Online Appendix to Accompany

"Planning frictions and the incomplete take-up of health investments: The case of HPV vaccination"

Julian Peña
Universidad del Rosario

Stanislao Maldonado
Tecnologico de Monterrey

This version:August 14, 2026

Not for Publication

A Conceptual framework: full derivation and proofs

This appendix derives the framework summarized in Section 2. We model how ownership reminders and time-specific planning prompts affect the decision to complete the HPV vaccination schedule, identify the channels each message may activate, and establish the assumptions needed to interpret the experimental comparisons.

A.1 Decision problem

A parent decides whether to complete her daughter's HPV vaccination schedule. Let $x_i \in \{0, 1\}$ denote the completion decision, where $x_i = 1$ if the daughter receives the second dose and $x_i = 0$ otherwise. The net value of completion before accounting for reference-dependent utility or message effects is

$$\theta_i = B_i - C_i, \tag{A.1}$$

where B_i is the parent's perceived value of completing the HPV schedule and C_i is the full cost of vaccination. It includes transportation, time, and hassle costs, uncertainty about when to go, and the cognitive cost of translating an intention into action.

Let $t \in \mathcal{T}$ denote the experimental condition, where

$$\mathcal{T} = \{C, P, O, D, OD\}.$$

Here, C denotes the pure control condition, P the placebo message, O the ownership reminder, D the time-specific planning prompt, and OD a combined treatment.

Treatments may affect completion through salience, reference points, and implementation costs. Let $s_i(t)$ capture the salience effect of receiving a message from the health authority, including institutional contact, personalization, and attention to vaccination. We allow salience to vary across message conditions; equality across arms is an additional assumption used below.[1]

[1] Karlan et al. (2016) model how reminders can mitigate undersaving by directing attention to future

Ownership language may strengthen an entitlement reference point, $e(t)$, which captures the extent to which the parent perceives the dose as assigned to her daughter.[2] A planning prompt may strengthen a plan reference point, $p(t)$, by making a specific day and time the expected moment for action.[3]

A planning prompt may also reduce the cost of deciding when and how to act. An inconvenient suggested time may instead create mismatch costs if it disrupts an alternative plan.[4] We write this net implementation effect as $\omega_i a(t)$, where $a(t) = 1$ for treatments with a suggested day and time and $a(t) = 0$ otherwise. The parameter ω_i can be positive, zero, or negative.

Let $\kappa_i(t) \geq 0$ denote a message-burden cost that lowers the payoff from completion through confusion, fatigue, or complexity from combining messages. We set this cost to zero outside the combined treatments as a simplifying restriction.[5]

The parent has reference-dependent preferences over the entitlement and plan dimensions. The binary outcome records completion rather than adherence to the suggested time, so we abstract from deviations from that time. Let $L_i(e, p)$ denote the loss from failing to act when

expenses. We draw on this attention channel and represent it through an additive term, separately from reference-dependent utility and implementation costs.

[2]The endowment-effect and psychological-ownership literatures motivate the role of perceived possession in valuation (Thaler, 1980; Kahneman et al., 1990; Pierce et al., 2001). Vaccination experiments provide evidence that reservation messages can increase uptake (Milkman et al., 2021; Dai et al., 2021). Our interpretation requires the parent to adopt the perceived allocation as a benchmark; it does not require physical possession of the dose.

[3]Koch and Nafziger (2011) model self-chosen goals as reference points that make shortfalls psychologically costly. We apply this logic to a suggested vaccination time if the parent adopts it as a benchmark. The implementation-intentions literature examines how concrete plans help translate intentions into behavior (Gollwitzer, 1999; Gollwitzer and Sheeran, 2006), including vaccination (Milkman et al., 2011).

[4]Reis (2006) models the resource costs of forming and updating plans. A suggested vaccination time may save some of this effort. Because parents remain free to attend at another time, mismatch costs require a response that disrupts existing arrangements; an inconvenient suggestion that is simply ignored need not raise costs.

[5]This term affects the relative payoff from completion. A nuisance from receiving an SMS that affects completion and noncompletion equally would cancel from the utility comparison and leave the action threshold unchanged.

the entitlement and plan reference points are e and p. We assume

$$L_i(0,0) = 0, \qquad L_{i,e}(e,p) \geq 0, \qquad L_{i,p}(e,p) \geq 0. \tag{A.2}$$

The parameter $\lambda_i > 0$ captures the weight placed on reference-dependent losses.[6]

The utility from choosing x_i under treatment t is

$$U_i(x_i;t) = x_i\left[\theta_i + s_i(t) + \omega_i a(t) - \kappa_i(t)\right] - (1-x_i)\lambda_i L_i(e(t),p(t)). \tag{A.3}$$

Completion yields the net vaccination payoff, adjusted by salience, implementation effects, and message burden. Noncompletion avoids the action cost but entails a reference-dependent loss when entitlement or plan reference points are present.

The parent completes the schedule if $U_i(1;t) \geq U_i(0;t)$. Using (A.3), this condition is equivalent to

$$\theta_i + s_i(t) + \omega_i a(t) - \kappa_i(t) + \lambda_i L_i(e(t),p(t)) \geq 0. \tag{A.4}$$

Define the behavioral shift generated by treatment t as

$$\Gamma_i(t) = s_i(t) + \omega_i a(t) - \kappa_i(t) + \lambda_i L_i(e(t),p(t)). \tag{A.5}$$

The completion rule can then be written as

$$x_i(t) = 1 \quad \text{if and only if} \quad \theta_i + \Gamma_i(t) \geq 0. \tag{A.6}$$

[6]The loss term draws on prospect theory and subsequent models of reference-dependent preferences (Kahneman and Tversky, 1979; Tversky and Kahneman, 1991; Kőszegi and Rabin, 2006). The normalization assigns no reference loss to noncompletion when both reference points are zero; the nonnegative partial derivatives imply that strengthening either reference cannot reduce that loss. We specify reference points as functions of the assigned message, without imposing consistency between expectations and choices as in Kőszegi and Rabin (2006).

A.2 Treatment mapping

The experimental arms can be mapped into the model as follows:

$$\begin{aligned}
C: &\quad s_i(C)=0, \quad a(C)=0, \quad \kappa_i(C)=0, \quad e(C)=e_0, \quad p(C)=p_0,\\
P: &\quad s_i(P)>0, \quad a(P)=0, \quad \kappa_i(P)=0, \quad e(P)=e_0, \quad p(P)=p_0,\\
O: &\quad s_i(O)>0, \quad a(O)=0, \quad \kappa_i(O)=0, \quad e(O)=e_0+\delta_E, \quad p(O)=p_0,\\
D: &\quad s_i(D)>0, \quad a(D)=1, \quad \kappa_i(D)=0, \quad e(D)=e_0, \quad p(D)=p_0+\delta_P,\\
OD: &\quad s_i(OD)>0, \quad a(OD)=1, \quad \kappa_i(OD)\geq 0, \quad e(OD)=e_0+\delta_E, \quad p(OD)=p_0+\delta_P.
\end{aligned} \tag{A.7}$$

The placebo message captures the common features of personalized contact with the Health Secretariat while leaving HPV-specific reference points unchanged. Ownership language raises perceived entitlement by $\delta_E \geq 0$, while the planning prompt strengthens the plan reference point by $\delta_P \geq 0$ and affects implementation costs through ω_i. The combined treatment shifts both reference points and may impose an additional burden, $\kappa_i(OD)$.

The loss function allows entitlement and plan reference points to be complements, substitutes, or additively separable. The effect of combining messages therefore depends in part on whether their reference-point channels reinforce each other or overlap.

A.3 Propositions and proofs

Proposition A1. Threshold rule and marginal parents. A parent completes the vaccination schedule under treatment t if and only if

$$\theta_i \geq -\Gamma_i(t), \tag{A.8}$$

where $\Gamma_i(t)$ is defined in (A.5). For any comparison of two conditions, completion changes only for parents whose θ_i lies between the corresponding action thresholds.

Proof. From (A.3),

$$U_i(1;t) = \theta_i + s_i(t) + \omega_i a(t) - \kappa_i(t)$$

and

$$U_i(0;t) = -\lambda_i L_i(e(t), p(t)).$$

The parent completes the schedule if $U_i(1;t) \geq U_i(0;t)$, which is equivalent to

$$\theta_i + s_i(t) + \omega_i a(t) - \kappa_i(t) + \lambda_i L_i(e(t), p(t)) \geq 0.$$

Using the definition of $\Gamma_i(t)$, this becomes

$$\theta_i + \Gamma_i(t) \geq 0,$$

or, equivalently,

$$\theta_i \geq -\Gamma_i(t).$$

Parents above both thresholds complete under either condition, while those below both thresholds complete under neither. Treatment effects therefore arise among parents whose net valuation lies between the two thresholds. □

Proposition A2. Ownership reminders increase completion if they create entitlement. Suppose the ownership reminder increases the entitlement reference point, so that $e(O) > e(P)$, while leaving the plan reference point and the planning component unchanged. Assume also that generic salience is the same in the placebo and ownership arms, $s_i(O) = s_i(P)$. If $L_{i,e}(e, p) > 0$, then the ownership reminder weakly increases the behavioral incentive to complete the schedule relative to the placebo message. The effect on completion is strictly positive if a positive mass of parents satisfies

$$-\Gamma_i(O) \leq \theta_i < -\Gamma_i(P). \tag{A.9}$$

Proof. Using (A.5), the difference in behavioral shifts between the ownership reminder

and the placebo message is

$$\Gamma_i(O) - \Gamma_i(P) = \lambda_i \left[L_i(e_0 + \delta_E, p_0) - L_i(e_0, p_0) \right],$$

where we use $s_i(O) = s_i(P)$, $a(O) = a(P) = 0$, and $\kappa_i(O) = \kappa_i(P) = 0$. Since $\delta_E \geq 0$, $\lambda_i > 0$, and $L_{i,e}(e, p) \geq 0$, this difference is weakly positive. If $L_{i,e}(e, p) > 0$ over the relevant range and $\delta_E > 0$, it is strictly positive.

By Proposition A1, the ownership reminder lowers the action threshold from $-\Gamma_i(P)$ to $-\Gamma_i(O)$. Since $\Gamma_i(O) \geq \Gamma_i(P)$, we have $-\Gamma_i(O) \leq -\Gamma_i(P)$. Parents with

$$-\Gamma_i(O) \leq \theta_i < -\Gamma_i(P)$$

complete under the ownership reminder but not under the placebo message. If there is positive mass of such parents, the treatment effect is strictly positive. □

Proposition A3. Planning prompts increase completion if planning benefits and plan-reference effects dominate mismatch costs. Suppose the time-specific planning prompt creates a plan reference point, so that $p(D) > p(P)$, and affects implementation costs through ω_i. Assume also that generic salience is the same in the placebo and planning-prompt arms, $s_i(D) = s_i(P)$. Relative to the placebo message, the planning prompt weakly increases the behavioral incentive to complete the schedule if

$$\omega_i + \lambda_i \left[L_i(e_0, p_0 + \delta_P) - L_i(e_0, p_0) \right] \geq 0. \tag{A.10}$$

If this condition holds for all parents, completion weakly increases in the population. The increase is strict if a positive mass of parents crosses the completion threshold.

Proof. Using (A.5), the difference in behavioral shifts between the planning prompt and

the placebo message is

$$\Gamma_i(D) - \Gamma_i(P) = \omega_i + \lambda_i \left[L_i(e_0, p_0 + \delta_P) - L_i(e_0, p_0) \right],$$

where we use $s_i(D) = s_i(P)$, $a(D) = 1$, $a(P) = 0$, and $\kappa_i(D) = \kappa_i(P) = 0$. The first term, ω_i, captures the net implementation effect of the suggested day and time: planning benefits net of mismatch costs. The second term captures the reference-dependent loss from failing to follow the plan reference point.

If this expression is weakly positive, then $\Gamma_i(D) \geq \Gamma_i(P)$, so the planning prompt weakly lowers the action threshold. If the inequality is strict for a positive mass of parents satisfying

$$-\Gamma_i(D) \leq \theta_i < -\Gamma_i(P),$$

then those parents complete under the planning prompt but not under the placebo message. When the incentive shift is nonnegative for all parents, these switches imply a strictly positive aggregate effect. □

Proposition A4. The ranking of ownership reminders and planning prompts is theoretically ambiguous. Under equal salience across the placebo, ownership, and planning arms, the content-specific behavioral shifts relative to placebo are

$$\Delta_i^O = \lambda_i \left[L_i(e_0 + \delta_E, p_0) - L_i(e_0, p_0) \right], \tag{A.11}$$

and

$$\Delta_i^D = \omega_i + \lambda_i \left[L_i(e_0, p_0 + \delta_P) - L_i(e_0, p_0) \right]. \tag{A.12}$$

The model does not determine whether $\Delta_i^O > \Delta_i^D$ or $\Delta_i^D > \Delta_i^O$. Accordingly, the treatment effect of the ownership reminder can be larger than, smaller than, or similar to the treatment effect of the planning prompt.

