implications in our analysis because, as we discuss in section 5, we find no differences between the pure and the experimental control groups.

errors because our intervention is equivalent to a randomized individual design (Abadie et al., 2023). Yet we decided to adopt a more conservative approach by clustering standard errors at the locality level. This decision was made due to the likelihood that parents or girls from the same neighborhoods share characteristics, so the assumption of independence in the individual randomized design is violated. As suggested by MacKinnon et al., (2023), in scenarios like this, it may be safer to cluster at a coarser level than the one in which the treatment was assigned.

4.2. Robustness checks

Estimating equation (1) represents a series of methodological challenges that need to be addressed. First, it considers many control and treatment groups, creating issues of multiple inference. To address this issue, we pre-specified in our PAP the use of the Benjamini and Hochberg (1995) procedure to control for the false discovery rate (FDR), i.e. the proportion of false positives among all significant results. We implement the algorithm proposed by Anderson (2008) to estimate the q-values for all the regressions to be estimated.[36]

Secondly, clustering standard errors at the level of locality may not work well in our setting. The cluster-robust variance estimator (CRVE) is only consistent when the number of clusters goes to infinity, the within-cluster error correlations are the same for all clusters, and each cluster has the same number of observations (MacKinnon & Webb 2017). None of these conditions are valid here. For instance, we only have 19 clusters in the data, and localities differ a lot in terms of size.

We address this issue using two complementary methodologies. First, we implement the wild cluster bootstrap (WCB) method proposed by Cameron et al., (2008). This strategy usually yields very accurate inferences, but its performance tends to deteriorate as the number of clusters becomes smaller. Interestingly, WCB usually under-rejects in this scenario, which implies that is over-conservative. Nevertheless, we use it here following advice by MacKinnon et al., (2023), who suggest using a variant of WCB in any empirical exercise. In particular, we implement the WCB using 1,000 repetitions with a Radamacher distribution for the error weights.

[36] Recall that the q-value is the estimated proportion of false positives among all discoveries (e.g. rejected null hypotheses). We focus on the FDR because has many advantages over statistical corrections for multiple inferences based on the family-wise error rate (FWER). The focus on the FWER is recommended when the cost of making an incorrect inference is high (e.g. budget allocation decisions). Our interventions are low-cost, so the consequences of an incorrect inference are lower. FDR is suitable for cases where scholars are exploring different alternatives to solve a policy problem, so they may be willing to tolerate some higher level of error in exchange for more statistical power to capture differences across treatment groups. See Anderson (2008) for details.

Because the WCB does not work well in all instances, it is recommended to complement it with alternative inferential procedures (MacKinnon et al., 2023). Therefore, we also implement the small sample correction for the estimation of the CRVE proposed by Pustejovsky & Tipton (2018). This correction is based on the generalization of the CRVE proposed by Bell & McCafrey (2002) using bias-reduced linearization methods, in conjunction with Satterthwaite approximations for t-tests. By modifying the standard errors and test statistics to incorporate information about the distribution of the estimator under small sample conditions, this approximation is expected to have a better performance in our setting.

Thirdly, the set of pre-treatment covariates in equation (1) was not pre-specified. As explained earlier, the administrative records of the HSB do not contain detailed information about parents, limiting the number of potential covariates. Still, the ad-hoc selection of covariates can lead to specification bias. As a robustness exercise, we implement the double-selection LASSO estimator proposed by Belloni et al., (2014), so only relevant covariates will be selected in the main regression while controlling for false discoveries. Then, we can check whether the main results are sensitive to the selection of covariates.

Finally, we also address contamination bias.[37] While running a simple linear regression using ordinary least squares (OLS) recovers the average treatment effect (ATE) with experimental data for the single treatment case, recent scholarship by Goldsmith-Pinkham et al., (2024) has shown that this is not the case for multitreatment scenarios. In this scenario, linear regressions fail to estimate convex-weighted averages of causal effects, so the coefficient associated with each treatment arm is contaminated by a non-convex average of the effects of the other treatment arms. We use the methods proposed by Goldsmith-Pinkham et al., (2024) to measure the extent of contamination bias in our experiment and to estimate alternative estimators robust to this issue.

4.3. Estimating the factorial design

We also estimate the long model from our factorial design. To do so, we interact the social norm treatment status with the assignment to the planning tool P_j for parent j. We estimate models of the following form:

[37] Contamination bias was not pre-specified in our PAP because this is a very recent development in the econometric literature for the analysis of experiments.

$$(2)\ \ Y_{ij} = \alpha + \lambda_1 C_j^1 + \sum_{k=1}^{K} \beta_k Z_j^k + \rho P_j + \sum_{k=1}^{K} \delta_k \left(Z_j^k . P_j \right) + \phi_1 \left(C_j^1 . P_j \right) + \theta_s + X_{ij}^{'} \delta + \varepsilon_{ij};$$

where δ_k is the coefficient of interest for interaction between the planning tool treatment P_j and the social norm treatment Z_j^k. Notice that this equation is slightly different from equation (1) because the policy control group was excluded from the design. This was decided because the government's message already incorporated a planning tool. Therefore, to prevent any potential issues with interpreting the results, none of the individuals assigned to the policy control group received an additional message. We dropped all these observations from the sample for this exercise. Moreover, none of the parents in the pure control group received messages with a planning tool. This was decided to preserve a large sample of parents not exposed to any kind of messages for the "short model" in the social norm experiment. Hence, only the experimental control group C_j^1 was cross-randomized with the planning tool. The interaction $C_j^1 . P_j$ captures the differential effect of receiving a second message with a planning tool for those who received a placebo message. All the other variables were defined above.

4.4. Heterogeneous effects

We also estimate heterogeneous treatment effects by interacting treatment status with a pre-treatment variable of interest W_j for parent j. We estimate (pre-specified) models of the following form:

$$(3)\ \ Y_{ij} = \sum_{l=0}^{L} \lambda_l C_j + \sum_{k=1}^{K} \beta_k Z_j + \gamma W_j + \sum_{k=1}^{K} \varphi_k \left(Z_j . W_J \right) + \theta_s + X_{ij}^{'} \delta + \varepsilon_{ij};$$

where φ_k is the coefficient of interest for interaction between the pre-treatment variable W_j and the social norm treatment Z_j^k. All the other variables were defined above. The pre-treatment covariates of interest are the socioeconomic stratum (a dummy variable equal to one of the parent belongs to strata 1, 2 or 3), displacement by civil war, ethnicity, enrollment in health subsidized regime, and access to health insurance.

5. Results

5.1. Main results for the aggregate social norm treatment

Table 4 presents the results of versions of equation (1) where treatment arms were collapsed into relevant aggregated treatment groups. In Panel A of Table 4, we report the results of a version of equation (1) where all social norm treatments were aggregated into a single treatment variable. This was pre-specified in our PAP to test whether social norms, regardless of their content, can foster vaccination against HPV. This also reduces the potential hypotheses to test to just a single one, so concerns regarding multiple hypothesis testing are minimized. The dependent variable was rescaled using a scale factor of 100 to ease interpretation. In all regressions, we incorporate both the experimental and policy control groups to explore the effect of the other behavioral elements used in the design of the text messages and to provide a comparison between our behavioral design and the communication strategy previously employed by the HSB. For this reason, the pure control group will serve as a benchmark in the empirical analysis. We include a vector of randomization strata (locality by age) in all regressions. Standard errors are clustered at the locality level.

Column (1) estimates the effect of this aggregated treatment variable on whether a parent vaccinated his or her daughter during the campaign window. The estimated coefficient is 0.907 percentage points, statistically significant at the 99% confidence level. Considering an average vaccination rate in the pure control group of 5.56%, this effect represents an increase of 16.3% in the HPV vaccination rate. This effect is relatively large in comparison with the international literature. Most studies about using social norms and similar soft interventions on vaccination tend to have small effects, around 2 percentage points. Patel et al., (2023) estimate an effect of 1.8 percentage points for the case of the influenza vaccination rates. Milkman et al., (2022) find that reminders increase flu vaccination by 2 percentage points. Busso et al., (2015) report an increase of 2.2 percentage points for childhood immunization in Guatemala. Although our estimated effect is lower, the effect size is larger compared to the control group because the vaccination rate for this group is lower than in other papers.

Column (1) also reports the results of comparing the experimental and policy control groups with the pure control group. Both coefficients are small and not statistically significant at the usual confidence levels. To offer more evidence on this matter, we implement a Wald test for the null hypothesis that the estimated coefficient is equal to 0 for both the experimental and the policy control groups individually. In the first case, we cannot reject the null hypothesis that the experimental control group is not statistically different than the pure control group (F statistic=0.13, p-value=0.72). These results suggest that none of the other behavioral elements, included in the message sent to the experimental control group, have a significant effect on HPV vaccine take-up. This is important because it shows that the estimated effect of the aggregated social norm treatment does not depend on the other elements of the message content, implying that the estimated effect is only driven by the normative content of the sent messages.

In the case of the policy control group, we also cannot reject the null hypothesis that the policy control group is not different than the pure control group (F-statistic=1.92, p-value=0.18). This suggests that the messages designed by the HSB to boost vaccination rates have the same results as not sending messages at all. Interestingly, the estimated coefficients are even negative, albeit not statistically significant at the usual confidence levels. In any case, this provides evidence suggesting that the content of the messages matters when it comes to understanding the effects of social norms interventions. By controlling for the role of non-normative content of text messages using the experimental group, we can isolate the role of the social norm content in explaining the estimated effects.

The results for the experimental and policy control groups are also useful to rule out the possibility that the messages work just as reminders. Interventions based on the use of text messages always face the possibility that the estimated effect depends on the message content or is explained by the effect of being contacted regardless of the content of the message. Consequently, receiving a social norm treatment can induce a change of behavior because of the message content or because the message reminds the parent about the pending vaccine. By showing that parents exposed to the experimental and policy control are not jointly statistically different than the pure control group (F-statistic=1.49, p-value=0.25), we can rule out the possibility that results are not explained by the social norm content. Because both the experimental and the policy control groups received messages and yet we find no difference between them and the pure control group that did not receive messages, we can safely conclude that the estimated coefficients for the aggregated social norm treatment reflect the effect of the message content rather than the effect of receiving a message. Hence, we can rule out the interpretation that our social norm treatments only work as reminders.

The previous estimates were computed using cluster standard errors at the locality level. As mentioned above, we have only 19 localities in the sample, which raises concerns about the performance of the CRVE in our setting. We implement the wild cluster bootstrap suggested by

Cameron et al., (2008). The corrected t-statistic for the aggregated social norm treatment variable is 3.56 (p-value=0.006), well above the usual confidence levels. We also implement the small sample correction for cluster robust standard errors proposed by Pustejovsky & Tipton (2016) finding consistent results (S.E.=0.26, p-value= 0.0062). Hence, our result is robust to the small number of clusters.

Although we aggregated the social norms treatment to minimize concerns of multiple hypothesis testing, the regression reported in column (1) is still affected by this concern because coefficients for both the experimental and policy controls were also estimated. As discussed in section 4, we control for this issue by computing the Benjamini and Hochberg's (1995) q-values using the methodology outlined by Anderson (2008). The estimated q-values are reported in brackets. Our previous results are not affected by implementing this correction. None of the estimated q-values change the qualitative results. The main coefficient of interest is significant at the same confidence levels as before (q-value=0.007). This suggests that our results are robust to multiple hypothesis testing.

We explore the robustness of these main results by performing additional exercises. In column (2) we report the results of estimating the same econometric specification with covariates. By controlling for additional covariates, we expect to improve efficiency and remove bias due to a potential imperfect randomization. Although this second issue was addressed during the design phase when random assignment was performed using the rerandomization algorithm proposed by Morgan & Rubin (2012), we use this exercise to check whether this was achieved in the data. We control for a set of indicator variables that capture membership to health insurance providers (EPS), enrollment in the contributory regime, in the subsidized regime, lack of health insurance, membership to ethnic groups, being displaced as a consequence of civil conflict, being a Colombian national, and membership to low socio-economic strata. Results are unaffected by including controls. The estimated coefficient and the standard errors are almost the same. In consequence, the main results are insensitive to the inclusion of additional covariates. These results are also robust to multiple hypothesis testing since the estimated q-values do not change the qualitative interpretation of the main results.

One concern with the previous regression is the ad-hoc selection of covariates. Because the set of covariates is limited in our setting and mainly composed of indicator variables, we did not pre-specify the set of covariates to be used in the linear regression model with controls in our

PAP. To check whether our results are sensitive to the choice of covariates, we run the double LASSO selection algorithm proposed by Belloni et al., (2014). Results are reported in column (3). We pre-selected a set of covariates to be always included in the regression analysis. These include enrollment in the contributory regime, in the subsidized regime, lack of health insurance, membership to ethnic groups, being displaced as a consequence of civil conflict, being a Colombian national, and membership to low socio-economic strata. The set of indicator variables that capture membership to health insurance providers are considered to be evaluated by the LASSO algorithm for inclusion among the set of regressors. We use the plugin iterative formula to select the optimal value of the LASSO penalty parameter. The main results remain unaffected by the choice of covariates after applying the LASSO algorithm. These results are also robust to multiple hypothesis testing.

We also address whether the main results in column (1) are sensitive to contamination bias. We implement the decomposition proposed by Goldsmith-Pinkham et al., (2024) and compute the own-treatment effects. A large difference between the coefficients estimated in column (1) and the estimated own-treatment effects would indicate the presence of contamination bias. Results are reported in column (4). We find that the estimated own-treatment effects are indistinguishable from the estimated coefficients for the econometric specification in column (1). These results are also robust to multiple hypothesis testing. As a result, we find no evidence in favor of contamination bias in our setting.

Tables A.3 and A.4 in the Online Appendix present the alternative estimators proposed by Goldsmith-Pinkham et al., (2024) to correct for contamination bias. In addition to own-treatment effects, we provide estimated coefficients for unweighted ATE and weighted estimates using both variance-minimizing and easiest-to-estimate common weighting schemes. Coefficients remain consistent across alternative methodologies.

5.2. Dynamics of the aggregated treatment effects

We have shown that the main results from this section are not sensitive to a battery of robustness checks. In this section, we take advantage of the detailed administrative records from the HSB to explore the dynamics of the treatment effect over the period under analysis.

Figure 5 shows the dynamics of the treatment effects. Each period reports the cumulative effect of the social norm intervention at a given point in time. The horizontal axis shows the date for which the treatment effect was estimated. The vertical axis reports the estimated effect measured in percentage points (using a scale from 0 to 100). The dashed lines indicate the intervention period. Pre-treatment data covers two weeks before the delivery of the first message in our SMS campaign (October 21st) and 12 weeks after the end of the intervention. Before October 21st, we observe no differences between the social norm treatment group and the pure control group. Many weeks after the end of the campaign, we find noticeable effects of social norms messages on HPV vaccination.

One limitation of the preceding graphical analysis is that the experimental dataset is subject to right-censoring. This means that we only observe the behavior of participants up to the end of the analyzed period, leaving out those who are vaccinated afterward. To address this challenge, we complement the previous analysis by employing a Kaplan-Meier survival analysis. This allows us to investigate the transition from non-vaccination to vaccination status resulting from the social norm intervention within our study timeframe (Kaplan & Meier, 1958). This enables researchers to evaluate the impact of the social norm intervention on the timing of vaccination, offering valuable insights into its effectiveness.[38]

Figure 6 presents the Kaplan-Meier survival function for the case of the aggregated social norms treatment. The horizontal axis gives the number of days considered in the experimental timeframe. We consider 130 days after receiving the first message, 144 days including the two pretreatment weeks. The vertical axis reports the proportion of vaccinated girls. Dashed vertical lines indicate dates that treatment began and ended. All participants were right-censored 130 days after the first message was delivered. The figure tracks the percentage of participants in the pure control group (red line) versus the social norm treatment (blue line) who had obtained the first dose of the HPV vaccine at Bogota's health system by a given day from the first message date. We observe a clear, widening, and persistent gap between the social norm treatment group and the pure control group during the period under study. This suggests that the social norm intervention not only increased the number of vaccinated participants but also accelerated the vaccination

[38] The Kaplan-Meier approach is particularly well-suited for analyzing time-to-event data, such as the duration until vaccination following receipt of text message interventions. It adeptly handles censoring, accommodating cases where individuals do not experience the event of vaccination during the study period. Furthermore, this method is non-parametric, eliminating the need to assume a specific distribution of survival times. Finally, Kaplan-Meier curves provide a visual representation of the probability of being vaccinated over time. See the Chapter 17 of Cameron & Trivedi (2005) for technical details.

process. Since accelerating vaccination also benefits society, as illustrated by the recent COVID-19 epidemic, we interpret this as an additional advantage of using social norms in this setting.[39]

5.3. The effects of static versus dynamic norms

We have established that receiving a message crafted with social norm content has a positive and statistically significant effect on HPV vaccination uptake. In this section, we explore whether messages with dynamic norms outperform those with static designs in terms of increasing vaccination against HPV. To do so, we aggregated the treatment arms with static designs (injunctive, positive descriptive and negative descriptive norms) and those with dynamic designs (dynamic and trending norms) into two treatment variables. In this way, we can test whether a differential effect of dynamic designs can be identified in the data.

Panel B of Table 4 presents the main findings of this section. Our analysis reveals that the static design is causally linked to a 0.66 percentage point increase in HPV vaccination rates, a result significant at the 95% confidence level. This represents a moderate increase of 12% in the uptake of HPV vaccination compared to the mean of the pure control group. Interestingly, the impact of the dynamic design is nearly double that of the static approach, with an estimated increase of 1.276 percentage points (S.E.= 0.372). This is a 23% increase in HPV vaccination rates. A relatively large effect in comparison with similar studies in other settings, as discussed earlier.

To check whether this larger effect for the dynamic design is not a statistical artifact, we implement a Wald test for the null hypothesis that the coefficient for the dynamic design is equal to the coefficient of the static design. We reject this null hypothesis at the usual confidence levels (F-statistic=3.02, p-value= 0.0991). In consequence, we can safely conclude that the estimated difference between these two designs is real.

Consistent with the evidence already discussed, we also find that the experimental and policy controls are not statistically different than the pure control group. This reinforces the idea that the other elements of the messages besides the social norm content play no role in terms of increasing HPV vaccination and rule out the concern that sending messages only reminds people about HPV vaccination. Moreover, these results are robust to multiple hypothesis testing, as suggested by the estimated q-values.

These results call for a more nuanced view of the role of static designs in settings with low adoption of vaccination. Although we can identify a larger effect of the dynamic design over the

[39] See, for instance, Castillo et al., (2021) for a discussion about this.

static one, we still find that the static norms can work in settings where the adoption of the desired behavior is low. Contrary to the common sense in the social norms literature, we find no evidence regarding the potentially harmful effects of static designs in settings with minority behaviors (Bicchieri & Dimant 2022). If anything, the estimated effects are moderate and positive. Therefore, the negative perspective of some scholars on this matter appears to be overstated.

We implement the same battery of robustness checks for these results. The main results in this section are robust to the inclusion of additional covariates (column 2), to the inclusion of covariates that were selected using the double LASSO algorithm (column 3), and to contamination bias (column 4). These results remain qualitatively unaffected by correcting for multiple comparisons using the Benjamini & Hochberg's (1995) methodology for controlling for the false discovery rate.

In Figure A.5 in the Online Appendix, we report the analysis of the dynamic treatment effect for both the static and dynamic designs. The results are consistent with the main results discussed in this section, showing a larger effect for the dynamic design. We also report in Figure A.6 in the Online Appendix the Kaplan-Meier survival analysis for HPV vaccination. We find a larger effect of the dynamic design versus the static one in accelerating the decision to vaccinate a daughter against HPV.

5.4. Dissentangling the effects of static and dynamic norms

We have shown that the dynamic design of our social norm experiment is more effective than the static one. In this section, we seek to explore what types of static and dynamic norms explain the results. Recall that our static design includes injunctive, positive descriptive, and negative descriptive norms. On the other hand, the dynamic design was comprised of dynamic and trending norms. By exploring the performance of each norm individually, we seek to shed light on this issue.

Table 5 presents the results. We report individual coefficients for each type of social norm treatment (5 in total), plus the two control groups discussed above. Because multiple hypothesis testing is more relevant in this case, we pay more attention to the estimated q-values. Column 1 presents the results for the simple linear specification without additional controls. As above, neither the experimental control group nor the policy control group are statistically different from zero. Therefore, we focus our discussion on the effect of each type of static and dynamic norm.

We start by discussing the results of disentangling the effects of the static design. Let's consider the positive descriptive norm. We find no evidence that the positive descriptive norm affects HPV vaccination. The estimated coefficient is close to zero (0.243 percentage points) and statistically insignificant. So, we find no evidence of the so-called "boomerang effect" of descriptive minority norms (Cialdini et al., 1990; Bicchieri & Dimant 2022; Schultz et al., 2007). The backfire effect might still be present in the population that corrected overstated beliefs of the descriptive norm, as in Schultz et al. (2007), but this is unlikely because HPV vaccination rates are so low in our setting that we should expect most people to have held overstated beliefs. So, we should have expected a large negative effect, since most participants would have updated their beliefs downwards compared to those who should have updated their beliefs upwards. Hence, it is unlikely that this lack of effect is explained by heterogeneity in beliefs.[40]

In contrast, the negative descriptive norm has a positive effect on HPV vaccination. The estimated coefficient is 0.615 percentage points, statistically significant at the 95% confidence level. This implies an 11.7% increase in the vaccination rate compared to the pure control group. This is a large effect compared to other papers in the literature. This suggests that using a loss framing can enhance the effect of social norms on HPV vaccination and provides additional support to the idea that the concern for the so-called "boomerang effect" is not relevant in our setting. This result also goes against the findings of Salovey and Williams-Piehota (2004), who suggest that positive descriptive norms are more effective when the goal is to increase the adoption of health-preventing behaviors. To verify whether this differential effect of the negative descriptive over the positive descriptive norm is consistent with the data, we implement a Wald test for the null hypothesis that the coefficient of the negative descriptive norm is equal to the positive descriptive one. We reject this null hypothesis at the usual confidence levels (F-statistic=1.25, p-value=0.2788). Consequently, we can safely discard the idea that the estimated effect of the negative descriptive social norm is a statistical artifact.

Let's consider the injunctive norm now. The estimated coefficient is 1.088 percentage points (S.E.=0.477), significant at the 95% confidence level. This represents a 20% increase in

[40] Of course, our setting limits the strength of our conclusion since beliefs on current vaccination rates held by the participants are not elicited, impeding analysis of heterogenous effects of descriptive norms on HPV vaccinations. In our opinion, being this a valid concern, it is hard to address from a practical point of view. Eliciting beliefs in large-scale interventions is costly and unfeasible in most cases. Therefore, there is a trade-off between the costs and benefits of eliciting beliefs in the context of large-scale interventions. Policymakers will need to take into account this trade-off at the time of planning and implementing social norm interventions.

HPV vaccination in comparison to the mean of the pure control group. This effect is almost two times larger than the one reported for the negative descriptive norm. This differential effect is not a consequence of sampling variability. A Wald test for the equality of coefficient between the negative descriptive and the injunctive norms is rejected at the usual confidence levels (F-statistic=0.6, p-value=0.4502). Supporting the focus theory of normative conduct by Cialdini et al., (1990), we find evidence that adding an injunctive norm to an otherwise descriptive norm message increases the impact on HPV vaccinations.

The accumulated evidence so far points to a relevant role of static designs in improving HPV vaccination rates, despite that HPV vaccination is largely a minority behavior. In the case of the injunctive norms, a common view in the existing literature is that this type of norms can overcome the "boomerang effect" of descriptive norms on minority behaviors (Allcott 2011; Jacobson et al., 2022; Schultz et al., 2007). However, although we find that the effect of injunctive norms is larger, negative descriptive norms are also effective in improving HPV vaccination. Accordingly, advertising minority norms via descriptive designs is not necessarily harmful as suggested by some scholars (Clislaghi & Heise 2018, Richter et al., 2018).

We turn now to exploring the effect of dynamic designs. In the case of the dynamic norms, we estimate a coefficient of 1.274 percentage points (significant at the 99% confidence level). Moreover, this effect is similar to the estimated coefficient for the trending norm (1.278 percentage points, S.E.=0.298). In fact, we cannot rule out the null hypothesis of equality of coefficients between the dynamic and trending norms using a Wald test (F-statistic=0, p-value=0.99). This represents a large effect of dynamic and trending social norms on HPV vaccination, a 23% increase compared to the pure control group mean in both cases. Moreover, these effects are similar to those reported for the injunctive norm treatment. A Wald test for the equality of coefficients for the injunctive, dynamic, and trending norm cannot reject the null hypothesis (F-statistic=0.19, p-value=0.8303).

These results support recent work that finds dynamic norms impactful in increasing behaviors that have not yet been adopted by a majority (Cheng et al., 2022; Mortensen et al., 2017; Sparkman & Walton 2017; Milkman et al., 2022). These results are important and very useful from a policy perspective. However, the fact that the estimated effects are not statistically different than those reported for injunctive norms, suggest that caution is required to conclude that dynamic and trending norms always outperform static designs in settings with minority behaviors.

