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

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Abstract

Failure to complete vaccination schedules represents an important form of health underinvestment in developing economies. We study this problem in the context of HPV vaccination in Colombia, where many girls initiate but do not complete the schedule. Messages framing a vaccine as reserved for the recipient can increase uptake, but they may be less effective when the relevant barrier is planning rather than entitlement. We test this hypothesis in a randomized controlled trial with parents of girls located inside and outside Bogota who had initiated but not completed HPV vaccination. The experiment compares ownership reminders with time-specific planning prompts: messages suggesting a specific day and time for vaccination without assigning an appointment or changing the formal choice set. The planning-prompt treatment and the combined treatments increase second-dose completion by 7.94 to 9.10 percentage points, relative to 8.63 percent in the control group, while the ownership reminder increases completion by 4.58 percentage points. This ranking is consistent with planning and implementation frictions being more binding than entitlement frictions among marginal households, although the experiment does not directly measure these mechanisms.

Keywords: Planning prompts; ownership reminders; health investments.
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1 Introduction

Many valuable health investments in developing countries require more than initial adoption. Individuals may begin preventive treatment without returning for a follow-up visit, or receive a first vaccine dose without completing the schedule.[1] Initial take-up reveals that individuals attach some value to the investment, making noncompletion difficult to explain by lack of demand alone. Noncompletion may instead reflect the need to remember, plan, and act after a delay (Ajzen, 1991; Gollwitzer, 1999). We study whether helping individuals translate an intention to invest into concrete action reduces these completion failures in the context of vaccination (Webb and Sheeran, 2006; Gollwitzer and Sheeran, 2006).

Vaccination provides a useful setting to study this problem. Vaccines generate large private and social returns and have prevented millions of deaths in low- and middle-income countries (Li et al., 2021).[2] Yet incomplete schedules remain common. The literature extensively examines reasons for non-vaccination, including beliefs about safety, adverse events, disease severity, and disease risk (Azarpanah et al., 2021; Brewer et al., 2007), but less is known about increasing completion after initiation. Completion differs from initial adoption: parents who start a schedule may still fail to complete it because of procrastination, limited prospective memory, overconfidence in memory capacity, or the cognitive costs of translating intentions into concrete action (Nuscheler and Roeder, 2016; McDaniel and Einstein, 2007; Ericson, 2011; Tasoff and Letzler, 2014). Incomplete vaccination thus exemplifies the intention-action gap.[3]

This margin may be particularly consequential in developing economies, where poverty

[1] These investments require follow-through after initiation: childhood vaccination schedules involve repeated visits (Banerjee et al., 2010), while tuberculosis treatment requires sustained adherence (Alipanah et al., 2018).

[2] Li et al. (2021) estimate that vaccination against ten pathogens in 98 low- and middle-income countries prevented 37 million deaths in 2000–2019 and could avert 32 million more by 2030.

[3] Reviews of the intention–behavior gap show that intentions imperfectly predict subsequent behavior and that interventions that increase intentions often produce smaller behavioral changes (Webb and Sheeran, 2006). Research on implementation intentions shows that specifying when, where, and how to act can help translate intentions into behavior (Gollwitzer and Sheeran, 2006), including preventive health behaviors such as influenza vaccination (Milkman et al., 2011).

compounds cognitive demands with larger material and institutional costs. Scarcity can divert attention from future tasks toward urgent needs (Shah et al., 2012).[4] Poor households also face frequent consequential decisions with fewer tools to automate or simplify routine actions (Bertrand et al., 2006; Schilbach et al., 2016). Consistent with these constraints, full childhood immunization is concentrated among wealthier households in 46 low- and middle-income countries (Hajizadeh, 2018).

A growing literature shows that light-touch reminders can reduce noncompletion when individuals intend to act but fail to follow through. In vaccination, Milkman et al. (2021) and Dai et al. (2021) show that messages suggesting a vaccine has been reserved for the recipient can increase uptake. Beyond providing information, these ownership reminders may create a sense of entitlement over the vaccine, making non-vaccination feel like failing to claim something already assigned. Consistent with a large literature in behavioral economics on reference dependence and loss aversion (Kahneman and Tversky, 1979; O'Donoghue and Sprenger, 2018), they can shift the reference point from whether to obtain the vaccine to whether to give up a reserved opportunity.[5]

However, ownership reminders may leave an implementation problem unresolved: a parent told that a vaccine is reserved for her daughter must still decide when to go, coordinate the visit, and act. This matters when noncompletion reflects difficulty translating general intentions into concrete plans more than opposition to vaccination. A planning prompt instead makes a specific course of action focal without changing the formal choice set. In our setting, this message suggests a specific vaccination day and time without assigning an appointment, restricting alternatives, or changing what happens under inaction. Their

[4]In complementary field evidence, Mani et al. (2013) find that financial concerns reduce cognitive performance among low-income but not affluent participants; the same Indian farmers perform worse before than after harvest.

[5]This interpretation follows Milkman et al. (2021), whose best-performing flu-vaccination text reminders used "reserved for you" language, and Buttenheim et al. (2022), who describe this framing as combining default expectations, reciprocity, and psychological ownership. In COVID-19 vaccination, Dai et al. (2021) use "claim your dose" to induce psychological ownership, while Patel et al. (2022) interpret reservation messages through loss aversion.

relative effectiveness in increasing completion is therefore an empirical question with direct implications for designing follow-through interventions.

We study this question in a randomized controlled trial among parents of girls in and outside Bogotá, Colombia, who had received the first dose of the human papillomavirus (HPV) vaccine but not completed the schedule. HPV vaccination is an important preventive health investment because HPV is causally associated with cervical cancer. Colombia offers a relevant setting: initially high coverage under the national campaign fell sharply after a widely publicized health episode in Carmen de Bolivar, linked to the vaccine in public debate (Martínez et al., 2015). HPV vaccination consequently became a contested investment, while completion remained low.[6] Focusing on parents of first-dose recipients allows us to study completion among households that had revealed some willingness to vaccinate.

Parents were randomly assigned to six mutually exclusive conditions: pure control, placebo message, ownership reminder, planning prompt, and two treatments combining the planning prompt with an ownership reminder 24 or two hours before the suggested vaccination time. The pure control identifies each complete communication package's effect relative to no SMS; the placebo tests whether contact from the health authority on an unrelated health topic changes HPV completion. The ownership and planning arms most directly compare the two behavioral approaches. Comparing planning alone with the combined packages tests gains from adding ownership language; comparing the combined arms isolates whether reminder timing matters.

These contrasts motivate a simple conceptual framework: a parent completes the schedule when its perceived net value outweighs transportation, time, hassle, and planning costs. Messages may affect this decision by changing salient reference points. An ownership reminder may create an entitlement reference point, making noncompletion feel like failing to claim something assigned to the daughter. A planning prompt may instead create a plan

[6]First-dose coverage fell from 97.5 percent in 2013 (Nogueira-Rodrigues, 2019) to 14 percent in 2016, when second-dose coverage was 9 percent (Simas et al., 2019). Online Appendix Figure A-1 presents the national series through 2022.

reference point, making a specific day and time focal and reducing the cognitive cost of deciding when to act. Their relative effectiveness is theoretically ambiguous. Combining them need not yield additive effects if they operate on overlapping margins, the suggested time is inconvenient, or additional messages create burden or confusion.

The experiment's scale and administrative records let us evaluate realized vaccination rather than stated intentions. The study includes 5,907 randomized parent records, each linked to one eligible daughter. Vaccination providers report completed doses to Bogota's administrative vaccination registry (PAIWEB), allowing us to observe second-dose completion for the full randomized sample and exact vaccination dates for dynamic analysis. This avoids conventional survey attrition and lets us examine whether effects emerge during the campaign and persist after message delivery. We registered the experiment and its pre-analysis plan before requesting or analyzing final outcome data.[7]

Arm-specific estimates show that both approaches increase completion, with larger effects from planning prompts. Relative to a pure-control completion rate of 8.63 percent, ownership reminders and planning prompts increase second-dose completion by 4.58 and 8.67 percentage points, respectively; a direct test rejects equality. The combined treatments increase completion by 7.94 points with ownership reminders 24 hours before the suggested time and 9.10 points with reminders two hours before. These effects are statistically indistinguishable from the effect of planning alone, indicating no detectable gain from adding ownership language.[8]

These results remain stable across alternative specifications, comparison groups, multiple-

[7]The experiment is registered with the AEA registry (AEARCTR-0009705). The pre-analysis plan is reproduced in Online Appendix B.

[8]Ownership and planning raise completion by about 53 and 100 percent over our pure-control mean. In U.S. influenza vaccination, Milkman et al. (2011) find a 4.2-percentage-point gain from date-and-time planning prompts over a standard reminder mailing; the best reservation messages yield 4.6 and 2.9 points in primary-care and pharmacy megastudies, respectively (Milkman et al., 2021, 2022). In Guatemala, Busso et al. (2015) report age-appropriate vaccination completion gains of 2.2 points overall and 4.6 among children due for vaccination. In a Bogotá experiment, Maldonado et al. (2026) find that dynamic social norms increase first-dose HPV vaccination by about 1.28 points, or 23 percent. Our planning estimate is large by these benchmarks; the ownership estimate is close to the larger reminder effects. Vaccines, baseline rates, follow-up periods, and control conditions differ; we study second-dose completion among families that had already initiated vaccination.

testing corrections, and randomization inference. Treatment-control gaps persist after message delivery, indicating increased cumulative completion rather than merely shifting vaccination to the suggested date. Larger effects among younger girls and families inside Bogotá were not accompanied by pre-specified directional hypotheses. Planning costs an estimated USD 0.38 per additional completion, compared with USD 0.72 for ownership.[9] These results are consistent with planning and implementation frictions being more binding than entitlement frictions among marginal parents, although the experiment does not directly measure the underlying psychological processes.

We make three contributions. First, we contribute to the growing literature applying behavioral insights to vaccination decisions (Betsch et al., 2015; Brewer et al., 2017). Studies have increased vaccination through more reliable access and incentives (Banerjee et al., 2010), reminders (Milkman et al., 2021, 2022), and implementation-intention prompts (Milkman et al., 2011). Although some examine full immunization schedules (Banerjee et al., 2010; Gibson et al., 2017), much evidence concerns whether eligible individuals receive a vaccine. We instead study whether parents whose daughters had received a first HPV dose complete the schedule after a delay. Initial vaccination reveals some willingness to invest, allowing us to focus on follow-through after adoption. We thus extend the behavioral-vaccination literature from initial take-up to completion, a central margin in multi-stage health investments and especially relevant when intentions must be remembered, planned, and executed over time (Webb and Sheeran, 2006; Gollwitzer and Sheeran, 2006).

Second, we connect two literatures developed largely in parallel. Research on implementation intentions and planning prompts shows that linking goals to concrete future occasions

[9]In a social-norm experiment, Maldonado et al. (2026) report USD 5.53 per additional first HPV dose and USD 3.93 for the more effective norm designs; our planning estimate is about one tenth of the latter. Other studies report USD 7.50 per additional child completing age-appropriate vaccination in Guatemala (Busso et al., 2015) and USD 7.90 per additional return visit for childhood vaccination in Nigeria (Kawakatsu et al., 2020). For social-media campaigns, Athey et al. (2023) estimate USD 5.68 per additional COVID-19 vaccination by linking experimental belief changes to observational vaccination rates. These comparisons use reported prices and differ in outcomes, cost coverage, and price years. Our ratios cover direct SMS and personnel costs within existing infrastructure, excluding vaccine provision and household costs, rather than full social costs per additional vaccination.

can improve follow-through (Gollwitzer, 1999; Gollwitzer and Sheeran, 2006; Milkman et al., 2011; Rogers et al., 2015). Complementary evidence shows that reservation language can increase vaccination by making a dose feel allocated to the recipient (Milkman et al., 2021, 2022; Dai et al., 2021; Buttenheim et al., 2022; Patel et al., 2022). Both approaches can be effective, but evidence comparing them within the same experiment, population, and outcome is limited. Beyond testing another reminder, we compare two leading behavioral approaches to completion and assess whether their effects are additive.

Third, we contribute to experimental research on digital vaccination outreach through public systems. Text messages and reminder letters can increase HPV vaccination and completion of multi-dose schedules (Rand et al., 2017; Tull et al., 2019; Chao et al., 2015). Embedding the intervention in a local health authority's existing infrastructure also connects the study to large-scale field experimentation and behavioral interventions in public administration (Banerjee et al., 2025; Muralidharan and Niehaus, 2017; Benartzi et al., 2017). Cost-effectiveness estimates translate these effects into direct implementation costs. These features show how administrative data and low-cost messaging can support comparison and refinement of behavioral designs within an operational vaccination system, beyond sending reminders.

The rest of the paper is organized as follows. Sections 2–5 present the conceptual framework, institutional background, research design, and empirical model, respectively. Section 6 reports the main results, including dynamic effects, treatment-effect heterogeneity, and robustness to multiple-inference adjustments. Section 7 presents the cost-effectiveness analysis, and Section 8 concludes.

2 Conceptual framework

We use a simple framework to compare ownership reminders and planning prompts. The framework draws on models of reference-dependent preferences (Kőszegi and Rabin, 2006), limited attention and reminders (Karlan et al., 2016), and self-regulation through goal

setting (Koch and Nafziger, 2011). We adapt these elements to the decision to complete the HPV vaccination schedule and clarify why the relative effectiveness of the two messages is theoretically ambiguous.