Proof. The ownership reminder affects completion through the entitlement reference point, while the planning prompt affects completion through both the plan reference point and the net implementation term ω_i. The model imposes no restriction linking δ_E, δ_P, λ_i, the marginal effects of $L_i(e, p)$, and ω_i. Therefore, there are admissible parameter values under which

$$\Delta_i^O > \Delta_i^D$$

and admissible parameter values under which

$$\Delta_i^D > \Delta_i^O.$$

For example, if entitlement losses are large and planning benefits are small or mismatch costs are large, then $\Delta_i^O > \Delta_i^D$. Conversely, if planning benefits and plan-reference losses are large relative to entitlement losses, then $\Delta_i^D > \Delta_i^O$. Hence the ranking is theoretically ambiguous. □

Proposition A5. Reference-point interactions and message burden determine whether the combined incentive shift is additive. Under equal salience across message arms, the content-specific behavioral shift of the combined treatment relative to placebo is

$$\Delta_i^{OD} = \omega_i - \kappa_i(OD) + \lambda_i \left[L_i(e_0 + \delta_E, p_0 + \delta_P) - L_i(e_0, p_0) \right]. \tag{A.13}$$

Define the interaction between the entitlement and plan reference points as

$$\begin{aligned} \mathcal{I}_i = {} & L_i(e_0 + \delta_E, p_0 + \delta_P) - L_i(e_0 + \delta_E, p_0) \\ & - L_i(e_0, p_0 + \delta_P) + L_i(e_0, p_0). \end{aligned} \tag{A.14}$$

For the specified reference-point shifts, $\mathcal{I}_i > 0$ indicates complementarity, $\mathcal{I}_i = 0$ additivity, and $\mathcal{I}_i < 0$ substitution or overlap. Relative to the sum of the individual content-specific

shifts,

$$\Delta_i^{OD} - (\Delta_i^O + \Delta_i^D) = \lambda_i \mathcal{I}_i - \kappa_i(OD). \tag{A.15}$$

Proof. Using the definition of $\Gamma_i(t)$, and assuming equal generic salience across message arms, the combined treatment has the content-specific shift

$$\Delta_i^{OD} = \omega_i - \kappa_i(OD) + \lambda_i \left[L_i(e_0 + \delta_E, p_0 + \delta_P) - L_i(e_0, p_0) \right].$$

The sum of the individual content-specific shifts is

$$\Delta_i^O + \Delta_i^D = \omega_i + \lambda_i \left[L_i(e_0 + \delta_E, p_0) - L_i(e_0, p_0) \right] + \lambda_i \left[L_i(e_0, p_0 + \delta_P) - L_i(e_0, p_0) \right].$$

Subtracting this expression from Δ_i^{OD} gives

$$\begin{aligned} \Delta_i^{OD} - (\Delta_i^O + \Delta_i^D) &= -\kappa_i(OD) + \lambda_i \Big[L_i(e_0 + \delta_E, p_0 + \delta_P) - L_i(e_0 + \delta_E, p_0) \\ &\qquad - L_i(e_0, p_0 + \delta_P) + L_i(e_0, p_0) \Big] \\ &= \lambda_i \mathcal{I}_i - \kappa_i(OD). \end{aligned}$$

The interaction term measures how the loss from shifting both reference points differs from the sum of the losses induced by shifting each separately. The combined incentive shift is smaller than the sum of the separate shifts when reference points overlap or message burden outweighs their complementarity. □

Proposition A6. Dynamic effects depend on the persistence of salience and reference points. Suppose the salience, entitlement, and plan-reference effects evolve after a message according to:

$$s_{it}(k) = \delta_s^k s_i(t), \qquad e_t(k) = e_0 + \delta_e^k \left[e(t) - e_0 \right], \qquad p_t(k) = p_0 + \delta_p^k \left[p(t) - p_0 \right], \tag{A.16}$$

where k is time since the message and $0 < \delta_s, \delta_e, \delta_p \leq 1$. Holding implementation effects and message burden fixed, the incentive shift is weakly decreasing in time since the message.

Proof. At time k, the behavioral shift is

$$\Gamma_i(t,k) = s_{it}(k) + \omega_i a(t) - \kappa_i(t) + \lambda_i L_i(e_t(k), p_t(k)).$$

For decay parameters below one, salience and the message-induced reference-point increments decline with k; a parameter equal to one implies persistence of that component. Since $L_i(e,p)$ is weakly increasing in both arguments, the reference-dependent component cannot rise as the reference points return toward their baseline levels. Holding the remaining terms fixed, the incentive shift is therefore largest when the message is received. This result concerns the incentive to act at a given time; cumulative completion gaps need not decline because vaccinations already completed remain recorded.

A response concentrated near the suggested vaccination date requires an additional timing channel. For example, the plan reference point may become strongest around that date, rather than decay from the moment the message arrives. Such concentration would be consistent with a plan-based response, although timing alone cannot distinguish it from salience or implementation effects. □

A.4 Link to the empirical design

Let Y_i denote second-dose completion. We relate the treatment comparisons to the estimating equation

$$Y_i = \alpha + \beta_P P_i + \beta_O O_i + \beta_D D_i + \beta_{OD} OD_i + X_i'\gamma + \varepsilon_i, \tag{A.17}$$

where the omitted category is the pure control group and X_i denotes pre-treatment covariates. In this specification, β_P captures the effect of the implemented mental health message relative to no message. Comparing each active treatment with this placebo yields:

$$\tau_O = \beta_O - \beta_P, \qquad \tau_D = \beta_D - \beta_P, \qquad \tau_{OD} = \beta_{OD} - \beta_P. \tag{A.18}$$

Under equal salience and the treatment mapping, τ_O captures the completion response to the entitlement shift; τ_D captures the joint response to the plan reference and implementation effects; and τ_{OD} captures their combination net of message burden. These interpretations depend on the maintained assumptions. The small, statistically insignificant placebo estimate indicates no detectable effect of the mental health message, but does not establish equal salience across arms or rule out a reminder effect from HPV-specific content.

The equality $\tau_O = \tau_D$ tests whether the implemented ownership and planning messages have the same average effect on completion. The model imposes neither equality nor a ranking. The comparison does not separately identify the contribution of each friction, since the messages may differ in how strongly they activate their intended channels or draw attention to vaccination.

The combined treatment allows us to test whether effects on completion relative to placebo are additive:

$$\tau_{OD} = \tau_O + \tau_D. \tag{A.19}$$

This equality is not implied by additive reference points and zero message-burden costs. Under equal salience, these conditions imply $\Delta_i^{OD} = \Delta_i^O + \Delta_i^D$, an equality of individual incentive shifts. Because completion follows a threshold rule, either message alone may induce the same parent to act, or both may be needed to induce a parent who responds to neither alone. The distribution of these responses determines whether the combined completion effect is below, equal to, or above the sum of the separate effects. Departures from additivity therefore do not by themselves identify substitution or complementarity between reference points. Comparisons with the single-message arms also vary the number of contacts; the two combined arms hold content and contact frequency constant while varying reminder timing.

The framework identifies the channels that each message may activate and the assumptions needed to interpret the treatment comparisons. Their relative effectiveness remains an empirical question.

References

Anderson, Michael L. 2008. "Multiple Inference and Gender Differences in the Effects of Early Intervention: A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects." *Journal of the American Statistical Association* 103 (484): 1481–1495.

Benjamini, Yoav, and Yosef Hochberg. 1995. "Controlling the False Discovery Rate: A Practical and Powerful Approach to Multiple Testing." *Journal of the Royal Statistical Society: Series B (Methodological)* 57 (1): 289–300.

Benjamini, Yoav, Abba M. Krieger, and Daniel Yekutieli. 2006. "Adaptive Linear Step-Up Procedures That Control the False Discovery Rate." *Biometrika* 93 (3): 491–507.

Chernozhukov, Victor, Mert Demirer, Esther Duflo, and Iván Fernández-Val. 2025. "Fisher–Schultz Lecture: Generic Machine Learning Inference on Heterogeneous Treatment Effects in Randomized Experiments, with an Application to Immunization in India." *Econometrica* 93 (4): 1121–1164.

Dai, Hengchen, Silvia Saccardo, Maria A. Han, Lily Roh, Naveen Raja, Sitaram Vangala, Hardikkumar Modi, Shital Pandya, Michael Sloyan, and Daniel M. Croymans. 2021. "Behavioural Nudges Increase COVID-19 Vaccinations." *Nature* 597 (7876): 404–409.

Gollwitzer, Peter M. 1999. "Implementation Intentions: Strong Effects of Simple Plans." *American Psychologist* 54 (7): 493–503.

Gollwitzer, Peter M., and Paschal Sheeran. 2006. "Implementation Intentions and Goal Achievement: A Meta-Analysis of Effects and Processes." In *Advances in Experimental Social Psychology*, edited by Mark P. Zanna, Volume 38. 69–119, Academic Press.

Kahneman, Daniel, Jack L. Knetsch, and Richard H. Thaler. 1990. "Experimental Tests of the Endowment Effect and the Coase Theorem." *Journal of Political Economy* 98 (6): 1325–1348.

Kahneman, Daniel, and Amos Tversky. 1979. "Prospect Theory: An Analysis of Decision under Risk." *Econometrica* 47 (2): 263–291.

Karlan, Dean, Margaret McConnell, Sendhil Mullainathan, and Jonathan Zinman. 2016. "Getting to the Top of Mind: How Reminders Increase Saving." *Management Science* 62 (12): 3393–3411.

Koch, Alexander K., and Julia Nafziger. 2011. "Self-Regulation through Goal Setting." *The Scandinavian Journal of Economics* 113 (1): 212–227.

Kőszegi, Botond, and Matthew Rabin. 2006. "A Model of Reference-Dependent Preferences." *The Quarterly Journal of Economics* 121 (4): 1133–1165.

Milkman, Katherine L., John Beshears, James J. Choi, David Laibson, and Brigitte C. Madrian. 2011. "Using Implementation Intentions Prompts to Enhance Influenza Vaccination Rates." *Proceedings of the National Academy of Sciences* 108 (26): 10415–10420.

Milkman, Katherine L., Mitesh S. Patel, Linnea Gandhi, Heather N. Graci, Dena M. Gromet, Hung Ho, Joseph S. Kay, et al. 2021. "A Megastudy of Text-Based Nudges Encouraging Patients to Get Vaccinated at an Upcoming Doctor's Appointment." *Proceedings of the National Academy of Sciences* 118 (20): e2101165118.

Pierce, Jon L., Tatiana Kostova, and Kurt T. Dirks. 2001. "Toward a Theory of Psychological Ownership in Organizations." *Academy of Management Review* 26 (2): 298–310.

Reis, Ricardo. 2006. "Inattentive Consumers." *Journal of Monetary Economics* 53 (8): 1761–1800.

Thaler, Richard. 1980. "Toward a Positive Theory of Consumer Choice." *Journal of Economic Behavior & Organization* 1 (1): 39–60.

Tversky, Amos, and Daniel Kahneman. 1991. "Loss Aversion in Riskless Choice: A Reference-Dependent Model." *The Quarterly Journal of Economics* 106 (4): 1039–1061.

Appendix Tables

Table A-1: Variable Definitions

Component	Variable or construct	Definition and coding	Source or timing
Panel A. Outcome and measurement window			
Outcome	`vaccinated`	Indicator equal to one if administrative records show that the girl completed the HPV second dose during the follow-up window, and zero otherwise.	Administrative vaccination records
Outcome date	`vaccination_date`	Date of observed HPV second-dose completion among girls who completed the schedule; missing for non-completers.	Administrative vaccination records
Measurement window	Follow-up window	Completion is measured from April 25, 2022, through July 30, 2022. This is the window used for the final outcome and Kaplan-Meier curves.	Post-randomization
Campaign window	SMS campaign	The first SMS round was delivered on May 4, 2022, and the last SMS round was delivered on June 11, 2022.	Implementation records
Panel B. Treatment assignment			
Treatment arm	`treat`	Randomized arm indicator: 1 pure control; 2 placebo control; 3 ownership reminder; 4 planning prompt; 5 planning prompt plus ownership, 24h reminder; 6 planning prompt plus ownership, 2h reminder.	Random assignment
Pure control	`pure_control`	Indicator for the no-message control group.	Random assignment
Placebo control	`placebo_control`	Indicator for receiving an SMS from the Health Secretariat unrelated to HPV vaccination.	Random assignment
Ownership reminder	`vaccine_reserv_only`	Indicator for receiving SMS language stating that an HPV vaccine was reserved for the daughter.	Random assignment
Planning prompt	`soft_default_only`	Indicator for receiving SMS language suggesting a specific day and time for completing the HPV vaccination schedule.	Random assignment
Combined treatment, 24h	`sft_dft_vaccine_reserv1`	Indicator for receiving planning-prompt language plus an ownership reminder sent 24 hours before the suggested vaccination time.	Random assignment
Combined treatment, 2h	`sft_dft_vaccine_reserv2`	Indicator for receiving planning-prompt language plus an ownership reminder sent 2 hours before the suggested vaccination time.	Random assignment
Panel C. Randomization strata			
Strata	`strata`	Numeric fixed-effect identifier formed by baseline location crossed with the age-group indicator used in the randomization.	Pre-treatment administrative data
Location stratum	`strata_location`	Baseline location category used in the randomization strata.	Pre-treatment administrative data
Bogota indicator	`bogota`	Derived indicator equal to one if `strata_location` is Bogota D.C., and zero otherwise.	Constructed from strata
Age stratum	`age`	Indicator equal to one for girls ages 9–10 at baseline, and zero otherwise.	Pre-treatment administrative data
Panel D. Baseline covariates			
Age	`age_girl`	Girl's age in years at baseline.	Pre-treatment administrative data
Health insurer	`sanitas`, `salud_total`, `famisanar`, `compensar`, `capital_salud`, `other_eps`	Mutually exclusive indicators for the girl's health insurer; `other_eps` collects remaining insurers.	Pre-treatment administrative data
Health regime	`regime_contri`, `regime_subsi`, `uninsured`, `another_regime`	Indicators for contributory scheme, subsidized scheme, uninsured status, and other health-regime category.	Pre-treatment administrative data
Ethnicity	`ethnicity`	Indicator equal to one if the administrative records identify the girl as belonging to an ethnic group.	Pre-treatment administrative data
Displacement status	`displaced`	Indicator equal to one if the administrative records identify the girl as displaced by the armed conflict.	Pre-treatment administrative data
Nationality	`colombian`	Indicator equal to one if the girl's recorded nationality is Colombian.	Pre-treatment administrative data

Notes: This table defines the variables used in the econometric analysis. The sample includes 5,907 randomized parent records, each linked to one eligible girl; 819 girls completed the HPV second dose during the measurement window. The pure-control completion rate over this window is 8.6 percent. Treatment indicators identify mutually exclusive randomized assignments. Baseline covariates and strata are measured before treatment assignment.