These results are robust to a set of additional analyses. First of all, these results are robust to controlling for multiple hypothesis testing. The estimated q-values do not change the qualitative interpretation of the main results, although the statistical strength of the relationship between the social norm treatments and HPV vaccination is weaker, except for the trending norms (significant at the 99% confidence level). Secondly, the estimated coefficients are robust to controlling for pre-treatment covariates (column 2). Thirdly, the estimated coefficients are also robust to controlling for pre-treatment covariates selected by the double LASSO algorithm (column 3). Finally, results are robust to contamination bias (column 4).

We complement this discussion with a graphical analysis of the dynamics of the treatment effects, as in Figure 5. Figure 7 reports such analysis for each treatment arm. Figure 7.a shows that the injunctive norm is very effective in increasing vaccination rates in the period under analysis. In contrast, the positive descriptive norm shows no effect over the analyzed weeks (Figure 7.b). The negative descriptive norm is effective, yet its effect is weak and non-noticeable up to the end of the experiment timeframe (Figure 7.c). Both the dynamic and the trending norm show an increasing effect over the period under analysis, noticeable even before the end of the SMS campaign (Figures 7.d and 7.e). The graphical analysis is consistent with the evidence reported above.

We also analyze the effect of variations of social norm messages on the timing of vaccination using the Kaplan-Meier methodology outlined above. Results are reported in Figure 8. For all the social norm treatments, except the positive descriptive norm (Figure 8.b), we see a clear gap between the proportion of vaccinated girls that were exposed to a social norm intervention versus the pure control group. The gap is larger for the injunctive (Figure 8.a), the dynamic (Figure 8.d), and the trending social norms (Figure 8.e). Consequently, the evidence is consistent regarding the role of static and dynamic social norms.

5.5. Interacting social norms with planning tools

In this section, we leverage the full factorial design outlined in equation (2) in Section 4. As a reminder, we cross-randomized the social norm treatments with a planning tool, specifically a message containing a link providing information about the nearest vaccination point. We aim to examine whether parents exposed to both the social norm treatment and this planning tool are more inclined to vaccinate their daughters against HPV compared to those exposed solely to either the social norm or the planning tool independently.

Table 6 presents the results for the 7x2 full factorial design. We initiate the analysis by concentrating on the "short model" for the planning tool. In column (1), we present its effect on HPV vaccination uptake. We excluded the policy control group from the analysis because its message content already incorporates a planning tool; hence, we designed our experiment to exclude this control group from the analysis of the "long model". The effect of the planning tool is nearly zero and statistically insignificant. This provides further evidence that simply receiving a message may be insufficient to prompt variation in HPV vaccination rates.

In column (2), we report the results for the “long model” using the aggregate social norm variable discussed in Panel A of Table 4. The regression model includes both treatment variables (social norm and planning tool) along with their interaction, effectively reducing the model to a 3x2 factorial design. The interaction between the social norm treatment and the planning tool is not statistically significant. However, both variables' levels are significant. The level of the social norm treatment is 0.929 percentage points (S.E.=0.2868, t-statistic=3.24), while for the planning tool, it is 0.916 percentage points (S.E.=0.4541, t-statistic=2.02). This suggests that receiving an additional message with a planning tool for those exposed to a social norm message diminishes the effectiveness of the social norm campaign.

Column (3) presents the results for the "long model" using the aggregation of treatment arms for the static and dynamic social norms as detailed in panel B of Table 4. The results align closely with those reported in column (2). Firstly, the planning tool is statistically significant once the "long model" is estimated (estimated coefficient=0.916 percentage points, S.E.=0.454), albeit with a weak effect. Secondly, we observe that both the static and dynamic designs are also statistically significant, although the coefficient for the dynamic design is larger. Thus, these findings suggest that there are no differential effects between the static and dynamic social norm interventions for parents who were exposed to an additional message containing a planning tool. For the subsample of parents exposed solely to the planning tool without exposure to the static and dynamic social norm interventions, we find that the planning tool marginally enhance HPV vaccination rates.

This trend persists in the “long model” for all the individual social norm treatments, although the estimated coefficients are noisy. We present these results in column (4). Except for the interaction between the positive descriptive norm and the planning tool, which is notably negative, all other interactions are statistically zero, although the effect sizes are relatively large, making it hard to draw conclusive interpretations. The estimated coefficients for the dynamic and trending norms remain as large as those reported in Tables 4 and 5; however, some of them are no longer statistically significant. The same observation applies to the injunctive norm. These findings may indicate a power issue, as recent studies in statistical literature suggest that estimating sample sizes for testing interactions may require an increase in sample size by a factor of 16 (Gelman 2018).

We also report a positive and statistically significant effect of positive social norms on HPV vaccine uptake for those not exposed to the planning tool (estimated coefficient=0.915, S.E.=0.340). This implies a potential "boomerang effect" of sending an additional message with a planning tool in this setting, a phenomenon that deserves further exploration. Although most coefficients are not statistically significant for the interactions, they are consistently negative. Given that we have already established that those parents who receive only one message with a planning tool are more likely to vaccinate their daughters, these negative coefficients for the interactions likely indicate a negative response from parents due to receiving a higher number of messages.[41]

The lack of sufficient statistical power does not allow us to be conclusive regarding the effects reported here. If the negative interactions were effectively all zero, more efficient estimates can be obtained with the "short model." However, if these interactions are not zero, then the "long model" provides consistent estimators for the main treatments (Muralidharan et al., 2023). Regrettably, designing experiments to precisely estimate these interactions requires exceedingly large sample sizes, something rarely observed in practice. Consequently, we can only speculate as to whether the observed interactions are truly zero or merely noisy estimates of genuine empirical relationships that reflect complementarities between social norms and planning tools. Regardless of this challenge, it is crucial to underscore that the estimated treatment effects encompass composite effects, comprising a weighted average of interactions with other treatments (Muralidharan et al., 2023).

Our emphasis on the "short model" was pre-specified in the PAP and was justified by our main interest in testing the effectiveness of many potential treatments (Cochran & Cox 1957). Our

[41] Experimental evidence about sending additional reminders is consistent with this finding. In an experiment for fundraising, the authors found that an additional reminder increased the rates of unsubscribing from the mailing list of participants by 76 percent. See Damgaard & Gravert (2018).

objective is to ascertain what strategies prove effective among various alternative social norm interventions and to assess the potential of promising approaches in enhancing HPV vaccination rates. While exploring these potential complementarities is of intrinsic academic and policy interest, we also provide the results of the "long model" to uphold academic transparency and ensure comprehensive reporting of the main results of this study.

5.6. Heterogenous effects

In this section, we investigate the presence of heterogeneous effects resulting from social norm interventions. We employ the (pre-specified) regression models outlined in equation (3). To explore this heterogeneity, we concentrate on four pre-specified covariates: low socioeconomic status, displacement due to civil conflict, membership in ethnic groups, and access to health insurance. An additional variable, Colombian nationality, was initially pre-specified but ultimately excluded due to insufficient variability. Instead, we substituted this variable with an indicator for whether the parent belongs to the subsidized health regime.

The main results are presented in Table 7. Each column corresponds to a heterogeneous effect based on one of the aforementioned covariates. Column (1) presents the results of the heterogeneity analysis based on socioeconomic status. We find no evidence of such heterogeneity. The interaction between exposure to a social norm intervention and belonging to a low socioeconomic stratum is not statistically significant. This pattern persists across the remaining covariates studied. We observe no evidence of heterogeneous responses based on displacement due to social conflict (column 2), ethnicity (column 3), access to the subsidized health regime (column 4), or lack of access to insurance in our setting. However, it is important to note that effect sizes are substantial in some cases (e.g., displacement due to social conflict and ethnicity), suggesting that sample sizes may not be sufficiently large to confidently rule out the presence of such heterogeneities.

6. Cost-effectiveness

Panel A of Table 8 presents the estimated costs associated with implementing the social norms intervention outlined in this study. We will perform this exercise from the perspective of the government. Since the HSB already possesses the technological infrastructure for delivering text messages, we identify personnel and message expenses as the main costs. Regarding personnel expenses, we anticipate a technical expert dedicating a total of 64 hours to the campaign, including tasks such as design, testing, and message delivery over 8 weeks. At a rate of COP 40,000 per hour

(USD 10.6), this amounts to USD 679 for a part-time tech professional. Regarding SMS expenses, the cost per message was COP 8. With the complete factorial design, a total of 386,436 messages were delivered, resulting in a total cost of USD 820. Therefore, the overall cost of the intervention amounts to USD 1,499.

The intervention reached 29,906 participants through text messages. When we divide the total cost of the intervention by this number, we find an estimated cost of USD 0.05 per girl. It's important to note that this cost accounts only for current expenses.[42] We also do not factor in the cost of HPV vaccines, as they are distributed free of charge to eligible girls. Additionally, we exclude the private costs incurred by parents to transport their daughters to vaccination points, as signing an informed consent is required by law at the time of vaccination. Therefore, our cost-effectiveness analysis solely focuses on the design and implementation of a social norm campaign under the assumption that vaccines are accessible at no cost, parental opportunity costs are disregarded, and no capital investments are necessary from the city's health authorities.

Panel B of Table 8 reports the cost-effectiveness ratios for the social norms interventions. By dividing the total cost per girl by the estimated ITT effect on HPV vaccination resulting from receiving a social norm message (regardless of the type of norm), we can calculate the cost per additional vaccinated girl. The cost-effectiveness ratio is USD 5.53 for the average social norm intervention. If we specifically examine injunctive or dynamic social norm interventions, this ratio reduces to USD 3.93. These cost ratios rank among the lowest for similar interventions utilizing SMS campaigns, as indicated by a systematic review of digital technology's role in enhancing vaccination uptake (Wang et al., 2023).[43] Other studies closely related to the use of vaccination reminders have reported larger cost-effectiveness ratios compared to those estimated in this study. For example, Busso et al., (2015) found a cost ratio of USD 7.5 in Guatemala, while Kawakatsu et al., (2020) estimated this ratio at USD 7.9 in Nigeria. Using data from 800 experiments on social media advertising, Athey et al., (2023) estimated a cost per additional vaccination of USD 5.7. Therefore, our intervention demonstrates high cost-effectiveness in comparison to similar studies.

7. Concluding remarks

[42] This reflects the current situation in our setting, where no extra investments are needed to execute a campaign like this, thanks to prior investments made by the HSB for communication with citizens. Without these prior investments, the cost of implementing this strategy would have been higher.

[43] According to Wang et al., (2023), the results for cost per additional vaccinated and/or return case ranged from USD 2.14 to USD 17.79 when compared with no intervention in the studies surveyed in their review.

HPV infection is a leading cause of cervical cancer and various other cancers (e.g. anal, penile, vaginal, and oropharyngeal cancer). Vaccination significantly reduces the risk of developing these cancers by preventing HPV infections. It not only protects individuals who receive the vaccine but also contributes to herd immunity, lowering the overall prevalence of HPV in the population. Cancers associated with HPV can impose a substantial economic burden on individuals and healthcare systems, particularly in the developing world. Unfortunately, the low HPV vaccination rates observed in many countries represent a missed opportunity to prevent HPV-related diseases, save lives, and enhance public health. Consequently, efforts to improve HPV vaccination rates are crucial.

To address this issue, we conducted a field experiment using a text message campaign to increase HPV vaccinations in Bogota, Colombia, a country that has experienced a dramatic reduction in its HPV vaccination rates in recent years. We targeted parents with daughters between 9 and 17 who need the first dose of the HPV vaccine. We tested five social norm messages, exploiting static and dynamic designs. Compared to previous studies that measure intention, the administrative data from the HSB has the advantage of testing the effect of social norm messaging on actual HPV vaccinations.

Our main finding provides robust evidence of the impact of many variations of social norms on increasing HPV vaccinations in our setting. We find that one type of static norm (injunctive) and the two variations of dynamic norms are equally effective in increasing HPV vaccination. These effects are strongly significant and robust to different specifications and robustness checks. These include strategies for addressing the low number of clusters, controlling for multiple hypothesis testing, the inclusion of covariates, and contamination bias.

Our results support recent evidence on the impact of dynamic norms on the increasing adoption of minority behaviors (Mortensen et al., 2017; Sparkman & Walton 2017). It is also consistent with the findings of the literature on injunctive norms as an element to overcome the shortcomings of descriptive social norms on minority behaviors (Allcott 2011; Jacobson et al., 2022; Schultz et al., 2007). One significant contribution of our paper is the development of a research design enabling the testing of various social norm framings. This design facilitates a clear comparison among alternative social norm theories.

Our results do not entirely support the common view in the literature that descriptive norms, particularly with social information about minority adoption, in isolation may induce a 'boomerang effect' (Bicchieri and Dimant 2022). On the contrary, we observe that negative descriptive norms also lead to increased HPV vaccination rates. Although we do not identify any effect of positive descriptive norms on HPV vaccination, there is no evidence supporting the occurrence of the "boomerang effect" in this scenario either. While we lack access to pre-treatment levels of individual beliefs about HPV vaccine adoption, it's improbable that this result can be attributed to heterogeneity in these beliefs. Given the low adoption rate of HPV vaccines in our setting (16%), it's expected that most participants have held upward-biased beliefs. Thus, a descriptive norm intervention would have likely hindered HPV vaccination rates if the "boomerang effect" were valid. This highlights the need for future research into the generality of the so-called "boomerang effect."

Finally, our research contributes to the general research on the relationship between social norms and behavior change. Our results are relevant for the design of strategies that aim to increase vaccinations and other health-preventing behaviors. Highlighting that others are increasingly adopting a minority behavior is likely to increase that behavior. Including a component of injunctive norms that communicates what others approve of, e.g., an emoticon, is likely to increase the impact of a social norm message. Even incorporating loss-framing in descriptive social norms can be effective.

The implications of this study's findings are relevant for developing cost-effective public social norms interventions. This study's estimated cost per marginal vaccinated girl was approximately USD 5.53. However, had the dynamic social norm (or the injunctive norm) been implemented across all groups, the cost per marginal vaccinated girl would have decreased to USD 3.93. These results highlight the importance of experiments that find effective social norms interventions for the target population, as they can help keep the costs low when implemented at scale. Furthermore, given the link between HPV vaccination and reduced risk of cervical cancer, social norm interventions may ultimately lower public resources allocated to cancer-related medical care.

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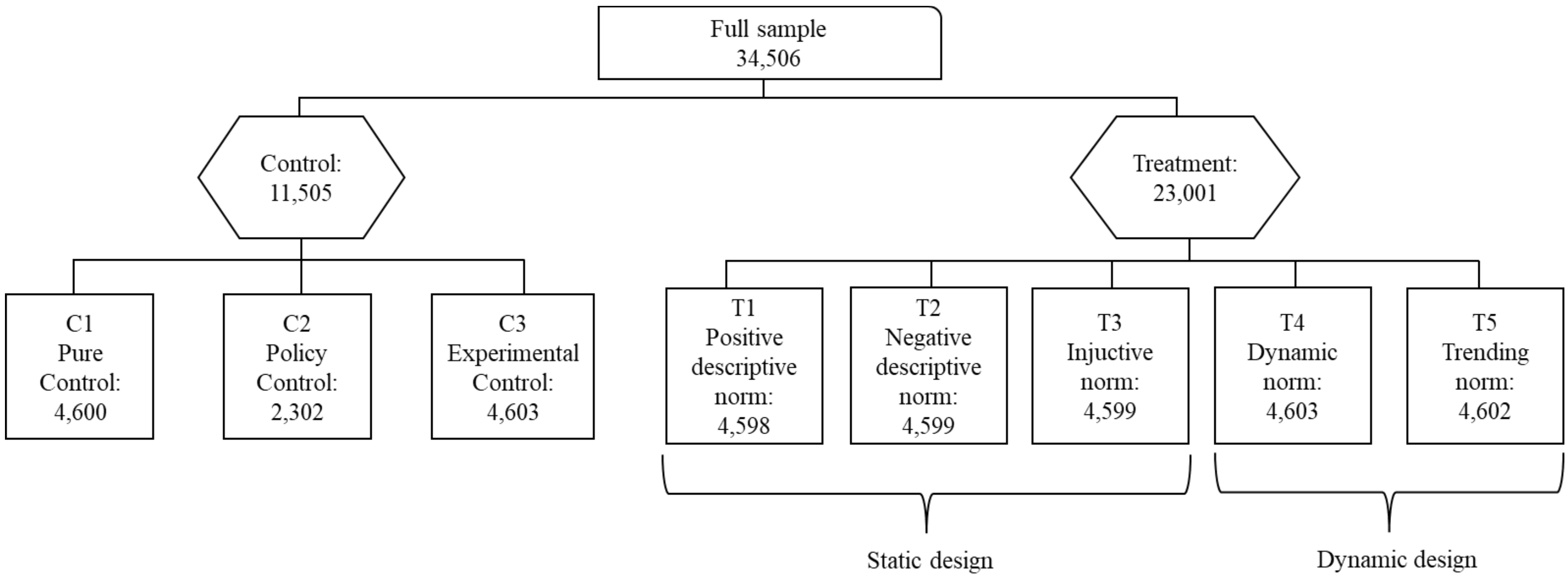
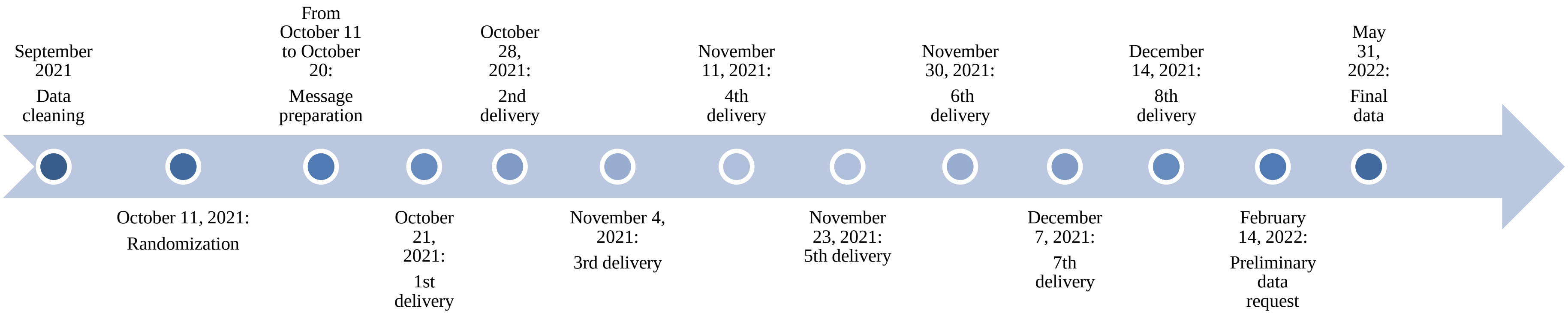
Tables and Figures

Figure 1. Graphical representation of experimental groups



Note: Authors' elaboration.

Figure 2. Timeline of experiment

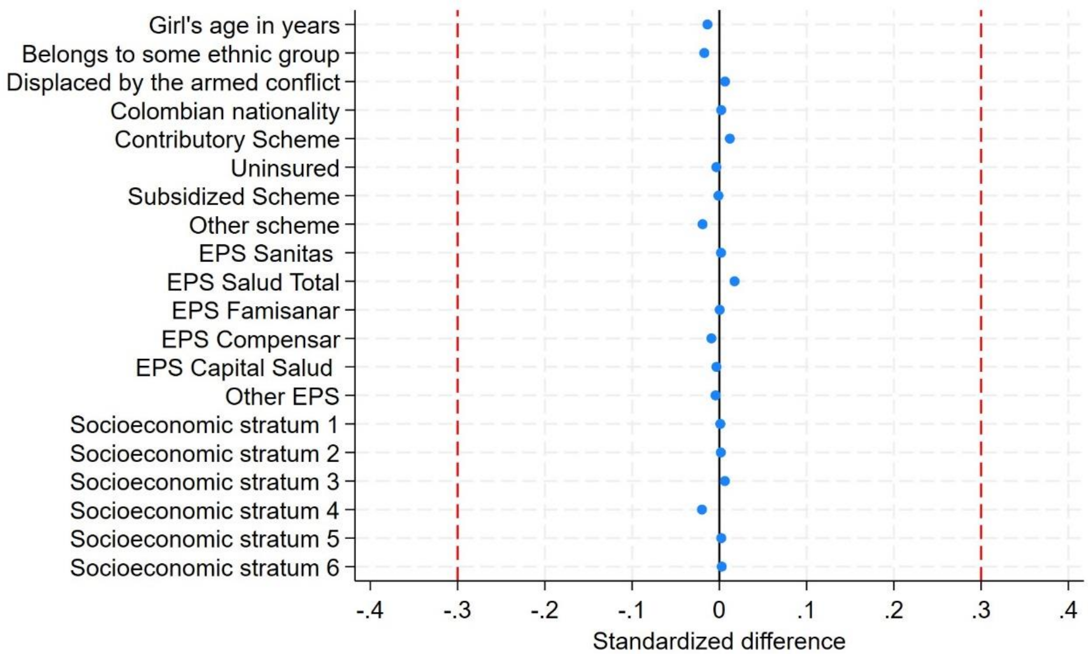


Note: Authors' elaboration.

Figure 3. Sampling frame construction for the social norms experiment

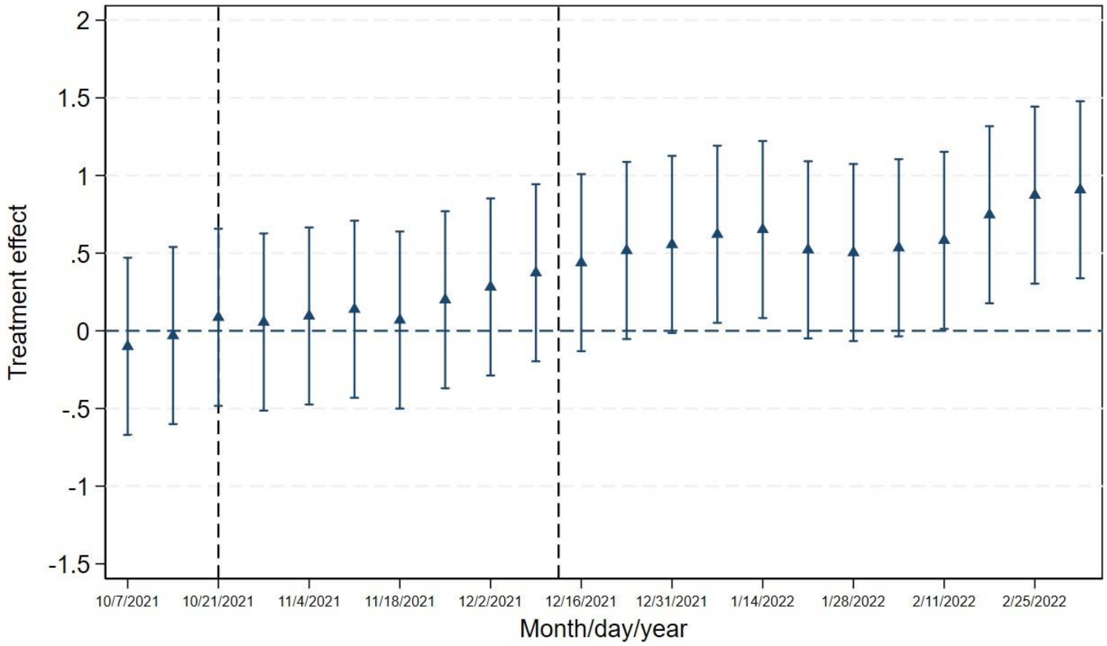
Note: Authors' elaboration.

Figure 4. Pre-treatment balance plot



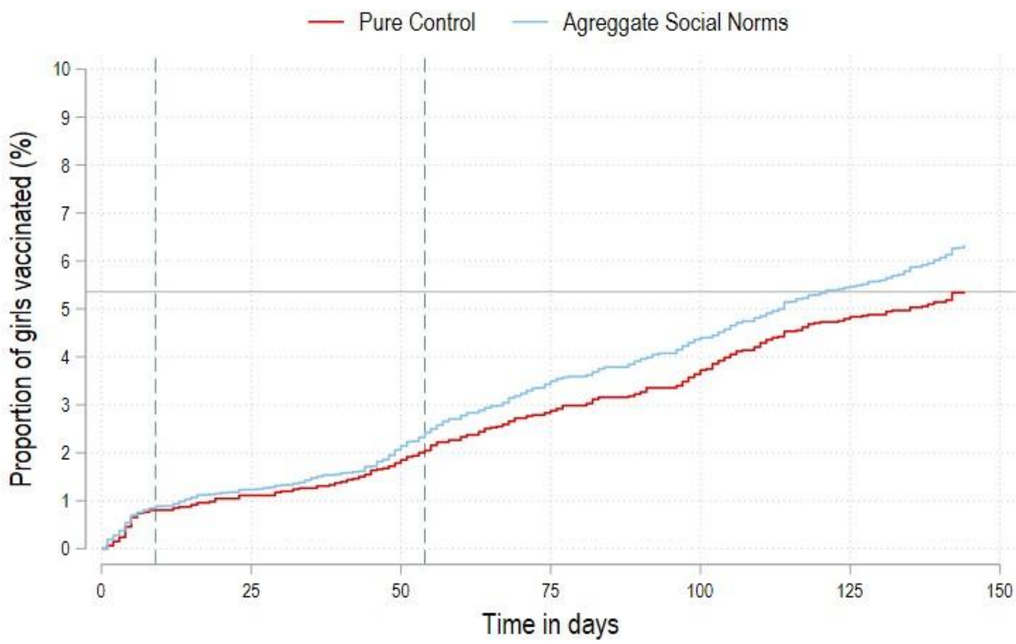
Note: Authors' elaboration. This figure reports the standardized differences between the average social norm treatment and the pure control group. Each dot represents a value of the standardized difference for a given covariate. Dashed red lines at the |0.3| of a standardized difference delineate the threshold at which a difference between the means between the treatment and the control group is considered statistically significant. This value was proposed by Imbens (2015).