In our framework, a parent decides whether to complete her daughter's HPV vaccination schedule. Let $x_i \in \{0, 1\}$ denote completion, 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 $\theta_i = B_i - C_i$, where B_i is the parent's perceived value of completing the HPV schedule and C_i is the full cost of taking the daughter to be vaccinated. This cost includes transportation and time costs, but also hassle costs, uncertainty about when to go, and the cognitive cost of converting a general intention into a concrete action.[10] In a benchmark without reference-dependent utility or message effects, the parent completes the schedule whenever $\theta_i \geq 0$.

The interventions may affect completion by changing the way parents evaluate action and inaction. An ownership reminder can create an entitlement reference point: the parent may perceive the vaccine as something assigned to her daughter and may evaluate noncompletion as failing to claim a reserved good.[11] A planning-prompt message can create a plan reference point: a specific day and time becomes the focal or expected moment for vaccination.[12] This message does not create an administrative default because no appointment is formally scheduled, parents remain free to vaccinate at another time, and inaction still results in

[10]Models of task completion distinguish the costs of carrying out an activity from its subsequent rewards (O'Donoghue and Rabin, 1999). Reis (2006) explicitly models the resource costs of acquiring and processing information and determining a plan of action. These contributions motivate treating the cost of vaccination as broader than its monetary price.

[11]Reference-dependent models allow a change in the reference point to alter the valuation of otherwise unchanged outcomes (Tversky and Kahneman, 1991). In expectations-based formulations, failing to obtain an anticipated good can be experienced as a loss even if the good was not previously owned (Kőszegi and Rabin, 2006). In our setting, this interpretation requires the parent to adopt the perceived allocation as a reference point; it does not require a physical transfer of the dose or a change in her vaccination options.

[12]Koch and Nafziger (2011) model goals as reference points that motivate effort by making shortfalls psychologically costly. Their model concerns goals chosen by an individual facing self-control problems. We draw on the reference-point role of goals to interpret the suggested vaccination time: if the parent adopts the proposed plan as a benchmark, failing to complete the schedule may carry a psychological cost in addition to the forgone health benefit.

non-vaccination. Instead, it makes one course of action psychologically salient.

Formally, let t denote the experimental condition. The treatment can affect five objects. First, $s_i(t)$ captures the salience effect of receiving a message from the health authority, including institutional contact, personalization, and attention drawn to the vaccination decision. We allow $s_i(t)$ to vary across message conditions; equal salience is an additional assumption used in the propositions.[13] Second, ownership language may strengthen the entitlement reference point, $e(t)$, which indexes the extent to which the dose is perceived as assigned to the daughter. Third, the suggested day and time may strengthen the plan reference point, $p(t)$, which indexes the extent to which that time serves as a benchmark for action.[14]

The remaining two objects capture costs associated with acting and with the message itself. A planning-prompt message may reduce planning and implementation costs, but an inconvenient suggested time may also create mismatch costs if it disrupts an alternative plan.[15] Let $\omega_i a(t)$ denote this net implementation effect, where $a(t) = 1$ for treatments that include a suggested day and time and $a(t) = 0$ otherwise. The parameter ω_i can be positive, zero, or negative. Finally, let $\kappa_i(t) \geq 0$ denote an additional cost of completing the schedule generated by confusion, fatigue, or complexity from combining messages. We set this term to zero outside the combined treatments as a simplifying restriction.[16]

[13]Karlan et al. (2016) model how reminders can mitigate undersaving caused by limited attention to future expenses. We draw on this attention channel and summarize its effect through an additive term, separately from reference-dependent utility and implementation costs.

[14]We specify these indices as functions of the assigned message, without imposing the consistency between expectations and choices required by Kőszegi and Rabin (2006). O'Donoghue and Sprenger (2018) review alternative approaches to reference-point formation. Appendix A allows initial levels e_0 and p_0, which need not be zero, and represents ownership and planning content as nonnegative shifts δ_E and δ_P in the corresponding indices.

[15]In the model of Reis (2006), forming and updating plans require resources. This motivates allowing a suggested vaccination time to save parents the effort of choosing when to act. Because parents remain free to attend at another time, an inconvenient suggestion need not raise implementation costs if the parent ignores it and retains any existing arrangements. A negative ω_i requires an additional response, such as displacing a feasible arrangement and leaving the parent to coordinate a new visit.

[16]In the utility specification in Appendix A, $\kappa_i(t)$ lowers the payoff from completing the schedule. A nuisance from receiving an SMS that affects completion and noncompletion equally would cancel from the comparison and would not change the action threshold. We use this term to represent that possible burden separately from the net implementation effect of the suggested time, ω_i.

The parent has reference-dependent preferences over the entitlement and plan dimensions. In this binary framework, completing the schedule entails no reference loss; we abstract from deviations from the suggested vaccination time. If she does not complete the schedule, she falls short of the reference points created by the message. Let $L_i(e, p)$ denote this reference-dependent loss, with $L_{i,e}(e, p) \geq 0$ and $L_{i,p}(e, p) \geq 0$, and let $\lambda_i > 0$ be the weight placed on such losses.[17] The parent completes the schedule under treatment t if

$$\theta_i + \Gamma_i(t) \geq 0, \tag{1}$$

where

$$\Gamma_i(t) = s_i(t) + \omega_i a(t) - \kappa_i(t) + \lambda_i L_i(e(t), p(t)). \tag{2}$$

Equation (1) shows that the interventions affect completion by shifting marginal parents across the action threshold. Parents with very high values of θ_i complete the schedule even without a message, while parents with very low values of θ_i do not complete even after receiving one. Messages can change completion decisions by shifting attention, reference points, or implementation costs among parents close enough to the action threshold. The experiment studies this response among parents whose daughters had initiated but not completed the HPV vaccination schedule.[18]

The model clarifies the interpretation of the experimental comparisons. The placebo message provides a benchmark for the common features of receiving a personalized message from the Health Secretariat. The ownership reminder adds an entitlement reference point by stating that the vaccine is reserved for the daughter. The planning-prompt treatment adds a plan reference point by suggesting a specific day and time for vaccination and may

[17] Our binary formulation retains the loss component of reference-dependent preferences (Tversky and Kahneman, 1991; Kőszegi and Rabin, 2006). We impose $L_i(0, 0) = 0$ so that noncompletion generates no reference loss when neither reference is present; the nonnegative partial derivatives mean that strengthening either reference cannot reduce this loss.

[18] For a treatment t that raises the completion incentive relative to the pure control C, a parent switches to completion when $-\Gamma_i(t) \leq \theta_i < -\Gamma_i(C)$. Being marginal therefore depends on both the initial decision margin and the shift induced by the message.

reduce the cost of deciding when to act. The combined treatments activate both reference points, but their effect on completion need not equal the sum of the effects of ownership and planning alone.[19]

The ranking between ownership reminders and planning prompts is therefore theoretically ambiguous. Ownership reminders can be more effective if noncompletion reflects weak entitlement or if parents respond strongly to the possibility of losing a reserved good. Planning prompts can be more effective when parents intend to complete the schedule but fail to choose and execute a specific moment for doing so. The empirical ranking identifies the relative effectiveness of the implemented messages. A larger planning effect is consistent with planning and implementation frictions, but may also reflect the plan reference point or differences in salience. A larger ownership effect is consistent with a stronger response to perceived entitlement. Because the experiment does not measure these psychological processes or how strongly each message activates them, the ranking cannot establish which friction is more binding. Appendix A presents the full derivation and proofs.

3 Background

Cervical cancer remains one of the most important preventable causes of mortality among women in developing countries. In 2020, approximately 342,000 women died from cervical cancer, and close to 90% of these deaths occurred in low- and middle-income countries (Sung et al., 2021). Persistent infection with the human papillomavirus (HPV) is the main cause of cervical cancer, explaining more than 95% of cervical cancer cases (WHO, 2022). The availability of HPV vaccines therefore created an extraordinary opportunity to prevent a large share of this disease burden. However, the returns to this preventive investment depend on completing the vaccination schedule. This distinction is central for our study. Low vaccination

[19]Proposition A5 in Appendix A decomposes shifts in individual incentives. Reference-point interactions and message-burden costs can make these shifts nonadditive. Even when the shifts are additive, completion effects need not be: each message may move some of the same parents across the threshold, while the combination may induce others to act who would not respond to either message alone. Aggregate effects therefore depend on the distribution of parents around the threshold as well as on the incentive shifts.

coverage can reflect low initial demand, but incomplete schedules among girls who already received a first dose reveal a different problem: households may start a valuable preventive investment and still fail to complete it.

Colombia provides a useful setting to study this margin. At the time of the experiment in 2022, HPV vaccination was available free of charge to girls aged 9–17 through the Expanded Program on Immunization (PAI).[20] The country initially achieved unusually high levels of HPV vaccination after introducing a national school-based campaign in 2012. According to Nogueira-Rodrigues (2019), Colombia reached a first-dose HPV vaccination coverage rate of 97.5% in 2013, the second-highest rate worldwide after Australia. The school-based model was important because it reduced the costs of acting for households. Parents did not need to search for a vaccination point, choose a date, coordinate transportation, or remember a future visit in the same way they would under a facility-based model. In this sense, the original design of the campaign compressed many of the implementation frictions that usually prevent preventive health investments from being completed.

This trajectory changed after the events in Carmen de Bolívar in 2014. Around 500 girls who had recently received the HPV vaccine reported symptoms such as headaches, fainting, and numbness of extremities. Some families and anti-vaccine groups attributed these events to the HPV vaccine, generating a public controversy around the program. Subsequent epidemiological work by the Colombian Ministry of Health and Social Protection and the National Institute of Health found that it was not plausible to associate HPV vaccination with the events in Carmen de Bolívar (Martínez et al., 2015). This conclusion is consistent with the international evidence.[21] Nevertheless, the episode changed the environment in which HPV vaccination decisions were made. The vaccine became more contested, the school-based

[20]During the 2022 campaign, the schedule consisted of two doses six months apart, and the Health Secretariat invited families to attend vaccination sites without an appointment (Barreto, 2022). National guidance also provided for vaccination at public and private sites regardless of insurance affiliation (Ministerio de Salud y Protección Social de Colombia, 2015). Free access therefore did not eliminate the time and coordination required for a return visit.

[21]For example, Phillips et al. (2018) review 109 studies and conclude that HPV vaccines have an acceptable safety profile.

delivery strategy was disrupted, and a subsequent Constitutional Court ruling reinforced the requirement for informed consent.[22]

These institutional features matter for our research question because completion involves both authorization and follow-through by households. Completing the HPV vaccination schedule therefore required parents to pay attention to the pending dose, decide when to go, coordinate the visit, and execute the action after some delay. This is precisely the type of setting in which completion failures may arise even when households are not opposed to vaccination. A parent may value the vaccine and still postpone the visit, forget the relevant date, fail to coordinate the logistics, or never transform a general intention into a specific plan.

The decline in HPV vaccination coverage after Carmen de Bolívar was substantial. By 2016, coverage had fallen to 14% for the first dose and 5% for the complete course (Simas et al., 2019). Although different institutions have attempted to restore confidence and increase uptake, progress has been uneven.[23] Despite these efforts, national coverage remained low. In 2019, first-dose coverage was 34.1% nationally, while second-dose coverage was only 11.1%. The situation was even more pronounced in Bogotá, where first-dose coverage was 25.7% and second-dose coverage was only 6% (Ministerio de Salud y Protección Social de Colombia, 2020). These figures indicate that the second-dose margin is not a minor administrative problem. It is a central component of the low-protection equilibrium that emerged after the collapse of the original campaign.

Our experiment focuses on this completion margin. The target population consists of parents whose daughters had already received the first dose of the HPV vaccine but had not

[22]Vorsters et al. (2020) describe the retreat from school-based HPV vaccination after the Carmen de Bolívar crisis and identify restoring school delivery as a policy priority. In Judgment T-365/17, the Constitutional Court reaffirmed that HPV vaccination is voluntary and required prior informed consent from recipients and, for minors, their parents or legal representatives (Corte Constitucional de Colombia, 2017). The ruling established a procedural requirement; it did not estimate its effect on vaccination coverage.

[23]For example, the Arauca initiative led by the Liga Colombiana Contra el Cáncer engaged girls, parents, teachers, healthcare providers, and local media through tailored information materials, talks, and training for health personnel. Reported first-dose coverage rose from about 5% in 2017 to 41% in 2018 and 83% in 2019 (Castro et al., 2020).

completed the schedule. This feature matters for interpretation. These households are not equivalent to households that never initiated vaccination. By accepting the first dose, they revealed some willingness to vaccinate, which makes noncompletion more consistent with follow-through frictions than with complete rejection of the vaccine. This setting therefore allows us to study whether light-touch messages can help households complete a preventive health investment that has already been initiated.

The comparison between ownership reminders and planning prompts is motivated by this context. Ownership reminders may increase completion if parents respond to the idea that the vaccine has been reserved for their daughter and that not returning implies failing to claim an assigned good. Planning prompts may increase completion if the relevant barrier is instead the absence of a concrete plan. By making a specific day and time focal, these messages can help parents transform a general intention to vaccinate into an actionable step. In this sense, Bogotá and the surrounding areas provide a natural setting to test whether second-dose completion is more responsive to entitlement-based reminders or to interventions that reduce planning and implementation frictions.