Table A-2: Descriptive Statistics and Covariate Balance: Placebo-Control Benchmark

	Descriptive statistics			Standardized differences relative to placebo control				
Variable	Mean	SD	Placebo-control mean	Pure control	Ownership	Planning prompt	Planning+Own. 24h	Planning+Own. 2h
Girl's age (years)	11.252	2.013	11.278	-0.020	-0.017	-0.030	-0.001	-0.007
Age 9–10 years	0.461	0.498	0.460	0.002	0.002	0.001	0.003	0.001
Inside Bogota	0.628	0.483	0.627	0.002	0.001	0.001	0.001	0.003
EPS Sanitas	0.147	0.354	0.131	0.038	0.042	0.061	0.064	0.076
EPS Salud Total	0.101	0.301	0.105	-0.041	-0.016	-0.020	0.010	-0.003
EPS Famisanar	0.204	0.403	0.202	0.035	0.031	-0.021	0.003	-0.022
EPS Compensar	0.184	0.387	0.183	-0.008	0.032	0.005	-0.029	0.014
EPS Capital Salud	0.096	0.295	0.101	-0.017	-0.059	0.006	0.004	-0.020
Other EPS	0.268	0.443	0.279	-0.016	-0.040	-0.023	-0.038	-0.036
Contributory scheme	0.813	0.390	0.801	0.068	0.035	0.024	0.033	0.028
Subsidized scheme	0.123	0.328	0.131	-0.059	-0.049	-0.006	-0.015	-0.018
Uninsured	0.013	0.113	0.012	0.000	0.026	0.026	-0.009	0.000
Other health regime	0.051	0.220	0.056	-0.032	-0.004	-0.046	-0.032	-0.022
Belongs to an ethnic group	0.005	0.071	0.005	-0.051	0.014	0.026	-0.032	0.026
Displaced by armed conflict	0.012	0.110	0.010	0.019	0.046	-0.000	-0.021	0.069
Colombian nationality	0.963	0.189	0.959	0.049	-0.011	0.021	0.037	0.010
Observations	5907		985	985	983	986	984	984

Notes: This table reports descriptive statistics and covariate balance for the final analysis sample. The first two columns report the full-sample mean and standard deviation. The third column reports the mean for the placebo-control group. The remaining columns report signed standardized differences between each listed arm and the placebo-control group, computed as the difference in means divided by the square root of the average of the two group-specific variances. Values close to zero indicate stronger balance. The covariates are measured before treatment assignment and come from administrative records. The pure control group received no message; the placebo-control group received an SMS unrelated to HPV vaccination content; ownership refers to language indicating that an HPV vaccine was reserved for the daughter; and planning prompt refers to language suggesting a specific day and time for completing the vaccination schedule.

Table A-3: Alternative Benchmarks for Main ITT Effects

	(1) Pure-control benchmark	(2) Placebo-control benchmark	(3) Pooled-control benchmark
Placebo control	0.0143	–	–
	(0.0130)		
Ownership reminder	0.0458***	0.0315**	0.0386***
	(0.0139)	(0.0143)	(0.0125)
Planning prompt	0.0867***	0.0724***	0.0795***
	(0.0147)	(0.0150)	(0.0133)
Planning prompt + ownership, 24h	0.0794***	0.0652***	0.0723***
	(0.0146)	(0.0149)	(0.0133)
Planning prompt + ownership, 2h	0.0910***	0.0768***	0.0839***
	(0.0148)	(0.0152)	(0.0135)
Benchmark mean	0.0863	0.0995	0.0929
Benchmark observations	985	985	1,970
Regression observations	5907	5907	5907
Strata fixed effects	Yes	Yes	Yes
Pre-treatment covariates	Yes	Yes	Yes
P-value: active arms equal	0.015	0.015	0.015

Notes: The dependent variable is an indicator equal to one if the girl completed the HPV second dose during the study period, measured in administrative vaccination records. Coefficients and standard errors are reported in the original 0–1 scale of the dependent variable. Column 1 estimates effects relative to the pure control group and includes the placebo-control arm as a separate non-omitted treatment indicator. Column 2 estimates effects relative to the placebo control group and includes the pure control group as a separate non-omitted category; the placebo row is therefore not applicable in this column. Column 3 pools the pure and placebo controls as the omitted benchmark; the placebo-control row is therefore part of the benchmark. All specifications are linear probability models with randomization-strata fixed effects and the same pre-treatment administrative covariates used in the covariate-adjusted specification of Table 3: EPS indicators, health-regime indicators, ethnicity, displacement status, Colombian nationality, and girl's age. The equality-test p-value tests equality of the four active treatment arms: ownership reminder, planning prompt, planning prompt plus ownership at 24 hours, and planning prompt plus ownership at 2 hours. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-4: Full Multiple-Hypothesis Testing Q-Values

Hypothesis	Benchmark or contrast	Estimate	Robust SE	Robust p	BH q	BKY q
Panel A. Main ITT vs pure control ($m = 5$)						
Placebo control	Pure control	0.0143	(0.0130)	0.274	0.274	0.058
Ownership reminder	Pure control	0.0458	(0.0139)	0.001	0.001	≤ 0.001
Planning prompt	Pure control	0.0867	(0.0147)	< 0.001	≤ 0.001	≤ 0.001
Planning prompt + ownership, 24h	Pure control	0.0794	(0.0146)	< 0.001	≤ 0.001	≤ 0.001
Planning prompt + ownership, 2h	Pure control	0.0910	(0.0148)	< 0.001	≤ 0.001	≤ 0.001
Panel B. Active arms vs placebo control ($m = 4$)						
Ownership reminder	Placebo control	0.0315	(0.0143)	0.028	0.028	0.007
Planning prompt	Placebo control	0.0724	(0.0150)	< 0.001	≤ 0.001	≤ 0.001
Planning prompt + ownership, 24h	Placebo control	0.0652	(0.0149)	< 0.001	≤ 0.001	≤ 0.001
Planning prompt + ownership, 2h	Placebo control	0.0768	(0.0152)	< 0.001	≤ 0.001	≤ 0.001
Panel C. Active arms vs pooled controls ($m = 4$)						
Ownership reminder	Pure + placebo controls	0.0386	(0.0125)	0.002	0.002	≤ 0.001
Planning prompt	Pure + placebo controls	0.0795	(0.0133)	< 0.001	≤ 0.001	≤ 0.001
Planning prompt + ownership, 24h	Pure + placebo controls	0.0723	(0.0133)	< 0.001	≤ 0.001	≤ 0.001
Planning prompt + ownership, 2h	Pure + placebo controls	0.0839	(0.0135)	< 0.001	≤ 0.001	≤ 0.001
Panel D. Mechanism contrasts ($m = 6$)						
Any message	Pure control	0.0757	(0.0106)	< 0.001	≤ 0.001	≤ 0.001
Ownership cue	Pure control	0.0458	(0.0139)	0.001	0.002	0.001
Planning prompt	Pure control	0.0867	(0.0147)	< 0.001	≤ 0.001	≤ 0.001
Ownership cue x planning prompt	Pure control	-0.0530	(0.0215)	0.014	0.017	0.008
2h vs 24h timing	24h combined arm	0.0116	(0.0165)	0.484	0.484	0.088
Ownership cue - planning prompt	Internal contrast	-0.0409	(0.0158)	0.010	0.015	0.007

Notes: The dependent variable is an indicator equal to one if the girl completed the HPV second dose during the study period, measured in administrative vaccination records. Estimates come from covariate-adjusted linear probability models with randomization-strata fixed effects, Eicker-Huber-White robust standard errors, and the same pre-treatment covariates used in Table 3. Panels A–C report treatment-arm contrasts under the benchmark definitions used in Tables 3 and A-3. Panel D reports the mechanism and component-model hypotheses from Table 4; the ownership-planning row reports the linear contrast between the ownership and planning-prompt coefficients. BH q-values control the false discovery rate within each panel using the procedure of Benjamini and Hochberg (1995). BKY q-values use the two-stage sharpened adjustment of Benjamini et al. (2006), following Anderson (2008), with each panel defining a family of hypotheses. Coefficients and standard errors are reported in the original 0–1 scale of the dependent variable.

Table A-5: Generic Machine Learning: Classification Analysis

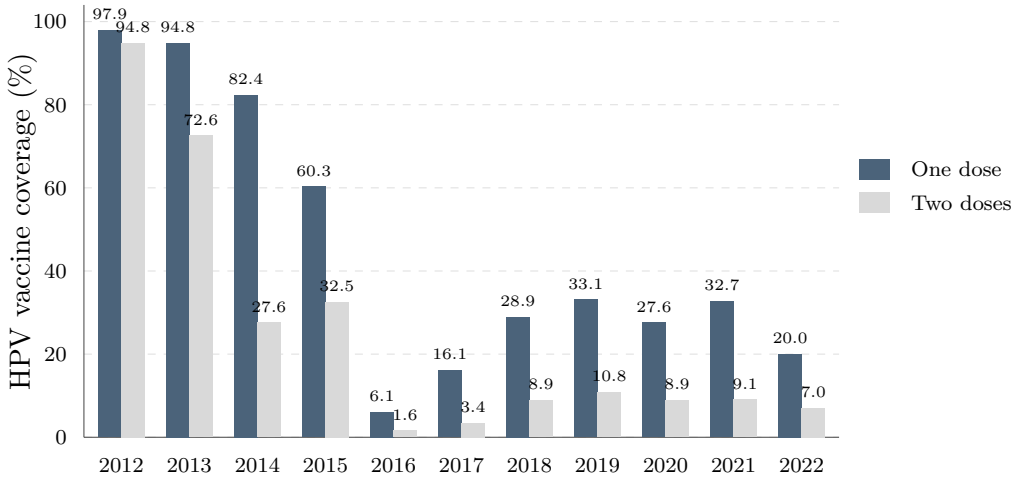
Characteristic	G1: lowest predicted gain	G5: highest predicted gain	Difference	90% CI	p-value
EPS Sanitas	8.47	28.79	17.73	[13.90, 22.12]	< 0.001
EPS Salud Total	15.15	1.17	-14.76	[-18.02, -11.70]	< 0.001
EPS Famisanar	20.44	17.63	-3.36	[-7.74, 1.47]	0.185
EPS Compensar	14.59	21.22	6.08	[2.60, 10.75]	0.004
EPS Capital Salud	4.12	11.62	7.24	[4.04, 10.29]	< 0.001
Contributory regime	84.59	79.39	-4.33	[-8.46, 0.10]	0.080
Uninsured	2.67	0.20	-2.37	[-3.85, -0.93]	0.001
Subsidized regime	6.10	14.44	8.48	[4.96, 11.99]	< 0.001
Ethnic minority	1.84	0.00	-1.84	[-3.05, -0.65]	0.003
Displaced	0.85	1.42	0.47	[-0.71, 1.78]	0.460
Colombian national	94.13	99.30	5.15	[2.81, 7.35]	< 0.001
Girl's age	13.92	9.55	-4.42	[-4.58, -4.26]	< 0.001
Age 9-10	0.52	95.37	94.46	[92.73, 96.28]	< 0.001
Inside Bogota	6.04	99.80	92.77	[90.59, 95.01]	< 0.001
Subsidized or uninsured	9.89	15.00	5.92	[2.04, 9.41]	0.004

Notes: The table reports classification analysis (CLAN) following Chernozhukov et al. (2025). G1 and G5 are the quintiles with the lowest and highest predicted treatment gains, respectively. The outcome is completion of the second HPV vaccine dose. The analysis compares assignment to any of the four active messages with the pure-control arm and excludes the placebo. For each of 100 sample splits, one half of the observations is used to predict treatment gains from pre-treatment characteristics and the other to estimate the target parameters. Estimates and confidence limits are aggregated across splits using medians. Binary characteristics are expressed as percentages, and differences as percentage points; age is measured in years. The 90% confidence interval and p-value refer to the difference between G5 and G1. Because each statistic is aggregated separately, the reported difference need not equal the difference between the displayed group means.

Appendix Figures

Figure A-1 documents the national trajectory of HPV vaccination coverage following the introduction of the vaccine in Colombia. The PAI series shows a sharp decline in both one-dose and two-dose coverage after the initial years of the program and persistently low coverage through 2022.

Figure A-1: HPV Vaccination Rates in Colombia Since the Introduction of the Vaccine in 2012



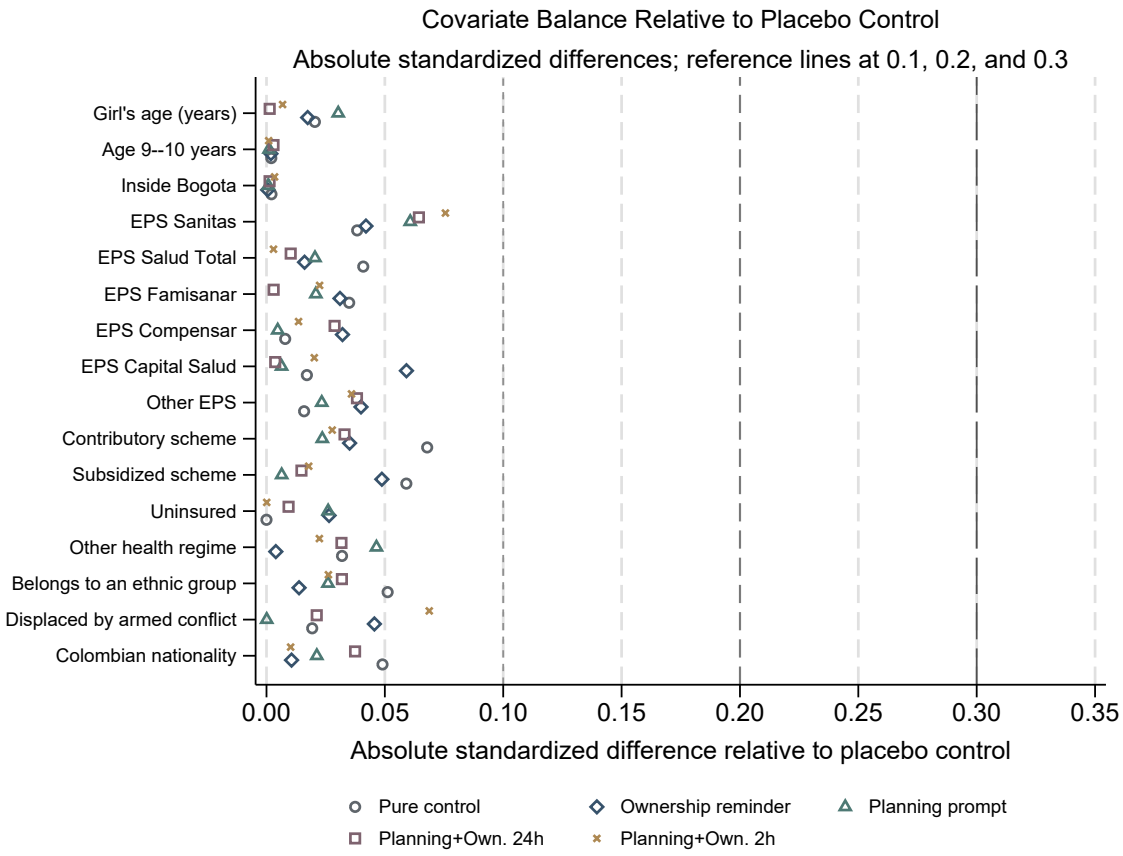
Notes: The bars report annual national HPV vaccination coverage for one and two doses. Source: Authors' elaboration based on data from the Information System of the Expanded Immunization Program (PAI) of the Ministry of Health and Social Protection of Colombia.