Figure 5. Effect of social norms on HPV vaccination over time



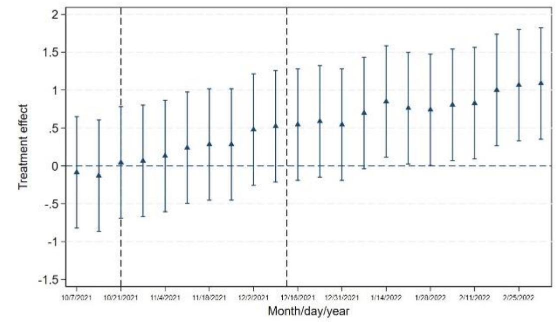
Note: Authors' elaboration. Horizontal axis gives date at which the outcome was measured. Vertical axis is the treatment effect in percentage points, estimated by pooling data from all outcome periods, estimating Eq.(1) with period dummy variables and one treatment dummy per period. Each triangle measures the cumulative effect of the social norm intervention over time. Vertical bars represent 95% confidence intervals, where standard errors are clustered at the locality level. Dashed vertical lines indicate dates that treatment began and ended.

Figure 6. Kaplan-Meier survival function for HPV vaccination

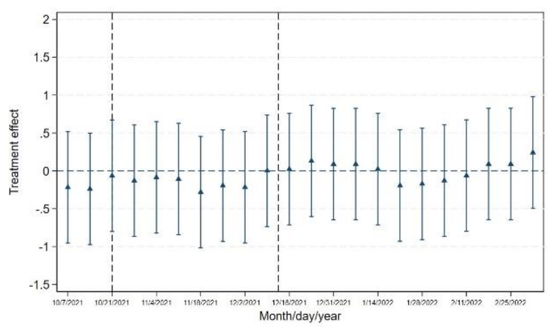


Note: Authors' elaboration. Horizontal axis gives the number of days after receiving the first message. Vertical axis is the proportion of vaccinated girls. Dashed vertical lines indicate dates that treatment began and ended. All participants were right-censored at 130 days after the first reminder date (144 days if we include the two pre-intervention weeks). The solid horizontal line indicates the proportion of girls in the pure control group that had been vaccinated by the end of 130 days after the first message.

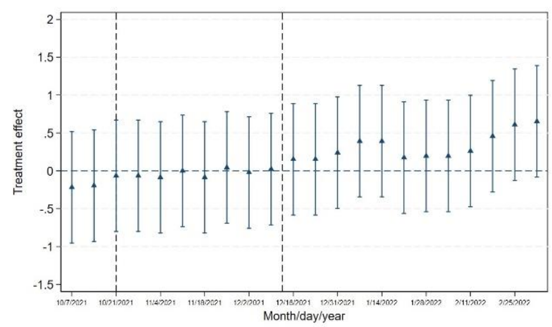
Figure 7. Effect of social norms on HPV vaccination over time by type of norm



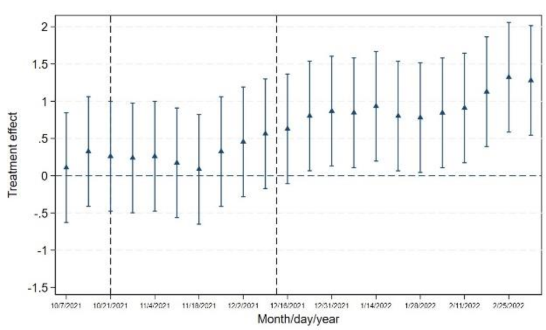
(a) Injunctive norm



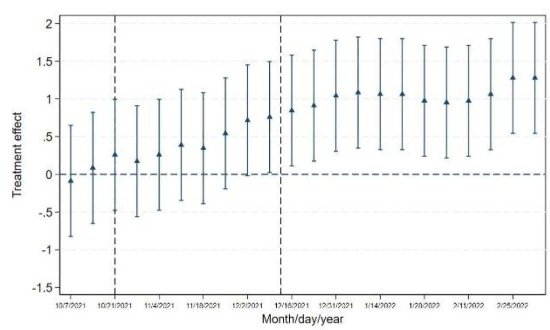
(b) Positive descriptive norm



(c) Negative descriptive norm



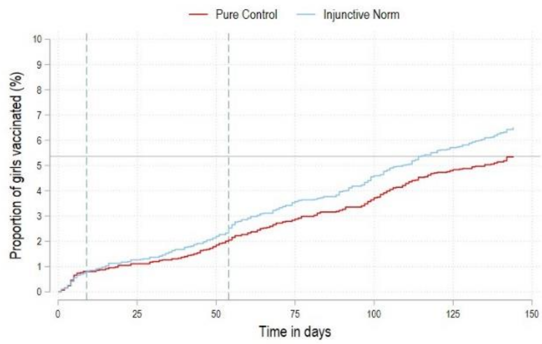
(d) Dynamic norm



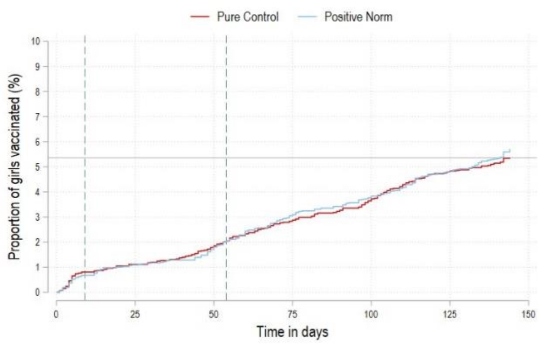
(e) Trending norm

Note: Authors' elaboration. Horizontal axis gives date at which the outcome was measured. Vertical axis is the treatment effect in percentage points, estimated by pooling data from all outcome periods, estimating Eq.(1) with period dummy variables and one treatment dummy per period. Each triangle measures the cumulative effect for each social norm intervention over time. Vertical bars represent 95% confidence intervals, where standard errors are clustered at the locality level. Dashed vertical lines indicate dates that treatment began and ended.

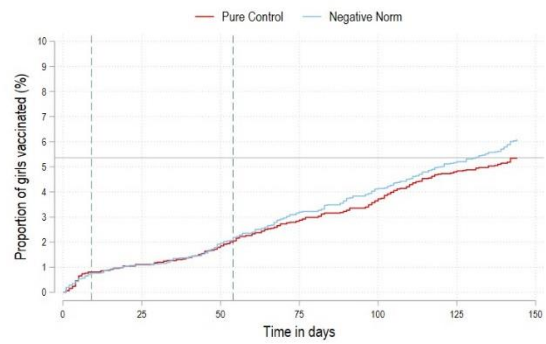
Figure 8. Kaplan-Meier survival function for HPV vaccination by type of norm



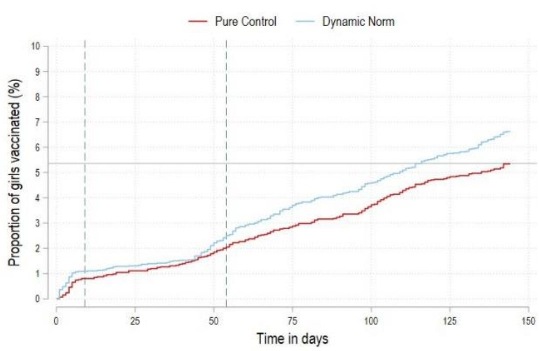
(a) Injunctive norm



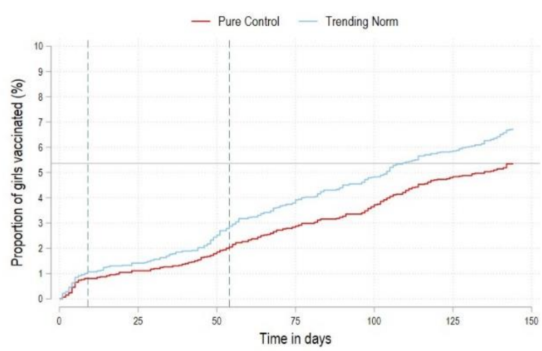
(b) Positive descriptive norm



(c) Negative descriptive norm



(d) Dynamic norm



(e) Trending norm

Note: Authors' elaboration. Horizontal axis gives the number of days after receiving the first message. Vertical axis is the proportion of vaccinated girls. Dashed vertical lines indicate dates that treatment began and ended. All participants were right-censored at 130 days after the first message was delivered. The solid horizontal line indicates the proportion of girls in the pure control group that had been vaccinated by the end of 130 days after the first message.

Table 1. Text message content by social norm treatment

Treatment	Text message content	Social norm element
Pure control	*No message*	None
Experimental control	*Hi [Name of the parent]. Vaccinate her against HPV: give her all the protection. Secretariat of Health*	None
Policy control	*Vaccinate them: Give your son or daughter all the protection. Look up http://aldm.co/Eq2vT9s for the nearest location. Secretariat of Health*	None
Positive descriptive norm	*Hi [Name of the parent]. 3 of every 10 parents in your locality vaccinated their daughter against HPV and protected them from cancer. Secretariat of Health.*	Descriptive norm
Negative descriptive norm	*Hi [Name of the parent]. 7 of every 10 parents in your locality lost the opportunity to vaccinate their daughter against HPV and protect them from cancer. Secretariat of Health*	Descriptive norm
Injunctive norm	*Hi [Name of the parent]. 7 of every 10 parents in your locality lost the opportunity to vaccinate their daughter against HPV and protect them from cancer :(. Secretariat of Health*	Descriptive and Injunctive norm (emoticon)
Dynamic norm	*Hi [Name of the parent]. Since 2016, 3 of every 10 parents in Bogota began vaccinating their daughters against HPV to protect them from cancer. Secretariat of Health*	Dynamic norm
Trending norm	*Hi [Name of the parent]. 3 of every 10 parents in Bogota vaccinated their daughter to protect them from cancer, an increase of 128% since 2016. Secretariat of Health*	Dynamic norm

Note: Authors' elaboration.

Table 2. Descriptive statistics of experimental sample

	Mean	Std.Dev.	Min	Max	N
Daughter's Age	10.74	1.719	9	17	34,506
EPS Sanitas	0.132	0.338	0	1	34,506
EPS Salud Total	0.112	0.315	0	1	34,506
EPS Famisanar	0.193	0.395	0	1	34,506
EPS Compensar	0.147	0.354	0	1	34,506
EPS Capital Salud	0.110	0.313	0	1	34,506
Other EPS	0.306	0.461	0	1	34,506
Contributory Scheme	0.772	0.419	0	1	34,506
Uninsured	0.038	0.191	0	1	34,506
Subsidized Scheme	0.145	0.352	0	1	34,506
Other scheme	0.045	0.208	0	1	34,506
Ethnic group	0.007	0.085	0	1	34,506
Displaced by armed conflict	0.016	0.126	0	1	34,506
Colombian nationality	0.989	0.102	0	1	34,506
Stratum 1	0.125	0.331	0	1	34,506
Stratum 2	0.474	0.499	0	1	34,506
Stratum 3	0.310	0.462	0	1	34,506
Stratum 4	0.060	0.238	0	1	34,506
Stratum 5	0.019	0.136	0	1	34,506
Stratum 6	0.012	0.109	0	1	34,506

Note: Authors' elaboration. All observable characteristics of the sample are coded as dummy variables and receive a value of 1 if they apply to the girl's record. Variables labelled with "EPS" denote the name of the insurance provider. Contributory insurance refers to insurance plans in which the employee contributes a portion of the premium, with the employer covering the remainder. Uninsured, subsidized insurance, ethnic group, displacement due to armed conflict, Colombian nationality, and contributory insurance are binary variables. "Stratum low" is also binary and was derived by aggregating the two lowest strata used by the local government of Bogotá to define low socioeconomic status.

Table 3. Balance table of covariates per treatment arm and orthogonality test

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Pure control	Experimental control	Policy control	Positive descriptive norm	Negative descriptive norm	Injunctive social norm	Dynamic norm	Trending norm	Joint orthogonality test	
									Control groups	All groups
Daughter's age	10.766	10.73	10.74	10.75	10.74	10.75	10.73	10.74	0.707	0.960
	(0.026)	(0.025)	(0.036)	(0.026)	(0.025)	(0.025)	(0.025)	(0.025)		
Ethnic group	0.009	0.008	0.005	0.007	0.008	0.006	0.007	0.009	0.419	0.613
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)		
Displaced by armed conflict	0.016	0.016	0.013	0.015	0.016	0.018	0.018	0.017	0.609	0.814
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)		
Colombian nationality	0.990	0.990	0.986	0.990	0.988	0.990	0.990	0.991	0.332	0.510
	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)		
Contributory Scheme	0.770	0.764	0.765	0.774	0.769	0.781	0.780	0.771	0.309	0.500
	(0.006)	(0.006)	(0.009)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)		
Uninsured	0.038	0.039	0.038	0.034	0.039	0.045	0.035	0.037	0.978	0.205
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)		
Subsidized Scheme	0.143	0.151	0.154	0.151	0.144	0.132	0.140	0.146	0.261	0.121
	(0.005)	(0.005)	(0.008)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)		
Other scheme	0.049	0.046	0.043	0.041	0.048	0.042	0.045	0.046	0.623	0.645
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)		
EPS Sanitas	0.131	0.133	0.128	0.137	0.130	0.129	0.133	0.131	0.957	0.964
	(0.005)	(0.005)	(0.007)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)		
EPS Salud Total	0.108	0.106	0.113	0.108	0.113	0.119	0.113	0.116	0.428	0.513
	(0.005)	(0.005)	(0.007)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)		
EPS Famisanar	0.193	0.191	0.192	0.191	0.190	0.199	0.197	0.189	0.987	0.940
	(0.006)	(0.006)	(0.008)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)		
EPS Compensar	0.150	0.147	0.153	0.144	0.150	0.140	0.148	0.149	0.818	0.821
	(0.005)	(0.005)	(0.008)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)		
EPS Capital Salud	0.109	0.113	0.119	0.119	0.104	0.100	0.106	0.111	0.396	0.089
	(0.005)	(0.005)	(0.007)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)		
Other EPS	0.308	0.309	0.296	0.302	0.312	0.312	0.303	0.302	0.729	0.811
	(0.007)	(0.007)	(0.010)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)		

Stratum 1	0.125	0.126	0.120	0.124	0.126	0.129	0.116	0.131	0.917	0.500
	(0.005)	(0.005)	(0.007)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)		
Stratum 2	0.474	0.469	0.483	0.472	0.477	0.472	0.486	0.465	0.725	0.604
	(0.007)	(0.007)	(0.010)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)		
Stratum 3	0.307	0.313	0.305	0.312	0.306	0.310	0.309	0.314	0.887	0.987
	(0.007)	(0.007)	(0.010)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)		
Stratum 4	0.064	0.062	0.060	0.062	0.058	0.058	0.058	0.060	0.608	0.901
	(0.004)	(0.004)	(0.005)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)		
Stratum 5	0.018	0.019	0.020	0.017	0.021	0.017	0.018	0.021	0.977	0.788
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)		
Stratum 6	0.012	0.012	0.012	0.013	0.012	0.014	0.013	0.009	0.987	0.525
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)		
Observations	4,600	4,603	2,302	4,598	4,599	4,599	4,603	4,602		

Authors' elaboration. Columns 1 to 8 present, for each treatment arm, the mean and the standard error (in parentheses) for each covariate. Column 9 displays the p-value of a joint orthogonality test for all control groups, assessing the null hypothesis that the means of control groups are equal for a given covariate. Column 10 provides the p-value of a joint orthogonality test for all treatment and control groups, evaluating the null hypothesis that the means of all treatment and control arms are equal for a given covariate. The last row indicates the number of observations for each treatment arm. All observable characteristics of the sample are coded as dummy variables and receive a value of 1 if they apply to the girl's record. Variables labeled with "EPS" denote the name of the insurance provider. Contributory insurance refers to insurance plans in which the employee contributes a portion of the premium, with the employer covering the remainder. Uninsured, subsidized insurance, ethnic group, displacement due to armed conflict, Colombian nationality, and contributory insurance are binary variables. "Stratum low" is also binary and was derived by aggregating the two lowest strata used by the local government of Bogotá to define low socioeconomic status.

Table 4. The effect of static and dynamic social norms on HPV vaccine uptake

	(1)	(2)	(3)	(4)
	Dependent variable: (1=if daughter was vaccinated)*100			
	Panel A: Aggregate treatment			
Experimental control	0.254	0.271	0.269	0.251
	(0.703)	(0.705)	(0.705)	(0.703)
	[0.722]	[0.705]	[0.703]	[0.726]
Policy control	-0.785	-0.781	-0.772	-0.786
	(0.566)	(0.577)	(0.564)	(0.566)
	[0.275]	[0.289]	[0.257]	[0.273]
Social norms treatment	0.907***	0.910***	0.905***	0.907***
	(0.255)	(0.260)	(0.257)	(0.254)
	[0.007]	[0.008]	[0.002]	[0.007]
	Panel B: Subgroup aggregation for the treatment			
Experimental control	0.254	0.271	0.269	0.251
	(0.703)	(0.705)	(0.705)	(0.703)
	[0.722]	[0.705]	[0.723]	[0.726]
Policy control	-0.785	-0.781	-0.773	-0.786
	(0.566)	(0.577)	(0.564)	(0.566)
	[0.244]	[0.257]	[0.228]	[0.243]
Static design	0.661**	0.671**	0.663**	0.660**
	(0.257)	(0.263)	(0.262)	(0.257)
	[0.039]	[0.041]	[0.023]	[0.039]
Dynamic design	1.276***	1.267***	1.268***	1.275***
	(0.372)	(0.374)	(0.372)	(0.371)
	[0.012]	[0.014]	[0.003]	[0.012]
Mean dependent variable	5.56	5.56	5.56	5.56
Controls	No	Yes	Yes	No
Model	OLS	OLS	Double-Lasso	Robust to contamination bias
Number of observations	34,506	34,506	34,506	34,506

Note: * significant at 10%; ** significant at 5%; *** significant at 1%. Huber-White standard errors, clustered at the locality level, are reported in parentheses. Benjamin and Hochberg's (1995) q-values are reported in brackets and were estimated using the algorithm outlined in Anderson (2008). The dependent variable has been rescaled using a scale factor of 100 to facilitate the interpretation of the results. Panel A presents the results of a linear regression model where all the social norm treatments were collapsed into a single treatment variable. Panel B reports the results of a similar exercise where the static social norms treatments (positive descriptive, negative descriptive, and injunctive norms) and dynamic social norms treatments (dynamic and trending norms) were collapsed into single treatment variables. Column 1 reports the results of a simple linear regression of the treatment on the dependent variable without covariates, controlling for membership in the experimental and policy control groups. The reference group is the pure control group. Column 2 presents the results of the previous exercise, including control variables. Column 3 reports the results of a linear regression where covariates were selected using the double-selection LASSO algorithm developed by Belloni et al., (2014). Column 4 reports the results of an exercise to evaluate the magnitude of contamination bias in the linear regressions reported in Column 1. We apply the decomposition proposed by Goldsmith-Pinkham et al., (2024) to estimate own-treatment effect components from the estimated regression, thus removing contamination bias. The mean of the dependent variable was calculated for the pure control group. The number of observations is reported in the last row.

Table 5. Disentangling the effects of static and dynamic social norms on HPV vaccine uptake

	(1)	(2)	(3)	(4)
	Dependent variable: (1=if daughter was vaccinated)*100			
Experimental control	0.254	0.271	0.269	0.251
	(0.703)	(0.705)	(0.705)	(0.703)
	[0.722]	[0.705]	[0.703]	[0.726]
Policy control	-0.785	-0.781	-0.773	-0.786
	(0.566)	(0.577)	(0.564)	(0.566)
	[0.256]	[0.270]	[0.240]	[0.255]
Positive descriptive norm	0.243	0.234	0.232	0.240
	(0.374)	(0.382)	(0.379)	(0.374)
	[0.611]	[0.640]	[0.631]	[0.618]
Negative descriptive norm	0.651**	0.659**	0.655**	0.651**
	(0.259)	(0.258)	(0.261)	(0.258)
	[0.062]	[0.057]	[0.036]	[0.062]
Injunctive social norm	1.088**	1.120**	1.102**	1.085**
	(0.477)	(0.481)	(0.475)	(0.477)
	[0.062]	[0.057]	[0.036]	[0.062]
Dynamic norm	1.274**	1.258**	1.256**	1.275**
	(0.545)	(0.543)	(0.541)	(0.544)
	[0.062]	[0.057]	[0.036]	[0.062]
Trending norm	1.278***	1.277***	1.280***	1.275***
	(0.298)	(0.302)	(0.301)	(0.298)
	[0.004]	[0.004]	[0.001]	[0.004]
Mean of dependent variable	5.56	5.56	5.56	5.56
Controls	No	Yes	Yes	No
Model	OLS	OLS	Double -Lasso	Robust to contamination bias
Number of observations	34,506	34,506	34,506	34,506

Note: * significant at 10%; ** significant at 5%; *** significant at 1%. Huber-White standard errors, clustered at the locality level, are reported in parentheses. Benjamin and Hochberg's (1995) q-values are reported in brackets and were estimated using the algorithm outlined in Anderson (2008). The dependent variable has been rescaled using a scale factor of 100 to facilitate the interpretation of the results. Column 1 reports the results of a simple linear regression of the treatment on the dependent variable without covariates, controlling for membership in the experimental and policy control groups. The reference group is the pure control group. Column 2 presents the results of the previous exercise, including control variables. Column 3 reports the results of a linear regression where covariates were selected using the double-selection LASSO algorithm developed by Belloni et al., (2014). Column 4 reports the results of an exercise to evaluate the magnitude of contamination bias in the linear regressions reported in Column 1. We apply the decomposition proposed by Goldsmith-Pinkham et al., (2024) to estimate own-treatment effect components from the estimated regression, thus removing contamination bias. The mean of the dependent variable was calculated for the pure control group. The number of observations is reported in the last row.

Table 6. Full factorial model for social norms and planning tool experiment

	(1)	(2)	(3)	(4)
	Dependent variable: (1=if daughter was vaccinated)*100			
Experimental control	-0.524	0.0338	0.0338	0.0339
	(0.556)	(0.580)	(0.580)	(0.580)
Social norms treatment		0.929***		
		(0.287)		
Static design			0.753**	
			(0.321)	
Dynamic design			1.192***	
			(0.379)	
Positive descriptive norm				0.915**
				(0.340)
Negative descriptive norm				0.261
				(0.368)
Injunctive social norm				1.084
				(0.644)
Dynamic norm				0.996
				(0.726)
Trending norm				1.388***
				(0.313)
Planning tool-link	0.255	0.916*	0.916*	0.916*
	(0.163)	(0.454)	(0.454)	(0.454)
Experimental control x link		-0.475	-0.475	-0.475
		(0.641)	(0.641)	(0.641)
Social norm treatment x link		-0.960		
		(0.596)		
Static design x link			-1.102	
			(0.643)	
Dynamic design x link			-0.749	
			(0.576)	
Positive norm x link				-2.260***
				(0.612)
Negative norm x link				-0.137
				(0.804)
Injunctive norm x link				-0.908
				(0.893)
Dynamic norm x link				-0.361
				(0.883)
Trending norm x link				-1.136
				(0.791)
Mean of dependent variable	5.56	5.56	5.56	5.56
Number of observations	34,501	34,501	34,501	34,501

Note: * significant at 10%; ** significant at 5%; *** significant at 1%. Huber-White standard errors, clustered at the locality level, are reported in parentheses. The dependent variable has been rescaled using a scale factor of 100 to enhance result interpretation. Column 1 presents the outcomes of a simple linear regression of the planning tool on the dependent variable without covariates, while controlling for membership in the experimental group, with the pure control group serving as the reference. Column 2 displays the results of the full factorial model, where all the social norms treatments were combined into a single treatment variable and interacted with the planning tool treatment. Column 3 displays the results of the full factorial model, where static social norms treatments (positive descriptive, negative descriptive, and injunctive norms) and dynamic social norms treatments (dynamic and trending norms) were each combined into two separate treatment variables, each interacted with the planning tool treatment. Column 4 reports the findings of the full factorial model where all social norm treatments are interacted with the planning tool treatment. The mean of the dependent variable was calculated for the pure control group. The total number of observations is provided in the last row.