4 Research design

4.1 The intervention

The intervention was developed with the Bogotá Health Secretariat, the Liga Colombiana Contra el Cáncer, the Inter-American Development Bank, the American Cancer Society, and the Universidad del Rosario. It targeted parents or guardians of girls who had received a first HPV vaccine dose but had not completed the schedule. This population had revealed some demand for vaccination, but completing the investment still required a subsequent action. We used SMS because the Health Secretariat already relied on this channel and could link its communication system to administrative immunization records. The intervention therefore changed the content of an existing public-sector tool rather than creating a parallel delivery system.

The campaign ran for six weeks, from May 4 to June 11, 2022. Messages were delivered in weekly cycles, and parents stopped receiving them once the administrative records showed that their daughters had completed the schedule.[24] All message conditions shared two features: the SMS was personalized with the recipient's first name and identified the Health Secretariat as the sender. These elements held personalization and the institutional source constant while the experimental arms varied the behavioral content. Figure 1 summarizes the design, implementation, and administrative follow-up sequence.

The experiment included six conditions: a pure control, a placebo message, an ownership reminder, a planning prompt, and two treatments combining the planning prompt with ownership language. Table 1 summarizes the conditions, and Figure A-2 reproduces the messages. Parents in the pure control received no SMS. Those assigned to the placebo received a personalized message from the Health Secretariat with general recommendations for protecting their mental health, but no reference to HPV, vaccination, or their daughter's incomplete schedule. The placebo thus retained institutional contact and personalization without drawing attention to the vaccination decision.

The ownership treatment told the parent that "there is an HPV vaccine reserved for your daughter." The wording adapted the reservation message studied by Milkman et al. (2021) to a decision made by a parent on behalf of a child. It framed a vaccine dose as assigned to the daughter, while leaving the parent responsible for deciding when and where to complete the schedule. The treatment consequently combined a reminder about the pending dose with language intended to create a sense of entitlement over the vaccine.

The planning treatment used appointment language to suggest a specific Saturday at 10 a.m. for vaccination. Saturday was selected because vaccination sites were operating and parents were expected to face fewer work-related constraints. We describe this treatment as

[24]Each weekly cycle comprised one SMS in the placebo, ownership-only, and planning-only arms and two SMS in each combined arm. Over six cycles, these schedules implied a maximum of six and twelve messages per parent, respectively. The realized number could be lower because messaging stopped after completion appeared in the administrative records.

a planning prompt because it translated an open-ended intention into a concrete day and time while leaving parents free to decide when and where to vaccinate.[25]

The two combined treatments first delivered the planning prompt and then followed it with the ownership reminder. The second message arrived either 24 hours or two hours before the suggested vaccination time. Because the content and number of messages were the same across these two arms, their comparison isolates whether bringing the ownership cue closer to the intended action changes the response. Comparisons between a combined arm and the planning-only arm have a different interpretation, since they vary both ownership content and the number of contacts.

4.2 Estimands, reminder effects, and comparison groups

The six-arm design supports several estimands with different substantive meanings. Our principal benchmark is the pure control. Comparisons with this group identify the intention-to-treat effect of assigning each complete communication package relative to receiving no SMS. The resulting estimates include the behavioral content of the active message as well as any response to being contacted, receiving a personalized communication from the Health Secretariat, and having the pending HPV dose made salient. This estimand is the most relevant for a health authority deciding whether to add the message package to its existing vaccination activities.

The placebo provides a second counterfactual. Its comparison with the pure control tests whether a personalized Health Secretariat message about an unrelated health topic changes HPV completion. Comparisons between an active treatment and the placebo instead recover the incremental effect of an HPV-specific communication relative to institutional contact without HPV content. The latter contrast does not isolate ownership or planning from the effect of reminding parents about HPV and their daughter's incomplete schedule, because the

[25]The SMS used the word "appointment," but the implementing institutions did not schedule individual appointments or reserve slots at particular facilities. Vaccination remained free, and parents could attend any participating site at another time. Thus, the message made a suggested time salient without changing access or the formal choice set; inaction still resulted in non-vaccination.

placebo is not a generic HPV reminder. We therefore use the placebo to assess contact and common message features rather than as the sole benchmark for the behavioral mechanisms.

The PAP also specified a pooled comparison group that combines the pure and placebo controls. Pooling improves precision by increasing the size of the benchmark, but it merges two counterfactuals with different interpretations. We consequently report pooled-control estimates as robustness checks. At the aggregate level, we first estimate the effect of assignment to any of the four active messages relative to the pure control. We then use the arm-specific estimates to determine whether this average masks differences across ownership, planning, and combined communication strategies.

The ownership-only and planning-only arms provide the cleanest comparison of the two active message packages. Both groups received one personalized HPV message from the same institutional sender, so their difference identifies the relative effectiveness of the implemented contents. It does not directly identify the psychological process responsible for that difference. Similarly, comparing the planning-only arm with either combined arm assesses whether adding the ownership reminder produces further gains, but the contrast varies both behavioral content and the number of contacts. Only the comparison between the two combined arms holds content and contact frequency constant while changing the timing of the ownership reminder.

The PAP pre-specified positive effects for the individual messages and for assignment to any active message. It also anticipated complementarity between planning and ownership and specified comparisons using the pure, placebo, and pooled controls. The directional ranking between planning prompts and ownership reminders was not pre-specified. We treat that ranking as a theory-guided contrast supported by the original experimental design, while distinguishing it from the PAP's confirmatory hypotheses.

4.3 Experiment

The unit of random assignment was the parent or guardian contact associated with an eligible girl. The final sample contains 5,907 randomized parent records, each linked to one eligible

daughter and assigned to one of the six mutually exclusive conditions reported in Table 1 and Figure 3. Throughout the paper, we refer to parents as the experimental participants because the messages were directed to them and they made the vaccination decision; completion outcomes are measured for their daughters. Assignment was divided almost equally across the arms: 985 parents entered the pure control, 985 the placebo, 983 the ownership arm, 986 the planning arm, and 984 each of the two combined arms. In total, 1,970 parents were assigned to a control condition and 3,937 to an active message.

We implemented a blocked randomized design using the cross-product of baseline location and the girl's age group. Location distinguishes girls inside Bogotá from those outside the city, while the age indicator separates girls ages 9–10 from those ages 11–17. These variables were available before treatment and were expected to predict vaccination behavior. Within the four strata, assignment followed the rerandomization procedure proposed by Morgan and Rubin (2012). Candidate allocations were evaluated using the main health-insurer indicators and health-system regimes before the final assignment was selected.[26] Section 4.5 assesses balance using the broader set of administrative covariates available for the experimental sample.

The experiment and analysis plan were registered in the AEA RCT Registry before the final outcome data were requested and analyzed. The intervention was implemented within the Health Secretariat's regular public-health communication activities, and the institutional review board at Universidad del Rosario approved the protocol under study DVO005 1663-CV1287. The research team handled parent and vaccination records under the confidentiality protections applicable to the administrative data.[27]

[26]Blocking and rerandomization serve related but distinct purposes. Crossing the two location categories with a binary age group avoids fragmenting the sample into many age-specific cells. Rerandomization then uses additional baseline information to improve balance within this design (Morgan and Rubin, 2012). The assignment script evaluated indicators for Sanitas, Salud Total, Famisanar, Compensar, and Capital Salud, together with contributory-regime, subsidized-regime, and uninsured status. These are the covariates used to select the allocation; Section 4.5 assesses balance on the broader set of observed baseline characteristics.

[27]The PAP also described proposed experiments among parents of unvaccinated girls. Institutional restrictions prevented their implementation. The present paper examines only the experiment conducted among parents of girls with incomplete vaccination schedules.

Treatment assignment does not ensure that the registered telephone number was active or that the recipient read the SMS. The delivery system does not allow us to establish exposure at the individual level, so all estimates are interpreted as intention-to-treat effects of assignment to a message. Unobserved delivery or reading failures may attenuate the estimates. They do not threaten the internal validity of the randomized comparisons unless the probability of exposure differs systematically across assigned conditions for reasons induced by the treatment.

Administrative records allow us to observe the outcome for the full randomized sample without relying on a follow-up survey.[28] This feature limits conventional attrition and avoids self-reported vaccination measures. At the same time, the outcome captures only doses reported through the administrative information system. The estimates should therefore be interpreted as effects on recorded completion, which is the outcome available to the implementing health authority.

4.4 Data and sample

The study combines randomized assignments with administrative records from the Bogotá Health Secretariat. Vaccination providers report HPV doses to PAIWEB, the information system of Colombia's Expanded Program on Immunization. These records indicate whether an eligible girl received the second dose and provide the corresponding date. The administrative data also contain the baseline variables used in the analysis: the girl's age and location, health insurer, health-system regime, nationality, recorded ethnicity, and displacement status. All were measured before assignment and are defined in Appendix Table A-1. Although the intervention was implemented with the Bogotá Health Secretariat, the resulting records include girls located both inside and outside Bogotá.

The target population consisted of parents of girls ages 9–17 who had received one

[28] In their review of COVID-19 vaccination interventions, Batteux et al. (2022) found that 32 of 39 studies measured intentions and seven measured behavior, including vaccination rates or scheduled appointments. Our administrative outcome records completion of the second HPV dose.

HPV vaccine dose but had not completed the schedule. Eligibility also required enough information to identify a parent or guardian and a telephone number for delivering the SMS. We constructed the sampling frame from Health Secretariat records that had not been included in an earlier program of HPV vaccination experiments. These records had been excluded because of geographic restrictions, missing locality information, or an insufficient interval between doses when the earlier frame was assembled.[29]

Figure 2 traces the construction of the sample. The original administrative frame contained 100,465 girls with incomplete schedules. Of these, 7,412 records excluded from the earlier program for the reasons described above were reconsidered for the present experiment. We then removed 1,464 girls who completed the schedule before implementation, 29 who had turned 18, and 12 records with duplicate or invalid telephone numbers. These exclusions yielded the final sample of 5,907 parent records.

The primary outcome equals one when PAIWEB records completion of the second HPV dose during the analysis window. The dataset contains 819 completions among the 5,907 randomized observations and retains the exact vaccination date for the dynamic analysis. We measure completion from April 25 through July 30, 2022. The window begins shortly before the first SMS on May 4 and extends seven weeks beyond the final delivery on June 11, allowing us to examine both pre-campaign differences and the persistence of the treatment effects. The administrative records provide one additional week of follow-up beyond the period specified in the PAP. We use the full window to take advantage of all available outcome data; excluding the additional week leaves the magnitude, ranking, and statistical interpretation of the central treatment estimates unchanged.

Administrative measurement provides broad outcome coverage and avoids relying on self-reports, but it also defines the boundaries of the analysis. The sample excludes records

[29]The earlier program evaluated several communication strategies among families of unvaccinated girls and girls with incomplete schedules. For the present experiment, the records reconsidered included girls outside Bogotá, girls in Sumapaz, records without locality information, and girls who had not yet satisfied the six-month interval between doses. We updated eligibility before implementation.

without usable parent or telephone information, and PAIWEB contains few socioeconomic or attitudinal characteristics. We cannot directly observe household income, parental education, vaccine beliefs, or planning constraints. These restrictions limit external-validity and mechanism analyses, although they do not alter the internal validity of treatment comparisons within the randomized sample.

4.5 Summary statistics and pre-treatment balance

Table 2 describes the 5,907 girls in the experimental sample. Their average age was 11.25 years, 46.1% were ages 9–10, and 62.8% were located inside Bogotá. Age and location also defined the randomization strata. Most girls were enrolled in the contributory health-insurance scheme (81.3%), while 12.3% belonged to the subsidized scheme, 1.3% were recorded as uninsured, and 5.1% belonged to another regime. The sample was predominantly Colombian (96.3%); 0.5% were recorded as belonging to an ethnic group and 1.2% as displaced by the armed conflict.

The small shares of uninsured girls, ethnic-group members, and displaced girls limit the precision of subgroup estimates along these dimensions. More generally, the administrative records do not contain household income, parental education, vaccine beliefs, or direct measures of planning constraints. The descriptive statistics therefore characterize the dimensions observed by the health authority, but they cannot establish how the experimental sample compares with eligible households along unmeasured socioeconomic characteristics or attitudes.

The randomization produced comparable groups on the available baseline variables. We calculate signed standardized differences between each non-pure-control arm and the pure control. All absolute differences are below 0.1, and the largest is approximately 0.075.[30] Table 2 and Figure 4 provide the complete results. Appendix Table A-2 and Appendix Figure A-3 repeat the exercise using the placebo as the benchmark; the differences again remain

[30]As a point of reference, Imbens (2015) describes normalized differences of up to 0.3 as modest in his analysis of experimental data. Our covariates satisfy the more stringent balance criterion of 0.1.

below 0.1 in absolute value.

These checks support the successful implementation of the blocked rerandomization procedure, and the conclusion does not depend on whether the pure or placebo control serves as the reference group. They cover every pre-treatment characteristic available in the administrative records. As in any randomized design, balance in observed covariates does not demonstrate balance in each unobserved characteristic, but random assignment provides the basis for causal inference within the experimental sample.

5 Empirical model

5.1 Basic model

We estimate intention-to-treat effects using linear probability models.[31] The outcome, y_{ij}, equals one if PAIWEB indicates that vaccinee i, linked to parent j, completed the second HPV vaccine dose during the analysis window and zero otherwise. Random assignment identifies the causal effect of assignment to each SMS condition within the experimental sample. The estimand is not the effect of reading a message, since individual exposure is unobserved, but the policy-relevant effect of assigning the communication package through the Health Secretariat's delivery system.