Figure A-2: SMS Content by Treatment Arm

Experimental arm	Message content
Placebo control	Hello Fransheska. Take care of your mental health. Exercise, meditate, eat well and get adequate sleep. Secretary of Health.
Ownership reminder	Hello Lucila. There is an HPV vaccine reserved for your daughter. Secretary of Health.
Planning prompt	Hello Fransheska. You have an appointment on May 21 at 10 a.m. to vaccinate your daughter against HPV. Secretary of Health.
Planning prompt + ownership reminder, 24h	**Message 1** (62 hours prior to the appointment): Hello Maria. You have an appointment on May 21 at 10 a.m. to vaccinate your daughter against HPV. Secretary of Health. **Message 2** (24 hours prior to the appointment): Hello Maria. Remember that there is an HPV vaccine reserved for your daughter. Secretary of Health.
Planning prompt + ownership reminder, 2h	**Message 1** (62 hours prior to the appointment): Hello Raquel. You have an appointment on May 21 at 10 a.m. to vaccinate your daughter against HPV. Secretary of Health. **Message 2** (2 hours prior to the appointment): Hello Raquel. Remember that there is an HPV vaccine reserved for your daughter. Secretary of Health.

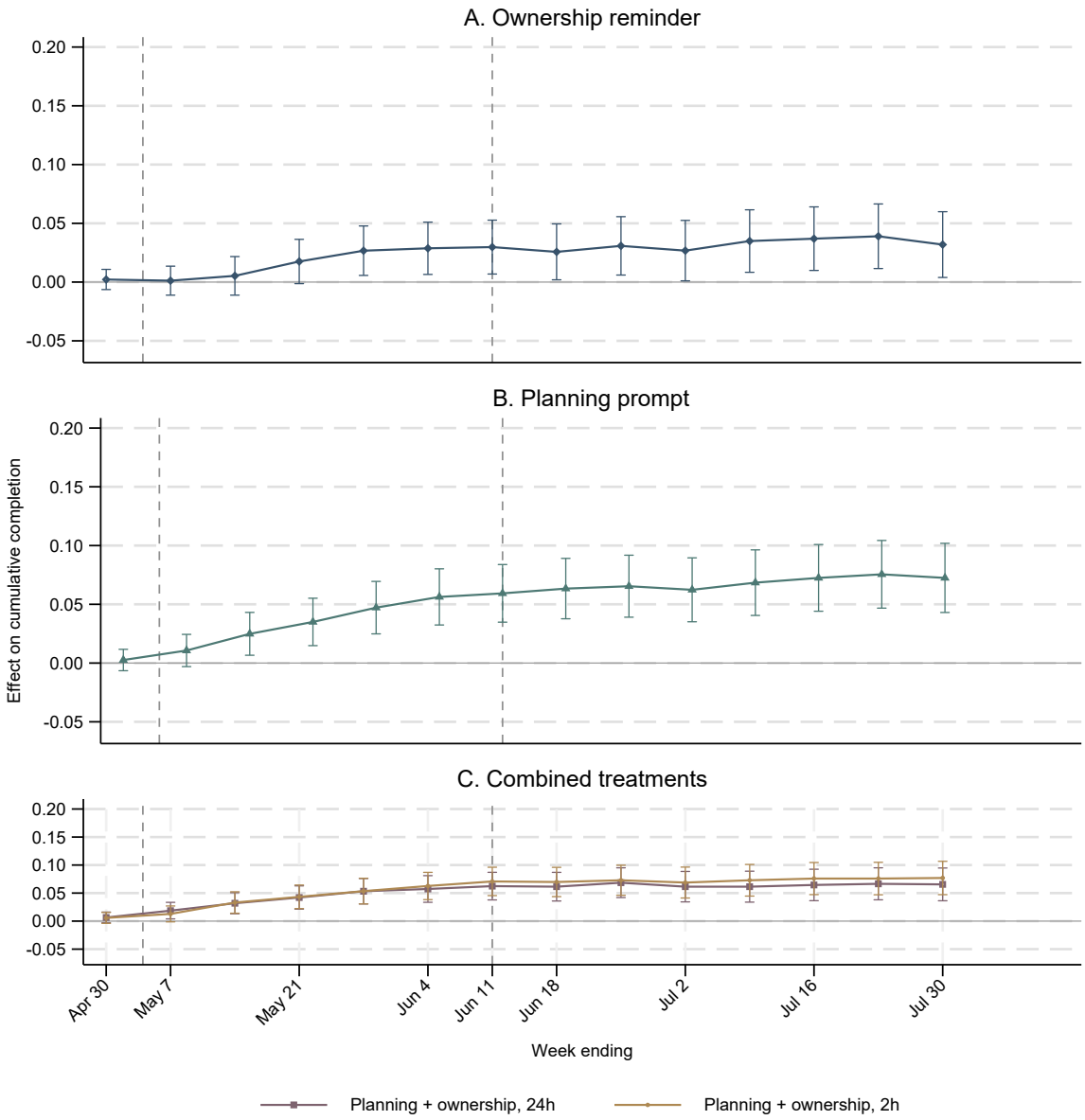
Notes: The figure presents the message content for each treatment arm. The displayed names illustrate personalization: each message addressed the parent by first name. All messages identified the Health Secretariat as the sender. The placebo message provided mental health advice without HPV-vaccination content; the pure control group received no SMS. Although planning prompts used appointment language, the day and time were suggestions for vaccination. No individual appointment was scheduled or vaccination slot reserved.

Figure A-3: Covariate Balance Plot Relative to Placebo Control



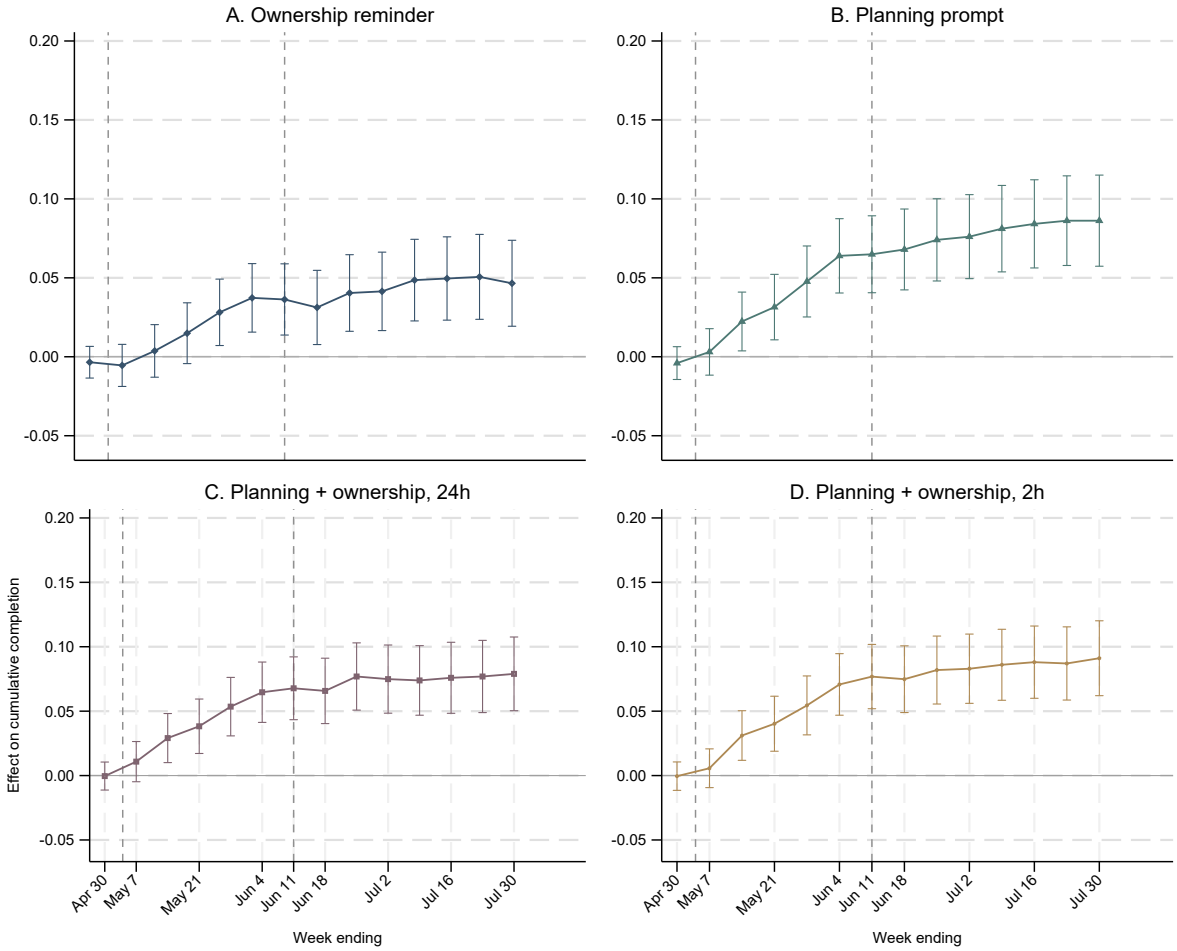
Notes: The figure reports absolute standardized differences in baseline covariates between each non-placebo-control arm and the placebo control group. Standardized differences are computed as the difference in group means divided by the square root of the average of the two group-specific variances. Vertical reference lines mark conventional thresholds of 0.1, 0.2, and 0.3 in absolute standardized-difference units. Values closer to zero indicate stronger covariate balance. Covariates are measured before treatment assignment and come from administrative records.

Figure A-4: Dynamic Treatment Effects Relative to Placebo Control



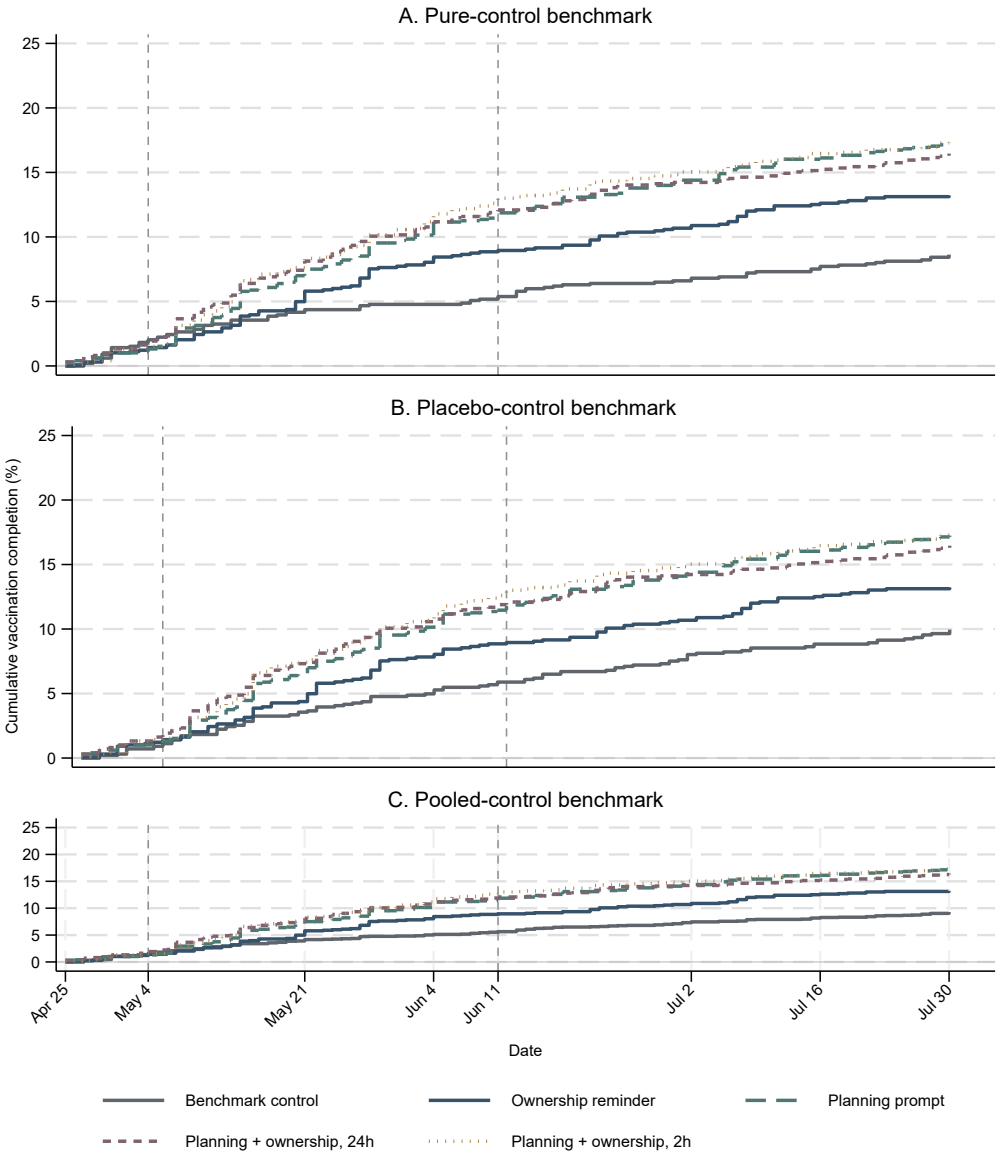
Notes: The figure reports weekly treatment effects on cumulative HPV second-dose completion relative to the placebo-control group. Panel A reports the ownership reminder arm; Panel B reports the planning-prompt arm; Panel C reports the two combined planning-prompt plus ownership-reminder arms. Outcomes are cumulative indicators equal to one if the girl had completed the second dose by the end of each calendar week. Estimates come from linear probability models in the stacked girl-week panel and include week fixed effects, randomization-strata fixed effects, and the same baseline covariates used in the main ITT specification. Vertical dashed lines mark the first SMS delivery on May 4, 2022, and the last SMS delivery on June 11, 2022. Error bars are 95 percent confidence intervals based on standard errors clustered at the girl level.