Table 7. Heterogeneous effects of social norm interventions

	(1)	(2)	(3)	(4)	(5)
	Dependent variable: (1= if daughter was vaccinated)*100				
Experimental control	0.256	0.254	0.251	0.259	0.257
	(0.705)	(0.704)	(0.702)	(0.701)	(0.706)
Policy control	-0.788	-0.786	-0.797	-0.778	-0.785
	(0.567)	(0.565)	(0.566)	(0.567)	(0.563)
Social norms treatment	0.975**	0.951***	0.884***	1.024***	0.889***
	(0.377)	(0.234)	(0.253)	(0.280)	(0.257)
Stratum low	0.521				
	(0.420)				
Social norms treatment x stratum low	-0.114				
	(0.429)				
Displaced by the armed conflict		-0.440			
		(1.417)			
Social norms treatment x displaced by the armed conflict		-2.617			
		(1.989)			
Ethnic group			-3.548**		
			(1.532)		
Social norms treatment x ethnic group			2.518		
			(1.522)		
Subsidized scheme				-0.646	
				(0.465)	
Social norms treatment x subsidized scheme				-0.821	
				(0.568)	
Uninsured					-4.417***
					(0.519)
Social norms treatment x uninsured					0.404
					(0.331)
Mean of dependent variable	5.56	5.56	5.56	5.56	5.56
Control type	Pure control	Pure control	Pure control	Pure control	Pure control
Number of observations	34,506	34,506	34,506	34,506	34,506

Note: * significant at 10%; ** significant at 5%; *** significant at 1%. Huber-White standard errors, clustered at the locality level, are reported in parentheses. The dependent variable has been rescaled using a scale factor of 100 to improve result interpretation. Each column presents the results of estimating equation 2 for every covariate. The regression model and covariates were pre-specified in the pre-analysis plan, except for the subsidized scheme. This variable replaced the covariate "Colombian nationality," which was excluded due to its lack of variability (99% of the sample consisted of Colombian nationals). The average of the dependent variable was computed for the pure control group. The total number of observations is provided in the last row.

Table 8. Cost-effectiviness analysis

Panel A: Costs	
Implementation costs (USD)	
Labor costs	678.94
SMS costs	819.90
Total costs	1,498.84
Girls covered	29,906
Total cost per girl	0.05
Panel B: Cost-effectiveness	
Average impact of social norm treatment	0.0091
Cost per girl	5.53
Average impact of dynamic/injunctive social norm treatment	0.0128
Cost per girl	3.93

Note: All amounts are in 2021 dollars on the first day of the intervention. The Online Appendix discusses the cost assumptions. A girl is covered if she receives a message regardless of her treatment status.

Online Appendix to Accompany "Building a shield together: Addressing low vaccine uptake against cancer through social norms"*

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This version: April 10th, 2024

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Appendix A: Figures and Tables

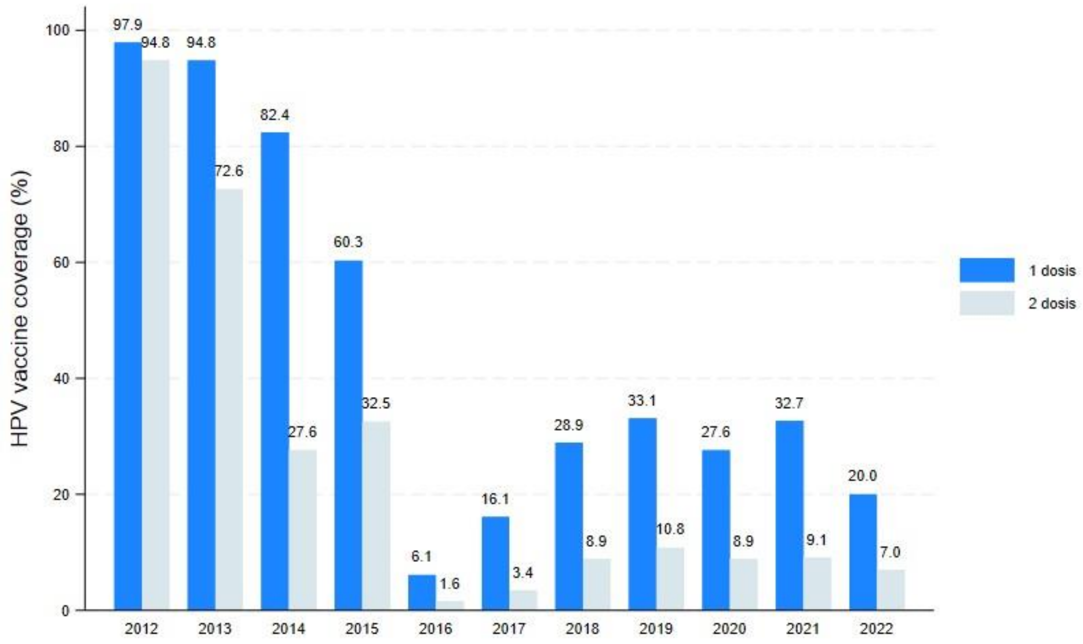


Figure A.1 HPV vaccination rates in Colombia since the introduction of the vaccine in 2012.

Note: Authors' elaboration based on data from the Information System of the Expanded Immunization Program (PAI) of the Ministry of Health and Social Protection of Colombia.

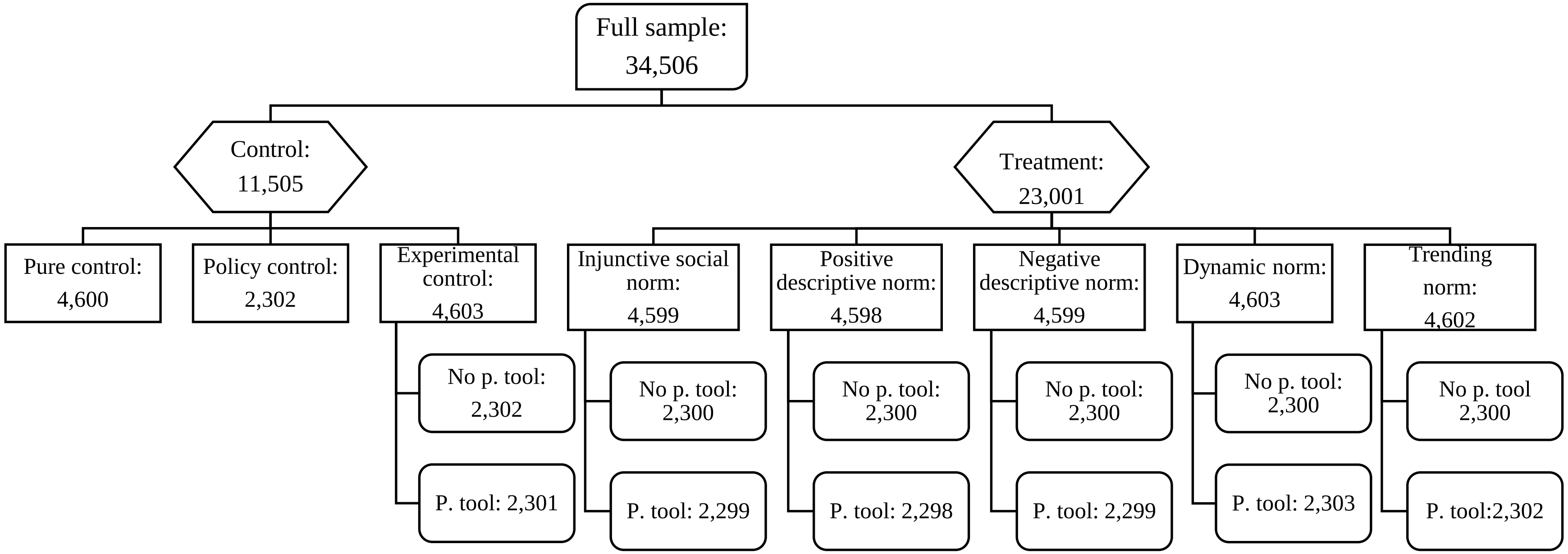


Figure A.2 Graphical representation of full factorial experimental design.

Note: Authors' elaboration.

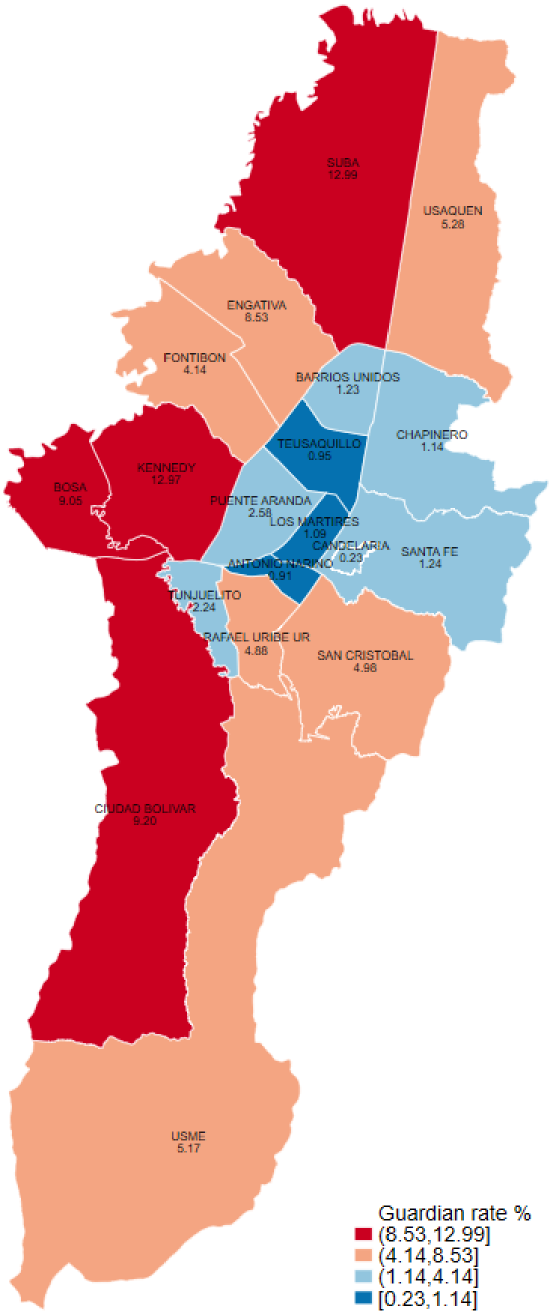
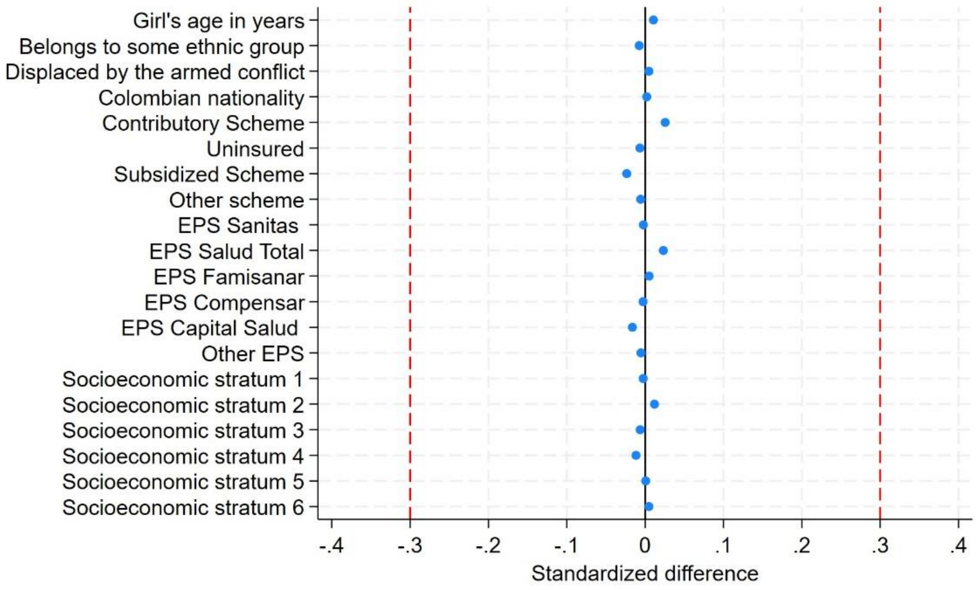


Figure A.3 Distribution of Bogota's target population by locality

Note: Authors' elaboration.

(a) Experimental control



(b) Policy control

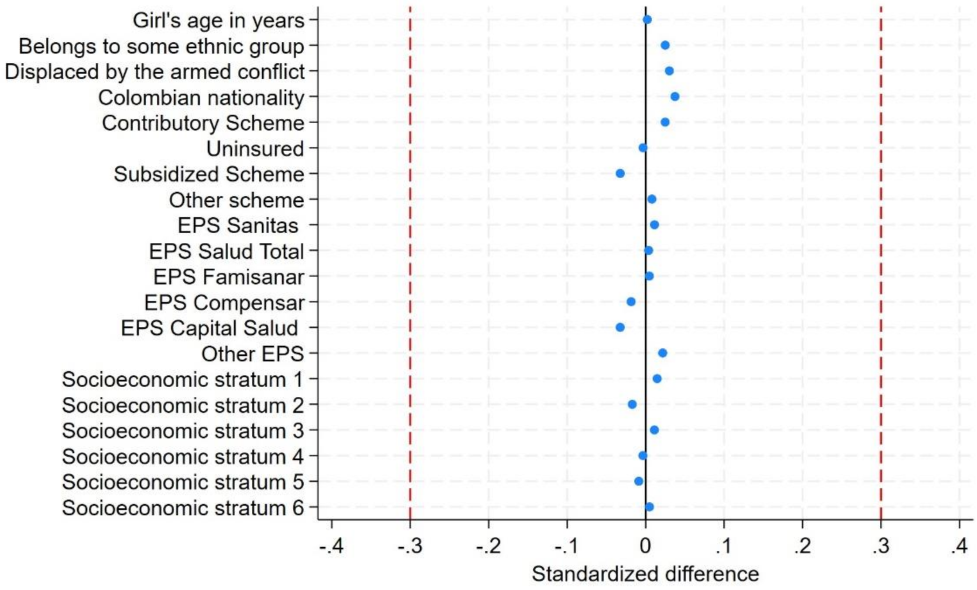
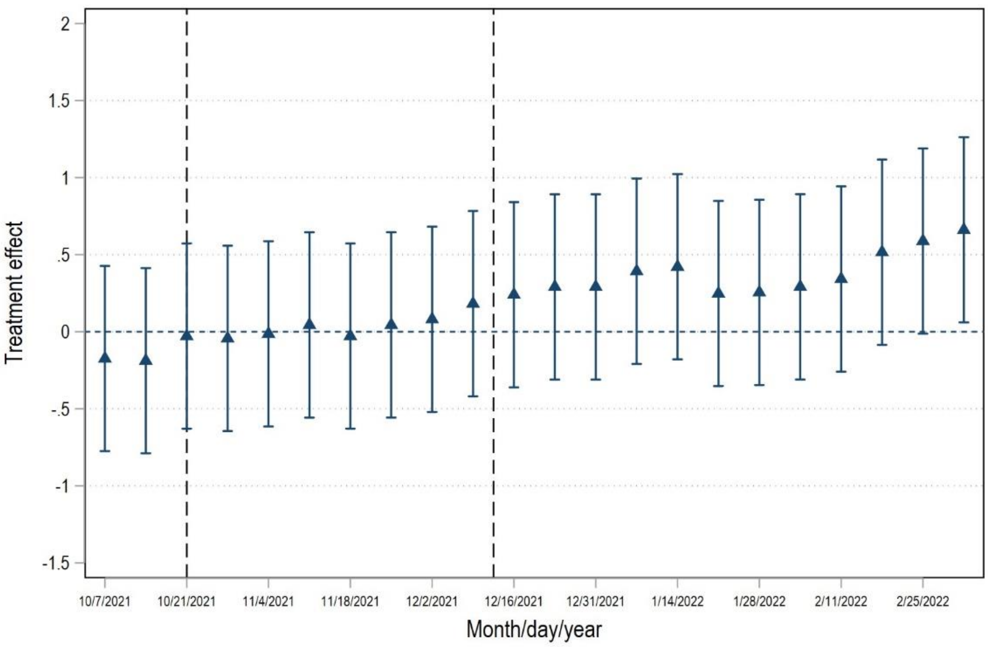


Figure A.4 Pre-treatment balance plot

Note: Authors' elaboration. This figure reports the standardized differences between the average social norm treatment and the experimental control group (a) and policy control group (b). Each dot represents a value of the standardized difference for a given covariate. Dashed red lines at the |0.3| of a standardized difference delineate the threshold at which a difference between the means between the treatment and the control group is considered statistically significant. This value was proposed by Imbens (2015).

(a) Static design



(b) Dynamic design

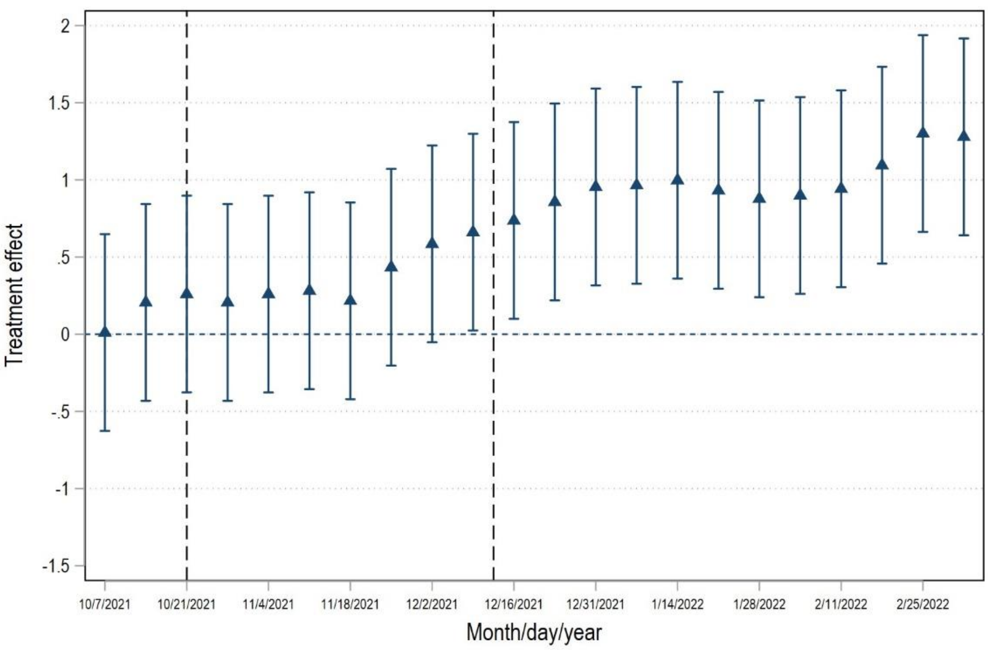
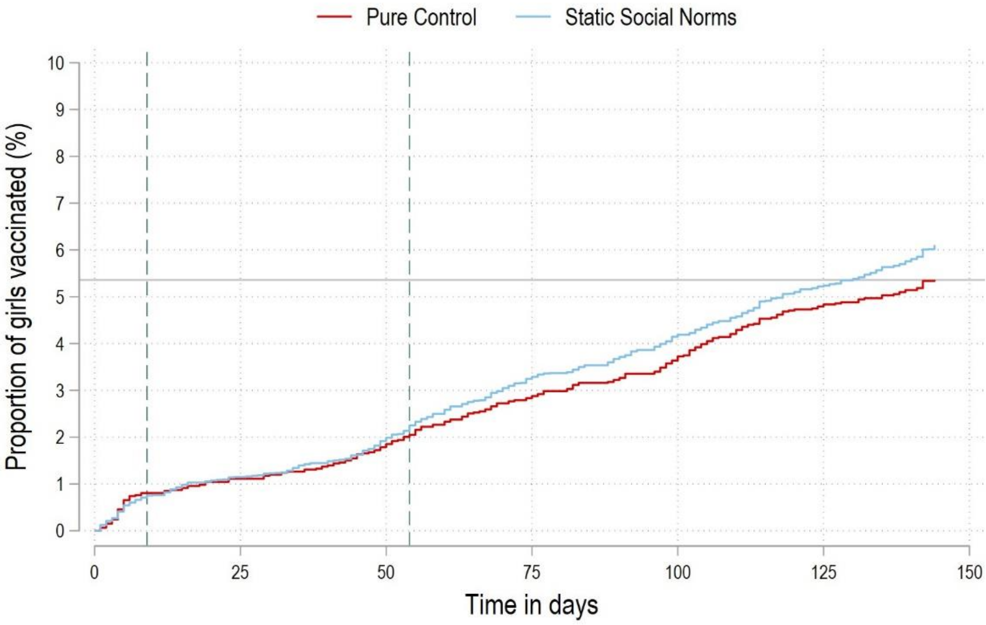


Figure A.5 Effect of social norms on HPV vaccination over time

Note: Authors' elaboration. Horizontal axis gives date at which the outcome was measured. Vertical axis is the treatment effect in percentage points, estimated by pooling data from all outcome periods, estimating Eq.(1) with period dummy variables and one treatment dummy per period. Each triangle measures the cumulative effect of the social norm intervention over time. Vertical bars represent 95% confidence intervals, where standard errors are clustered at the locality level. Dashed vertical lines indicate dates that treatment began and ended.

(a) Static design



(b) Dynamic design

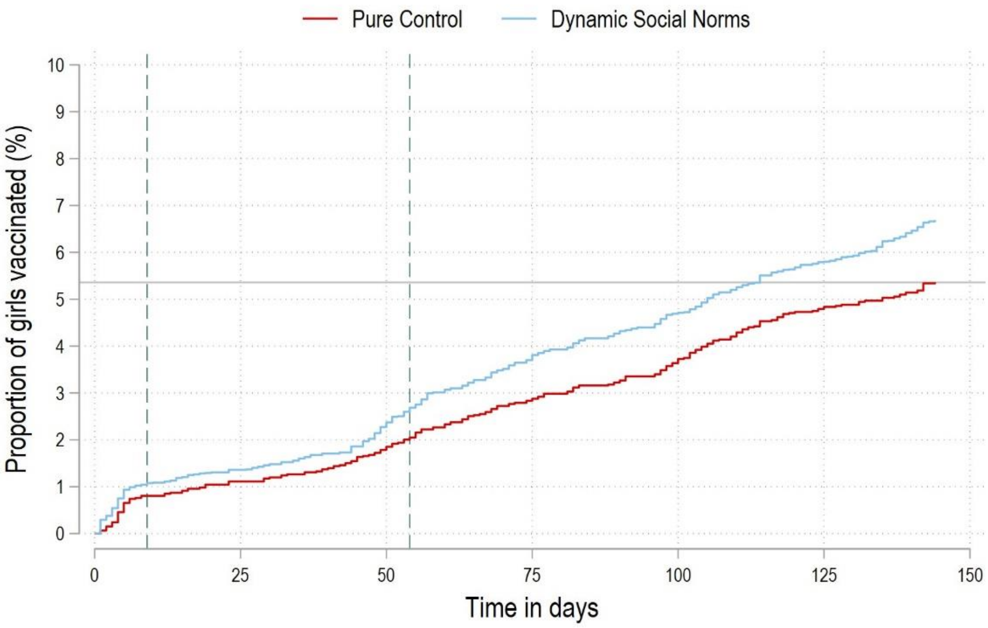


Figure A.6 Kaplan-Meier survival function for HPV vaccination

Note: Authors' elaboration. Horizontal axis gives the number of days after receiving the first message. Vertical axis is the proportion of vaccinated girls. Dashed vertical lines indicate dates that treatment began and ended. All participants were right-censored at 130 days after the first reminder date (144 days if we include the two pre-intervention weeks). The solid horizontal line indicates the proportion of girls in the pure control group that had been vaccinated by the end of 130 days after the first message.