Our arm-specific specification is

$$y_{ij} = \alpha + \lambda P_j + \sum_{k=1}^{K} \beta_k Z_j^k + \theta_S + X_{ij}'\delta + \varepsilon_{ij}. \tag{3}$$

In Equation (3), P_j indicates assignment of parent j to the placebo condition, while Z_j^k contains indicators for the four active treatments: the ownership reminder, the planning prompt, and the two combined arms with different reminder timings. The term θ_S represents fixed effects for the randomization strata defined by location and age group, while X_{ij} contains

[31] Our outcome is binary, so differences in mean completion rates correspond to differences in vaccination probabilities. The linear specification expresses treatment contrasts directly on this scale and facilitates tests comparing the experimental arms. See Angrist (2001) for a discussion of linear models for estimating causal effects with limited dependent variables.

pre-treatment administrative covariates for vaccinee i and parent j.

The pure control is the omitted category. The coefficient λ therefore captures the effect of the placebo message relative to receiving no SMS, while each β_k measures the effect of assignment to active treatment k relative to the pure control. We estimate the model on the original 0–1 outcome scale and multiply the coefficients by 100 when reporting percentage-point effects. All specifications include the four randomization-strata fixed effects. The preferred model additionally controls for age in years; indicators for the main health insurers and health-system regimes; and indicators for ethnicity, displacement status, and Colombian nationality. These controls can improve precision, but identification follows from random assignment rather than covariate adjustment.[32]

Inference uses Eicker–Huber–White heteroskedasticity-robust standard errors. Assignment occurred at the parent-contact level, and each randomized parent record contributes one eligible girl. Household clustering therefore offers no meaningful additional correction under the effective individual-level structure of the data (Abadie et al., 2023). The dynamic specifications described below are different because each girl contributes repeated weekly observations; for those models, we cluster standard errors at the girl level.

We first summarize the experiment with an aggregate specification. This model replaces the four active-arm indicators with A_j, an indicator for assignment to any active message, while retaining a separate indicator for the placebo. The coefficient on A_j estimates the average effect of the active-message family relative to the pure control. Arm-specific estimates then show whether this average conceals differences across ownership, planning, and combined packages.

We next reparameterize Equation (3) to map the experimental arms into the conceptual framework. The specification retains the placebo indicator and replaces the four active-treatment indicators with the ownership cue, the planning prompt, their interaction, and the

[32]We report both the strata-only and covariate-adjusted estimates so that the contribution of the adjustment set remains transparent.

contrast between the two-hour and 24-hour reminders. The indicator for assignment to any active message is not included because it is exactly collinear with the ownership, planning, and interaction terms. This parameterization allows us to compare τ_O with τ_D and to assess whether the combined treatment satisfies $\tau_{OD} = \tau_O + \tau_D$. We use linear contrasts to test equality between the ownership and planning coefficients, whether the interaction is zero, and whether reminder timing matters. We also test equality across all active arms and across the three arms containing a planning prompt.[33]

The dynamic analysis examines when the treatment effects emerge and whether they persist after message delivery. We reshape the data into a stacked girl-week panel covering the 14 calendar weeks ending April 30 through July 30, 2022. The weekly outcome indicates whether the girl had completed the second dose by the end of each week. We estimate arm-specific effects relative to the pure control using treatment-by-week interactions, week fixed effects, randomization-strata fixed effects, and the preferred baseline covariates. Standard errors are clustered at the girl level to account for repeated observations.

Kaplan–Meier-style curves provide a descriptive complement to the weekly regressions. Every observation shares the same administrative end date, and the data contain no differential censoring. The curves should consequently be read as cumulative vaccination rates by date rather than as estimates of a separate duration-model parameter. Their role is to show the timing and persistence of completion, not to change the causal estimand defined by assignment to treatment.

5.2 Robustness checks

We assess whether the main findings depend on the covariate set or estimator. The strata-only specification relies on the randomized design, whereas the preferred model adds all available

[33]The ownership-by-planning coefficient measures the departure from additivity for the combined arm with a 24-hour reminder. The two-hour reminder coefficient captures the difference between the two combined arms, so the departure from additivity for the two-hour arm is the sum of these coefficients. Because the combined arms include a second contact, the interaction should not be interpreted as a pure psychological interaction between ownership and planning.

baseline administrative controls. Similar estimates across these models would indicate that the results are not driven by adjustment choices. Because the PAP did not fully determine which baseline variables should enter the adjusted model, we also implement double-selection LASSO (Belloni et al., 2014). Treatment indicators and randomization-strata fixed effects remain variables of interest, while the procedure selects predictive controls from the same administrative covariate set.[34]

We assess the sensitivity of the arm-specific estimates to the contamination bias described by Goldsmith-Pinkham et al. (2024).[35] We also change the omitted comparison group, re-estimating the models relative to the placebo and to the pooled pure-plus-placebo control. These specifications implement the estimands described in Section 4.2 while retaining the same strata fixed effects, baseline controls, and robust standard errors.

Multiple treatment arms create a second inferential concern. Following the PAP, we apply the Benjamini–Hochberg false-discovery-rate procedure (Benjamini and Hochberg, 1995) to the five contrasts comparing the placebo and the four active treatments with the pure control. Appendix Table A-4 reports Benjamini–Hochberg and two-stage sharpened false-discovery-rate q-values for the pure-control, placebo-control, pooled-control, and mechanism-contrast families.[36] We complement this correction with stratified randomization inference.[37]

[34]The procedure selects controls through separate LASSO regressions for the vaccination outcome and each variable of interest. The final outcome regression includes the union of the controls selected in these regressions. A covariate can therefore enter the adjustment set through its predictive relevance for either the outcome or the variables of interest.

[35]With multiple treatments and heterogeneous effects, a covariate-adjusted regression coefficient can include contributions from other treatments' effects. We estimate the own-treatment effect component (OWN) by subtracting an estimate of those contributions from the partly linear coefficient. This retains the weighted component associated with the treatment of interest. Comparing OWN with the conventional estimates assesses the sensitivity of our results to contamination bias.

[36]False discovery rate control limits the expected proportion of false rejections among rejected hypotheses, whereas familywise error control limits the probability of at least one false rejection. This less stringent criterion can improve power when evaluating several potentially effective message strategies. For each procedure, a reported q-value is the smallest FDR level at which a hypothesis is rejected within its testing family. The two-stage sharpened q-values use the adaptive procedure of Benjamini et al. (2006), which estimates the number of true null hypotheses, following the implementation described by Anderson (2008).

[37]The sharp null states that each girl's vaccination outcome is the same under all experimental conditions. Under this hypothesis, observed outcomes remain fixed when treatment labels are reassigned. The permutation p-value measures how often the resulting statistic is at least as extreme as the observed one. This provides a complementary assessment of statistical significance to conventional inference based on robust standard

Treatment assignments are permuted 10,000 times within the original strata, and two-sided p-values compare the absolute robust t-statistic under the observed allocation with its permutation distribution. Figure 7 reports these distributions and the observed estimates.

We examine treatment-effect heterogeneity using conventional subgroup interactions and machine-learning methods. The conventional analysis excludes the placebo and compares the four active arms with the pure control. For a pre-treatment characteristic W_j, we estimate

$$y_{ij} = \alpha + \sum_{k=1}^{K} \beta_k Z_j^k + \gamma W_j + \sum_{k=1}^{K} \phi_k \left(Z_j^k \cdot W_j \right) + \theta_S + X_{ij}'\delta + \varepsilon_{ij}, \tag{4}$$

where ϕ_k captures how the intention-to-treat effect of active treatment k varies with W_j. We first interact each characteristic with an indicator for any active treatment and then estimate arm-specific interactions. The models otherwise retain the strata fixed effects, baseline controls, and robust standard errors used in the preferred ITT specification.

The subgroup dimensions are age 9–10, residence inside Bogotá, subsidized or uninsured status, Colombian nationality, and displacement by armed conflict. The PAP anticipated heterogeneity by health regime, insurance access, nationality, ethnicity, and displacement. Age and location defined the randomization strata but were not accompanied by directional heterogeneity hypotheses. We therefore interpret their interactions as additional design-based analyses rather than confirmatory tests of pre-specified directional predictions. Ethnicity does not appear in the main heterogeneity table because only 0.5% of the sample is recorded as belonging to an ethnic group. Estimates for nationality and displacement also require caution because the smaller side of each comparison contains few observations.[38]

The GenericML framework of Chernozhukov et al. (2025) provides a more flexible assessment of treatment-effect heterogeneity. This analysis compares assignment to any active message with the pure control and excludes the placebo. The best linear predictor (BLP)

errors.

[38]These limitations concern the precision and interpretation of subgroup estimates, not identification of the average treatment effects in the full randomized sample.

assesses whether the predicted treatment-effect score captures variation in treatment effects. Group average treatment effects (GATES) summarize effects across five groups ranked by predicted gains, while classification analysis (CLAN) compares the baseline characteristics of the groups with the lowest and highest predicted gains.[39]

All estimates rely on the standard no-interference assumption. The administrative records do not contain communication networks among parents, so the study cannot measure whether recipients shared message content with control households. Such diffusion would reduce the difference in exposure between experimental conditions and would generally attenuate the estimated intention-to-treat contrasts. The available data do not allow us to test this possibility directly.

6 Results

6.1 Main results for the aggregate message treatment

Assignment to any of the four active messages increased second-dose completion by 7.57 percentage points relative to the pure control group, with a standard error of 1.06 percentage points. This effect is large relative to the pure-control completion rate of 8.63 percent: it represents an increase of approximately 88 percent over the control mean. The estimated gain is also large relative to average effects reported in other vaccination messaging experiments.[40] Panel A of Table 4 reports this aggregate estimate. Its magnitude is remarkably stable across the alternative specifications described in Section 5, ranging from 7.53 to 7.57 percentage

[39]For each split, one half of the sample is used to train predictions from baseline characteristics, and the other half is used to estimate BLP, GATES, and CLAN. This separation limits overfitting in the assessment of treatment-effect heterogeneity. We use 100 sample splits and four learners: random forest, cross-validated elastic net, support vector machine, and XGBoost. Repeating the split reduces dependence on any single partition. The PAP contemplated GenericML if sufficient baseline information became available, but it did not specify the final learners, number of splits, or reporting choices. We therefore treat the implemented analysis as structured supplementary evidence rather than a fully specified confirmatory test.

[40]As a point of reference, Maldonado et al. (2026) estimate that social-norm messages increase first-dose HPV vaccination by 0.907 percentage points, or 16.3 percent relative to a control mean of 5.56 percent. Average increases in influenza vaccination are 1.8 percentage points in Patel et al. (2023) and 2.0 points in Milkman et al. (2022), while Busso et al. (2015) report a 2.2-point increase in age-appropriate childhood vaccination in Guatemala. The studies differ in vaccines, baseline uptake, control conditions, and follow-up periods; our estimate concerns second-dose completion among families that had already initiated vaccination.

points as we add pre-treatment covariates or use double-selection LASSO.

The placebo message increased completion by 1.43 percentage points relative to the pure control, but the estimate is not statistically different from zero. Thus, the data provide no evidence of a contact-only effect comparable in magnitude to the effects of the active messages, although the confidence interval does not rule out a smaller response.[41] The aggregate effect nevertheless masks substantial variation across message content. We next examine the arm-specific estimates to compare ownership reminders with planning prompts.

6.2 The effects of ownership reminders versus planning prompts

The arm-specific estimates show that both behavioral approaches increased second-dose completion, but planning prompts produced a larger response. Table 3 reports the intention-to-treat effects for each experimental arm. In the preferred covariate-adjusted specification, the ownership reminder increased completion by 4.58 percentage points, while the planning prompt increased it by 8.67 percentage points.[42] Both estimates are statistically significant at the one-percent level. Relative to the 8.63-percent pure-control mean, the ownership reminder increased completion by approximately 53 percent, whereas the planning prompt approximately doubled it. A direct test rejects equality between the two treatment effects ($p = 0.010$).

Combining the planning prompt with an ownership reminder did not generate larger effects. The combined treatment increased completion by 7.94 percentage points when the ownership reminder arrived 24 hours before the suggested vaccination time and by 9.10 percentage points when it arrived two hours before. These estimates are close to the effect of

[41] Because the placebo message did not mention HPV vaccination, this comparison does not isolate a generic vaccination-salience effect. It instead captures the effect of contact with the health authority through a message unrelated to HPV vaccination.

[42] Our planning estimate of 8.67 percentage points is about twice the 4.2-point gain from date-and-time prompts in Milkman et al. (2011), where the comparison group received a standard reminder mailing. The 4.58-point ownership estimate is close to the 4.6-point gain from the best reservation-message package in the primary-care megastudy of Milkman et al. (2021) and above the corresponding 2.9-point gain in the pharmacy megastudy of Milkman et al. (2022). Using the full observation period of the primary-care experiment, Patel et al. (2023) report a 3.1-point gain for the best reservation package.

the planning prompt alone. Indeed, the null hypothesis that the three treatments containing a planning prompt have equal effects cannot be rejected ($p = 0.778$). The evidence therefore indicates that neither adding ownership language nor changing the reminder timing produced a detectable improvement over the planning prompt.