Figure A-5: Arm-by-Arm Dynamic Treatment Effects



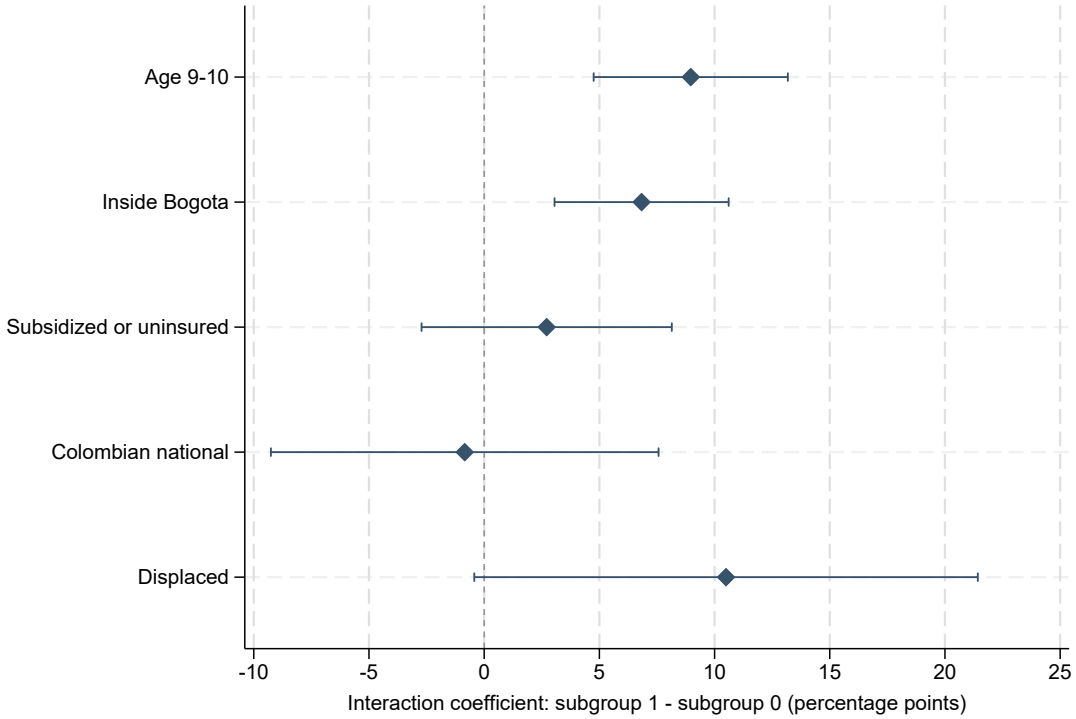
Notes: Each panel reports weekly treatment effects on cumulative HPV second-dose completion relative to the pure control group for one active treatment arm. Outcomes are cumulative indicators equal to one if the girl had completed the second dose by the end of each calendar week. Estimates come from linear probability models in the stacked girl-week panel and include week fixed effects, randomization-strata fixed effects, and the same baseline covariates used in the main ITT specification. Vertical dashed lines mark the first SMS delivery on May 4, 2022, and the last SMS delivery on June 11, 2022. Error bars are 95 percent confidence intervals based on standard errors clustered at the girl level.

Figure A-6: Kaplan-Meier Completion Curves by Alternative Control Benchmark



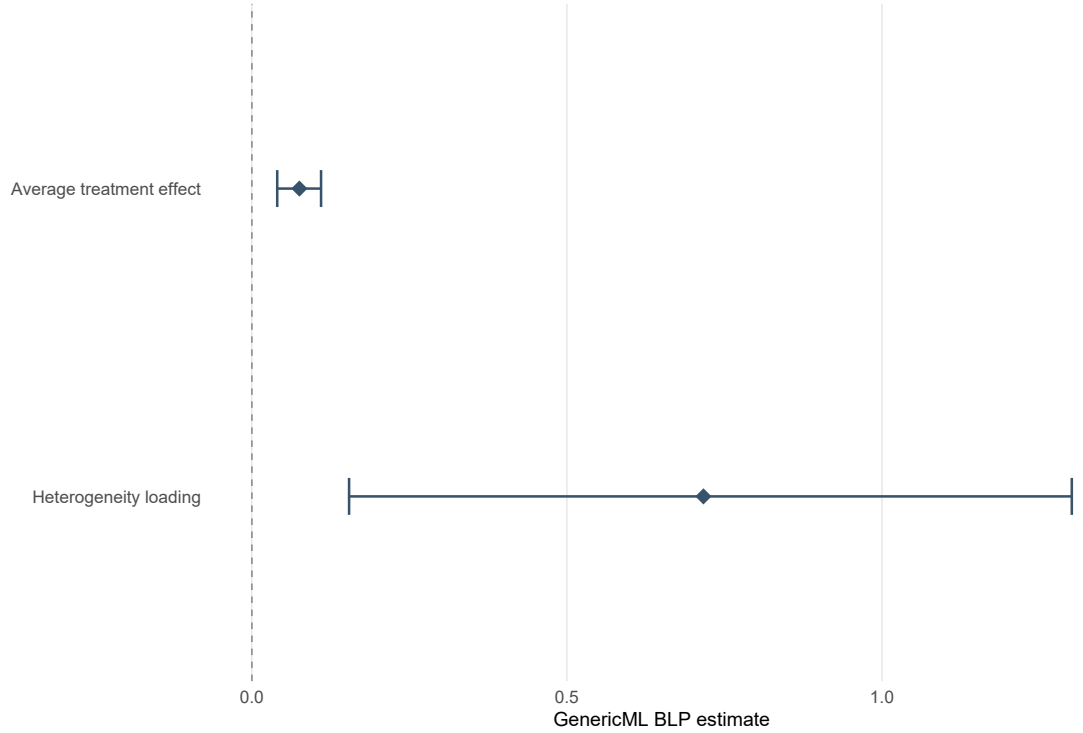
Notes: Each panel plots cumulative HPV second-dose completion during the administrative follow-up window for one control benchmark and the four active treatment arms. Panel A uses the pure control group as the benchmark; Panel B uses the placebo-control group; Panel C pools the pure and placebo control groups. The time axis begins on April 25, 2022, the first observed vaccination date in the follow-up data, and ends on July 30, 2022. Vertical dashed lines mark the first SMS delivery on May 4, 2022, and the last SMS delivery on June 11, 2022. Because there is only administrative censoring at the end of follow-up, the plotted curves are equivalent to cumulative completion rates by date.

Figure A-7: Heterogeneity Coefficient Plot



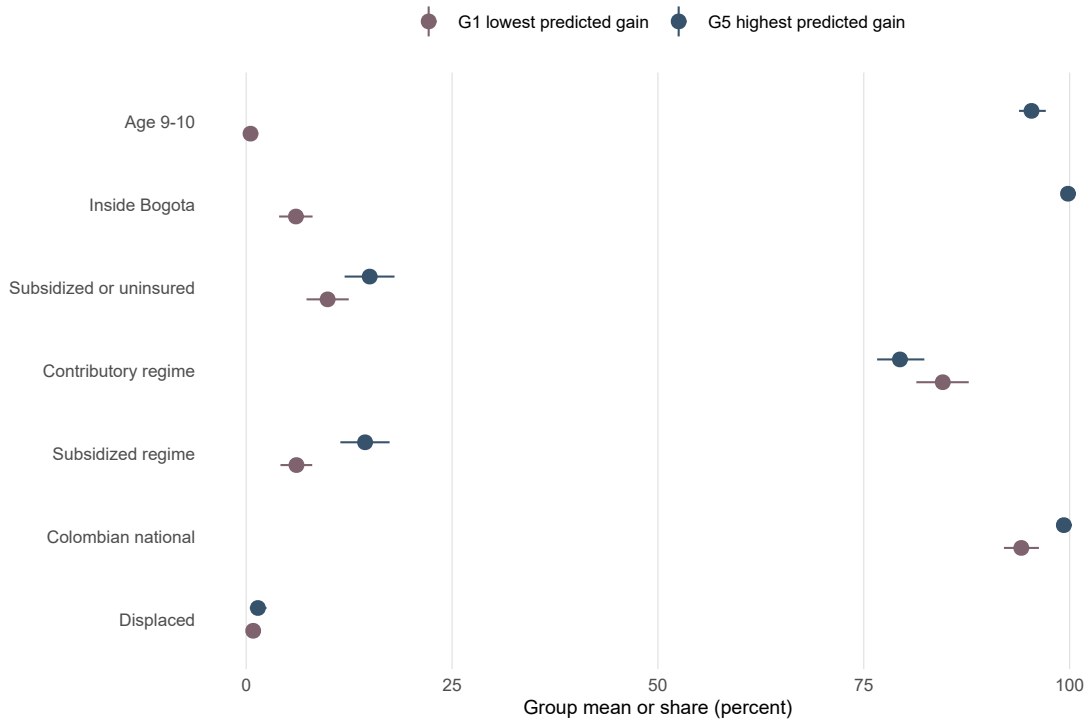
Notes: Each point is the interaction coefficient between the indicator for receiving any of the four active messages and the pre-specified subgroup indicator shown on the vertical axis. The estimation sample excludes the placebo-control arm and compares the pure control group with the four active treatment arms. Coefficients are reported in percentage points. All specifications are linear probability models with randomization-strata fixed effects and the same pre-treatment administrative covariates used in Table 3. Horizontal bars report 95 percent confidence intervals based on Eicker-Huber-White robust standard errors. The corresponding arm-specific interaction estimates are reported in Table 6.

Figure A-8: Generic Machine Learning: Best Linear Predictor



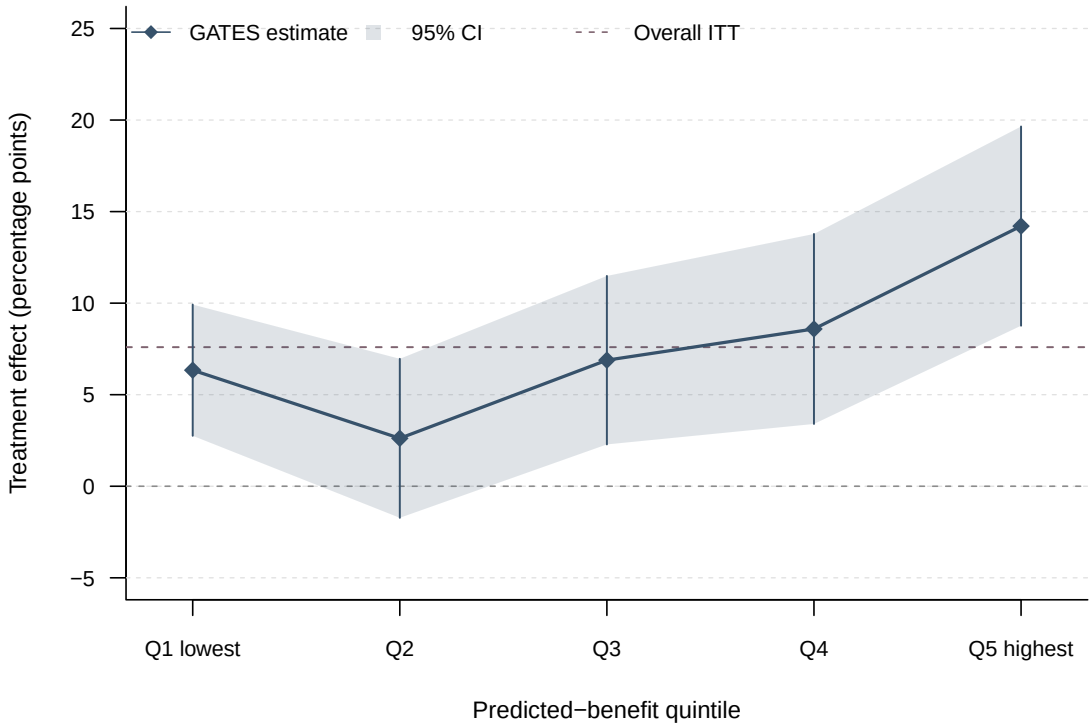
Notes: The figure reports the best linear predictor (BLP) of treatment effects using the framework of Chernozhukov et al. (2025). The outcome is completion of the second HPV vaccine dose. The analysis compares assignment to any of the four active messages with the pure-control arm and excludes the placebo. The coefficient β_1 measures the average treatment effect on the 0–1 completion scale; β_2 measures whether the predicted-gain score captures treatment-effect heterogeneity. Testing $\beta_2 = 0$ assesses the predictive content of that score. For each of 100 sample splits, one half of the observations is used to predict treatment gains from pre-treatment characteristics and the other to estimate the target parameters. Estimates and confidence limits are aggregated across splits using medians. Horizontal bars show 90% confidence intervals.

Figure A-9: Generic Machine Learning: Classification Analysis



Notes: The figure reports classification analysis (CLAN) following Chernozhukov et al. (2025). It compares selected baseline characteristics in the quintiles with the lowest (G1) and highest (G5) predicted treatment gains. The outcome is completion of the second HPV vaccine dose. The analysis compares assignment to any of the four active messages with the pure-control arm and excludes the placebo. For each of 100 sample splits, one half of the observations is used to predict treatment gains from pre-treatment characteristics and the other to estimate the target parameters. Estimates and confidence limits are aggregated across splits using medians. Points show group shares in percent, and horizontal bars show 90% confidence intervals. Table A-5 reports all characteristics and tests of the differences between G5 and G1.

Figure A-10: Group Average Treatment Effects Using a Cross-Fitted Regression-Tree Score



Notes: The figure reports treatment effects by quintile of predicted gains from regression-tree outcome models. The outcome is completion of the second HPV vaccine dose. The analysis compares assignment to any of the four active messages with the pure-control arm and excludes the placebo. Predictions use five-fold cross-fitting: separate outcome models for treated and control observations are estimated on four folds using pre-treatment covariates and randomization-strata indicators, then applied to the remaining fold. Each parent's predicted gain is the difference between these predictions. Quintiles rank these gains from lowest to highest. Group effects are estimated jointly in a linear probability model with treatment-by-quintile interactions, quintile fixed effects, randomization-strata fixed effects, and pre-treatment covariates. Effects are expressed in percentage points. The shaded region shows 95% confidence intervals based on Eicker–Huber–White robust standard errors, and the dashed line shows the overall covariate-adjusted intention-to-treat effect. This exercise assesses sensitivity to the construction of the predicted-gain score relative to Figure 8. The joint test of equal effects across quintiles has $p = 0.024$; the linear trend test has $p = 0.004$.