Table A1. p-values for the orthogonality test of pairwise comparisons between treatment arms

	C1 vs C2	C1 vs C3	C1 vs T1	C1 vs T2	C1 vs T3	C1 vs T4	C1 vs T5	C2 vs C3	C2 vs T1	C2 vs T2	C2 vs T3	C2 vs T4	C2 vs T5
Daughter's age	0.247	0.542	0.705	0.414	0.723	0.338	0.442	0.741	0.433	0.725	0.416	0.845	0.696
Ethnic group	0.643	0.115	0.284	0.563	0.144	0.282	0.998	0.219	0.543	0.908	0.317	0.540	0.644
Displaced by armed conflict	0.937	0.359	0.802	0.934	0.515	0.378	0.744	0.328	0.742	0.872	0.567	0.422	0.805
Colombian nationality	0.997	0.156	0.677	0.376	0.837	0.833	0.524	0.155	0.679	0.375	0.839	0.836	0.526
Contributory Scheme	0.517	0.613	0.663	0.936	0.214	0.254	0.873	0.980	0.278	0.570	0.058	0.074	0.419
Uninsured	0.876	0.995	0.241	0.912	0.129	0.341	0.657	0.893	0.184	0.964	0.173	0.268	0.549
Subsidized Scheme	0.282	0.216	0.272	0.926	0.131	0.710	0.684	0.725	0.982	0.326	0.010	0.148	0.503
Other scheme	0.519	0.291	0.088	0.925	0.148	0.426	0.553	0.592	0.289	0.582	0.422	0.881	0.958
EPS Sanitas	0.839	0.714	0.458	0.832	0.690	0.768	0.982	0.594	0.589	0.677	0.547	0.927	0.857
EPS Salud Total	0.779	0.594	0.925	0.443	0.093	0.513	0.250	0.445	0.852	0.295	0.050	0.350	0.153
EPS Famisanar	0.821	0.867	0.799	0.735	0.475	0.666	0.645	0.986	0.978	0.911	0.347	0.510	0.815
EPS Compensar	0.759	0.714	0.449	0.904	0.194	0.827	0.970	0.537	0.652	0.669	0.321	0.930	0.788
EPS Capital Salud	0.536	0.252	0.156	0.440	0.164	0.654	0.745	0.524	0.424	0.164	0.044	0.286	0.769
Other EPS	0.963	0.307	0.506	0.730	0.663	0.573	0.547	0.289	0.476	0.765	0.698	0.541	0.516
Stratum 1	0.860	0.596	0.856	0.847	0.549	0.196	0.353	0.500	0.720	0.987	0.673	0.142	0.453
Stratum 2	0.625	0.463	0.866	0.715	0.892	0.255	0.428	0.257	0.749	0.393	0.724	0.104	0.761
Stratum 3	0.542	0.850	0.626	0.933	0.781	0.873	0.494	0.493	0.903	0.488	0.740	0.653	0.941
Stratum 4	0.693	0.521	0.703	0.223	0.278	0.219	0.383	0.748	0.989	0.410	0.490	0.404	0.633
Stratum 5	0.942	0.666	0.697	0.451	0.475	0.997	0.411	0.710	0.643	0.496	0.431	0.938	0.453
Stratum 6	0.921	0.934	0.573	0.847	0.406	0.517	0.184	0.999	0.508	0.926	0.352	0.455	0.219

Authors' elaboration: Each column presents the p-value for the pairwise comparisons between treatment arms. Each test assesses the null hypothesis that the means of two treatment arms are equal to zero.C1 is the pure control group. C2 is the experimental control group. C3 is the policy control group. T1 is the positive descriptive norm treatment. T2 is the negative descriptive norm treatment. T3 is the injunctive norm treatment. T4 is the dynamic norm treatment. T5 is the trending norm treatment. All observable characteristics of the sample are coded as dummy variables and receive a value of 1 if they apply to the girl's record. Variables labeled with "EPS" denote the name of the insurance provider. Contributory insurance refers to insurance plans in which the employee contributes a portion of the premium, with the employer covering the remainder. Uninsured, subsidized insurance, ethnic group, displacement due to armed conflict, Colombian nationality, and contributory insurance are binary variables. "Stratum low" is also binary and was derived by aggregating the two lowest strata used by the local government of Bogotá to define low socioeconomic status.

Table A2. p-values for the orthogonality test of pairwise comparisons between treatment arms

	C3 vs T1	C3 vs T2	C3 vs T3	C3 vs T4	C3 vs T5	T1 vs T2	T1 vs T3	T1 vs T4	T1 vs T5	T2 vs T3	T2 vs T4	T2 vs T5	T3 vs T4	T3 vs T5	T4 vs T5
Daughter's age	0.759	0.963	0.742	0.866	0.990	0.661	0.980	0.559	0.694	0.641	0.879	0.967	0.540	0.674	0.847
Ethnic group	0.447	0.254	0.652	0.449	0.115	0.622	0.695	0.997	0.285	0.376	0.619	0.564	0.698	0.145	0.283
Displaced by armed conflict	0.473	0.395	0.154	0.109	0.241	0.867	0.368	0.258	0.564	0.463	0.335	0.683	0.818	0.745	0.579
Colombian nationality	0.076	0.503	0.111	0.110	0.049	0.194	0.833	0.836	0.825	0.276	0.274	0.129	0.997	0.666	0.669
Contributory Scheme	0.388	0.660	0.127	0.149	0.524	0.606	0.419	0.481	0.783	0.186	0.222	0.810	0.917	0.279	0.327
Uninsured	0.337	0.923	0.218	0.437	0.721	0.200	0.007	0.826	0.466	0.159	0.288	0.579	0.014	0.050	0.611
Subsidized Scheme	0.739	0.246	0.013	0.123	0.367	0.315	0.009	0.142	0.489	0.109	0.642	0.754	0.255	0.055	0.437
Other scheme	0.742	0.328	0.907	0.678	0.563	0.107	0.796	0.362	0.266	0.176	0.484	0.618	0.514	0.392	0.840
EPS Sanitas	0.332	0.846	0.967	0.544	0.700	0.339	0.254	0.654	0.471	0.852	0.612	0.814	0.487	0.673	0.785
EPS Salud Total	0.542	0.924	0.403	0.998	0.684	0.389	0.077	0.455	0.214	0.363	0.909	0.703	0.306	0.597	0.620
EPS Famisanar	0.968	0.913	0.454	0.603	0.835	0.933	0.333	0.493	0.837	0.292	0.441	0.903	0.778	0.240	0.372
EPS Compensar	0.323	0.789	0.151	0.586	0.691	0.380	0.588	0.590	0.472	0.156	0.734	0.874	0.280	0.207	0.857
EPS Capital Salud	0.985	0.074	0.021	0.129	0.380	0.029	0.005	0.062	0.274	0.536	0.745	0.272	0.345	0.086	0.440
Other EPS	0.632	0.193	0.169	0.574	0.596	0.312	0.271	0.919	0.949	0.928	0.363	0.343	0.318	0.300	0.969
Stratum 1	0.702	0.492	0.310	0.599	0.200	0.708	0.435	0.267	0.267	0.685	0.137	0.462	0.059	0.742	0.026
Stratum 2	0.383	0.663	0.398	0.845	0.167	0.594	0.975	0.191	0.533	0.616	0.439	0.247	0.202	0.512	0.053
Stratum 3	0.558	0.904	0.678	0.750	0.455	0.568	0.834	0.743	0.844	0.718	0.808	0.443	0.906	0.684	0.600
Stratum 4	0.740	0.725	0.809	0.719	0.946	0.402	0.482	0.397	0.624	0.894	0.992	0.729	0.886	0.831	0.721
Stratum 5	0.450	0.852	0.304	0.663	0.809	0.253	0.746	0.700	0.226	0.142	0.448	0.945	0.478	0.125	0.408
Stratum 6	0.592	0.940	0.453	0.546	0.305	0.450	0.788	0.933	0.059	0.306	0.401	0.255	0.854	0.031	0.049

Authors' elaboration: Each column presents the p-value for the pairwise comparisons between treatment arms. Each test assesses the null hypothesis that the means of two treatment arms are equal to zero.C1 is the pure control group. C2 is the experimental control group. C3 is the policy control group. T1 is the positive descriptive norm treatment. T2 is the negative descriptive norm treatment. T3 is the injunctive norm treatment. T4 is the dynamic norm treatment. T5 is the trending norm treatment. All observable characteristics of the sample are coded as dummy variables and receive a value of 1 if they apply to the girl's record. Variables labeled with "EPS" denote the name of the insurance provider. Contributory insurance refers to insurance plans in which the employee contributes a portion of the premium, with the employer covering the remainder. Uninsured, subsidized insurance, ethnic group, displacement due to armed conflict, Colombian nationality, and contributory insurance are binary variables. "Stratum low" is also binary and was derived by aggregating the two lowest strata used by the local government of Bogotá to define low socioeconomic status.

Table A.3 Contamination bias in the social norms experiment

	(1)	(2)	(3)	(4)
	Dependent variable: (1=if daughter was vaccinated)*100			
	Panel A: Aggregate treatment			
Experimental control	0.251	0.253	0.252	0.252
	(0.703)	(0.703)	(0.703)	(0.702)
Policy control	-0.786	-0.788	-0.786	-0.787
	(0.566)	(0.566)	(0.566)	(0.566)
Social norms treatment	0.907***	0.905***	0.907***	0.906***
	(0.254)	(0.255)	(0.254)	(0.255)
	Panel B: Subgroup aggregation for the treatment			
Experimental control	0.251	0.253	0.252	0.252
	(0.703)	(0.702)	(0.703)	(0.702)
Policy control	-0.786	-0.788	-0.786	-0.787
	(0.566)	(0.566)	(0.566)	(0.566)
Static design	0.660**	0.659**	0.660**	0.659**
	(0.257)	(0.257)	(0.257)	(0.257)
Dynamic design	1.275***	1.275***	1.277***	1.275***
	(0.372)	(0.372)	(0.372)	(0.372)
Mean dependent variable	5.56	5.56	5.56	5.56
Estimator	Own	ATE	EW	CW
Number of observations	34,506	34,506	34,506	34,506

Note: * significant at 10%; ** significant at 5%; *** significant at 1%. Huber-White standard errors, clustered at the locality level, are reported in parentheses. The dependent variable has been rescaled using a scale factor of 100 to facilitate the interpretation of the results. Panel A presents the results of a linear regression model where all the social norm treatments were collapsed into a single treatment variable. Panel B reports the results of a similar exercise where the static social norms treatments (positive descriptive, negative descriptive, and injunctive norms) and dynamic social norms treatments (dynamic and trending norms) were collapsed into single treatment variables. Column 1 reports the results of an exercise to evaluate the magnitude of contamination bias in the linear regressions reported in Column 1 in Table 4. We apply the decomposition proposed by Goldsmith-Pinkham et al., (2024) to estimate own-treatment effect components from the estimated regression, thus removing contamination bias. Column 2 presents the results for the estimation of the unweighted ATE. Column 3 shows the weighted ATE using the variance-minimizing weighting scheme. Column 4 reports the weighted ATE using the easiest-to-estimate common weighting scheme. The mean of the dependent variable was calculated for the pure control group. The number of observations is reported in the last row.

Table A.4 Contamination bias in the social norms experiment

	(1)	(2)	(3)	(4)
	Dependent variable: (1=if daughter was vaccinated)*100			
Experimental control	0.251	0.253	0.252	0.253
	(0.703)	(0.702)	(0.703)	(0.702)
Policy control	-0.786	-0.788	-0.786	-0.788
	(0.566)	(0.566)	(0.566)	(0.566)
Positive descriptive norm	0.240	0.240	0.241	0.240
	(0.374)	(0.374)	(0.374)	(0.374)
Negative descriptive norm	0.651**	0.646**	0.651**	0.646**
	(0.258)	(0.258)	(0.258)	(0.258)
Injunctive social norm	1.085**	1.086**	1.085**	1.086**
	(0.477)	(0.477)	(0.477)	(0.477)
Dynamic norm	1.275**	1.270**	1.275**	1.270**
	(0.544)	(0.544)	(0.544)	(0.544)
Trending norm	1.274***	1.277***	1.276***	1.277***
	(0.298)	(0.298)	(0.298)	(0.298)
Mean of dependent variable	5.56	5.56	5.56	5.56
Model	Own	ATE	EW	CW
Number of observations	34,506	34,506	34,506	34,506

Note: * significant at 10%; ** significant at 5%; *** significant at 1%. Huber-White standard errors, clustered at the locality level, are reported in parentheses. The dependent variable has been rescaled using a scale factor of 100 to facilitate the interpretation of the results. Column 1 reports the results of an exercise to evaluate the magnitude of contamination bias in the linear regressions reported in Column 1 in Table 4. We apply the decomposition proposed by Goldsmith-Pinkham et al., (2024) to estimate own-treatment effect components from the estimated regression, thus removing contamination bias. Column 2 presents the results for the estimation of the unweighted ATE. Column 3 shows the weighted ATE using the variance-minimizing weighting scheme. Column 4 reports the weighted ATE using the easiest-to-estimate common weighting scheme. The mean of the dependent variable was calculated for the pure control group. The number of observations is reported in the last row.

Appendix B: Pre-Analysis Plan

Nudging parents to increase HPV vaccine demand in Bogota, Colombia[1]

Stanislao Maldonado[2], Deborah Martinez[3], Lina Diaz[4], and Erika Rees-Punia[5]

January 20, 2022

Abstract

We study the effectiveness of a large-scale SMS campaign based on behavioral insights to boost vaccine take-up against the Human Papillomavirus in Bogota, Colombia. Messages were crafted taking advantage of behavioral science lessons, including social norms, beliefs, emotions, and a set of decision aids.

1. Introduction

The Human Papillomavirus (HPV) is one of the leading causes of cervical cancer, one of the major public health problems in the developing world. Vaccines against HPV have been available since 2006, yet vaccination coverage is very low. As with many other preventive health investments, adoption is shallow despite their large economic and social benefits.

In this project, we implement large-scale interventions based on behavioral science to boost vaccination rates using cost-effective strategies based on SMS campaigns with parents in Bogota, Colombia. Text messages for this campaign were designed based on behavioral insights, including social norms, beliefs, emotions, and a set of decision aids. These interventions were developed by

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[1] This pre-analysis plan was written by Stanislao Maldonado with inputs by Deborah Martinez, Lina Diaz, and Erika Rees-Punia. Julian Pena provided superb research assistance. Comments by Meenu Anand, Carlos Scartascini, and Carlos Castro are greatly appreciated. This version corresponds to the one registered at the AEA RCT registry on February 2nd, 2022, with minor edits to adjust changes in affiliation.

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a team of researchers at the American Cancer Society, the Behavioral Government Lab (Universidad del Rosario), and the Inter-American Development Bank, and implemented in partnership with the Health Secretariat (HS) of the City of Bogota, and the Liga Colombiana de Lucha contra el Cancer.

This plan outlines a pre-analysis plan for evaluating the effectiveness of the SMS campaign described above. Because the authors completed the plan before endline data was requested and analyzed (March 2022), this document provides a valuable reference in evaluating the results of these interventions.

The plan is outlined as follows: Section 2 reviews the background of the study, the setting of the experiment, and describes the interventions; Section 3 describes the sample and the data; Section 4 outlines the study hypotheses; Section 5 describes the research design, and Section 6 the estimation strategy. The Appendix offers details about the interventions and other aspects of our research design.

2. Motivation and Intervention Overview

2.1. Background

Preventive health investments have large economic and social benefits, yet these health technologies are often adopted at low rates (Newhouse 2021). HPV vaccination is a leading example of this phenomenon. Cervical cancer (CC), caused by infections from HPV, is a significant public health problem in the developing world. Despite that HPV vaccines have been available since 2006, vaccine coverage in most developing countries is still sub-optimal (WHO 2018). According to some estimates, only 15% of girls in the target age for HPV vaccination are fully protected (Bruni et al. 2021).

CC is the leading cause of death from cancer in Colombia's women aged 30 to 59. In addition to CC, HPV is also associated with oropharyngeal, anus, genital (vulva, vagina, and penis), head and neck cancer. Infection by this virus is widespread among women, and it is estimated that 70% of them will acquire HPV at some point in their lives. Currently, the risk of HPV infection can be reduced thanks to a vaccine administered in Colombia through the Expanded Program on Immunization (PAI). This vaccine is targeted to girls and adolescents from fourth grade (9 years or older) to eleventh grade. Non-schooled women from 9 to 18 years old are also covered.

In 2012, Colombia was one of the leaders in HPV vaccination coverage in Latin America. The vaccine became mandatory and was distributed free of charge by the National Committee for Immunization Practices. However, the program's success came to a halt after an outbreak of unknown etiology in the municipality of Carmen de Bolívar. Community members and vaccine opponents claimed this outbreak was associated with the HPV vaccine. This issue led to a public relations crisis that made the vaccine controversial. Eventually, these events led to the collapse of the school's HPV vaccination program. Although safety studies found no association between the HPV vaccine and Carmen de Bolívar’s events, vaccine coverage rates began to decline steadily, reaching their lowest point in 2016 with a 6.1% coverage for the first dose. Although coverage levels of HPV vaccination have been recovering over the past years, they are still far from the pre-Carmen de Bolivar levels, representing a challenge for the vaccination policy in Colombia.

2.2. Setting

The research team designed an SMS campaign to offer solutions to the above challenges. The team designed text messages based on behavioral science to induce parents to vaccinate their daughters against HPV. Taking advantage of essential lessons from the international literature and qualitative work carried out as part of this research project, the team decided to craft text messages that appeal to social norms, seek to modify beliefs, take advantage of emotions, and use a set of decision aids. These interventions were implemented in large-scale experiments with the population of eligible girls in Bogota, Colombia's largest city.

We implemented the SMS campaign using the existing technological structure of the HS and within its vaccination efforts. Due to the current institutional framework in Colombia, health providers report data to the HS about all eligible individuals for vaccination. These include information about their progress in terms of their vaccination schedules. This centralized information system will be instrumental in evaluating the effectiveness of our interventions.

Although each person can be affiliated with a private insurer or covered under the subsidized regime, Bogota’s health system allows citizens to be vaccinated at any vaccination point regardless of their health provider. This is true for all vaccines included in the PAI, as is the case for HPV vaccines. Therefore, we take advantage of this institutional feature to launch a city-wide SMS campaign to boost HPV vaccination rates across almost all city's localities.[6] Our target population can get HPV vaccines from their health providers and vaccination points run by the HS across different areas in the city.

2.3. Interventions

In this setting, we implemented two sets of experiments designed to increase vaccination rates among two groups: a) unvaccinated girls; and b) girls with incomplete HPV vaccination schedules. We treat these groups differently because the behavioral biases preventing HPV vaccination may differ. Because the target population is composed of minors, these interventions were targeted to their parents. The timeline for these experiments is presented in Figure 1.

We describe these experiments below.

2.3.1. Experiments for Unvaccinated Girls

For the first group, four experiments were implemented. The first experiment explored the role of social norms following a large literature on the matter in vaccination and other health areas. A second experiment exploited the role of beliefs, taking advantage of an extensive literature about the role of beliefs in vaccination decisions. A third experiment appealed to emotions to induce behavior change regarding HPV vaccination. Finally, the fourth experiment considered the use of decision aids that directly seek to modify behavior in contexts where there is a gap between intentions and actions.

a. Experiment 1: Social Norms

The social norm experiment exploited alternative ways to communicate the norms regarding HPV vaccination in our setting. Our treatments include a static (descriptive and injunctive norms) and a dynamic design (dynamic and trending norms). Table A1 in the Appendix describes the messages delivered as part of this intervention. As an example, a subset of parents in this experiment received an SMS with a descriptive social norm of the following form:

"Hello NAME. X of each 10 parents in your neighborhood vaccinated their daughters against HPV and protected them against cancer. Health Secretariat."

[6] Bogota is divided in 20 localities. We excluded one locality, Sumapaz, due to its small size in terms of eligible girls.

All messages include fixed elements across treatment arms proven effective in other settings. For instance, we added the parent's name and the sender's information in all the messages in this experiment. We tested other fixed elements in the experiments described below.

b. Experiment 2: Beliefs

The second experiment seeks to correct a set of beliefs regarding HPV. In particular, we were interested in correcting beliefs about HPV, HPV vaccination, and vaccine support by doctors and the government.

The messages in this experiment were designed to correct beliefs about HPV. These messages were targeted to issues of likelihood and severity. Regarding beliefs about HPV vaccines, we paid attention to effectiveness, safety, and cost issues. Finally, we targeted beliefs about government and health provider support for HPV vaccination.

Table A2 in the Appendix describes the messages sent in this experiment.

c. Experiment 3: Emotions

The third experiment explored the role of emotions. There is an important scholarship exploring the role of emotions in vaccination decisions. We designed messages that emphasized anticipated regret, worry, and soft shame to boost vaccination rates based on this research.

Table A3 in the Appendix describes the messages sent in this experiment.

d. Experiment 4: Decision Aids

The fourth experiment uses tools to induce a direct behavioral change, including an appeal to prosocial concerns, adapting them for an SMS format. We are particularly interested in the potential of soft-defaults, enhanced active choice, and pseudo sets in inducing behavioral change. We also explore the role of altruism, considering that previous communication strategies in our setting were built on this principle.

We acknowledge that many of these instruments are not easy to capture using an SMS format. Yet, we hope the messages crafted following these principles offer an alternative way to capture the potential of these instruments in practice.

Table A4 in the Appendix describes the messages sent in this experiment.

2.3.2. Experiments for Girls with Incomplete Vaccination Schedules

The second set of experiments was targeted at girls with incomplete vaccination schedules. We hypothesized that parents of girls with incomplete vaccination schedules are not necessarily affected by the same set of behavioral biases as those affecting parents of unvaccinated girls. To avoid sample constraints, we only implemented two experiments with this sub-population. The first one was based on social norms, following a similar structure to Experiment 1. The second experiment tested different decision aids to help parents close the intention-behavior gap. These experiments are described below.

e. Experiment 5: Social Norms

The fifth experiment used social norms with similar static and dynamic designs as Experiment 1. We introduced a minor variation to incorporate a comparison between qualitative and quantitative dynamic norms. In this way, we can offer a complete picture regarding the use of social norms to induce behavioral change in the context of vaccination.

Table A5 in the Appendix describes the messages sent in this experiment.

f. Experiment 6: Decision Aids

The second experiment introduced a set of decision aids to close the intention-action gap. It was presumed that parents of girls with incomplete vaccination schedules already expressed their desire to vaccinate their daughters, but failed to complete the process. Reminders, presumptive announcements, priming, and planning tools were used in this case. For each of these tools, variations were introduced to complement these messages. These variations were designed to elicit implementation intentions or to introduce an anchor. In the case of priming, question-behavior effects and mere-measurement effects were used.

Table A6 in the Appendix describes the messages sent in this experiment.

2.3.3. Cross-randomized Experiment: Planning Tools

All the treatments in the two sets of experiments were cross-randomized with planning tools. In other words, half of the sample in each of the six experiments described above also received a second message with a planning tool. These planning tools offer a link or a telephone number where parents can have access to information about the closest vaccination point and other related information about HPV vaccination.

Table A7 in the Appendix describes the messages sent in this experiment.

3. Sample and Data

3.1. Target Population

The target population for this intervention consists of parents with unvaccinated or partially vaccinated daughters ages 9-17 who are registered with a cellphone number in the administrative records of the HS. We were forced to discard many observations due to incomplete information, which has implications for the experiment's external validity. We describe this issue below.

The population of unvaccinated girls aged 9-17 in the city is 440,010. Given the nature of the intervention, we need to have information about parents, and therefore, we discarded 216,371 records due to incomplete information about parents. Because the intervention was planned to be delivered using text messages, we dropped those parents with no info about cellphone numbers. Hence, 63,602 observations were discarded in this step. Finally, because the experiments were block-randomized based on locality and girls' age, we dropped all the observations from neighbor localities outside Bogota or records without information regarding their locality. We also dropped records from Sumapaz, a tiny locality in Bogota, with only 41 observations. The final sample size for the experiments with unvaccinated girls is 131,124.

We constructed the sample size for the experiments with girls with incomplete vaccination schedules using the same procedure as above. The population of eligible girls is 93,542. After discarding observations as before, the final sample consists of 43,057 observations.

Figures 2A and 2B summarize the steps we use to construct the final sample for both sets of experiments.

3.2. Data

The primary source of data for the experiments is the HS administrative records. HS administers health records from all the institutional health providers in the city and directs the vaccination policy at the city level. These records are monthly updated, so the information about vaccination progress is handy to test the effectiveness of our interventions.

The main advantage of the HS records for evaluating the impact of our interventions is its coverage. We had access to all the records of eligible girls in the city. The main disadvantage is its limited set of pre-treatment covariates. These variables include enrollment to institutional health providers (EPS), type of health provider, access to health insurance, nationality, ethnicity, displacement by civil war, and daughter's age. Descriptive statistics for the experimental sample are reported in Table 1A for the experiment with unvaccinated girls and Table 1B for the case of girls with incomplete vaccination schedules. Unfortunately, detailed information about socioeconomic characteristics is not collected besides these variables.

Figure 4 shows the distribution of the target population by locality. A large fraction of girls and parents for the experiments are located in populated and low-income areas in the city.

3.3. Power calculations

We estimated the minimum sample size for each experiment assuming an individual randomized design with an effect size of 3 percentage points, 90% power, and a Chi-Square test for a difference in proportions with a 5% significance level. Because of the multiplicity of treatments, we adjusted for multiple testing using a Bonferroni correction for up to 17 comparisons in each experiment. Under these assumptions, the minimum sample size for each experiment is 13,578 subjects.

A critical parameter in power calculations is the effect size (Cohen 1988). A very influential experiment about vaccination in the US (Milkman et al. 2021) used 4.8 percentage points as an effect size to put our assumption in perspective. Therefore, our assumptions seem to be relatively conservative. Moreover, we implemented our experiments with sample sizes larger than this minimum requirement (see Tables A1 to A7).

Notice that we do not exploit the panel structure of our data in our main specifications (see Section 6). This could lead to potential gains in power as suggested by McKenzie (2012). This implies that our power analysis is more conservative than needed.

4. Hypotheses

Because of the nature of our data, we can test a limited set of hypotheses. The main hypothesis is related to the impact of the behavioral-based text messages on HPV vaccination take-up. The second set of hypotheses is related to exploring heterogeneous effects.

Because all the experiments have a similar treatment structure, we formulate a single hypothesis regarding the effectiveness of these interventions and one for the joint effect of both behavioral insights and planning tools.

4.1. Hypothesis about Impact on Outcomes

We consider a set of main individual hypotheses M about the impact on outcomes. We also consider aggregated or global hypothesis G for the effect of any behavioral treatment.

Additionally, we also consider a set of auxiliary hypotheses A regarding additional analysis with the main outcomes for some relevant comparisons.

The main individual hypotheses are the following:

Hypothesis M.1: Receiving a text message based on behavioral insights positively affects HPV vaccination take-up.