The magnitudes and ranking of these estimates remain stable across the alternative specifications described in Section 5, including the OWN estimates. The larger effects of the treatments containing a planning prompt are consistent with planning and implementation frictions being more binding than entitlement frictions among parents at the margin of completing the vaccination schedule. This interpretation is necessarily bounded by the experimental design. The treatment contrasts identify differences between the implemented message packages, but the experiment does not directly measure the psychological processes activated by either message.

6.3 Disentangling the effects of ownership reminders and planning prompts

Panel B of Table 4 uses the factorial structure of the experiment to separate the ownership and planning components. The specification includes an ownership cue, a planning prompt, their interaction, and an indicator distinguishing the two-hour from the 24-hour reminder. This parameterization maps the experimental arms into the entitlement and plan-reference components of the conceptual framework. The estimated ownership coefficient is 4.58 percentage points, while the planning coefficient is 8.67 percentage points. Their statistically significant difference of 4.09 percentage points confirms that the planning-prompt package generated a larger response than the ownership-reminder package. This comparison is consistent with the planning content adding value, although it does not isolate the underlying psychological channel from other differences between the implemented messages.

The interaction between the ownership cue and the planning prompt is negative and statistically significant. In the preferred specification, the interaction is -5.30 percentage points, with a standard error of 2.15 percentage points and a p-value of 0.014. The observed

effect of the combined 24-hour treatment is therefore approximately equal to the sum of the ownership and planning coefficients net of this negative interaction. Put differently, combining the two components did not produce additive gains.[43]

Reminder timing does not explain the absence of additional gains. Moving the ownership reminder from 24 hours to two hours before the suggested vaccination time increased completion by 1.16 percentage points, but the estimate is imprecise and not statistically different from zero ($p = 0.484$). The data therefore provide no evidence that bringing the ownership cue closer to the intended action improves completion. Taken together, these results suggest that the planning prompt accounts for most of the effect of the combined message packages.

6.4 Dynamics of the treatment effects

The treatment effects emerged during the SMS campaign and accumulated as parents had opportunities to complete the second dose. Figure 5 traces cumulative effects over the 14-week analysis window. Before the first SMS delivery, the estimated differences between each active treatment and the pure control are close to zero. The effects become visible during the campaign and increase over the subsequent weeks. Appendix Figure A-4 presents the corresponding trajectories relative to the placebo group, while Appendix Figure A-5 reports each active treatment separately relative to the pure control.

The dynamic estimates reproduce the ranking observed in the main intention-to-treat results. The ownership-reminder effect rises gradually and reaches approximately 4–5 percentage points by the end of the follow-up period. The planning-prompt effect grows more rapidly and reaches approximately 8–9 percentage points. The two combined-treatment series follow a similar trajectory and remain close to one another throughout the analysis window. This pattern reinforces the finding that adding an ownership reminder to the planning prompt did not produce a detectable additional gain.

[43]The interaction should be interpreted as a departure from additivity within the implemented message bundles. Because the combined treatments added a second contact as well as ownership language, the design does not identify a pure psychological interaction between entitlement and planning reference points.

The treatment-control gaps persist through July 30, several weeks after the final SMS delivery. This persistence indicates that the messages did not merely shift vaccination to the suggested date; they increased cumulative completion over the administrative follow-up period. The dynamic ranking also mirrors the arm-specific estimates in Table 3, with larger effects for every treatment containing a planning prompt.

Figure 6 provides complementary evidence using cumulative completion curves. The planning-prompt and combined-treatment curves separate most clearly from the pure control, while the ownership-reminder curve lies between them. By the end of the window, the ordering of the curves is consistent with the arm-specific estimates in Table 3. Appendix Figure A-6 shows the same qualitative ordering when the placebo group or the pooled control group serves as the benchmark.[44]

6.5 Additional results

The main findings are robust to alternative inference procedures. Table 5 reports Benjamini–Hochberg q-values for the five comparisons against the pure control. All four active-treatment effects remain significant ($q \leq 0.001$), while the placebo effect remains insignificant ($q = 0.274$). Stratified randomization inference preserves this pattern, with $p \leq 0.001$ for the active treatments and $p = 0.285$ for the placebo. The ownership–planning difference and their interaction also remain significant at the five-percent level after the Benjamini–Hochberg adjustment within the six-hypothesis family in Panel D of Appendix Table A-4 ($q = 0.015$ and $q = 0.017$, respectively). The correction therefore preserves both conclusions: planning has the larger effect, and the combined treatment produces less than additive gains.

Appendix Table A-3 further shows that the active-treatment effects remain positive and statistically significant when the placebo group or the pooled pure-plus-placebo group serves as the comparison category. These comparisons establish that the active message packages

[44]Because all observations share the same administrative end date and there is no differential censoring, the Kaplan–Meier-style curves are equivalent to descriptive cumulative vaccination rates by date. They show that the active messages accelerated completion and increased its final level, but they do not identify a separate duration-model parameter.

generated effects beyond those of the placebo message. However, they do not identify which behavioral mechanism produced the difference.

The average effect of the active messages varies by age and place of residence. Table 6 reports an effect of 12.43 percentage points for girls aged 9–10, compared with 3.46 percentage points for older girls. The difference between the two groups is 8.97 percentage points. Similarly, the estimated effect is 10.13 percentage points for families residing inside Bogotá and 3.30 percentage points for those outside the city, a difference of 6.83 percentage points. Both differences are statistically significant at the one-percent level. Joint tests also reject the null that the four arm-specific interactions are zero for each dimension ($p < 0.001$). Appendix Figure A-7 summarizes the interaction estimates and their confidence intervals across the reported subgroup dimensions.

The data provide less evidence of systematic heterogeneity by subsidized or uninsured status or Colombian nationality. The pooled treatment effect is 10.50 percentage points larger among displaced girls; this difference is statistically significant (standard error 5.57). These estimates should be interpreted cautiously. The relevant comparison cells for nationality and displacement are small, and only 63 observations are recorded as displaced. The resulting imprecision means that the absence of statistically significant differences by insurance status and nationality cannot be interpreted as evidence of homogeneous effects along these dimensions.[45]

The GenericML analysis provides complementary evidence on treatment-effect heterogeneity. The best linear predictor in Appendix Figure A-8 yields a positive heterogeneity coefficient ($\beta_2 = 0.717$, $p = 0.0141$), indicating that the predicted-gain score captures systematic variation in treatment effects. Figure 8 reports effects of 1.20 and 14.56 percentage points for the lowest and highest predicted-gain quintiles, respectively. Although the group-specific confidence intervals are wide, the estimated difference between the highest and lowest quintiles

[45]Age and location defined the randomization strata, but the available PAP text did not state directional heterogeneity hypotheses for these dimensions. We therefore interpret the corresponding estimates as design-based subgroup evidence rather than as confirmatory tests of pre-specified directional hypotheses.

is positive and statistically significant (13.88 percentage points; $p = 0.0137$).[46]

The classification analysis in Appendix Figure A-9 and Appendix Table A-5 shows that the group with the highest predicted gains includes younger girls (9.55 versus 13.92 years) and a larger share of families residing inside Bogotá (99.80 versus 6.04 percent). These profiles are consistent with the subgroup patterns reported above. Appendix Figure A-10 presents the alternative GATES-style implementation used as a robustness and quality-control exercise.

These machine-learning results should be viewed as structured supplementary evidence. The PAP did not fully specify the final implementation, and the administrative covariates do not include direct measures of household resources, vaccine beliefs, or planning constraints. The analysis can therefore document systematic variation in predicted treatment gains but cannot provide a complete account of the mechanisms behind that variation. This limitation is particularly relevant when interpreting the characteristics of the groups predicted to benefit most from the intervention.

7 Cost-effectiveness

The intervention generated additional second-dose completions at a low direct implementation cost. We assess cost-effectiveness from the perspective of the implementing government by combining the cost of the SMS campaign with the covariate-adjusted intention-to-treat effects reported in Section 6. The outcome is an additional girl completing the second HPV vaccine dose during the study period. Table 7 presents the cost assumptions and results, first for the campaign as a whole and then for each experimental arm.

The campaign required an estimated USD 162.38 in 2022 prices. SMS delivery accounted for USD 73.49: the intervention planned 41,340 messages at a unit cost of COP 8. The remaining USD 88.89 corresponds to ten hours of message programming and delivery, valued at COP 40,000 per hour. We convert all amounts using the exchange rate applied in the

[46] Estimates are aggregated across the 100 sample splits using medians. The median of the between-group contrast can therefore differ from the difference between the two group medians (Chernozhukov et al., 2025).

original project calculations, COP 4,500 per USD.[47]

A total of 4,922 participants were assigned to an SMS condition, including the placebo and the four active-message arms. Dividing total implementation costs by this population yields an average direct cost of approximately USD 0.033 per covered participant. Table 7 reports this amount rounded to USD 0.03, but the calculations use the unrounded value. For treatment k, we compute the cost per additional second-dose completion as

$$\widehat{c}_k = \frac{C/N_{\text{SMS}}}{\widehat{\tau}_k}, \tag{5}$$

where C denotes the total campaign cost, N_{SMS} is the number of participants assigned to an SMS condition, and $\widehat{\tau}_k$ is the corresponding covariate-adjusted intention-to-treat estimate expressed on the 0–1 scale.

Using the pooled effect of the four active messages (0.0757) and the campaign-wide average cost per covered participant in equation (5) yields USD 0.44 per additional second-dose completion. This ratio evaluates the pooled active-message effect under the same common cost assumption used for the arm-specific comparisons.

The treatment-specific estimates reinforce the empirical ranking reported in Section 6. The ownership reminder costs an estimated USD 0.72 per additional completion, compared with USD 0.38 for the planning prompt. The corresponding ratios are USD 0.42 for the combined treatment with the 24-hour reminder and USD 0.36 for the combined treatment with the two-hour reminder. The lower ratio for the planning prompt relative to the ownership reminder reflects its larger estimated effect under the common cost assumption, rather than a difference in the cost assigned to each covered participant. These direct implementation costs are low relative to reported cost-per-vaccination benchmarks.[48]

[47]The planned SMS volume reflects six weekly messages for participants assigned to the placebo, ownership-reminder, and planning-prompt arms, and twelve weekly messages for participants assigned to either combined-treatment arm.

[48]As a point of reference, Maldonado et al. (2026) report USD 5.53 per additional first HPV dose and USD 3.93 for the more effective norm designs. The planning-prompt ratio is approximately one tenth of the latter. Other reminders cost USD 7.50 per additional child completing age-appropriate vaccination in Guatemala

The numerical ordering among the three planning treatments should be interpreted cautiously. Although the combined two-hour treatment has the lowest ratio, the effects of the three treatments containing a planning prompt are statistically indistinguishable. Moreover, the arm-specific calculations apply the same average cost per covered participant to every treatment, even though each combined arm delivered an additional message. The calculation therefore does not allocate the higher messaging cost of the combined packages to those arms. The estimates support the conclusion that planning prompts are more cost-effective than ownership reminders under the implemented campaign, but they do not establish that a two-message package is more cost-effective than the planning prompt alone.

Table 7 also reports a ratio of USD 2.31 for the placebo message. We include this calculation for completeness, but it has no stable substantive interpretation. The placebo effect is small and statistically indistinguishable from zero, so minor changes in the estimated coefficient would generate large changes in the ratio. It should therefore not be used to compare policy alternatives.

These calculations cover the direct implementation cost of adding the SMS campaign to an existing vaccination system. They exclude vaccine procurement and administration costs, household transportation and opportunity costs, and capital investments in messaging infrastructure. The estimates therefore describe the communication campaign's cost per additional second-dose completion, rather than the full government or social cost of vaccination.[49]

Subject to these boundaries, the estimates show that time-specific planning prompts can produce sizable gains in vaccination completion at a very low direct implementation cost. This conclusion is most relevant for health authorities that already maintain vaccination

(Busso et al., 2015) and USD 7.90 per additional return visit for childhood vaccination in Nigeria (Kawakatsu et al., 2020). For social-media campaigns, Athey et al. (2023) estimate USD 5.68 per additional COVID-19 vaccination by linking experimental belief changes to observational vaccination rates. These reported-price comparisons provide context rather than a common-cost ranking: outcomes, cost coverage, and price years differ, and our estimates cover only SMS and personnel costs within existing infrastructure.

[49]The experiment does not identify effects on cervical cancer incidence, mortality, quality-adjusted life years, or future medical expenditures. Extrapolating to these outcomes would require epidemiological and cost parameters outside the experimental design.

registries and have the administrative capacity to identify incomplete schedules and deliver personalized messages at scale. In such settings, the planning prompt can complement existing information and access policies without requiring a formal appointment or a change in the available choice set.

8 Concluding remarks

Completing a health investment can remain difficult even after households incur the initial cost of adoption. We study this margin in a randomized controlled trial with 5,907 parents inside and outside Bogotá whose daughters had received a first HPV vaccine dose but had not completed the schedule. The experiment compares an ownership reminder, a time-specific planning prompt, and two packages combining both components. By studying completion after adoption, we focus on households with revealed willingness to vaccinate whose constraints need not be the same as those of parents who reject or never begin vaccination.

All four active messages increased completion, although their effects differed substantially with message content. Relative to a pure-control completion rate of 8.63 percent, the ownership reminder and planning prompt increased second-dose completion by 4.58 and 8.67 percentage points, respectively. A direct test rejects equality of these effects. The three treatments containing a planning prompt were statistically indistinguishable from one another, indicating no detectable gain from adding ownership language. The negative ownership-by-planning interaction further indicates that the implemented components produced less than additive gains. The placebo estimate, by contrast, was small and statistically indistinguishable from zero.