B Pre-Analysis Plan

The experiment was registered in the AEA RCT Registry under ID AEARCTR-0009705. The complete pre-analysis plan is reproduced below.[7]

[7]The document reproduced in this appendix is the original version available through the AEA RCT Registry at `https://www.socialscienceregistry.org/trials/9705`. Its original terminology, including references to "soft defaults" and "default options," is retained verbatim. The manuscript refers to the implemented intervention as a planning prompt.

Applying Behavioral Insights for Encouraging Parents to Vaccinate their Daughters against HPV in Colombia

Stanislao Maldonado* Julian Peña Duarte†

July 8, 2022

Pre-Analysis Plan

Abstract

We conducted a text SMS campaign leveraging the lessons from behavioral sciences to increase vaccine take-up against the Human Papillomavirus (HPV) in Colombia. In particular, these messages integrate behavioral insights in dimensions related to altruism, warm-glow giving, default options, reminders, and beliefs.

1 Introduction

In 2020 approximately 342,000 women died from cervical cancer (CC), and around 90 percent of the deaths occurred in low- and middle-income countries. (Sung et al., 2021). The main cause of cervical cancer is the Human Papillomavirus (HPV), preventable by a cost-effective solution such as vaccination. However, in Latin America, a significant reduction in HPV vaccination coverage has occurred in the years following its inclusion in the vaccination plans (Nogueira-Rodrigues, 2019). Consequently, there is a serious public health problem in the region. Since many women are still unimmunized and the long-term consequences are devastating. Hence, the interest of governments in promoting effective HPV vaccination

*Universidad del Rosario and Behavioral Government Lab. Email: stanislao.maldonado@urosario.edu.co
†Universidad del Rosario. Email: julian.penad@urosario.edu.co

campaigns.

For this project, we conducted large-scale experiments based on behavioral sciences to increase the uptake of the HPV vaccine in Colombia. Specifically, we implemented a behavioral-based short messaging service (SMS) campaign targeting parents in Colombia. Text messages include behavioral lessons such as altruism, warm-glow giving, default options, reminders, and beliefs. These interventions were implemented in partnership with the American Cancer Society, the Behavioral Government Lab (Universidad del Rosario), the Inter-American Development Bank, the Health Secretariat (HS) of the City of Bogotá, and the Liga Colombiana contra el Cáncer.

This document describes the pre-analysis plan to assess the impact of the SMS campaign. Since this plan was written before final data were requested and analyzed (August 2022), it provides an essential reference to evaluate the performance of these interventions. The plan is outlined as follows: Section 2 reviews the background of the study; Section 3 explains the setting of the experiment; Section 4 defines the interventions; Section 5 describes the sample and the data; section 7 summarizes the study hypotheses; Section 6 explains the power calculations; Section 8 describes the research design, and Section 9 the estimation strategy. The Appendix offers details about our research design.

2 Background

Colombia is a particular case regarding HPV vaccination, as according to Nogueira-Rodrigues (2019) in 2013, the vaccination coverage of first doses was 97.5%, the second-best rate worldwide after Australia. A significant part of its success is attributable to the campaign implemented in 2012 in schools across the country, where girls over nine years of age were vaccinated. Nevertheless, in 2014 an outbreak of unknown etiology occurred in the municipality of Carmen de Bolivar; around 500 girls who had been immunized against HPV presented headaches, fainting, and numbness of extremities. Some families in the municipality and

anti-vaccine movements claimed that the HPV vaccination was the cause of the outbreak. This case led to a huge controversy surrounding the vaccine and bad publicity in the media. So vaccination rates dropped precipitously, to the point that by 2016 coverage for first doses was 14% and 9% for second doses (Simas et al., 2019). However, the Colombian Ministry of Health and Social Protection, in conjunction with the National Institute of Health, conducted an epidemiological study in which they demonstrated that it was not possible to associate HPV vaccination with the events in Carmen de Bolivar (Martínez et al., 2015). Similarly, several international studies have confirmed the safety of the vaccine. In particular, Phillips et al. (2018) identified 109 studies demonstrating that all HPV vaccines have an acceptable safety profile.

Following the Carmen de Bolivar incident, much political instability was generated around the vaccine. Therefore, the Colombian state preferred to withdraw its support for HPV vaccination and adopt a more indifferent stance. The school campaign was interrupted, and the government decided to incorporate informed consent so that the parents would have the autonomy to decide whether to vaccinate their daughters. Provoking an additional barrier since other vaccines in the vaccination plan do not require informed consent.

Despite little government support, institutions such as the Liga Colombiana Contra el Cáncer and the American Cancer Society have made several efforts to improve HPV vaccination coverage in the country. In Arauca (a city in Colombia), for example, an educational campaign was developed targeting the different members of the HPV vaccination process. After the campaign, vaccination rates in Arauca went from 5% in 2017 to 41% in 2018 and 83 % in 2019 (Castro et al., 2020). So the Liga has made great progress. However, there is still great room for improvement in the rest of the country, especially in the teachings of behavioral sciences. Since its solutions, in many cases, are low cost and easy to implement.

3 Setting

Before designing any public policy, it is necessary to have a thorough understanding of the problem to be solved. In the case of low HPV vaccination rates, there is a vast literature on the reasons. One plausible explanation is the beliefs that people may form about the vaccine and HPV. Another possible cause is procrastination, with parents postponing HPV vaccination. We, therefore, developed a series of interventions that address these causes and take advantage of the altruistic behaviors of parents toward their daughters.

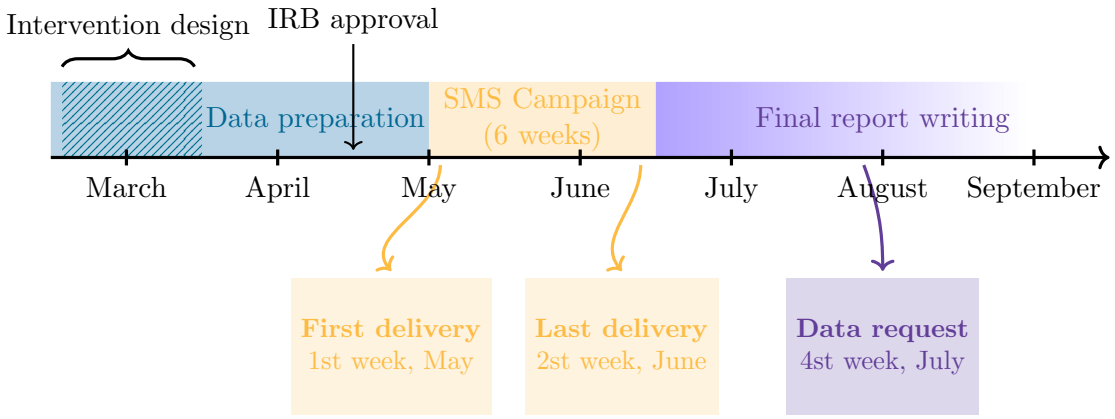
The implementation of the SMS campaign was possible due to the administrative data of HS. Colombia has an excellent immunization program (PAI) that covers the entire population, and it does not matter whether the person is insured in the health system. Regarding the information on the vaccines, this is uploaded to the application (PAIWEB) by the health providers. These reports will be crucial in evaluating the effectiveness of the interventions.

4 Interventions

The study included three types of experiments designed to improve the HPV vaccination rates in this setting. The first type of experiment focuses on altruism and its distinction with warm-glow giving. The second uses soft default options and some insights from Milkman et al. (2021). The third may aim to correct parents' beliefs about the HPV vaccine. Each experiment included two fixed components in its messages: the name of the person to whom the message was sent and the message's sender. We added the fixed components to make the message more personalized, as other settings have shown it to be effective. For the population of unvaccinated girls we implemented all three types of experiments.

In contrast, only the default options and milkman insights were applied to girls with incomplete HPV vaccination schedules. The reason for this was that there was not enough sample for girls with incomplete HPV vaccination schedules to conduct further experiments. The timeline

Figure 1: Experiment Timeline



of these experiments is presented in figure 1 and the interventions are described in detail below.

4.1 Experiment 1: Altruism and warm-glow

Experiment 1 exploits the altruism of fathers towards their daughters and their desire to protect them. Specifically, we present two versions: a soft and a strong version. In the soft version, the word "need" is used to show parents that their daughters need their help, which could be considered something usual because parents, in theory, are constantly helping their children. In addition, at the end of the message, it is emphasized that the parent will be protecting her daughter against cervical cancer. In the strong version, the imminent danger of cervical cancer is made salient by using the expression "Save your daughter," At the end, the message emphasizes that the parent will be giving her all the protection.

On the other hand, we added two treatments of the economic theory of Warm-glow. Specifically, one exploits intrinsic Warm-glow and the other extrinsic Warm-glow. Intrinsic Warm-glow refers to the personal satisfaction of helping others. Extrinsic Warm-glow indicates that external rewards may be involved, such as recognition and prestige. The content of the text messages for this intervention is presented in the graph (2).

Figure 2: SMS Content for experiment 1

Altruism: a soft feeling of protecting	Hello Dayana. Your daughter needs you, vaccinate her against HPV and protect her from cervical cancer. Secretary of Health.
Altruism: a strong feeling of protecting	Hello Luciana. Save your daughter from cervical cancer. Vaccinate her against HPV and give her all the protection. Secretary of Health.
Warm-glow Intrinsic	Hello Aura. Vaccinate your daughter against HPV and be glad to protect her from cervical cancer. Secretary of Health.
Warm-glow Extrinsic	Hello Silvia. Vaccinate your daughter against HPV and join the list of responsible parents who protect their daughters against cancer. Secretary of Health.

4.2 Experiment 2: Beliefs

This experiment provided information to correct parental beliefs regarding HPV vaccination and the severity of HPV. One of the treatments includes a short 2-minute YouTube video designed by La Liga Colombiana contra el Cáncer that provides general information about HPV vaccination. The other two treatments address the ideal timing of HPV vaccination and the severity of HPV globally. Figure (3) presents the messages used in this intervention.

Figure 3: SMS Content for experiment 2

Beliefs in general	Hello Sofia. For more information on HPV vaccination, watch a 2-minute video: https://youtu.be/FZOIYXwwLk0 . Secretary of Health.
Beliefs about vaccination timing	Hello Danna. Did you know that the sooner you vaccinate your daughter against HPV the better? Don't miss out on the benefits of doing it early. Secretary of Health.
Beliefs about severity global	Hello Laura. Did you know that more than 324,000 women worldwide die every year due to HPV? Vaccinate your daughter against HPV. Secretary of Health.

4.3 Experiment 3: Decision aids

In this intervention, we implemented the soft default options and one of the insights from Milkman et al. (2021). For the soft default options, we suggested a time and day for parents to go and vaccinate their daughters. Then, following the methodology of Milkman et al. (2021), the message "an HPV vaccine reserved for your daughter" is used as a reminder of the previous message. Because, as the authors explain, the language makes parents feel that the vaccine belongs to their daughters, and therefore they do not want to miss "their" dose. We carried out two versions of reminders: one 24 hours before the appointment and the other 2 hours before the appointment. In Figure (4) we can see in detail the wording of the text messages based on these dimensions.

Figure 4: SMS Content for experiment 3

Milkman's insight (1SMS)	Hello Valeria. There is an HPV vaccine reserved for your daughter. Secretary of Health.
Soft-default (1 SMS)	Hello Lucila. You have an appointment on June 21 at 10 a.m. to vaccinate your daughter against HPV. Secretary of Health.
Soft-default + Milkman's insight (2 SMS option 1)	**Message 1** (62 hours prior to the appointment): Hello Maria. You have an appointment on June 21 at 10 a.m. to vaccinate your daughter against HPV. Secretary of Health. **Message 2** (24 hours prior to the appointment): Hello Maria. Remember that there is an HPV vaccine reserved for your daughter. Secretary of Health.
Soft-default + Milkman's insight (2 SMS option 2)	**Message 1** (62 hours prior to the appointment): Hello Mariana. You have an appointment on June 21 at 10 a.m. to vaccinate your daughter against HPV. Secretary of Health. **Message 2** (2 hours prior to the appointment): Hello Mariana. Remember that there is an HPV vaccine reserved for your daughter. Secretary of Health.

5 Data

The Health Secretariat of Bogota provided the data for the experiments. One of the advantages of these data is that health providers upload information regarding vaccinations for all citizens. Therefore, we will be able to observe whether the girl was vaccinated or not. However, there are limited pre-treatment covariates. Among these covariates are the health care provider (EPS), if the person belongs to the contributory or subsidized regime, nationality, ethnicity, displacement by civil war, and daughter's age.

Regarding the sample, the first dataset corresponds to parents with unvaccinated daughters against HPV between 9 and 17 years of age. The second dataset corresponds to girls who have only one dose of the vaccine (incomplete schedule). Specifically, part of the excluded sample from an allied research study, "Nudging parents to increase HPV vaccine demand in Bogota, Colombia," was used. The reason for this data exclusion was that this information was required for the stratified randomization and the social norms experiment. However, in the experiments presented above, it is not necessary to have this information. Thus, we took advantage of these samples to conduct other interventions. Finally, the resulting sample for unvaccinated girls is 16,538 parents. At the same time, the sample for girls with incomplete schedules is 5,907 parents. Descriptive statistics for unvaccinated girls are presented in the table (1) and for girls with incomplete immunization schedules are presented in the table (2).

6 Power calculations

We estimated the minimum sample size for each experiment assuming an individual randomized design with an effect size of 4.25 percentage points, 90% power, and a Chi-Square test for difference of proportions at a 5% significance level. Because each experiment has up to 6 treatment arms, we adjusted for multiple testing using a Bonferroni correction for up to 6 comparisons in each experiment. Under these assumptions, the minimum sample size for each

experiment is 5,858 subjects. Figures (6) to (9) in the appendix show that each experiment has a larger sample size than required.