Hypothesis M.2: Receiving a text message with a planning tool positively affects HPV vaccination take-up.

Hypothesis M.3: Receiving both a text message based on behavioral insights and one with a planning tool has a larger positive effect on HPV vaccination take-up than the individual effects of these treatments.

All these hypotheses will be evaluated for a single behavioral treatment. Because we test many behavioral treatments, we will also test a general hypothesis about the effectiveness of receiving a message with a behavioral insight regardless of the behavioral principle in each experiment. The hypothesis is the following:

Hypothesis G.1: Receiving a text message based on *any* behavioral insights positively affects HPV vaccination take-up.

All these hypotheses are based on a comparison with the experimental control group. We also consider a set of auxiliary hypotheses related to the different control groups in our experimental designs:

Hypothesis A.1: There is no difference between receiving a placebo message (experimental control) and not receiving a message at all (pure control) in increasing HPV vaccination take-up.

Hypothesis A.2: Receiving the "business as usual" text message has a larger effect than not receiving a message at all (pure control) in increasing HPV vaccination take-up.

Hypothesis A.3: Receiving a text message based on behavioral insights has a larger effect on HPV vaccination take-up than receiving the "business as usual" text message.

4.2. Hypotheses about the Heterogeneity of Impacts

We don't have access to rich information on covariates for these experiments, limiting our ability to perform more sophisticated heterogeneous analysis. We will explore the heterogeneity of impacts on the socioeconomic stratum, access to health insurance, nationality, ethnicity, and displacement by civil war. The hypothesis is the following:

Hypothesis H.1: The individual characteristics of parents and girls are likely to determine HPV vaccination take-up.

We consider this hypothesis for each of the characteristics mentioned above for any given experiment.

5. Research Design

This section describes the experimental design. As noted above, we implemented six experiments in total. Because all of them followed a similar structure, we describe each of them in the Appendix.

5.1. Outcomes

The main outcome is a binary measure of whether a parent's daughter is vaccinated against HPV during the SMS campaign window (8 weeks).

5.2. Treatment Assignment

All eligible girls in the target population were randomly assigned to an experimental condition using a block-randomized design (Imbens and Rubin 2015, and Gerber and Green 2012). One of the girls' parents (typically their mothers) was assigned to one of the treatment or control groups. This decision was driven by the information available at the HS's records, which typically collects information about mothers. Notice that participants were assigned to only one of the aforementioned behavioral experiments.

The research team randomly selected parents into treatment after stratifying on locality and vaccinee's age using the re-randomization algorithm by Morgan and Rubin (2012). This algorithm avoids the risk of pre-treatment imbalance for a given set of covariates by allowing treatment re-randomization without affecting the design's statistical properties. Because stratification was based on age, we constructed a set of indicator variables to avoid the "curse of dimensionality," typically associated with using a categorical variable. For the sample of unvaccinated girls, a dummy variable equal to 1 for girls aged 9 and 10 years old was used for the stratification. In the case of the sample of girls with incomplete vaccination schedules, that dummy variable was equal to 1 for girls aged 9 to 12 years old. On average, these dummy variables split the experimental sample in half.

Figures 3A to 3G present the experimental design, and Tables A1 to A6 summarize the relevant information for each experiment. This information includes the behavioral principle, the fixed elements that complement the message, the SMS content, and the sample size for each treatment arm. For instance, the social norm experiment for the unvaccinated daughters (Experiment 1) includes eight treatment conditions (Figure 3A and Table A1). This experiment considers five treatment groups (positive descriptive norm, negative descriptive norm, injunctive norm, trending norm, and dynamic norm) and three control groups (described below). Each experimental arm has about 4,600 observations, and the total sample size is 34,506 observations. According to our power calculations, this sample size is larger than the minimum required.

Each experiment includes three control groups besides the experimental groups assigned to receive behavioral treatments. The **Pure Control Group** does not receive any message based on behavioral insights. One critical concern in this type of experiment is disentangling the effect of the message from the effect of the mean used to deliver that message. Therefore, including a pure control group is helpful to evaluate the role of receiving a message regardless of its content. The **Experimental Control Group** receives placebo messages that include the fixed elements described above. This element is essential because it controls the role of these fixed elements from the main behavioral insight delivered by the message. Finally, the **Policy Control Group** receives the "business as usual" message that the HS uses in its current communication strategy with the target population. The messages delivered to the control groups are also described in Tables A1 to A6.

A planning tool intervention was cross-randomized with the behavioral experiments described above. In Experiment 1, 2, 5, and 6, we sent a message with a link with information about the closest vaccination points. In Experiment 3, we sent a message with a cellphone number where participants could request information about HPV and HPV vaccination. In Experiment 4, both strategies were implemented: half of the experimental sample received a link, and the other half the telephone number. Table A7 presents the design of these experiments.

Because these interventions were implemented within the regular HS communication policy, participants were not informed that they were part of these experiments. This is standard practice for government interventions and was approved by IRB.

A weekly message was sent to the treatment sample for eight weeks. The timeline and exact day of SMS delivery during the intervention are reported in Figure 1.

5.3. Pre-treatment Balance

Tables 2A to 2F show balance in the limited set of pre-treatment covariates for each experiment. These tables suggest that the randomization strategy successfully achieved statistical balance across treatment arms.

6. Estimation Strategy

This section discusses the methodological steps to evaluate the impact of the SMS campaign on HPV vaccination take-up.

6.1. Pre-treatment Balance

To test balance, we will use two strategies to capture differences between treatment and control groups at the baseline:

- t-test.
- Standardized differences.

The joint equality of treatment arms will be assessed using standard F-tests.

6.2. Estimation of Treatment Effects

The impact analysis will be based on a standard intention to treat analysis (ITT). We will estimate models of the following form:

$$Y_{ij} = \alpha + \sum_{k=1}^{K} \beta_k Z_j + \theta_s + X_{ij}^{'}\delta + \varepsilon_{ij}; \tag{1}$$

where Y_{ij} is the outcome of interest for vaccinee i from parent j measured two months after the end of the SMS campaign, Z_j is an indicator for a parent being assigned to one of the treatment conditions, θ_s is a vector of randomization strata dummy variables (locality*age), X_{ij} is a set of pre-treatment covariates at the vaccinee and parent level, and ε_{ij} is the error term. Cluster standard errors at the parent level will be used, given that randomization was implemented at this level conditional on locality by age strata. β_k captures the ITT effect for each treatment arm in a given experiment, which is the effect of being selected to receive a text message based on a behavioral insight.

6.3. Heterogeneous Treatment Effects

Heterogeneous treatment effects will be estimated interacting treatment status with the pre-treatment variable of interest W_j for parent j. We will estimate models of the following form:

$$Y_{ij} = \alpha + \sum_{k=1}^{K} \beta_k Z_j + \gamma W_j + \sum_{k=1}^{K} \varphi_k \left(Z_j . W_J \right) + \theta_s + X_{ij}^{'} \delta + \varepsilon_{ij};$$

(2)

where φ_k is the coefficient of interest for interaction between the pre-treatment variable W_j and the treatment arm k. The pre-treatment covariates of interest are the socioeconomic stratum, access to health insurance, nationality, ethnicity, and displacement by civil war.

Alternatively, if we can expand our set of covariates from administrative records and data are sufficiently rich in terms of variability, we will use the generic machine learning strategy proposed by Chenozhukov et al. (2020).

6.4. Addressing Multiple Inference

We have multiple treatment arms in each experiment and pre-treatment characteristics to explore heterogeneities. To consider multiple hypothesis testing, we will implement the Benjamini and Hochberg (1995) procedure to control the false discovery rate.

6.5. Non-compliance

ITT estimates will capture the effect of receiving a text message, but this does not imply that they were read by those assigned to each treatment arm. Although a tiny fraction of those assigned to treatment did not read the messages, we will adjust by non-compliance using the standard IV approach.

6.6. Attrition

Because we will work with administrative records, attrition is not relevant in our setting. Nevertheless, Lee's (2009) bounds will be implemented if needed.

6.7. Issues with Factorial Designs

Recent scholarship has raised concerns about how factorial designs are analyzed and implemented in practice (Muralidharan et al. 2022). Our design is subjected to these issues because of the cross-randomization of planning tools in all the experiments discussed above.

Following Muralidharan and coauthors, we will estimate the "long model" (potentially in the Appendix), but our main focus is on the "short model" for the main results in the paper. Our interest in our experiments is the main effects, but it is also of secondary interest to evaluate whether there are positive complementarities between behavioral insights and planning tools. We will clarify that the results are weighted averages of interactions with other treatments for those specifications based on the "short model."

The nature of our interventions justifies our focus on the "short model" for the analysis. As recognized by the technical literature in experimental designs, analyzing the "short model" in factorial designs is appropriate when evaluating the effectiveness of many potential treatments or many variations of the same treatment (Cochran and Cox 1957). In this scenario, researchers are interested in learning what works among many alternatives and further test the power of promising approaches to affect outcomes. Our experiments are based on the same principle because they will be the first phase of a learning process to define which behavioral insights may be more appropriate to boost vaccination rates in our setting.

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Figure 1. Experiment Timeline

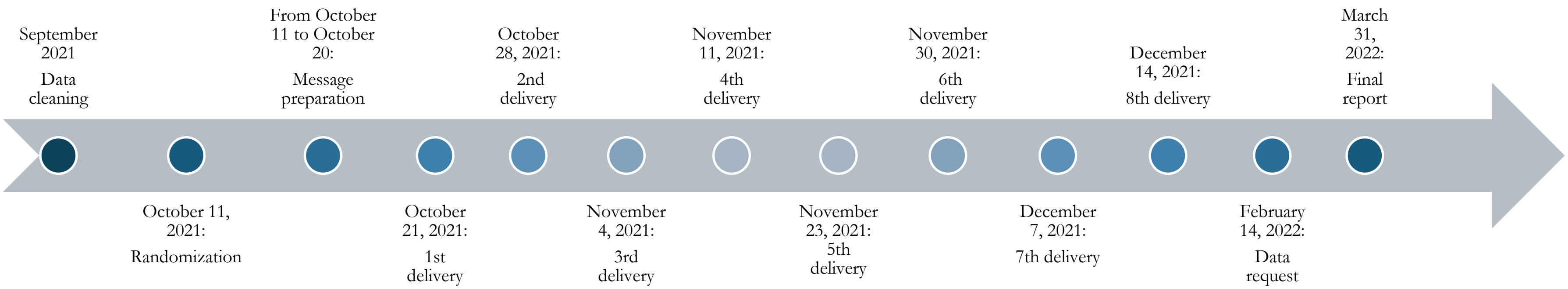


Figure 2A. Sample construction for experiments with unvaccinated girls

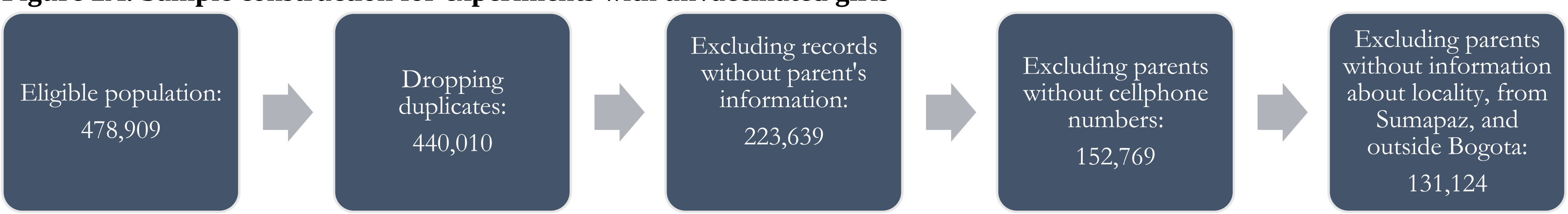


Figure 2B. Sample construction for experiments with incomplete vaccination schedules

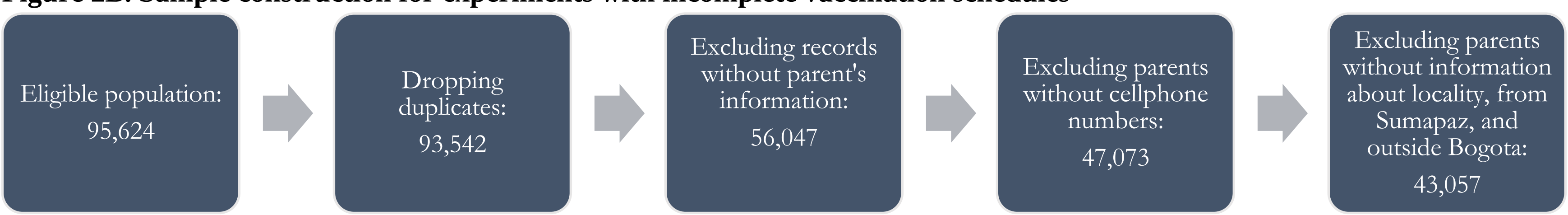
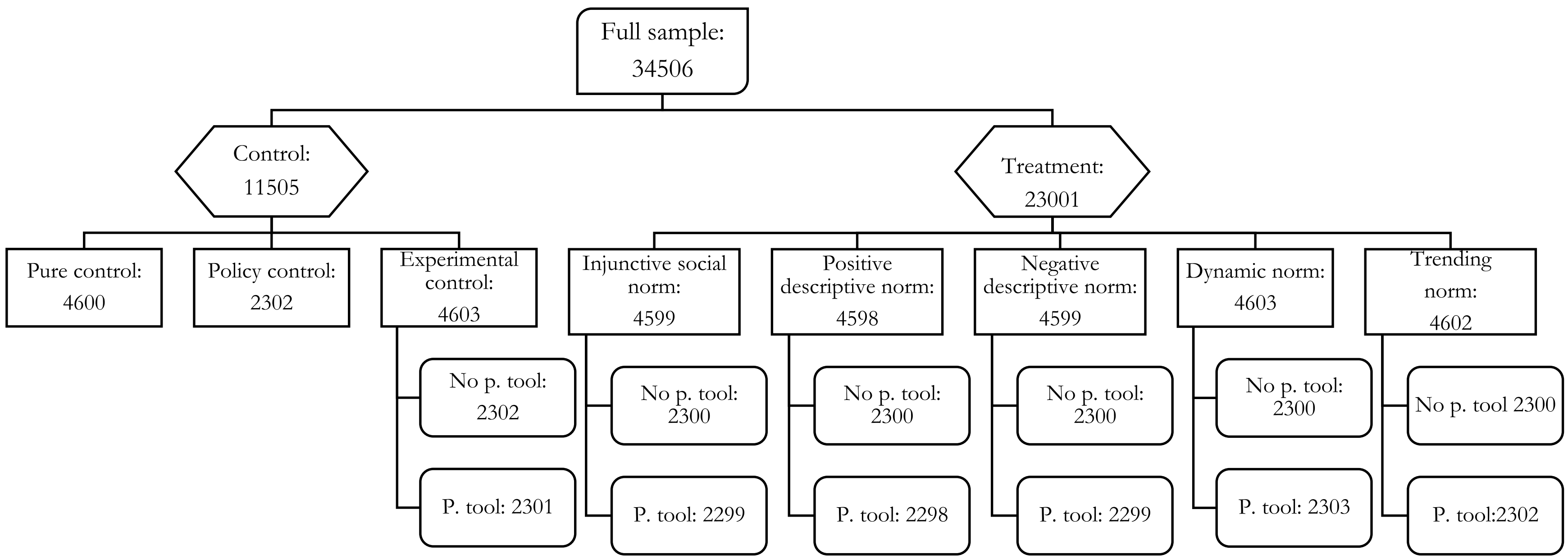
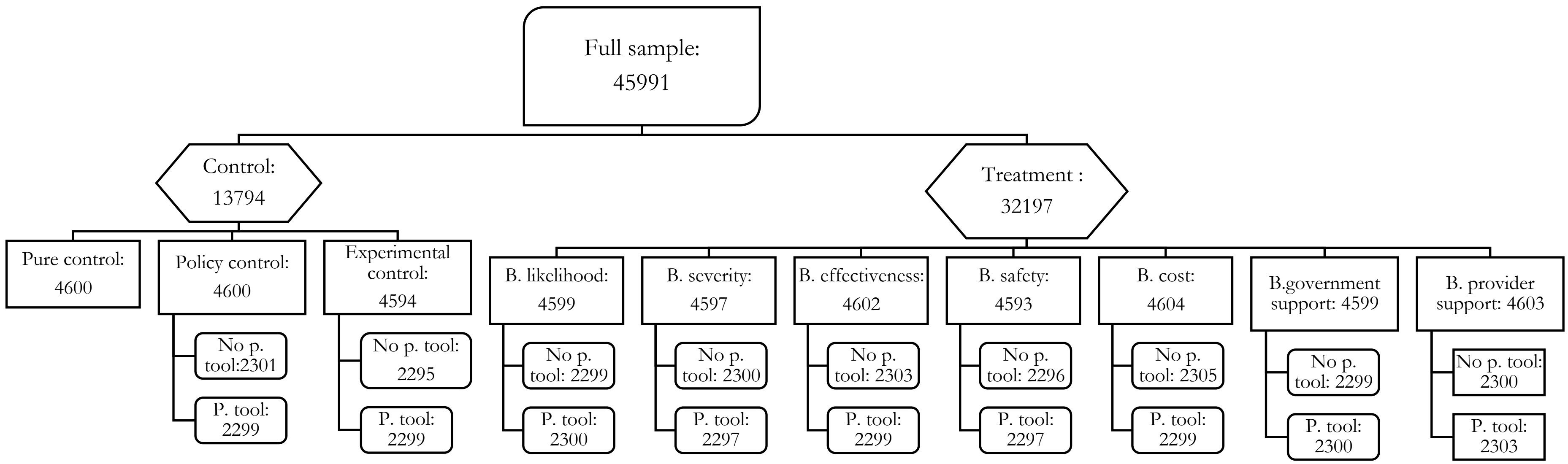


Figure 3A. Experimental Design for Experiment 1-Social Norms



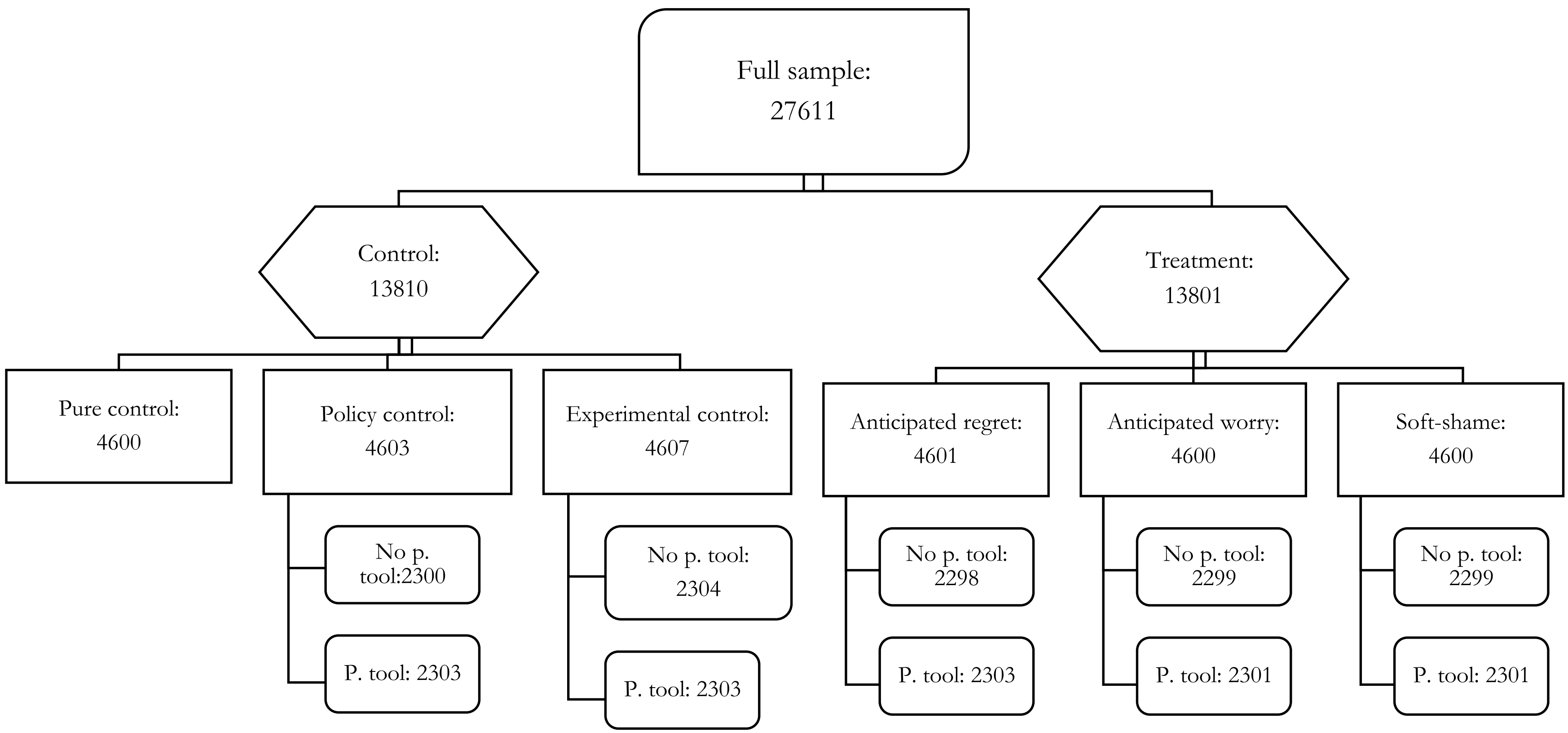
Source: Own elaboration.

Figure 3B. Experimental Design for Experiment 2-Beliefs



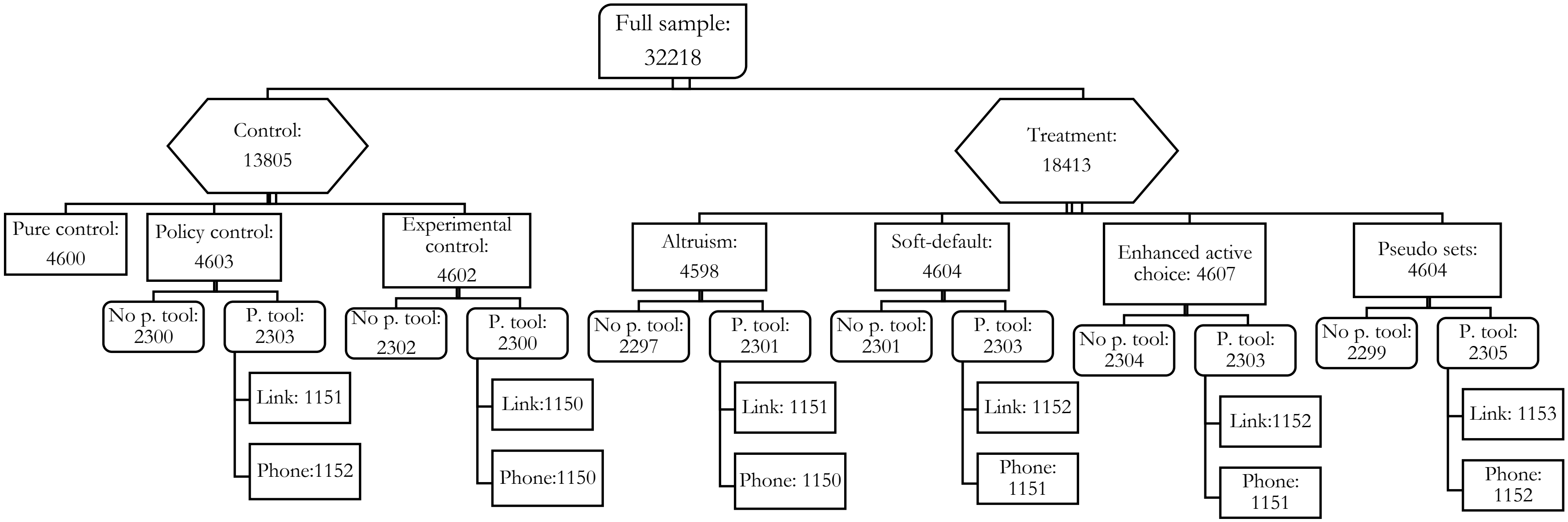
Source: Own elaboration.

Figure 3C. Experimental Design for Experiment 3- Emotions



Source: Own elaboration.

Figure 3D. Experimental Design for Experiment 4- Behavior



Source: Own elaboration.