This treatment ranking is consistent with planning and implementation frictions being especially relevant among parents at the completion margin. A parent who has accepted the first dose may still fail to select a feasible time or coordinate the return visit. A time-specific prompt can make a concrete occasion for action focal. In our setting, the recommendation improved follow-through without assigning an appointment, changing the formal default, or

restricting the choice set. The evidence does not, however, identify the psychological channel directly. We do not measure the underlying behavioral processes, and the effects may also reflect how parents interpret the messages or how well the suggested time fits their schedules.

The cost-effectiveness estimates reinforce the practical relevance of this result. The planning prompt required an estimated USD 0.38 in direct implementation costs per additional second-dose completion, compared with USD 0.72 for the ownership reminder. These amounts measure the incremental communication cost of adding the SMS campaign to an existing system, rather than the full social cost of vaccination. The low direct cost makes time-specific prompts a potentially useful complement to information and access policies when health authorities already maintain vaccination registries and can deliver personalized messages at scale.

The scope of the evidence remains specific to completion after initial vaccination. It does not establish that planning prompts can increase first-dose take-up or overcome vaccine refusal or supply constraints. Nor does the experiment compare a suggested time with a self-generated plan or a formally scheduled appointment. Future research should compare these alternatives and measure the intermediate mechanisms more directly. In particular, allowing households to select a feasible time could preserve the benefits of a concrete plan while reducing mismatch costs. Such comparisons would clarify when a planning prompt is sufficient and when a formal appointment is more effective.

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Tables

Table 1: Experimental Design and Treatment Arms

Arm	Treatment condition	SMS contact	Ownership cue	Planning prompt	Reminder timing	Observations	Share (%)
1	Pure control	No	No	No	–	985	16.7
2	Placebo control	Yes	No	No	–	985	16.7
3	Ownership reminder	Yes	Yes	No	–	983	16.6
4	Planning prompt	Yes	No	Yes	–	986	16.7
5	Planning prompt + ownership reminder	Yes	Yes	Yes	24 hours before	984	16.7
6	Planning prompt + ownership reminder	Yes	Yes	Yes	2 hours before	984	16.7
Total						5907	100.0

Notes: This table reports the randomized treatment arms in the HPV vaccination completion experiment. The sample includes parents of girls who had initiated but not completed the HPV vaccination schedule at baseline. The pure control group received no message. The placebo control group received an SMS unrelated to HPV vaccination content. The ownership cue refers to language indicating that an HPV vaccine was reserved for the daughter. The planning prompt refers to language suggesting a specific day and time for completing the vaccination schedule without assigning a formal appointment or changing the choice set. Reminder timing applies only to treatment arms combining the planning prompt with an ownership reminder.

Table 2: Descriptive Statistics and Covariate Balance

	Descriptive statistics			Standardized differences relative to pure control				
Variable	Mean	SD	Pure control mean	Placebo	Ownership	Planning prompt	Planning+Own. 24h	Planning+Own. 2h
Girl's age (years)	11.252	2.013	11.237	0.020	0.003	-0.010	0.019	0.014
Age 9–10 years	0.461	0.498	0.461	-0.002	-0.000	-0.001	0.001	-0.001
Inside Bogota	0.628	0.483	0.628	-0.002	-0.002	-0.001	-0.001	0.001
EPS Sanitas	0.147	0.354	0.144	-0.038	0.004	0.022	0.026	0.037
EPS Salud Total	0.101	0.301	0.092	0.041	0.025	0.020	0.051	0.038
EPS Famisanar	0.204	0.403	0.216	-0.035	-0.004	-0.056	-0.032	-0.057
EPS Compensar	0.184	0.387	0.180	0.008	0.040	0.013	-0.021	0.021
EPS Capital Salud	0.096	0.295	0.095	0.017	-0.042	0.023	0.021	-0.003
Other EPS	0.268	0.443	0.272	0.016	-0.024	-0.007	-0.022	-0.020
Contributory scheme	0.813	0.390	0.827	-0.068	-0.033	-0.044	-0.035	-0.040
Subsidized scheme	0.123	0.328	0.112	0.059	0.010	0.053	0.044	0.041
Uninsured	0.013	0.113	0.012	0.000	0.026	0.026	-0.009	0.000
Other health regime	0.051	0.220	0.049	0.032	0.028	-0.015	0.000	0.010
Belongs to an ethnic group	0.005	0.071	0.002	0.051	0.064	0.075	0.020	0.075
Displaced by armed conflict	0.012	0.110	0.012	-0.019	0.026	-0.019	-0.040	0.050
Colombian nationality	0.963	0.189	0.969	-0.049	-0.060	-0.028	-0.012	-0.039
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 pure control group. The remaining columns report signed standardized differences between each listed arm and the pure 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 3: Main ITT Effects on HPV Second-Dose Completion

	(1) Strata only	(2) Covariate adjusted	(3) Double-selection LASSO	(4) OWN
Placebo control	0.0133	0.0143	0.0143	0.0130
	(0.0130)	(0.0130)	(0.0130)	(0.0130)
Ownership reminder	0.0460***	0.0458***	0.0457***	0.0466***
	(0.0139)	(0.0139)	(0.0139)	(0.0139)
Planning prompt	0.0862***	0.0867***	0.0863***	0.0853***
	(0.0147)	(0.0147)	(0.0147)	(0.0148)
Planning prompt + ownership, 24h	0.0784***	0.0794***	0.0796***	0.0790***
	(0.0146)	(0.0146)	(0.0146)	(0.0146)
Planning prompt + ownership, 2h	0.0905***	0.0910***	0.0909***	0.0910***
	(0.0149)	(0.0148)	(0.0148)	(0.0148)
Pure-control mean	0.0863	0.0863	0.0863	0.0863
Observations	5907	5907	5907	5907
Strata fixed effects	Yes	Yes	Yes	Yes
Pre-treatment covariates	No	Yes	Selected	Yes
P-value: all noncontrol arms equal	0.000	0.000	0.000	0.000
P-value: planning arms equal	0.758	0.778	0.787	0.772

Notes: The dependent variable is an indicator equal to one if the girl received the follow-up HPV vaccine 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; for example, an estimate of 0.0460 corresponds to a 4.60 percentage-point increase in completion. The omitted category in all columns is the pure control group. Column 1 adjusts only for randomization-strata fixed effects. Column 2 adds pre-treatment administrative covariates: EPS indicators, health-regime indicators, ethnicity, displacement status, Colombian nationality, and girl's age. Column 3 reports double-selection LASSO estimates using the same treatment indicators and strata fixed effects as variables of interest and allowing the pre-treatment covariates to be selected controls. Column 4 reports the OWN estimator of Goldsmith-Pinkham et al. (2024), using the same pre-treatment covariates and strata; this estimator subtracts estimated contamination attributable to other treatments from the partly linear estimator. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Ownership Reminders Versus Planning Prompts

	(1) Strata only	(2) Covariate adjusted	(3) Double-selection LASSO	(4) OWN
Panel A. Global message effect				
Any active message	0.0753***	0.0757***	0.0756***	0.0759***
	(0.0106)	(0.0106)	(0.0106)	(0.0106)
Panel B. Message-component decomposition				
Ownership cue	0.0460***	0.0458***	0.0457***	0.0466***
	(0.0139)	(0.0139)	(0.0139)	(0.0139)
Planning prompt	0.0862***	0.0867***	0.0863***	0.0853***
	(0.0147)	(0.0147)	(0.0147)	(0.0148)
Ownership cue x planning prompt	-0.0539**	-0.0530**	-0.0524**	-0.0529**
	(0.0215)	(0.0215)	(0.0215)	(0.0218)
2h vs 24h timing	0.0121	0.0116	0.0113	0.0120
	(0.0166)	(0.0165)	(0.0165)	(0.0167)
Pure-control mean	0.0863	0.0863	0.0863	0.0863
Observations: Panel A	5907	5907	5907	5907
Observations: Panel B	5907	5907	5907	5907
Strata fixed effects	Yes	Yes	Yes	Yes
Pre-treatment covariates	No	Yes	Selected	Yes
P-value: ownership = planning	0.011	0.010	0.010	0.015
P-value: interaction = 0	0.012	0.014	0.015	0.015
P-value: 2h = 24h	0.465	0.484	0.493	0.472

Notes: The dependent variable indicates completion of the follow-up HPV vaccine dose during the study period, measured in administrative vaccination records. Coefficients and standard errors are reported on the 0–1 scale. The pure-control arm is the reference category; the placebo arm is retained as a separate category. Panel A pools the four active messages. Panel B reports the ownership and planning effects, their interaction, and the contrast between reminders sent two and 24 hours before the suggested time. Column 1 includes randomization-strata fixed effects. Column 2 adds pre-treatment EPS indicators, health-regime indicators, ethnicity, displacement status, Colombian nationality, and girl's age. Column 3 reports double-selection LASSO estimates, retaining treatment indicators and strata fixed effects and selecting among the pre-treatment covariates. Column 4 uses the OWN estimator of Goldsmith-Pinkham et al. (2024), with the same covariates and strata. In Panel B, columns 1–3 estimate a component model; column 4 reports the corresponding contrasts of arm-specific OWN effects. The interaction is the effect of the combined 24-hour arm minus the ownership-only and planning-only effects; the timing contrast is the difference between the combined two-hour and 24-hour arms. Standard errors and equality tests for these contrasts use the joint covariance matrix of the arm-specific estimates. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Multiple Inference and Robust Inference

	Estimate	Robust SE	Robust p	BH q	RI p
Placebo control	0.0143	(0.0130)	0.274	0.274	0.285
Ownership reminder	0.0458***	(0.0139)	0.001	0.001	0.001
Planning prompt	0.0867***	(0.0147)	< 0.001	< 0.001	< 0.001
Planning prompt + ownership, 24h	0.0794***	(0.0146)	< 0.001	< 0.001	< 0.001
Planning prompt + ownership, 2h	0.0910***	(0.0148)	< 0.001	< 0.001	< 0.001
Pure-control mean	0.0863				
Observations	5907				
Randomization strata fixed effects	Yes				
Pre-treatment covariates	Yes				
RI replications	10000				

Notes: The dependent variable is an indicator equal to one if the girl received the follow-up HPV vaccine dose during the study period, measured in administrative vaccination records. Estimates come from the covariate-adjusted linear probability model in Table 3; the omitted category is the pure control group. Coefficients and standard errors are reported in the original 0–1 scale of the dependent variable. Robust p-values use Eicker-Huber-White standard errors. BH q-values apply the Benjamini-Hochberg false-discovery-rate adjustment to the five treatment-arm contrasts reported in the table. RI p-values are two-sided stratified randomization-inference p-values based on the absolute robust t-statistic and 10000 permutations of the treatment assignment within randomization strata. Treatment was assigned at the parent-household level; because nearly all participating households contribute only one eligible girl to the final analysis sample, household clustering is not a meaningful additional correction. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Pre-Specified Heterogeneous Effects

	Age 9–10	Inside Bogota	Subsidized or uninsured	Colombian national	Displaced
Panel A. Pooled message effect					
Any message, subgroup = 0	0.0346***	0.0330***	0.0725***	0.0841**	0.0747***
	(0.0129)	(0.0115)	(0.0116)	(0.0415)	(0.0107)
Any message, subgroup = 1	0.1243***	0.1013***	0.0997***	0.0757***	0.1797***
	(0.0172)	(0.0154)	(0.0251)	(0.0109)	(0.0548)
Difference: subgroup 1 - subgroup 0	0.0897***	0.0683***	0.0271	-0.0084	0.1050*
	(0.0215)	(0.0193)	(0.0277)	(0.0429)	(0.0557)
Panel B. Arm-specific heterogeneity terms					
Ownership reminder x subgroup	0.0641**	0.0245	0.0744*	-0.0059	0.1651
	(0.0284)	(0.0257)	(0.0411)	(0.0532)	(0.1086)
Planning prompt x subgroup	0.1154***	0.0956***	0.0078	-0.0076	0.0592
	(0.0299)	(0.0264)	(0.0383)	(0.0636)	(0.1101)
Planning prompt + ownership, 24h x subgroup	0.1085***	0.0638**	0.0238	0.0175	0.0663
	(0.0298)	(0.0268)	(0.0403)	(0.0546)	(0.1149)
Planning prompt + ownership, 2h x subgroup	0.0707**	0.0894***	0.0031	-0.0388	0.0993
	(0.0302)	(0.0269)	(0.0400)	(0.0655)	(0.0872)
P-value: arm-specific interactions jointly zero	< 0.001	< 0.001	0.456	0.951	0.427
Pure-control mean, subgroup = 0	0.0734	0.0328	0.0915	0.0323	0.0874
Pure-control mean, subgroup = 1	0.1013	0.1179	0.0492	0.0881	0.0000
Subgroup observations	2,268	3,092	662	4,742	63
Regression observations	4,922	4,922	4,922	4,922	4,922
Strata fixed effects	Yes	Yes	Yes	Yes	Yes
Pre-treatment covariates	Yes	Yes	Yes	Yes	Yes