7 Hypotheses

The principal set of hypotheses concerns the impact of behavioral-based text messages on the uptake of HPV vaccination. Specifically, we will evaluate each behavioral treatment's main hypotheses (M). In addition, as we have many behavioral treatments, we will also test a set of general hypotheses (G) and subgroups hypotheses (S). The final set of hypotheses is about heterogeneous effects exploration.

7.1 Hypotheses about altruism and warm-glow

Hypothesis M.1: Receiving a text message based on altruism or warm-glow insights positively affects HPV vaccination take-up.

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

Hypothesis S.2: Receiving a text message based on *any* warm-glow insights (intrinsic or extrinsic) positively affects HPV vaccination take-up.

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

7.2 Hypotheses about beliefs

Hypothesis M.2: Receiving a text message that seeks to correct beliefs regarding HPV positively affects HPV vaccination take-up.

Hypothesis G.2: Receiving *any* text message that seeks to correct beliefs regarding HPV positively affects HPV vaccination take-up.

7.3 Hypotheses about decision aids

Hypothesis M.3: Receiving a text message that relies on decision aids positively affects the uptake of HPV vaccination.

Hypothesis M.4: Receiving both a text message based on default options and one with milkman's insight has a larger positive effect on HPV vaccination take-up than the individual effects of these treatments.

Hypothesis G.3: Receiving a text message based on *any* decision aids positively affects HPV vaccination take-up.

The above hypotheses are based on comparisons with the pure control group, the placebo control group, and the union of both control groups. We also consider an auxiliary hypothesis (A) related to the different control groups in our experimental designs:

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

7.4 Hypotheses about the Heterogeneity of Impacts

Due to the lack of covariates, we will not conduct sophisticated methods to analyze the heterogeneous effects. We will explore the heterogeneity of impacts on if the person belongs to the contributory or subsidized regime, 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 daughters are likely to influence HPV vaccination take-up. We consider this hypothesis for each of the above features for any experiment.

8 Research Design

As described above, we applied three types of experiments and four in total, three for the sample of unvaccinated girls and one for girls with incomplete vaccination schedules. The experimental design for each treatment is presented in figures (6) through (9).

8.1 Outcomes

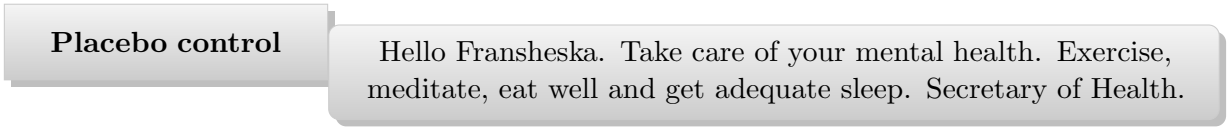
The primary outcome is a categorical variable that indicates whether a parent's daughter is vaccinated against HPV during the SMS campaign window (6 weeks) or during the 6 weeks after the end of the intervention.

8.2 Randomization

The experiment employed a randomized block design. Specifically, participants were divided into blocks according to their location (Bogotá or outside Bogotá) and the girl's age. Since we used age as a block variable, we constructed a binary variable to avoid the "curse of dimensionality ." This dummy variable equals 1 for girls aged 9 and 10 and 0 for the other cases. Participants were subsequently assigned to only one of the above behavioral experiments using the re-randomization algorithm suggested by Morgan and Rubin (2012).

Each experiment incorporates two control groups besides the behavioral treatment groups: the pure control group does not receive any message, and the placebo control receives a message about mental health care and contains the fixed elements. We intend to distinguish between the effect of sending a message and sending a behavioral-based text message with this placebo message. Figure 5 shows the content of the placebo control message.

Figure 5: SMS Content for placebo control



We sent weekly messages to the treatment sample for six weeks. The timeline of the experiment is presented in figure (1). Informed consent was not necessary for this project as it was implemented within the framework of the public policy of vaccination against HPV implemented by the health authorities of the District of Bogotá. In this scenario, the HS of the District of Bogotá already uses a communication campaign through text messages to encourage vaccination. The research team's contribution was to design a communication strategy based on behavioral sciences to improve the strategy already implemented by the city's health authorities. The IRB of the Universidad del Rosario approved this intervention.

8.3 Balance

Tables (3) to (6) report the analysis of the balance of randomization on the limited set of pre-treatment covariates for each experiment. These tables suggest that the randomization of the experiments ensured no difference between treatment arms in terms of observables.

9 Estimation Strategy

This section discusses the methods that will be employed to evaluate the impact of the SMS campaign on HPV vaccination take-up.

9.1 Estimation of Treatment Effects

We will estimate the impacts of interventions using an intention-to-treat (ITT) approach. We will estimate models as follows:

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

Where Y_{ij} is the outcome of interest for girl i from parent j measured six weeks after the end of the SMS campaign. Z_j denotes the treatment status of parent j. θ_s is a vector

of randomization strata dummy variables (location*age). $X_{ij}^{'}$ is a vector of pre-treatment covariates at the girl and parent level, and ε_{ij} is the error term. β_{κ} is our parameter of interest that will capture the ITT effect for each treatment arm.

9.2 Heterogeneous Treatment Effects

We will test the differential impact of the treatments, including the interaction terms of the pre-treatment variables of interest W_j for parent j with the treatment variables. Then we will estimate models of the following form:

$$Y_{ij} = \alpha + \sum_{\kappa=1}^{K} \beta_{\kappa} Z_j + \gamma W_j + \sum_{\kappa=1}^{K} \varphi_{\kappa}(Z_j.W_j) + \theta_s + X_{ij}^{'}\delta + \varepsilon_{ij} \tag{2}$$

Where the coefficients φ_{κ} recover the heterogeneous treatment effects. The pre-treatment covariates of interest are contributory regime, subsidized regime, access to health insurance, nationality, ethnicity, and displacement by civil war. In addition, if we can obtain more features from administrative records. We will apply the generic machine learning strategy proposed by Chernozhukov et al. (2018).

9.3 Addressing Multiple Inference

Multiple hypothesis testing is a concern for our study because we have multiple treatment arms and pre-treatment characteristics to explore heterogeneities. Therefore, we will implement the Benjamini and Hochberg (1995) method to control the false discovery rate.

9.4 Non-compliance

Given the nature of our study, parents cannot change their treatment status. However, the fact that we send a message does not guarantee that it will be read, so ITT estimates will capture the effect of receiving a text message with behavioral components.

9.5 Attrition

Since we work with administrative records, attrition will not be a problem in our study.

However, we could implement Lee (2009) if required.

10 References

Benjamini, Y., & Hochberg, Y. (1995). Controlling the false discovery rate: a practical and powerful approach to multiple testing. *Journal of the Royal statistical society: series B (Methodological)*, *57*(1), 289–300.

Castro, C., Marín, M., & Muñoz, N. (2020). Hpv vaccination in colombia. from a nightmare to a bright and promising dawn. *www.HPVWorld.com*, *105*.

Chernozhukov, V., Demirer, M., Duflo, E., & Fernandez-Val, I. (2018). *Generic machine learning inference on heterogeneous treatment effects in randomized experiments, with an application to immunization in india* (Tech. Rep.). National Bureau of Economic Research.

Lee, D. S. (2009). Training, wages, and sample selection: Estimating sharp bounds on treatment effects. *The Review of Economic Studies*, *76*(3), 1071–1102.

Martínez, M., Estévez, A., Quijada, H., Walteros, D., Tolosa, N., Paredes, A., ... others (2015). Brote de evento de etiología desconocida en el municipio de el carmen de bolívar, bolívar, 2014. *Informe Quincenal Epidemiológico Nacional*, *20*(3-4), 41–77.

Milkman, K. L., Patel, M. S., Gandhi, L., Graci, H. N., Gromet, D. M., Ho, H., ... others (2021). A megastudy of text-based nudges encouraging patients to get vaccinated at an upcoming doctor's appointment. *Proceedings of the National Academy of Sciences*, *118*(20).

Morgan, K. L., & Rubin, D. B. (2012). Rerandomization to improve covariate balance in experiments. *The Annals of Statistics*, *40*(2), 1263–1282.

Nogueira-Rodrigues, A. (2019). Hpv vaccination in latin america: global challenges and feasible solutions. *American Society of Clinical Oncology Educational Book*, *39*, e45–e52.

Phillips, A., Patel, C., Pillsbury, A., Brotherton, J., & Macartney, K. (2018). Safety of human papillomavirus vaccines: an updated review. *Drug safety*, *41*(4), 329–346.

Simas, C., Munoz, N., Arregoces, L., & Larson, H. J. (2019). Hpv vaccine confidence and cases of mass psychogenic illness following immunization in carmen de bolivar, colombia. *Human vaccines & immunotherapeutics*, *15*(1), 163–166.

Sung, H., Ferlay, J., Siegel, R. L., Laversanne, M., Soerjomataram, I., Jemal, A., & Bray, F. (2021). Global cancer statistics 2020: Globocan estimates of incidence and mortality worldwide for 36 cancers in 185 countries. *CA: a cancer journal for clinicians*, *71*(3), 209–249.

Tables

Table 1: Descriptive Statistics for Sample of Unvaccinated Girls

Variables	mean	sd	min	max	N
Girl's age in years	10.61	1.857	9	17	16,538
EPS Sanitas	0.0993	0.299	0	1	16,538
EPS Salud Total	0.0825	0.275	0	1	16,538
EPS Famisanar	0.150	0.357	0	1	16,538
EPS Compensar	0.117	0.321	0	1	16,538
EPS Capital Salud	0.0516	0.221	0	1	16,538
Other EPS	0.500	0.500	0	1	16,538
Contributory Scheme	0.693	0.461	0	1	16,538
Uninsured	0.0951	0.293	0	1	16,538
Subsidized Scheme	0.144	0.351	0	1	16,538
Other scheme	0.0680	0.252	0	1	16,538
Belongs to some ethnic group	0.00955	0.0973	0	1	16,538
Displaced by the armed conflict	0.0207	0.142	0	1	16,538
Colombian nationality	0.983	0.127	0	1	16,538

Table 2: Descriptive Statistics for the Sample of Girls with Incomplete Vaccination Schedules

Variables	mean	sd	min	max	N
Girl's age in years	11.25	2.013	9	17	5,907
EPS Sanitas	0.147	0.354	0	1	5,907
EPS Salud Total	0.101	0.301	0	1	5,907
EPS Famisanar	0.204	0.403	0	1	5,907
EPS Compensar	0.184	0.387	0	1	5,907
EPS Capital Salud	0.0963	0.295	0	1	5,907
Other EPS	0.268	0.443	0	1	5,907
Contributory Scheme	0.813	0.390	0	1	5,907
Uninsured	0.0130	0.113	0	1	5,907
Subsidized Scheme	0.123	0.328	0	1	5,907
Other scheme	0.0508	0.220	0	1	5,907
Belongs to some ethnic group	0.00508	0.0711	0	1	5,907
Displaced by the armed conflict	0.0124	0.110	0	1	5,907
Colombian nationality	0.963	0.189	0	1	5,907

Table 3: Randomization Balance Analysis. Experiment 1: Altruism and warm-glow

Variable	(1) Pure control Mean/SE	(2) Placebo control Mean/SE	(3) Altruism-soft Mean/SE	(4) Altruism-strong Mean/SE	(5) Warm-glow intrinsic Mean/SE	(6) Warm-glow extrinsic Mean/SE	T-test Difference (1)-(2)	(1)-(3)	(1)-(4)	(1)-(5)	(1)-(6)
EPS Sanitas	0.099	0.090	0.105	0.104	0.109	0.100	0.009	-0.006	-0.005	-0.010	-0.001
	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.008)					
EPS Salud Total	0.086	0.083	0.079	0.077	0.090	0.077	0.004	0.008	0.009	-0.003	0.009
	(0.008)	(0.008)	(0.008)	(0.007)	(0.008)	(0.007)					
EPS Famisanar	0.151	0.136	0.151	0.167	0.136	0.151	0.015	0.000	-0.016	0.015	0.000
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)					
EPS Compensar	0.114	0.119	0.123	0.103	0.108	0.119	-0.005	-0.009	0.011	0.006	-0.005
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)					
EPS Capital Salud	0.053	0.057	0.053	0.058	0.045	0.051	-0.003	0.001	-0.005	0.009	0.002
	(0.006)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)					
Other EPS	0.496	0.517	0.489	0.492	0.513	0.502	-0.020	0.007	0.004	-0.017	-0.006
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)					
Contributory Scheme	0.691	0.675	0.700	0.694	0.702	0.698	0.016	-0.009	-0.003	-0.011	-0.007
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)					
Uninsured	0.094	0.103	0.097	0.090	0.089	0.088	-0.009	-0.003	0.004	0.005	0.006
	(0.008)	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)					
Subsidized Scheme	0.148	0.159	0.136	0.150	0.147	0.143	-0.011	0.012	-0.002	0.001	0.005
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)					
Other scheme	0.068	0.063	0.067	0.066	0.062	0.071	0.005	0.001	0.002	0.006	-0.003
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)					
Belongs to some ethnic group	0.008	0.011	0.010	0.005	0.013	0.010	-0.003	-0.002	0.003	-0.006	-0.002
	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)					
Displaced by the armed conflict	0.024	0.026	0.024	0.015	0.017	0.017	-0.002	0.001	0.009*	0.008	0.008
	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)					
Colombian nationality	0.986	0.986	0.984	0.985	0.980	0.985	0.000	0.002	0.001	0.006	0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)					
Girl's age in years	10.586	10.623	10.615	10.570	10.627	10.619	-0.036	-0.028	0.016	-0.040	-0.033
	(0.052)	(0.052)	(0.052)	(0.050)	(0.052)	(0.053)					
N	1272	1272	1272	1273	1272	1272					

Notes: The value displayed for t-tests are the differences in the means across the groups. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table 4: Randomization Balance Analysis. Experiment 2: Beliefs