Figure 3E. Experimental Design for Experiment 5-Decision Aids

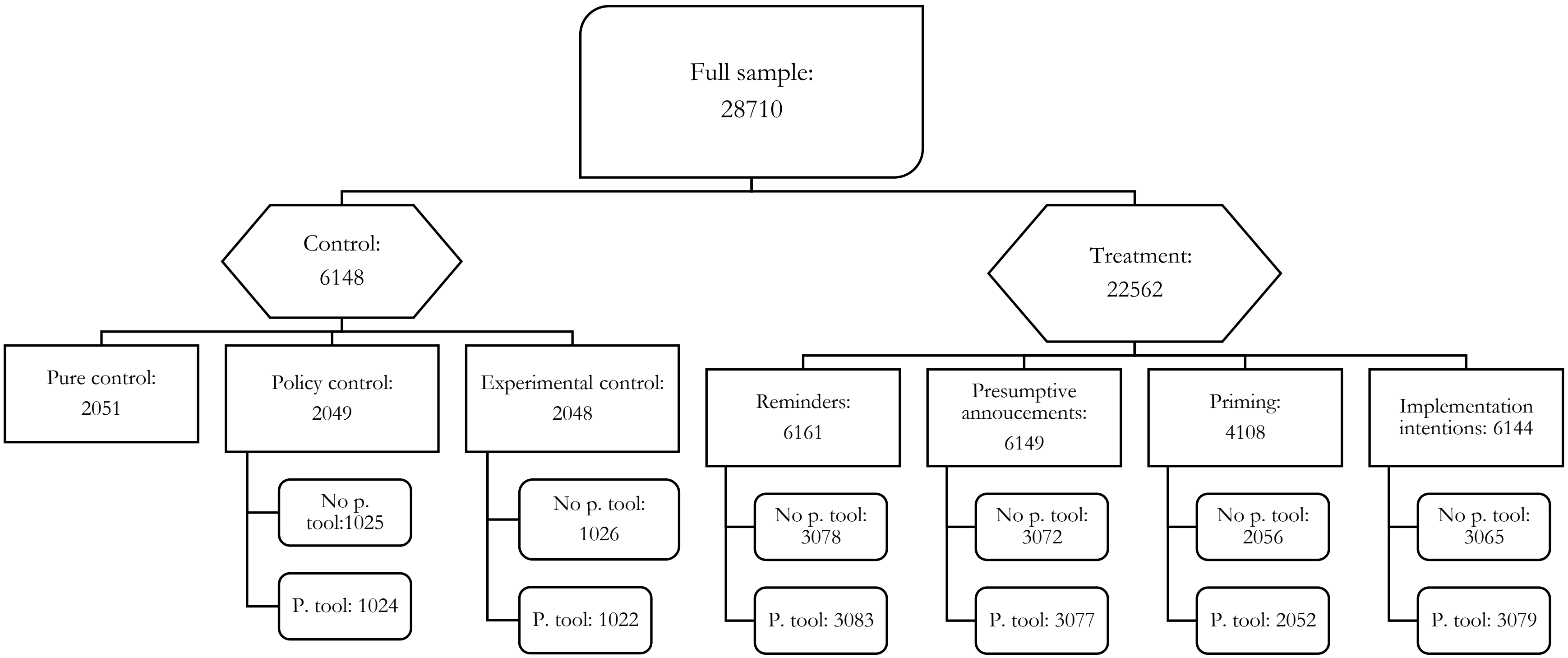
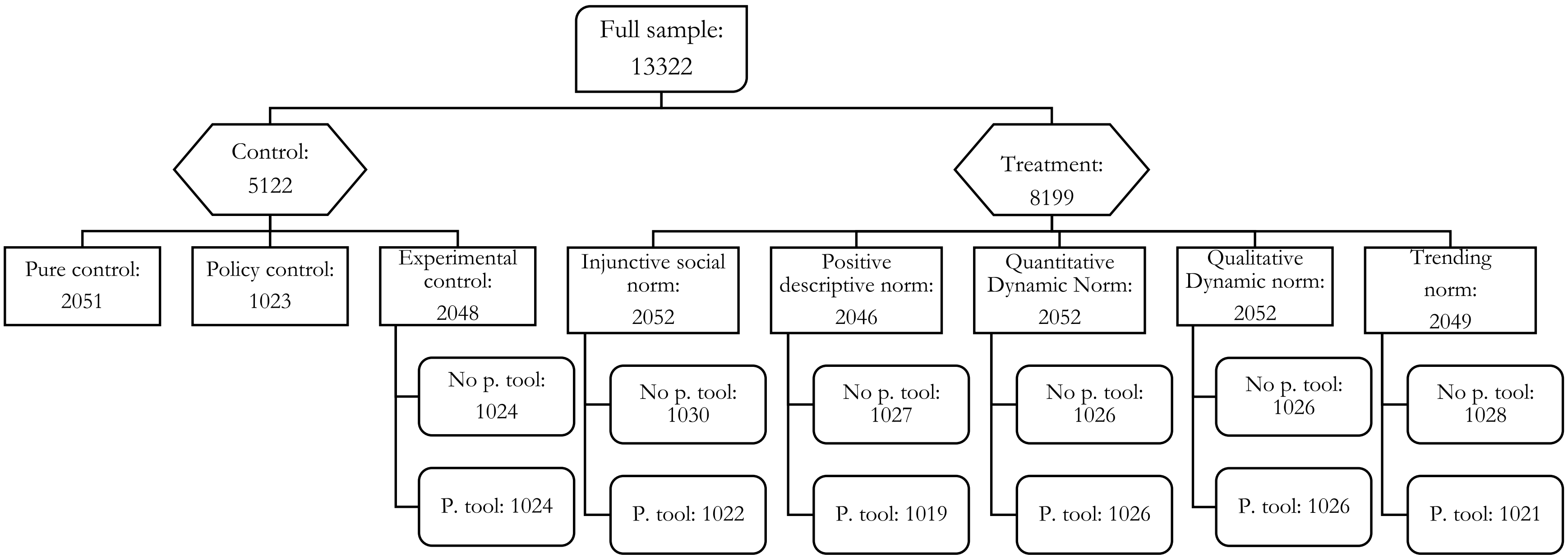
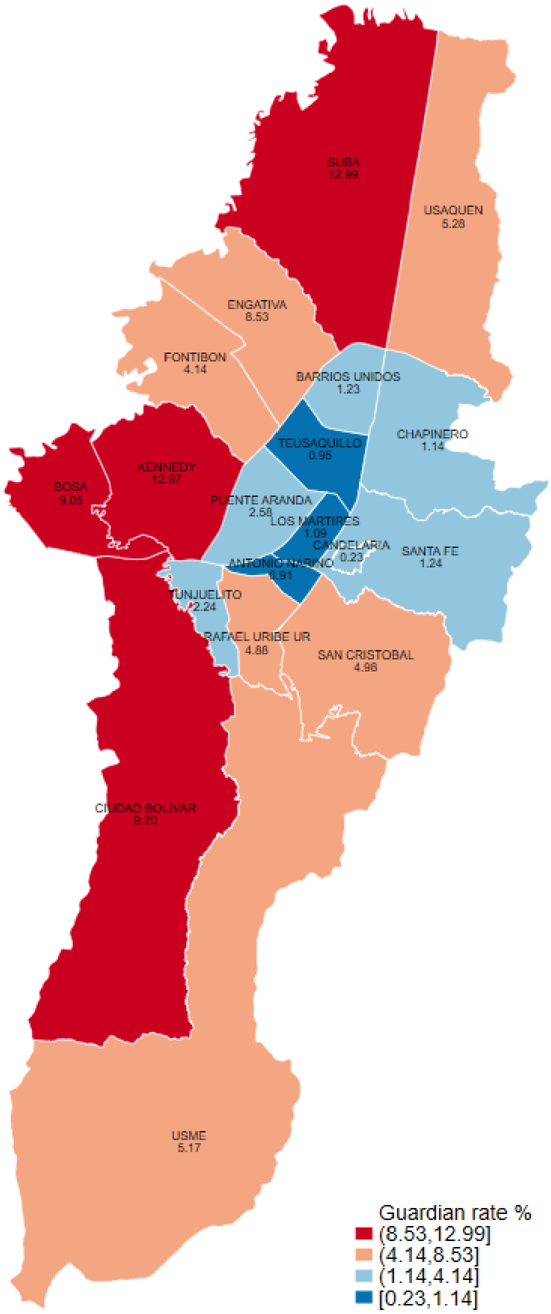


Figure 3F. Experimental Design for Experiment 6-Social Norms



Source: Own elaboration.

Figure 4. Distribution of Bogota's Target Population by Locality



Source: Own elaboration.

Table 1A. Descriptive Statistics for Sample of Unvaccinated Girls.

Variables	Mean	Std. Dev.	Min	Max	N
Daughter age	10.74	1.713	9	17	131,124
EPS Sanitas	0.136	0.342	0	1	131,124
EPS Salud Total	0.109	0.312	0	1	131,124
EPS Famisanar	0.191	0.393	0	1	131,124
EPS Compensar	0.149	0.356	0	1	131,124
EPS Capital Salud	0.111	0.314	0	1	131,124
Other EPS	0.305	0.460	0	1	131,124
Contributory Scheme	0.771	0.420	0	1	131,124
Uninsured	0.0374	0.190	0	1	131,124
Subsidized Scheme	0.146	0.354	0	1	131,124
Other scheme	0.0451	0.208	0	1	131,124
Ethnicity	0.00753	0.0864	0	1	131,124
Displaced	0.0169	0.129	0	1	131,124
Colombian nationality	0.989	0.103	0	1	131,124

Source: Own elaboration.

Table 1B. Descriptive Statistics for the Sample of Girls with Incomplete Vaccination Schedules.

Variables	Mean	Std. Dev.	Min	Max	N
Daughter age	12.53	2.108	9	17	43,057
EPS Sanitas	0.154	0.361	0	1	43,057
EPS Salud Total	0.125	0.330	0	1	43,057
EPS Famisanar	0.183	0.387	0	1	43,057
EPS Compensar	0.156	0.363	0	1	43,057
EPS Capital Salud	0.143	0.350	0	1	43,057
Other EPS	0.239	0.426	0	1	43,057
Contributory Scheme	0.761	0.427	0	1	43,057
Uninsured	0.0303	0.172	0	1	43,057
Subsidized Scheme	0.173	0.379	0	1	43,057
Other scheme	0.0354	0.185	0	1	43,057
Ethnicity	0.00557	0.0745	0	1	43,057
Displaced	0.0110	0.104	0	1	43,057
Colombian nationality	0.977	0.149	0	1	43,057

Source: Own elaboration.

Table 2A. Test for Randomization. Experiment 1-Social Norms.

Variables	Pure Control	Policy Control	Experimental Control	Injunctive Social Norm	Positive Descriptive Norm	Negative Descriptive Norm	Dynamic norm	Trending Norm	Joint H0 p-value
EPS Sanitas	0.131	0.128	0.133	0.129	0.137	0.130	0.133	0.131	0.964
	(0.005)	(0.007)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
EPS Salud Total	0.108	0.113	0.106	0.119	0.108	0.113	0.113	0.116	0.513
	(0.005)	(0.007)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
EPS Famisanar	0.193	0.192	0.191	0.199	0.191	0.190	0.197	0.189	0.940
	(0.006)	(0.008)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	
EPS Compensar	0.150	0.153	0.147	0.140	0.144	0.150	0.148	0.149	0.821
	(0.005)	(0.008)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
EPS Capital Salud	0.109	0.119	0.113	0.100	0.119	0.104	0.106	0.111	0.089
	(0.005)	(0.007)	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	
Other EPS	0.308	0.296	0.309	0.312	0.302	0.312	0.303	0.302	0.811
	(0.007)	(0.010)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	
Contributory Scheme	0.770	0.765	0.764	0.781	0.774	0.769	0.780	0.771	0.500
	(0.006)	(0.009)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	
Subsidized Scheme	0.143	0.154	0.151	0.132	0.151	0.144	0.140	0.146	0.121
	(0.005)	(0.008)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
Uninsured	0.038	0.038	0.039	0.045	0.034	0.039	0.035	0.037	0.205
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	
Other scheme	0.049	0.043	0.046	0.042	0.041	0.048	0.045	0.046	0.645
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	
Colombian nationality	0.990	0.986	0.990	0.990	0.990	0.988	0.990	0.991	0.510
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	
Ethnicity	0.009	0.005	0.008	0.006	0.007	0.008	0.007	0.009	0.613
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	
Displaced	0.016	0.013	0.016	0.018	0.015	0.016	0.018	0.017	0.814
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	
Daughter age	10.766	10.739	10.725	10.753	10.753	10.737	10.731	10.738	0.960
	(0.026)	(0.036)	(0.025)	(0.025)	(0.026)	(0.025)	(0.025)	(0.025)	0.964
Observations	4600	2302	4603	4599	4598	4599	4603	4602	

Source: Own elaboration.

Table 2B. Test for Randomization. Experiment 2-Beliefs.

Variables	Pure Control	Policy Control	Experimental Control	Beliefs about likelihood	Beliefs about severity	Beliefs about effectiveness	Beliefs about safety	Beliefs about cost	Beliefs about government support	Beliefs about provider support	Joint H0 p-value
EPS Sanitas	0.131	0.130	0.141	0.139	0.134	0.144	0.133	0.143	0.139	0.134	0.428
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
EPS Salud Total	0.108	0.111	0.103	0.106	0.114	0.102	0.104	0.117	0.106	0.110	0.296
	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	
EPS Famisanar	0.193	0.187	0.189	0.199	0.188	0.184	0.192	0.187	0.183	0.186	0.741
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	
EPS Compensar	0.150	0.152	0.154	0.153	0.154	0.149	0.152	0.144	0.159	0.156	0.784
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
EPS Capital Salud	0.109	0.111	0.111	0.107	0.108	0.104	0.116	0.113	0.102	0.113	0.578
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.005)	
Other EPS	0.308	0.308	0.302	0.297	0.301	0.316	0.304	0.297	0.310	0.302	0.589
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	
Contributory Scheme	0.770	0.773	0.773	0.774	0.772	0.769	0.777	0.768	0.784	0.767	0.755
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	
Subsidized Scheme	0.143	0.147	0.145	0.144	0.146	0.143	0.148	0.147	0.137	0.149	0.926
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
Uninsured	0.038	0.037	0.038	0.037	0.038	0.035	0.033	0.033	0.037	0.039	0.788
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	
Other scheme	0.049	0.043	0.044	0.045	0.044	0.053	0.042	0.052	0.042	0.045	0.069
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	
Colombian nationality	0.990	0.988	0.988	0.988	0.990	0.989	0.992	0.989	0.990	0.991	0.613

	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	
Ethnicity	0.009	0.009	0.008	0.010	0.009	0.006	0.008	0.006	0.008	0.005	0.096
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	
Displaced	0.016	0.017	0.022	0.016	0.015	0.019	0.016	0.019	0.016	0.019	0.323
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	
Daughter age	10.766	10.728	10.746	10.717	10.707	10.729	10.739	10.731	10.735	10.742	0.928
	(0.026)	(0.025)	(0.026)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.026)	
Observations	4600	4600	4594	4599	4597	4602	4593	4604	4599	4603	

Source: Own elaboration.

Table 2C. Test for Randomization. Experiment 3-Emotions.

Variables	Pure Control	Policy Control	Experimental Control	Anticipated Regret	Anticipated Worry	Soft-shame	Joint H0 p-value
EPS Sanitas	0.131	0.138	0.133	0.135	0.136	0.129	0.806
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
EPS Salud Total	0.108	0.110	0.114	0.106	0.105	0.116	0.536
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
EPS Famisanar	0.193	0.188	0.200	0.188	0.192	0.188	0.605
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	
EPS Compensar	0.150	0.150	0.148	0.151	0.142	0.149	0.882
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
EPS Capital Salud	0.109	0.114	0.103	0.106	0.111	0.109	0.628
	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)	
Other EPS	0.308	0.300	0.302	0.314	0.313	0.310	0.598
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	
Contributory Scheme	0.770	0.761	0.776	0.773	0.764	0.772	0.569
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	
Subsidized Scheme	0.143	0.154	0.140	0.143	0.150	0.141	0.306
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
Uninsured	0.038	0.038	0.040	0.035	0.039	0.042	0.500
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	
Other scheme	0.049	0.046	0.043	0.050	0.047	0.045	0.760
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	
Colombian nationality	0.990	0.986	0.988	0.990	0.990	0.988	0.326
	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	
Ethnicity	0.009	0.008	0.008	0.006	0.006	0.006	0.434
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	
Displaced	0.016	0.014	0.014	0.017	0.017	0.016	0.860
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	0.806
Daughter age	10.766	10.739	10.708	10.739	10.764	10.737	
	(0.026)	(0.025)	(0.025)	(0.025)	(0.026)	(0.025)	
Observations	4600	4603	4607	4601	4600	4600	

Source: Own elaboration.

Table 2D. Test for Randomization. Experiment 4-Decision Aids.

Variables	Pure control	Policy control	Experimental control	Altruism	Soft-default	Enhanced active choice	Pseudo sets	Joint H0 p-value
EPS Sanitas	0.131	0.138	0.147	0.139	0.144	0.137	0.129	0.157
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
EPS Salud Total	0.108	0.105	0.104	0.111	0.109	0.100	0.108	0.715
	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)	
EPS Famisanar	0.193	0.184	0.195	0.189	0.197	0.194	0.186	0.614
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	
EPS Compensar	0.150	0.143	0.151	0.151	0.142	0.152	0.146	0.708
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
EPS Capital Salud	0.109	0.120	0.108	0.114	0.108	0.113	0.118	0.360
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
Other EPS	0.308	0.310	0.295	0.296	0.300	0.304	0.313	0.370
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	
Contributory Scheme	0.770	0.762	0.784	0.774	0.772	0.769	0.751	0.010
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	
Subsidized Scheme	0.143	0.156	0.139	0.144	0.149	0.153	0.161	0.028
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
Uninsured	0.038	0.038	0.038	0.039	0.034	0.037	0.040	0.794
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	
Other scheme	0.049	0.044	0.039	0.043	0.045	0.041	0.047	0.274
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	
Colombian nationality	0.990	0.988	0.992	0.988	0.987	0.992	0.990	0.167
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	
Ethnicity	0.009	0.009	0.007	0.007	0.006	0.010	0.010	0.368
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	
Displaced	0.016	0.019	0.018	0.016	0.015	0.016	0.023	0.065
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	
Daughter age	10.766	10.749	10.759	10.738	10.715	10.758	10.740	0.844
	(0.026)	(0.025)	(0.026)	(0.025)	(0.025)	(0.026)	(0.025)	
Observations	4600	4603	4602	4598	4604	4607	4604	

Source: Own elaboration.

Table 2E. Test for Randomization. Experiment 5-Social Norms.

Variables	Pure control	Policy control	Experimental control	Injunctive social norm	Positive Descriptive Norm	Quantitative Dynamic Norm	Qualitative Dynamic Norm	Trending Norm	Joint H0 p-value
EPS Sanitas	0.155	0.153	0.156	0.154	0.143	0.154	0.145	0.149	0.926
	(0.008)	(0.011)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	
EPS Salud Total	0.114	0.115	0.137	0.118	0.125	0.123	0.123	0.111	0.250
	(0.007)	(0.010)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	
EPS Famisanar	0.190	0.179	0.170	0.205	0.179	0.185	0.180	0.179	0.192
	(0.009)	(0.012)	(0.008)	(0.009)	(0.008)	(0.009)	(0.008)	(0.008)	
EPS Compensar	0.154	0.146	0.141	0.154	0.154	0.158	0.148	0.169	0.382
	(0.008)	(0.011)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	
EPS Capital Salud	0.144	0.147	0.140	0.133	0.154	0.140	0.153	0.153	0.474
	(0.008)	(0.011)	(0.008)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	
Other EPS	0.243	0.260	0.255	0.237	0.244	0.240	0.251	0.239	0.764
	(0.009)	(0.014)	(0.010)	(0.009)	(0.010)	(0.009)	(0.010)	(0.009)	
Contributory Scheme	0.765	0.751	0.751	0.772	0.751	0.772	0.751	0.753	0.424
	(0.009)	(0.014)	(0.010)	(0.009)	(0.010)	(0.009)	(0.010)	(0.010)	
Subsidized Scheme	0.171	0.173	0.175	0.158	0.187	0.172	0.190	0.183	0.170
	(0.008)	(0.012)	(0.008)	(0.008)	(0.009)	(0.008)	(0.009)	(0.009)	
Uninsured	0.029	0.034	0.037	0.033	0.026	0.029	0.030	0.027	0.537
	(0.004)	(0.006)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	
Other scheme	0.035	0.042	0.037	0.037	0.036	0.027	0.029	0.037	0.347
	(0.004)	(0.006)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	
Colombian nationality	0.979	0.972	0.972	0.974	0.982	0.979	0.975	0.978	0.410
	(0.003)	(0.005)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	
Ethnicity	0.010	0.004	0.006	0.003	0.008	0.004	0.005	0.003	0.060
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	
Displaced	0.009	0.015	0.011	0.010	0.010	0.011	0.011	0.012	0.925
	(0.002)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	
Daughter age	12.584	12.540	12.519	12.498	12.581	12.569	12.519	12.553	0.865
	(0.047)	(0.067)	(0.047)	(0.046)	(0.047)	(0.047)	(0.047)	(0.047)	
Observations	2051	1023	2048	2052	2046	2052	2052	2049	

Source: Own elaboration.

Table 2F. Test for Randomization. Experiment 6-Decision Aids.

Variables	Pure control	Policy control	Experimental control	Reminder Simple	Reminder Simple + Eliciting implementation intentions	Reminder Simple + Anchoring (date)	Pres. annoucement Standard	Pres. annoucement Standard + Eliciting implementation intentions	Pres. annoucement Standard + Anchoring (date)	Priming Question-behavior Effect	Priming Mere-measurement Effect	Implementation intentions	Implementation intentions + Anchoring (this week as T9)	Implementation intentions+ Urgency (vaccinate this week)	Joint H0 p-val.
EPS Sanitas	0.155	0.142	0.157	0.154	0.164	0.160	0.169	0.146	0.155	0.156	0.150	0.153	0.167	0.155	0.585
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	
EPS Salud Total	0.114	0.131	0.137	0.132	0.136	0.132	0.121	0.122	0.131	0.118	0.114	0.120	0.133	0.124	0.285
	(0.007)	(0.007)	(0.008)	(0.007)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	
EPS Famisanar	0.190	0.181	0.173	0.184	0.174	0.177	0.189	0.196	0.169	0.189	0.189	0.187	0.178	0.186	0.610
	(0.009)	(0.009)	(0.008)	(0.009)	(0.008)	(0.008)	(0.009)	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)	(0.008)	(0.009)	
EPS Compensar	0.154	0.164	0.156	0.167	0.156	0.157	0.161	0.166	0.152	0.159	0.157	0.154	0.144	0.156	0.905
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	
EPS Capital Salud	0.144	0.149	0.134	0.142	0.137	0.140	0.125	0.141	0.141	0.145	0.139	0.154	0.150	0.148	0.536
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	
Other EPS	0.243	0.233	0.243	0.222	0.233	0.234	0.235	0.228	0.252	0.234	0.250	0.232	0.227	0.231	0.622
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.009)	(0.010)	(0.009)	(0.009)	(0.009)	
Contributory Scheme	0.765	0.748	0.758	0.773	0.772	0.761	0.780	0.769	0.761	0.759	0.753	0.755	0.756	0.764	0.584
	(0.009)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.009)	(0.009)	
Subsidized Scheme	0.171	0.178	0.164	0.166	0.162	0.174	0.163	0.170	0.172	0.175	0.175	0.178	0.179	0.174	0.947
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	
Uninsured	0.029	0.036	0.034	0.030	0.033	0.028	0.027	0.027	0.033	0.034	0.033	0.026	0.028	0.028	0.748
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	
Other scheme	0.035	0.039	0.044	0.032	0.033	0.038	0.029	0.034	0.034	0.032	0.039	0.041	0.038	0.035	0.428
	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	
Colombian nationality	0.979	0.977	0.974	0.977	0.974	0.980	0.981	0.978	0.975	0.973	0.979	0.981	0.979	0.978	0.784
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	

Ethnicity	0.010	0.006	0.006	0.004	0.004	0.009	0.005	0.003	0.007	0.004	0.005	0.006	0.005	0.004	0.151
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	
Displaced	0.009	0.013	0.013	0.005	0.009	0.012	0.010	0.008	0.010	0.009	0.018	0.013	0.015	0.011	0.032
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	
Daughter age	12.584	12.508	12.515	12.491	12.546	12.528	12.514	12.549	12.562	12.558	12.508	12.531	12.521	12.546	0.992
	(0.047)	(0.046)	(0.046)	(0.046)	(0.046)	(0.047)	(0.046)	(0.046)	(0.046)	(0.046)	(0.047)	(0.046)	(0.046)	(0.048)	
Observations	2051	2049	2048	2054	2056	2051	2049	2052	2048	2053	2055	2047	2051	2046	

Source: Own elaboration.

Table 2G. Test for Randomization. Planning Tools for Experiments 1, 2, 3, and 4.

Variables	Pure control	Planning tool-link	Planning tool-phone	Joint H0 p-value
EPS Sanitas	0.131	0.147	0.126	0.089
	(0.005)	(0.007)	(0.007)	
EPS Salud Total	0.108	0.115	0.107	0.612
	(0.005)	(0.007)	(0.006)	
EPS Famisanar	0.193	0.199	0.188	0.629
	(0.006)	(0.008)	(0.008)	
EPS Compensar	0.150	0.142	0.148	0.694
	(0.005)	(0.007)	(0.007)	
EPS Capital Salud	0.109	0.122	0.123	0.155
	(0.005)	(0.007)	(0.007)	
Other EPS	0.308	0.275	0.308	0.010
	(0.007)	(0.009)	(0.010)	
Contributory Scheme	0.770	0.778	0.757	0.262
	(0.006)	(0.009)	(0.009)	
Subsidized Scheme	0.143	0.147	0.154	0.489
	(0.005)	(0.007)	(0.008)	
Uninsured	0.038	0.029	0.040	0.076
	(0.003)	(0.003)	(0.004)	
Other scheme	0.049	0.047	0.049	0.920
	(0.003)	(0.004)	(0.004)	
Colombian nationality	0.990	0.988	0.988	0.835
	(0.001)	(0.002)	(0.002)	
Ethnicity	0.009	0.006	0.013	0.062
	(0.001)	(0.002)	(0.002)	
Displaced	0.016	0.018	0.013	0.427
	(0.002)	(0.003)	(0.002)	
Daughter age	10.766	10.683	10.743	0.165
	(0.026)	(0.034)	(0.036)	
Observations	4600	2297	2301	

Source: Own elaboration.