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. The estimation sample excludes the placebo-control arm and compares the pure control group with the four active active-message arms. Panel A reports covariate-adjusted effects of receiving any of the four active messages separately for observations with subgroup indicator equal to zero and one, plus the interaction difference. Panel B reports treatment-arm-specific interaction terms between each active treatment arm and the subgroup indicator. All specifications are linear probability models with randomization-strata fixed effects and the same pre-treatment administrative covariates used in Table 3. The age and Bogota subgroup main effects are absorbed by the randomization-strata fixed effects when collinear with the strata, but their treatment interactions are identified. Heterogeneity by Colombian nationality and displacement status should be interpreted cautiously because the corresponding reference or subgroup cells are small. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

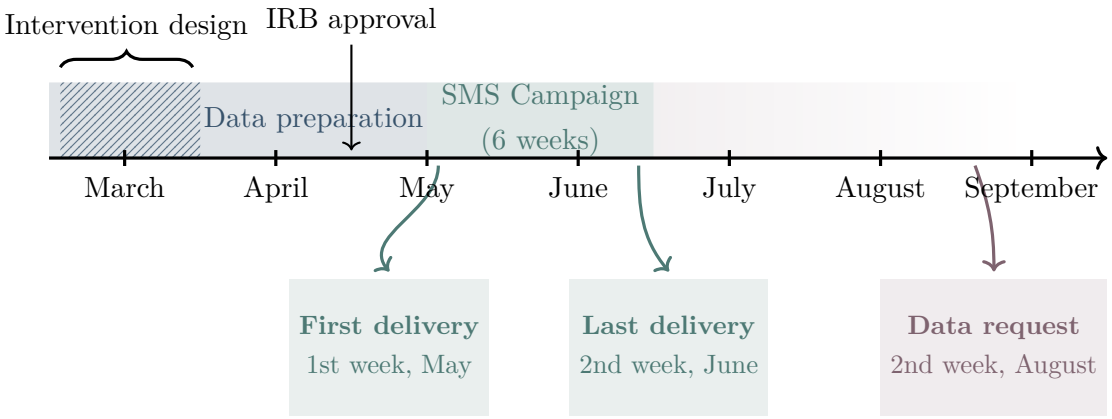
Table 7: Cost-Effectiveness

Panel A. Costs	
Item	Implementation costs (2022 USD)
Labor costs	88.89
SMS costs	73.49
Total costs	162.38
Participants covered	4,922
Total cost per participant	0.03
Planned SMS	41,340
Panel B. Cost-effectiveness	
Item	Estimate
Average impact of any active message	0.0757
Cost per additional vaccination	0.44
Impact of placebo message	0.0143
Cost per additional vaccination	2.31
Impact of ownership reminder	0.0458
Cost per additional vaccination	0.72
Impact of planning prompt only	0.0867
Cost per additional vaccination	0.38
Impact of planning prompt + ownership, 24h	0.0794
Cost per additional vaccination	0.42
Impact of planning prompt + ownership, 2h	0.0910
Cost per additional vaccination	0.36

Notes: Panel A reports implementation costs from the government perspective in 2022 USD. A participant is covered if assigned to any SMS arm, including the placebo and the four active messages; the pure control group receives no message. Panel B applies the same campaign-wide average cost per covered participant to each covariate-adjusted ITT effect on second-dose HPV vaccination. The numerator averages costs across all SMS arms, including the placebo; it does not allocate costs separately by treatment. The pooled active-message effect combines the ownership reminder, planning prompt, planning prompt plus ownership reminder at 24 hours, and planning prompt plus ownership reminder at 2 hours. The treatment-specific rows report the corresponding arm-specific ITT effects. All treatment effects are estimated relative to the pure control group with the same pre-treatment covariates and randomization-strata fixed effects as Table 3. Each SMS costs 8 COP and message programming/delivery required 10 labor hours at 40,000 COP per hour. The planned SMS volume is 41,340: six weekly SMS for the placebo, ownership-reminder, and planning-prompt arms, and twelve weekly SMS for each combined arm. USD values use 4,500 COP per USD. The exercise excludes vaccine costs, parental opportunity and transport costs, and capital investments in messaging infrastructure.

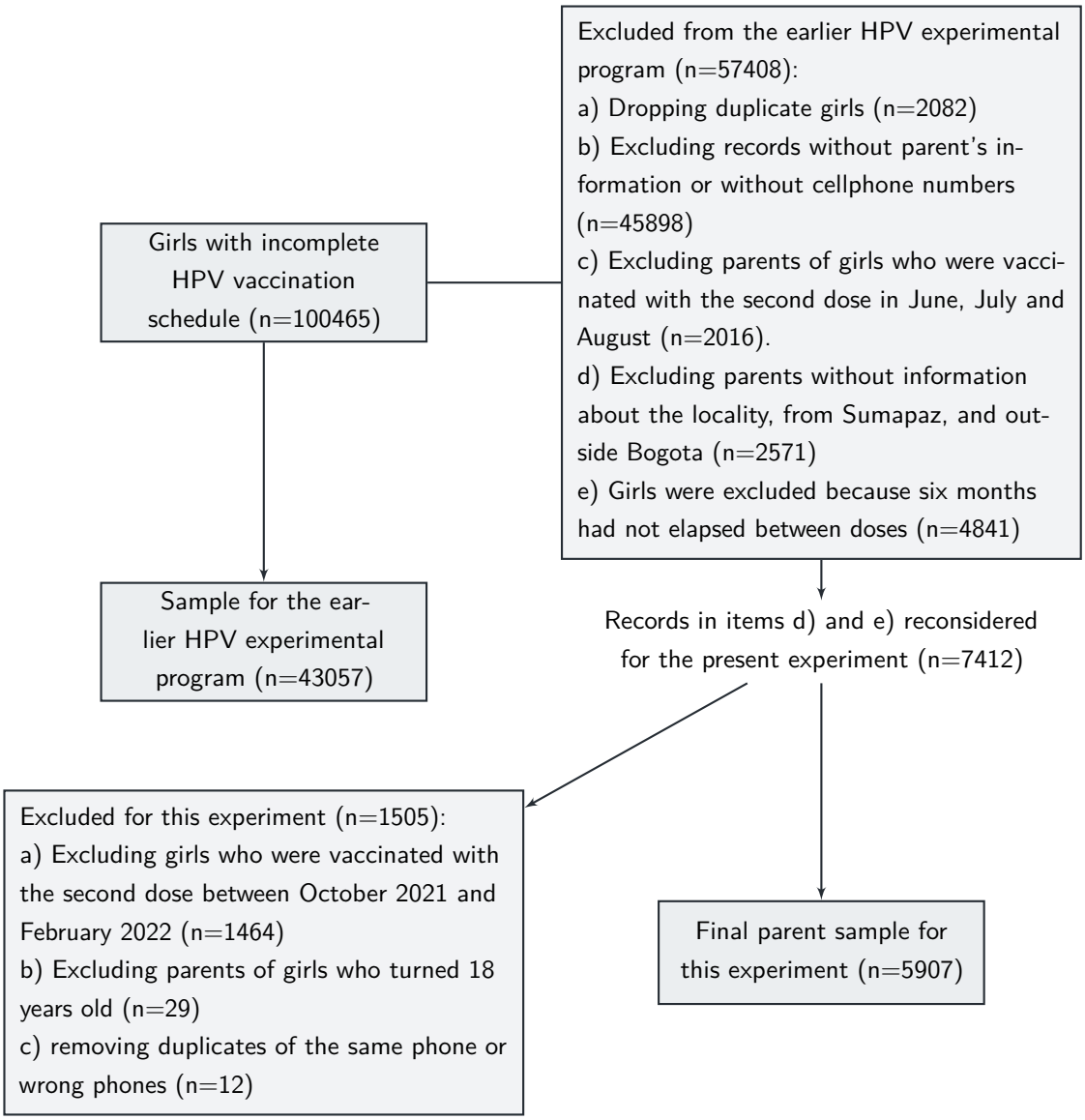
Figures

Figure 1: Experiment Timeline



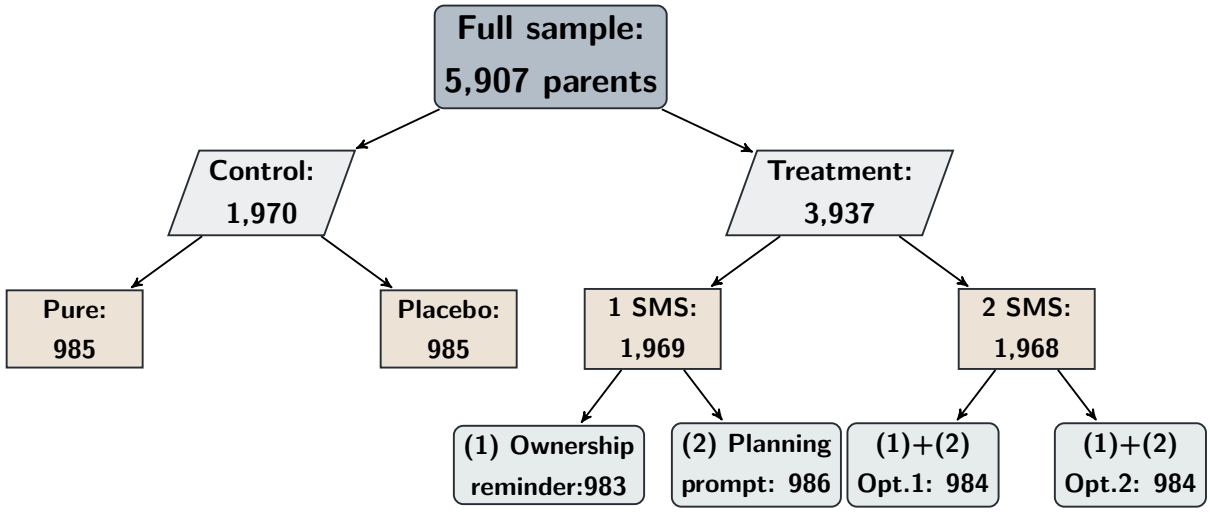
Notes: The figure summarizes the implementation timeline for the 2022 message experiment. The design and data-preparation phase preceded the six-week SMS campaign. The first SMS round was delivered on Wednesday, May 4, 2022, and the last round was delivered on Saturday, June 11, 2022. Outcome data were requested from administrative vaccination records in the second week of August 2022, allowing vaccination completion to be measured after the campaign window.

Figure 2: Sample construction for this experiment



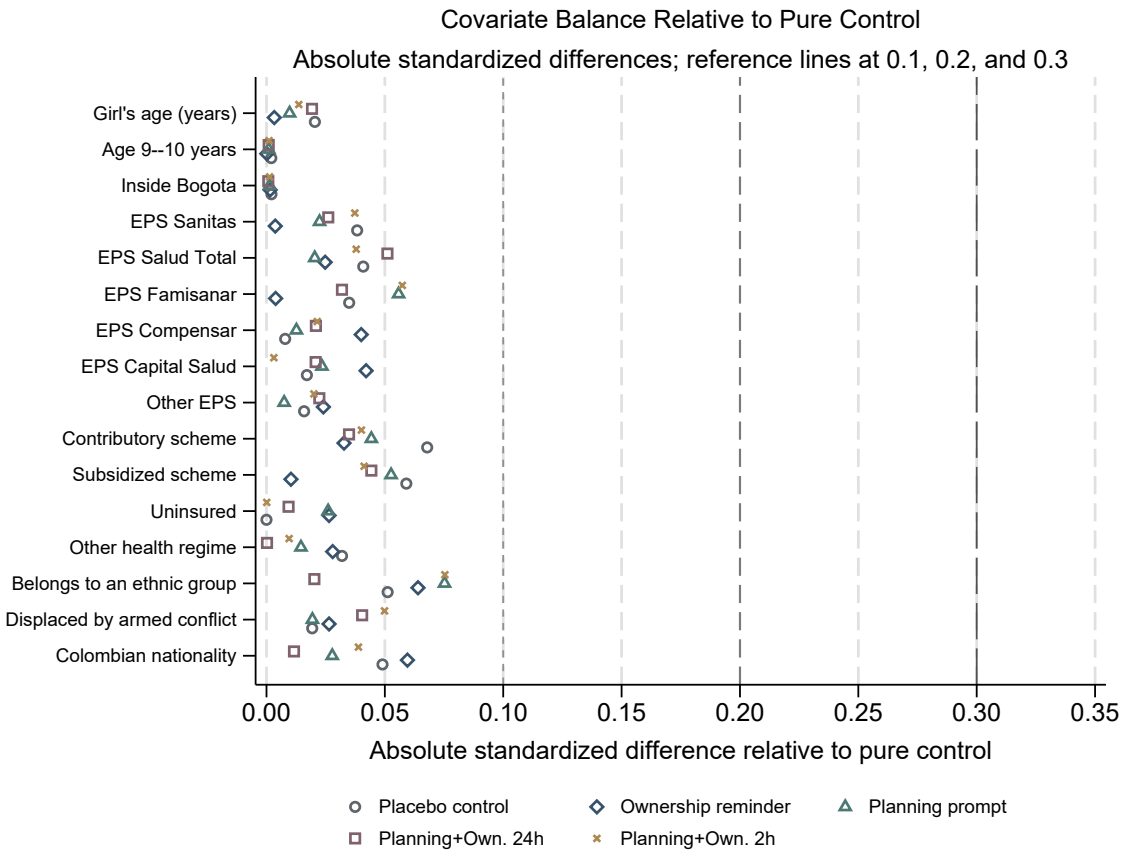
Notes: The figure traces the construction of the experimental sample from Health Secretariat records of girls with incomplete HPV vaccination schedules. An earlier program of HPV vaccination experiments used 43,057 records from this incomplete-schedule frame. Of the 57,408 excluded records, 7,412 were reconsidered for the present study: 2,571 had been excluded because of geographic restrictions or missing locality information and 4,841 because the six-month interval between doses had not elapsed. Updating eligibility and contact information removed another 1,505 records, leaving 5,907 randomized parent records, each linked to an eligible daughter.