Variable	(1) Pure control Mean/SE	(2) Placebo control Mean/SE	(3) Beliefs general (video) Mean/SE	(4) Beliefs vaccination timing Mean/SE	(5) Beliefs severity global Mean/SE	T-test Difference (1)-(2)	(1)-(3)	(1)-(4)	(1)-(5)
EPS Sanitas	0.099	0.090	0.096	0.089	0.093	0.009	0.003	0.010	0.006
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)				
EPS Salud Total	0.086	0.083	0.083	0.094	0.080	0.004	0.003	-0.008	0.006
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)				
EPS Famisanar	0.151	0.136	0.155	0.150	0.145	0.015	-0.004	0.001	0.006
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)				
EPS Compensar	0.114	0.119	0.113	0.108	0.132	-0.005	0.001	0.006	-0.018
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)				
EPS Capital Salud	0.053	0.057	0.047	0.053	0.050	-0.003	0.006	0.001	0.004
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)				
Other EPS	0.496	0.517	0.506	0.506	0.501	-0.020	-0.009	-0.010	-0.005
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)				
Contributory Scheme	0.691	0.675	0.691	0.672	0.692	0.016	0.000	0.019	-0.001
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)				
Uninsured	0.094	0.103	0.095	0.100	0.101	-0.009	-0.002	-0.006	-0.007
	(0.008)	(0.009)	(0.008)	(0.008)	(0.008)				
Subsidized Scheme	0.148	0.159	0.134	0.150	0.145	-0.011	0.013	-0.002	0.002
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)				
Other scheme	0.068	0.063	0.079	0.078	0.062	0.005	-0.012	-0.010	0.006
	(0.007)	(0.007)	(0.008)	(0.008)	(0.007)				
Belongs to some ethnic group	0.008	0.011	0.009	0.009	0.010	-0.003	-0.001	-0.001	-0.002
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)				
Displaced by the armed conflict	0.024	0.026	0.017	0.022	0.021	-0.002	0.008	0.002	0.003
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)				
Colombian nationality	0.986	0.986	0.987	0.980	0.981	0.000	-0.001	0.006	0.005
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)				
Girl's age in years	10.586	10.623	10.512	10.639	10.583	-0.036	0.075	-0.053	0.003
	(0.052)	(0.052)	(0.049)	(0.054)	(0.051)				
N	1272	1272	1272	1273	1272				

Notes: The value displayed for t-tests are the differences in the means across the groups. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table 5: Randomization Balance Analysis. Experiment 3: Decision aids (unvaccinated girls)

	(1)	(2)	(3)	(4)	(5)	(6)			T-test Difference		
Variable	Pure control Mean/SE	Placebo control Mean/SE	Milkman's insight Mean/SE	Soft-default Mean/SE	Version 1 Mean/SE	Version 2 Mean/SE	(1)-(2)	(1)-(3)	(1)-(4)	(1)-(5)	(1)-(6)
EPS Sanitas	0.099	0.090	0.105	0.106	0.099	0.097	0.009	-0.006	-0.007	0.000	0.002
	(0.008)	(0.008)	(0.009)	(0.009)	(0.008)	(0.008)					
EPS Salud Total	0.086	0.083	0.090	0.068	0.083	0.083	0.004	-0.003	0.019*	0.004	0.003
	(0.008)	(0.008)	(0.008)	(0.007)	(0.008)	(0.008)					
EPS Famisanar	0.151	0.136	0.138	0.153	0.160	0.160	0.015	0.012	-0.002	-0.009	-0.009
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)					
EPS Compensar	0.114	0.119	0.112	0.126	0.119	0.123	-0.005	0.002	-0.012	-0.006	-0.009
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)					
EPS Capital Salud	0.053	0.057	0.055	0.058	0.050	0.042	-0.003	-0.002	-0.005	0.004	0.012
	(0.006)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)					
Other EPS	0.496	0.517	0.500	0.489	0.490	0.495	-0.020	-0.004	0.007	0.006	0.001
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)					
Contributory Scheme	0.691	0.675	0.700	0.690	0.697	0.702	0.016	-0.009	0.001	-0.006	-0.011
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)					
Uninsured	0.094	0.103	0.098	0.086	0.098	0.099	-0.009	-0.004	0.007	-0.005	-0.006
	(0.008)	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)					
Subsidized Scheme	0.148	0.159	0.139	0.159	0.138	0.125	-0.011	0.009	-0.011	0.009	0.023*
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.009)					
Other scheme	0.068	0.063	0.063	0.064	0.067	0.074	0.005	0.005	0.003	0.001	-0.006
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)					
Belongs to some ethnic group	0.008	0.011	0.008	0.011	0.013	0.007	-0.003	-0.000	-0.003	-0.006	0.001
	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)					
Displaced by the armed conflict	0.024	0.026	0.022	0.021	0.026	0.018	-0.002	0.002	0.003	-0.002	0.006
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)					
Colombian nationality	0.986	0.986	0.981	0.978	0.983	0.989	0.000	0.005	0.008	0.002	-0.003
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)					
Girl's age in years	10.586	10.623	10.582	10.639	10.659	10.621	-0.036	0.004	-0.052	-0.072	-0.035
	(0.052)	(0.052)	(0.050)	(0.053)	(0.055)	(0.053)					
N	1272	1272	1271	1273	1272	1272					

Notes: The value displayed for t-tests are the differences in the means across the groups. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table 6: Randomization Balance Analysis. Experiment 3: Decision aids (girls with incomplete vaccination schedules)

Variable	(1) Pure Control Mean/SE	(2) Placebo Control Mean/SE	(3) Milkman's insight Mean/SE	(4) Soft-default Mean/SE	(5) Version 1 Mean/SE	(6) Version 2 Mean/SE	T-test Difference (1)-(2)	(1)-(3)	(1)-(4)	(1)-(5)	(1)-(6)
EPS Sanitas	0.144	0.131	0.145	0.152	0.153	0.158	0.013	-0.001	-0.008	-0.009	-0.013
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.012)					
EPS Salud Total	0.092	0.105	0.100	0.098	0.108	0.104	-0.012	-0.007	-0.006	-0.015	-0.011
	(0.009)	(0.010)	(0.010)	(0.009)	(0.010)	(0.010)					
EPS Famisanar	0.216	0.202	0.215	0.194	0.203	0.193	0.014	0.002	0.023	0.013	0.023
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)					
EPS Compensar	0.180	0.183	0.195	0.185	0.172	0.188	-0.003	-0.016	-0.005	0.008	-0.008
	(0.012)	(0.012)	(0.013)	(0.012)	(0.012)	(0.012)					
EPS Capital Salud	0.095	0.101	0.083	0.102	0.102	0.095	-0.005	0.012	-0.007	-0.006	0.001
	(0.009)	(0.010)	(0.009)	(0.010)	(0.010)	(0.009)					
Other EPS	0.272	0.279	0.261	0.269	0.262	0.263	-0.007	0.011	0.003	0.010	0.009
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)					
Contributory Scheme	0.827	0.801	0.815	0.810	0.814	0.812	0.026	0.013	0.017	0.013	0.015
	(0.012)	(0.013)	(0.012)	(0.012)	(0.012)	(0.012)					
Uninsured	0.012	0.012	0.015	0.015	0.011	0.012	0.000	-0.003	-0.003	0.001	-0.000
	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)					
Subsidized Scheme	0.112	0.131	0.115	0.129	0.126	0.125	-0.019	-0.003	-0.017	-0.014	-0.013
	(0.010)	(0.011)	(0.010)	(0.011)	(0.011)	(0.011)					
Other scheme	0.049	0.056	0.055	0.046	0.049	0.051	-0.007	-0.006	0.003	-0.000	-0.002
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)					
Belongs to some ethnic group	0.002	0.005	0.006	0.007	0.003	0.007	-0.003	-0.004	-0.005*	-0.001	-0.005*
	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)					
Displaced by the armed conflict	0.012	0.010	0.015	0.010	0.008	0.018	0.002	-0.003	0.002	0.004	-0.006
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)					
Colombian nationality	0.969	0.959	0.957	0.963	0.966	0.961	0.009	0.011	0.005	0.002	0.007
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)					
Girl's age in years	11.237	11.278	11.243	11.217	11.275	11.264	-0.042	-0.007	0.020	-0.039	-0.028
	(0.064)	(0.065)	(0.063)	(0.063)	(0.065)	(0.065)					
N	985	985	983	986	984	984					

Notes: The value displayed for t-tests are the differences in the means across the groups. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Figures

Figure 6: Experimental Design for experiment 1: Altruism and warm-glow

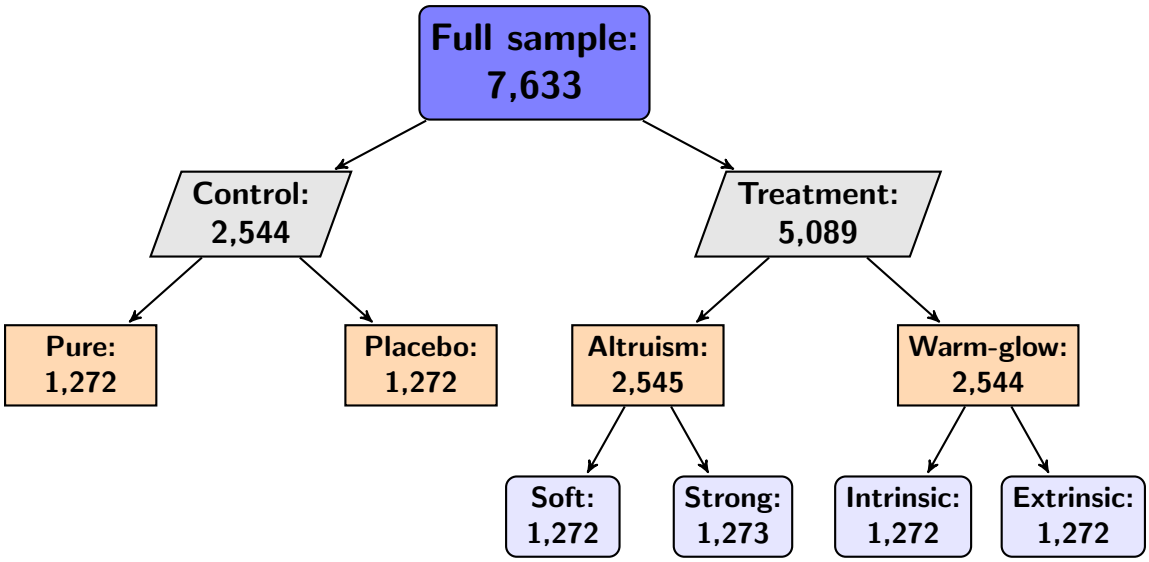


Figure 7: Experimental Design experiment for experiment 2: Beliefs

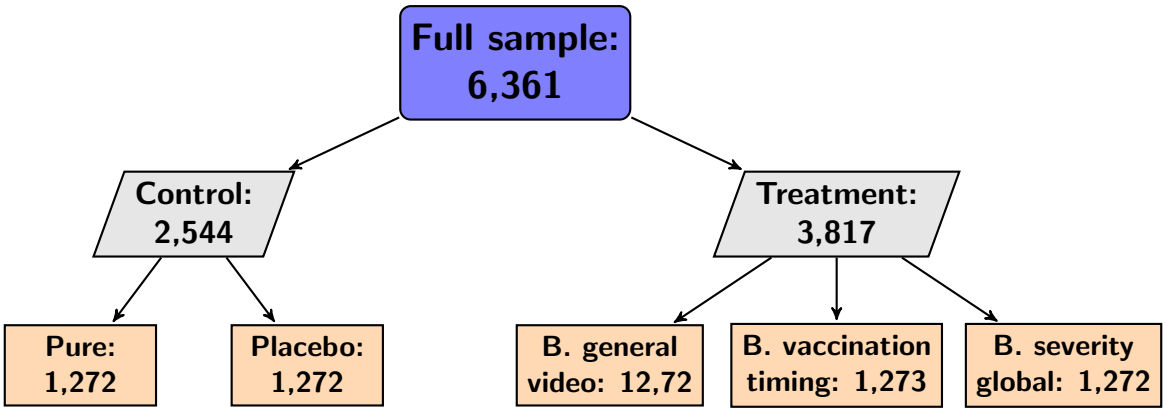


Figure 8: Experimental Design for experiment 3: Decision aids (unvaccinated girls)

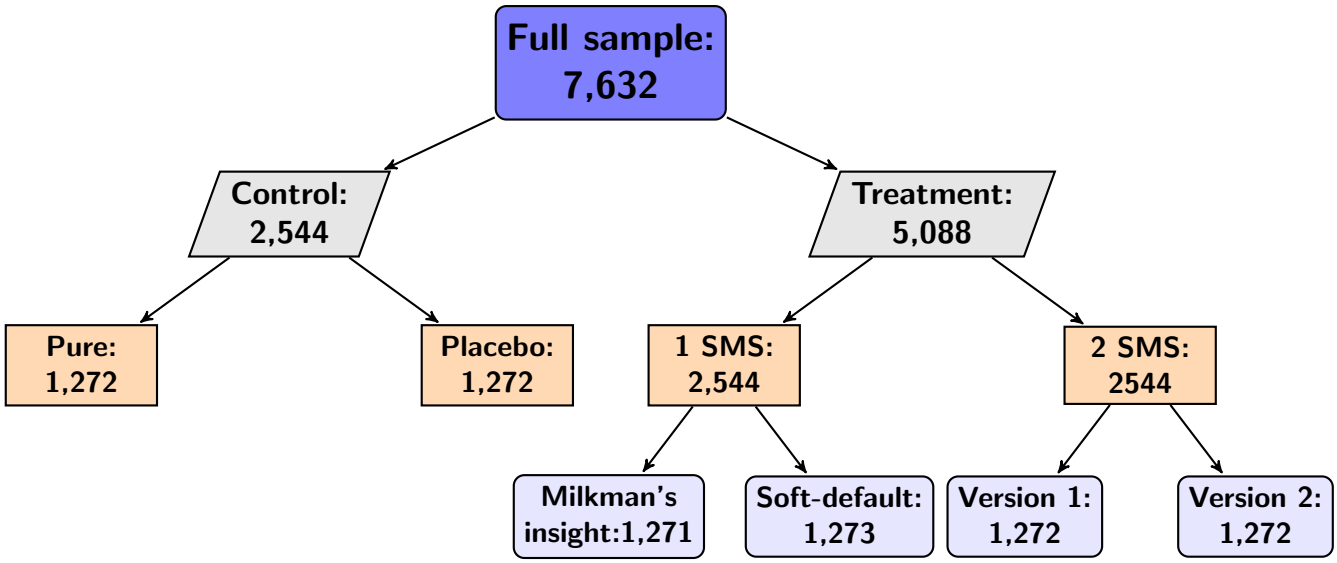


Figure 9: Experimental Design for experiment 4: Decision aids (incomplete schedules)