Table 2H. Test for Randomization. Planning Tools for Experiments 5 and 6.

Variables	Pure control	Planning tool-link	Joint H0 p-value
EPS Sanitas	0.155	0.140	0.255
	(0.008)	(0.011)	
EPS Salud Total	0.114	0.112	0.908
	(0.007)	(0.010)	
EPS Famisanar	0.190	0.191	0.943
	(0.009)	(0.012)	
EPS Compensar	0.154	0.161	0.595
	(0.008)	(0.011)	
EPS Capital Salud	0.144	0.151	0.610
	(0.008)	(0.011)	
Other EPS	0.243	0.245	0.923
	(0.009)	(0.013)	
Contributory Scheme	0.765	0.758	0.670
	(0.009)	(0.013)	
Subsidized Scheme	0.171	0.178	0.633
	(0.008)	(0.012)	
Uninsured	0.029	0.026	0.646
	(0.004)	(0.005)	
Other scheme	0.035	0.038	0.680
	(0.004)	(0.006)	
Colombian nationality	0.979	0.983	0.408
	(0.003)	(0.004)	
Ethnicity	0.010	0.007	0.413
	(0.002)	(0.003)	
Displaced	0.009	0.012	0.523
	(0.002)	(0.003)	
Daughter age	12.584	12.525	0.465
	(0.047)	(0.065)	
Observations	2051	1025	

Source: Own elaboration.

Table A1. SMS Content for Experiment 1. Social Norms.

Number		*Behavioral Principle*	*Fixed Element 1*	*Fixed Element 2*	*SMS Content*	*SMS content in English*	*Sample size*
1	Control	*Pure control*	*None*	*None*	*No message*	*No message*	*4,600*
2		*Policy control*	*None*	*Sender*	*Vacúnalo: bríndale a tu hijo o hija toda la protección. Consulta en https://bit.ly/ssaludbog el punto más cercano. Secretaría de Salud*	*Vaccinate him/her: give your son or daughter full protection. Consult https://bit.ly/ssaludbog for the nearest point. Secretary of Health*	*2,302*
3		*Experimental control*	*Personalized*	*Sender*	*Hola XXXXXXXXXX. Vacúnala contra el VPH: bríndale toda la protección. Secretaría de Salud*	*Hello XXXXXXXXXXXXXX. Vaccinate her against HPV: give her all the protection. Secretary of Health*	*4,603*
4	Static design	*Injunctive social norm*	*Personalized*	*Sender*	*Hola XXXXXXXXXX. X de cada 10 padres en tu localidad perdieron la opción de vacunar a sus hijas contra el VPH y cuidarlas del cáncer :(. Secretaría de Salud*	*Hello XXXXXXXXXX. X out of 10 parents in your locality lost the option of vaccinating their daughters against HPV and taking care of them from cancer :(. Secretary of Health*	*4,599*
5		*Positive descriptive norm*	*Personalized*	*Sender*	*Hola XXXXXXXXXX. X de cada 10 padres en tu localidad vacunaron a sus hijas contra el VPH y las protegieron contra el cáncer. Secretaría de Salud*	*Hello XXXXXXXXXXXXXX. X out of 10 parents in your locality vaccinated their daughters against HPV and protected them against cancer. Secretary of Health*	*4,598*
6		*Negative descriptive norm*	*Personalized*	*Sender*	*Hola XXXXXXXXXX. X de cada 10 padres en tu localidad perdieron la opción de vacunar a sus hijas contra el VPH y cuidarlas del cáncer. Secretaría de Salud*	*Hello XXXXXXXXXXXXXX. X out of 10 parents in your locality lost the option to vaccinate their daughters against HPV and take care of them against cancer. Secretary of Health*	*4,599*
7	Dynamic design	*Dynamic norm*	*Personalized*	*Sender*	*Hola XXXXXXXXXX. Desde 2016, 4 de cada 10 padres en Bogotá han empezado a vacunar a sus hijas contra el VPH cuidándolas del cáncer. Secretaría de Salud*	*Hello XXXXXXXXXXXXXX. Since 2016, 4 out of 10 parents in Bogota have started vaccinating their daughters against HPV guarding them against cancer. Secretary of Health*	*4,603*
8		*Trending norm*	*Personalized*	*Sender*	*Hola XXXXXXXXXX. 4 de cada 10 padres en Bogotá han vacunado a sus hijas contra el*	*Hello XXXXXXXXXX. 4 out of 10 parents in Bogotá have vaccinated their daughters against HPV caring for them from cancer,*	*4,602*

					VPH cuidándolas del cáncer, un alza del 128% desde 2016. Secretaría de Salud	*an increase of 128% since 2016. Secretary of Health*	

Overall Sample size: 34,506

Table A2. SMS Content for Experiment 2. Beliefs.

Number		Behavioral Principle	Fixed Element 1	Fixed Element 2	Fixed Element 3	SMS Content	SMS content in English	Sample size
1	Control	Pure control	None	None	None	No message	No message	4,600
2		Policy control	None	None	Sender	Vacúnalo: bríndale a tu hijo o hija toda la protección. Secretaría de Salud	Vaccinate him/her: give your son or daughter full protection. Secretary of Health	4,600
3		Experimental control	Personalized	Milkman's insight	Sender	Hola XXXXXXXXXX Hay una vacuna contra el VPH que espera a tu hija. Secretaría de Salud	Hello XXXXXXXXXX There is an HPV vaccine waiting for your daughter. Secretary of Health	4,594
4	Beliefs about VPH	Beliefs about likelihood	Personalized	Milkman's insight	Sender	Hola XXXXXXXXXX ¿Sabías que la probabilidad de contagio de VPH para tu hija es del 80%? Hay una vacuna contra el VPH que espera a tu hija. Secretaría de Salud	Hello XXXXXXXXXX Did you know that the probability of HPV infection for your daughter is 80%? There is an HPV vaccine waiting for your daughter. Secretary of Health	4,599
5		Beliefs about severity	Personalized	Milkman's insight	Sender	Hola XXXXXXXXXX ¿Sabías que en el 2018, 974 mujeres con VPH desarrollan cáncer en Bogotá? Hay una vacuna contra el VPH que espera a tu hija. Secretaría de Salud	Hello XXXXXXXXXXXXXX Did you know that in 2018, 974 women with HPV develop cancer in Bogota? There's an HPV vaccine waiting for your daughter. Secretary of Health	4,597
6	Beliefs about VPH vaccine	Beliefs about effectiveness	Personalized	Milkman's insight	Sender	Hola XXXXXXXXXX ¿Sabías que la vacuna del VPH reduce 89% el riesgo de cáncer cervical? Hay una vacuna contra el VPH que espera a tu hija. Secretaría de Salud	Hello XXXXXXXXXX Did you know that the HPV vaccine reduces the risk of cervical cancer by 89%? There is an HPV vaccine waiting for your daughter. Secretary of Health	4,602
7		Beliefs about safety	Personalized	Milkman's insight	Sender	Hola XXXXXXXXXX. ¿Sabes que la vacuna contra el VPH es la más segura del plan de vacunación?	Hello XXXXXXXXXX. Do you know that the HPV vaccine is the safest in the vaccination plan? There is a vaccine waiting	4,593

						Hay una vacuna que espera a tu hija. Secretaría de Salud	*for your daughter. Secretary of Health*	
8		*Beliefs about cost*	*Personalized*	*Milkman's insight*	*Sender*	*Hola XXXXXXXXXX ¿Sabías que la vacuna contra el VPH es gratuita? Hay una vacuna contra el VPH que espera a tu hija. Secretaría de Salud*	*Hello XXXXXXXXXX Did you know that the HPV vaccine is free? There is an HPV vaccine waiting for your daughter. Secretary of Health*	*4,604*
9	*Beliefs about government support*	*Beliefs about government support*	*Personalized*	*Milkman's insight*	*Sender*	*Hola XXXXXXXXXX La Secretaría de Salud recomienda que vacunes a tu hija contra el VPH. Hay una vacuna contra el VPH que espera a tu hija. Secretaría de Salud*	*Hello XXXXXXXXXX The Secretary of Health recommends that you vaccinate your daughter against HPV. There is an HPV vaccine waiting for your daughter. Secretary of Health*	*4,599*
10	*Beliefs about provider support*	*Beliefs about provider support*	*Personalized*	*Milkman's insight*	*Sender*	*Hola XXXXXXXXXX. Los médicos especialistas recomiendan la vacunación de tu hija contra el VPH. Hay una vacuna esperando por tu hija. Secretaría de Salud*	*Hello XXXXXXXXXX. Specialist doctors recommend vaccinating your daughter against HPV. There is a vaccine waiting for your daughter. Secretary of Health*	*4,603*

Overall Sample size: 45,991

Table A3. SMS Content for Experiment 3. Emotions.

Number	*Behavioral Principle*	*Fixed Element 1*	*Fixed Element 2*	*Fixed Element 3*	*Fixed Element 4*	*SMS Content*	*SMS content in English*	*Sample size*
1	*Pure control*	*None*	*None*	*None*	*None*	*No message*	*No message*	*4,600*
2	*Policy control*	*None*	*Closest vaccination point*	*Call for action*	*Sender*	*Vacúnalo: bríndale a tu hijo o hija toda la protección. Vacuna a tu hija y protégela del cáncer cervical. Secret. de Salud*	*Vaccinate him/her: give your son or daughter full protection. Vaccinate your daughter and protect her from cervical cancer. Secretary of Health*	*4,603*
3	*Experimental control*	*Personalized*	*Closest vaccination point*	*Call for action*	*Sender*	*Hola XXXXXXXXXX. Vacuna a tu hija contra el VPH. Consulta aquí el punto más cercano https://bit.ly/ssaludbog . Secret. de Salud*	*Hello XXXXXXXXXXX. Vaccinate your daughter against HPV. Check here for the nearest point https://bit.ly/ssaludbog . Secret. of health*	*4,607*
4	*Anticipated regret*	*Personalized*	*Closest vaccination point*	*Call for action*	*Sender*	*Hola XXXXXXXXXX. Vacuna tu hija contra el VPH y no lamentaras un cáncer después. Consulta aquí el punto más cercano https://bit.ly/ssaludbog . Secret. de Salud*	*Hello XXXXXXXXXX. Vaccine your daughter against HPV and you will not regret a cancer later. Check here the nearest point https://bit.ly/ssaludbog. Secret. of health*	*4,601*
5	*Anticipated worry*	*Personalized*	*Closest vaccination point*	*Call for action*	*Sender*	*Hola XXXXXXXXXX. Vacuna tu hija contra el VPH y no te preocupes por cancer despues. Consulta aquí el punto más cercano https://bit.ly/ssaludbog Secret. de Salud*	*Hello XXXXXXXXXX. Get your daughter vaccinated against HPV, and don't worry about cancer later. Check here the closest point https://bit.ly/ssaludbog. Secret. of health*	*4,600*

6	*Soft-shame*	*Personalized*	*Closest vaccination point*	*Call for action*	*Sender*	*Hola XXXXXXXXXX. Tu hija no tiene la vacuna contra el VPH :(Vacuna a tu hija. Consulta aquí el punto más cercano https://bit.ly/ssaludbog . Secret. de Salud*	*Hello XXXXXXXXXX. Your daughter is not vaccinated against HPV :(Vaccinate your daughter. Consult the nearest point at https://bit.ly/ssaludbog. Secret. of health*	*4,600*

Overall Sample size: 27,611

Table A4. SMS Content for Experiment 4. Decision Aids.

Number	*Behavioral Principle*	*Fixed Element 1*	*Fixed Element 2*	*Fixed Element 3*	*SMS Content*	*SMS content in English*	*Sample size*
1	*Pure control*	*None*	*None*	*None*	*No message*	*No message*	*4,600*
2	*Policy control*	*None*	*None*	*Sender*	*Vacúnalo: bríndale a tu hijo o hija toda la protección. Secretaría de Salud*	*Vaccinate him/her: give your son or daughter full protection. Secretary of Health*	*4,603*
3	*Experimental control*	*Personalized*	*Belief about cost*	*Sender*	*Hola XXXXXXXXXX. La vacuna contra el VPH es gratuita. Secretaría de Salud*	*Hello XXXXXXXXXXXXXX. The HPV vaccine is free. Secretary of Health*	*4,602*
4	*Altruism*	*Personalized*	*Belief about cost*	*Sender*	*Hola XXXXXXXXXX. Vacuna a tu hija contra el VPH y protégela del cáncer cervical. La vacuna es gratuita. Secretaría de Salud*	*Hello XXXXXXXXXXXXXX. Vaccinate your daughter against HPV and protect her from cervical cancer. The vaccine is free. Secretary of Health*	*4,598*
5	*Soft-default*	*Personalized*	*Belief about cost*	*Sender*	*Hola XXXXXXXXXX. Tienes una cita el xx de *MES* a las (X) am/pm para vacunar a tu hija contra el VPH. La vacuna es gratuita. Secretaria de Salud*	*Hello XXXXXXXXXXXXXX. You have an appointment on the XX of *MONTH* at (X) am/pm to have your daughter vaccinated against HPV. The vaccine is free. Secretary of Health*	*4,604*
6	*Enhanced active choice*	*Personalized*	*Belief about cost*	*None*	*Hola XXXXXXXXXX ¿Quieres vacunar gratis a tu hija contra VPH?* *1 = Sí* *2 = No, prefiero correr el riesgo que desarrolle un cáncer cervical. Secretaria de Salud*	*Hello XXXXXXXXXXXXXX Do you want to vaccinate your daughter against HPV for free?* *1 = Yes* *2 = No, I prefer to take the risk of her developing cervical cancer. Secretary of Health*	*4,607*
7	*Pseudo sets*	*Personalized*	*Belief about cost*	*Sender*	*Hola XXXXXXXXXX. Tu hija debe tener 21 vacunas en su carné de vacunación y aún le falta la vacuna contra el VPH. La vacuna es gratuita. Secretaria de Salud*	*Hello XXXXXXXXXXXXXX. Your daughter should have 21 vaccines on her immunization record and is still missing the HPV vaccine. The vaccine is free. Secretary of Health*	*4,604*

Overall Sample size: 32,218

Table A5. SMS Content for Experiment 5. Social Norms.

Number		Behavioral Principle	Fixed Element 1	Fixed Element 2	SMS Content	SMS content in English	Sample size
1	Control	Pure control	None	None	No message	No message	2,051
2		Policy control	None	Sender	Vacúnalo: bríndale a tu hijo o hija toda la protección. Consulta en https://bit.ly/ssaludbog el punto más cercano. Sec. de Salud	Vaccinate him/her: give your son or daughter full protection. Consult https://bit.ly/ssaludbog for the nearest point. Secretary of Health	1,023
3		Experimental control	Personalized	Sender	Hola XXXXXXXXXX. Ponle a tu hija la 2da vacuna contra el VPH: bríndale toda la protección. Sec. de Salud	Hello XXXXXXXXXXXXXXXX. Give your daughter the 2nd HPV vaccine: give her full protection. Sec. of Health	2,048
4	Static design	Injunctive social norm localidad	Personalized	Sender	Hola XXXXXXXXX. El XX% de padres de familia en tu localidad ya le pusieron la 2da vacuna contra el VPH a sus hijas. Faltas tu :(. Secretaria de Salud	Hello XXXXXXXXXXX. XX% of parents in your area have already given the 2nd HPV vaccine to their daughters. You are missing :(. Secretary of Health	2,052
5		Positive descriptive norm localidad	Personalized	Sender	Hola XXXXXXXXX. El XX% de padres de familia en tu localidad ya le pusieron la 2da vacuna contra el VPH a sus hijas. Sec. de Salud	Hello XXXXXXXXXXX. XX% of parents in your locality have already given the 2nd HPV vaccine to their daughters. Sec. of Health	2,046
6	Dynamic design	Quantitative Dynamic Norm localidad	Personalized	Sender	Hola XXXXXXXXX. El XX% de padres de familia en tu localidad ya le pusieron la 2da vacuna contra el VPH a sus hijas y cada vez se suman más. Sec. de Salud	Hello XXXXXXXXXXX. XX% of parents in your locality have already given the 2nd HPV vaccine to their daughters and many more are joining in. Sec. of Health	2,052
7		Qualitative Dynamic norm localidad	Personalized	Sender	Hola XXXXXXXXX. Cada vez mas padres de familia en tu localidad le ponen la 2da vacuna contra el VPH a sus hijas. Sec. de Salud	Hello XXXXXXXXXXX. More and more parents in your locality are giving the 2nd HPV vaccine to their daughters. Sec. of Health	2,052
8		Trending norm	Personalized	Sender	Hola XXXXXXXXX. Desde el 2016 aumentó 83% el número de padres de familia en Bogotá que le pusieron la 2da	Hello XXXXXXXXXXX. Since 2016 increased 83% the number of parents in Bogota who gave the 2nd HPV vaccine to their daughters. Sec. of Health	2,049

					vacuna contra el VPH a sus hijas. Sec. de Salud		

Overall Sample size: 15,373

Table A6. SMS Content for Experiment 6. Decision Aids.

Number		Behavioral Principle	Fixed Element 1	Fixed Element 2	Fixed Element 3	SMS Content	SMS content in English	Sample size
1	Control	Pure control	None	None	None	No message	No message	2,051
2		Policy control	None	None	Sender	Vacúnalo: bríndale a tu hijo o hija toda la protección. Secretaría de Salud	Vaccinate him/her: give your son or daughter full protection. Secretary of Health	2,049
3		Experimental control	Personalized	Milkman's insight	Sender	Hola XXXXXXXXXX. Hay una vacuna contra el VPH esperando por tu hija. Secretaría de Salud	Hello XXXXXXXXXXX. There is an HPV vaccine waiting for your daughter. Secretary of Health	2,048
4	Reminders	Simple	Personalized	Milkman's insight	Sender	Hola XXXXXXXXXX. Recuerda que a tu hija aun le falta la segunda dosis de la vacuna contra el VPH. Hay una vacuna esperandola. Secretaria de Salud	Hello XXXXXXXXXXX. Remember that your daughter is still missing her second dose of the HPV vaccine. There is a vaccine waiting for her. Secretary of Health	2,054
5		Simple+Eliciting implementation intentions	Personalized	Milkman's insight	Sender	Hola XXXXXXXXXX. Recuerda que a tu hija aun le falta la segunda dosis de la vacuna contra el VPH. Hay una vacuna esperándola. ¿La vacunaras? Secret. de Salud	Hello XXXXXXXXXX. Remember that your daughter is still missing her second dose of the HPV vaccine. There is a vaccine waiting for her. Will you vaccinate her? Secretary of Health	2,056
6		Simple+Anchoring (date)	Personalized	Milkman's insight	Sender	Hola XXXXXXXXXX. Recuerda que a tu hija aun le falta la segunda dosis de la vacuna contra el VPH. Hay una vacuna esperándola esta semana. Secret. de Salud	Hello XXXXXXXXXX. Remember that your daughter is still missing her second dose of the HPV vaccine. There is a vaccine waiting for her this week. Secretary of Health	2,051

7	*Presumptive announcements*	*Standard*	*Personalized*	*Milkman's insight*	*Sender*	*Hola XXXXXXXXXX. Es momento de aplicarle la segunda dosis de la vacuna contra el VPH a tu hija. Hay una vacuna esperandola. Secretaría de Salud*	*Hello XXXXXXXXXX. It is time to give your daughter the second dose of the HPV vaccine. There is a vaccine waiting for her. Secretary of Health*	*2,049*
8		*Standard+Eliciting implementation intentions*	*Personalized*	*Milkman's insight*	*Sender*	*Hola XXXXXXXXXX. Es momento de aplicarle la segunda dosis de la vacuna contra el VPH a tu hija. Hay una vacuna esperandola. ¿La vacunaras? Secretaría de Salud*	*Hello XXXXXXXXXX. It is time to give your daughter the second dose of the HPV vaccine. There is a vaccine waiting for her. Will you vaccinate her? Secretary of Health*	*2,052*
9		*Standard+Anchorin g (date)*	*Personalized*	*Milkman's insight*	*Sender*	*Hola XXXXXXXXXX. Es momento de aplicarle la segunda dosis de la vacuna contra el VPH a tu hija. Hay una vacuna esperandola esta semana. Secretaría de Salud*	*Hello XXXXXXXXXX. It is time to give your daughter the second dose of the HPV vaccine. There is a vaccine waiting for her this week. Secretary of Health*	*2,048*
10	*Priming*	*Question-behavior effect*	*Personalized*	*Milkman's insight*	*Sender*	*Hola XXXXXXXXXX. ¿Llevaras a tu hija a recibir la 2da dosis de la vacuna contra el VPH? 1: Si, 2: No. Hay una vacuna esperandola. Secret. de Salud*	*Hello XXXXXXXXXXX. Will you take your daughter to get the 2nd dose of the HPV vaccine? 1: Yes, 2: No. There is a vaccine waiting for her. Secretary of Health*	*2,053*
11		*Mere-measurement effect*	*Personalized*	*Milkman's insight*	*Sender*	*Hola XXXXXXXXXX. ¿Llevaras a tu hija a recibir la 2da dosis de la vacuna contra el VPH? 1: Seguro 2: Probablemente. Hay una vacuna esperandola. Secret. de Salud*	*Hello XXXXXXXXXX. Will you take your daughter to get the 2nd dose of the HPV vaccine? 1: Sure 2: Probably. There is a vaccine waiting for her. Secretary of Health*	*2,055*
12	*Planning tools*	*Implementation intentions*	*Personalized*	*Milkman's insight*	*Sender*	*Hola XXXXXXXXXX. Escoge el dia y la hora para aplicarle a tu hija la 2da dosis de la vacuna contra el VPH. Hay una vacuna esperandola. Secretaria de Salud*	*Hello XXXXXXXXXX. Choose the day and time to give your daughter the 2nd dose of the HPV vaccine. There is a vaccine waiting for her. Secretary of Health*	*2,047*

13		*Implementation intentions + Anchoring (this week as T9)*	*Personalized*	*Milkman's insight*	*Sender*	*Hola XXXXXXXXXX. Escoge el día y la hora esta semana para aplicarle a tu hija la 2da vacuna contra el VPH. Hay una vacuna esperandola. Secretaria de Salud*	*Hello XXXXXXXXXX. Pick a day and time this week to give your daughter the 2nd dose of the HPV vaccine. There is a vaccine waiting for her. Secretary of Health*	*2,051*
14		*Implementation intentions + Urgency (vaccinate this week)*	*Personalized*	*Milkman's insight*	*Sender*	*Hola XXXXXXXXXX. Escoge el día y la hora para aplicarle esta semana la 2da vacuna contra el VPH a tu hija. Hay una vacuna esperandola. Secretaria de Salud*	*Hello XXXXXXXXXX. Choose the day and time to give your daughter the 2nd HPV vaccine this week. There is a vaccine waiting for her. Secretary of Health*	*2,046*

Overall Sample size: 28,710

Table A7. SMS Content for Experiment 7. Cross-randomized Planning Tool.

Panel A. Experiments 1, 2, 3 and 4.

Number	*Behavioral Principle*	*Fixed Element 1*	*SMS Content*	*SMS content in English*	*Sample size*
1	*Pure control*	*None*	*No message*	*No message*	*4,600*
2	*Planning tool (link)*	*Sender*	*Haz tu cita para vacunar a tu hija contra el VPH en el sitio de vacunación más cercano: https://bit.ly/ssaludbog . Secretaria de Salud*	*Make your appointment to vaccinate your daughter against HPV at the nearest vaccination site: https://bit.ly/ssaludbog . Secretary of Health*	*2,297*
3	*Planning tool (telephone)*	*Sender*	*Llama al call center de la Secretaría de Salud al 6013295090 para más información sobre vacunación contra el VPH. Secretaria de Salud*	*Call the call center of the Secretary of Health at 6013295090 for more information on vaccination against HPV. Secretary of Health*	*2,301*

Overall Sample size: 9,198

Panel B. Experiments 5 and 6.

Number	*Behavioral Principle*	*Fixed Element 1*	*SMS Content*	*SMS content in English*	*Sample size*
1	*Pure control*	*None*	*No message*	*No message*	*2,051*
2	*Planning tool (link)*	*Sender*	*Haz tu cita para vacunar a tu hija contra el VPH en el sitio de vacunación más cercano: https://bit.ly/ssaludbog . Secretaria de Salud*	*Make your appointment to vaccinate your daughter against HPV at the nearest vaccination site: https://bit.ly/ssaludbog . Secretary of Health*	*1,025*

Overall Sample size: 3,076