Figure 3: Experimental design for the experiment



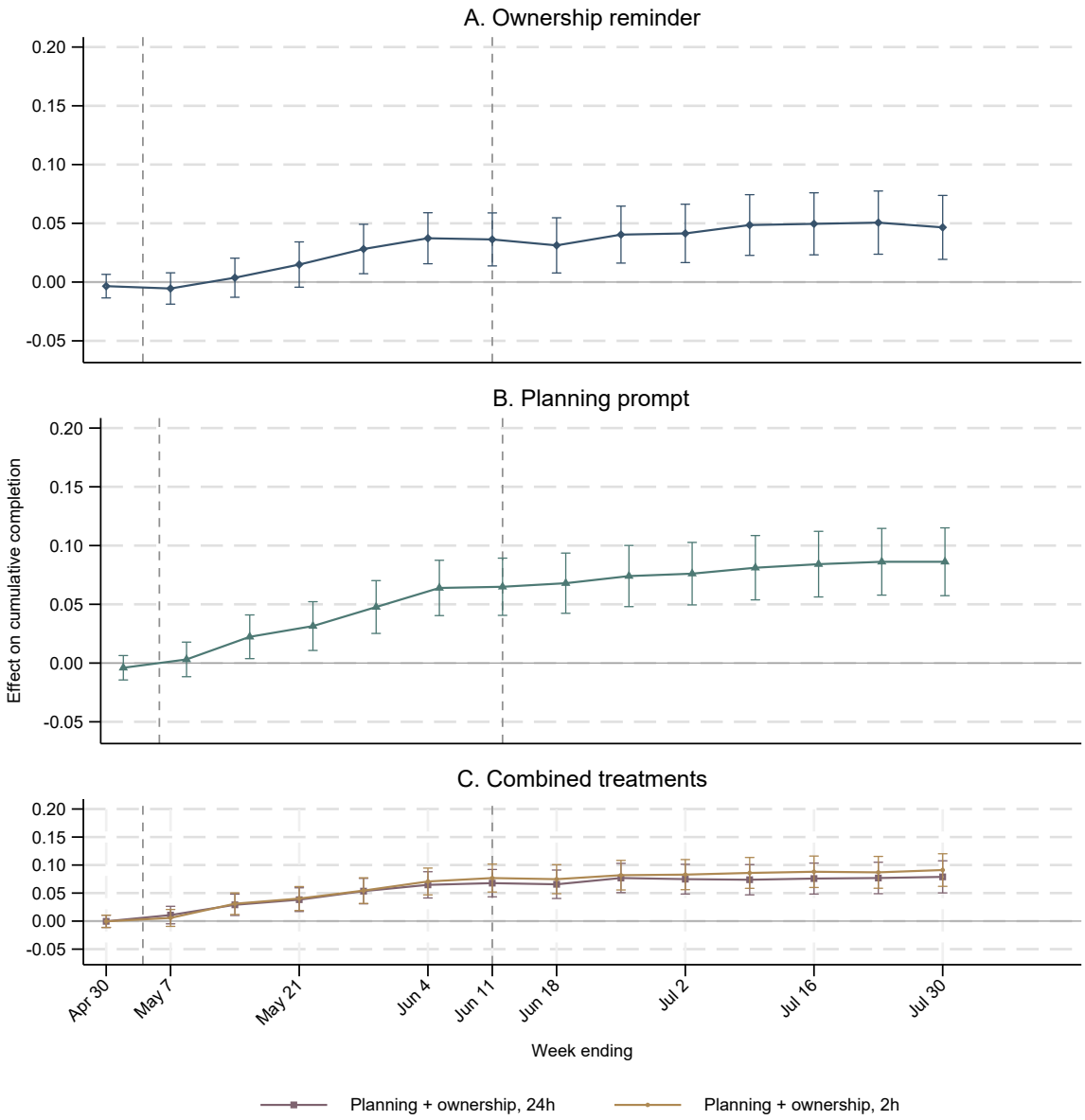
Notes: The figure reports the randomized assignment structure for the final experimental sample of 5,907 parents. The control group contains a pure control arm with no SMS and a placebo-control arm with non-behavioral SMS content. The active-treatment group contains two one-SMS arms, ownership reminder and planning prompt, and two two-SMS arms combining the planning prompt with an ownership reminder sent either 24 hours or 2 hours before the suggested vaccination time. The messages did not assign a formal appointment. Cell counts report the number of parents assigned to each arm after randomization.

Figure 4: Covariate Balance Plot Relative to Pure Control



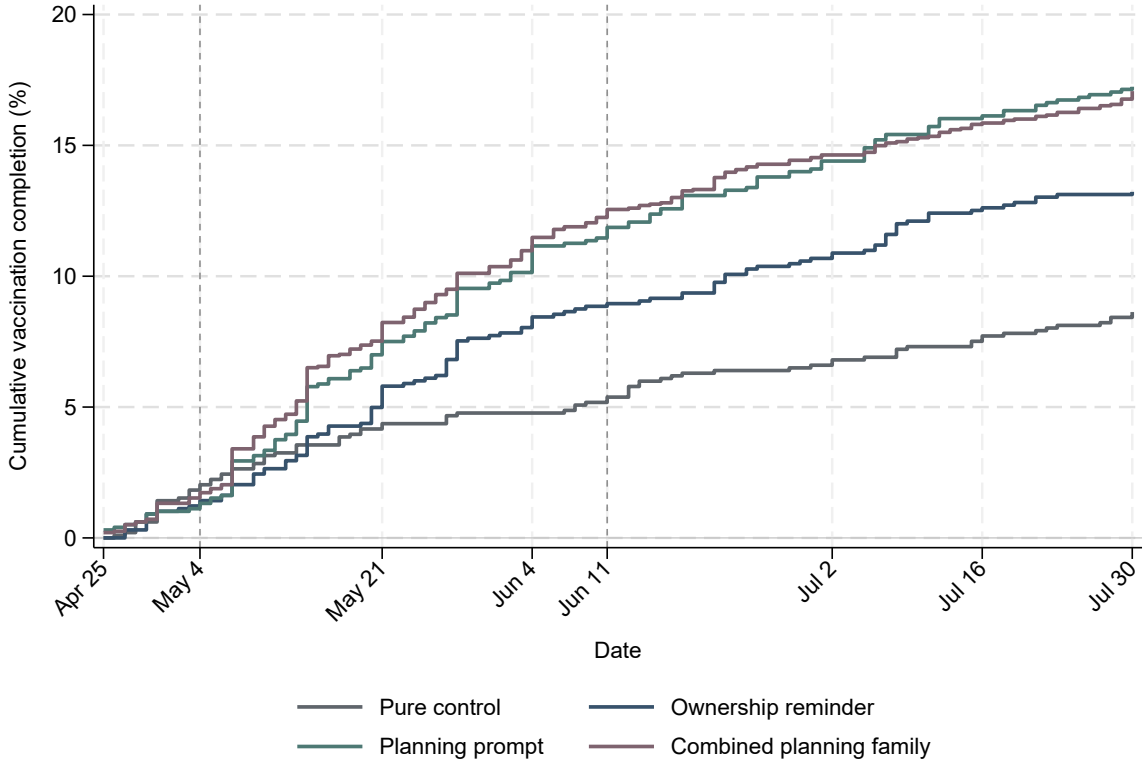
Notes: The figure reports absolute standardized differences in baseline covariates between each non-pure-control arm and the pure control group. Standardized differences are computed as the difference in group means divided by the square root of the average of the two group-specific variances. Vertical 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 5: Dynamic Treatment Effects



Notes: The figure reports weekly treatment effects on cumulative HPV second-dose completion relative to the pure 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.

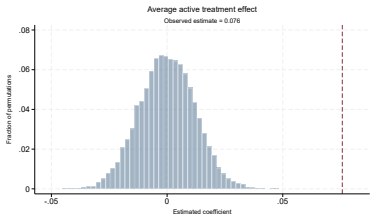
Figure 6: Kaplan-Meier Vaccination Completion



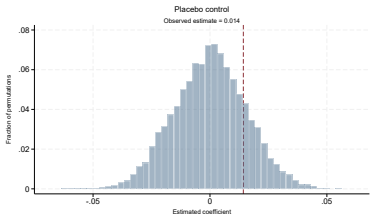
Notes: The figure reports Kaplan-Meier-style cumulative completion curves for HPV second-dose vaccination during the administrative follow-up window. The pure control arm received no SMS. The ownership-reminder curve corresponds to the ownership-only treatment arm; the planning-prompt curve corresponds to the planning-only treatment arm; and the combined planning family pools the two treatment arms that combined planning-prompt language with an ownership reminder. 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 7: Randomization-Inference Permutation Distributions

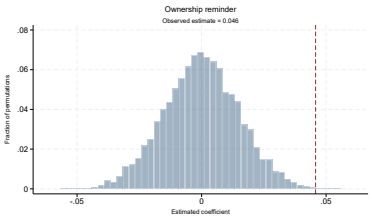
(a) Aggregate



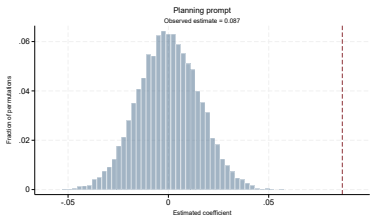
(b) Placebo control



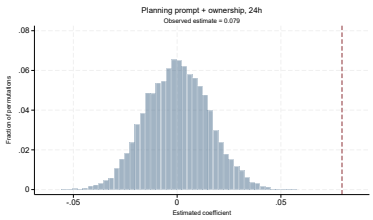
(c) Ownership reminder



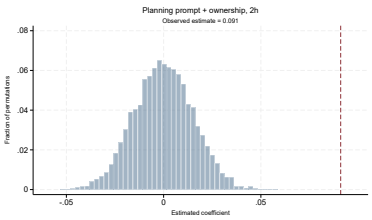
(d) Planning prompt



(e) Planning + ownership, 24h

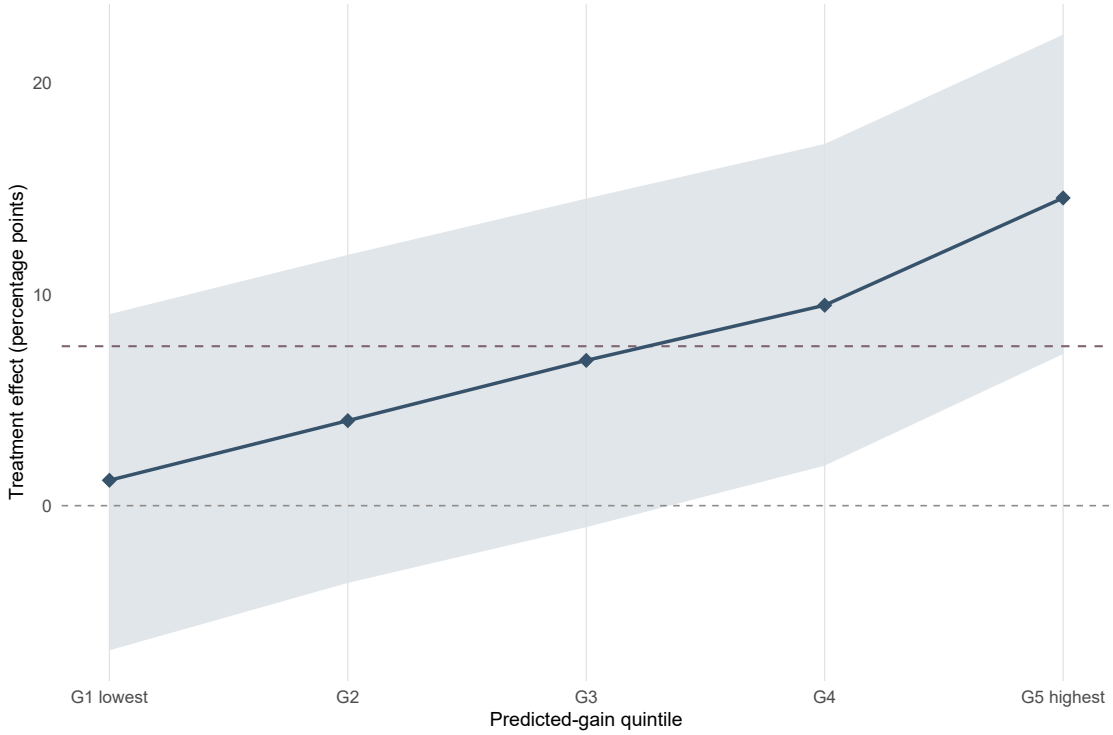


(f) Planning + ownership, 2h



Notes: Each panel plots the permutation distribution of the covariate-adjusted coefficient from 10000 treatment reassignments within randomization strata. The vertical line marks the observed estimate in the final analysis sample. The aggregate panel reports the equally weighted average of the four active-message treatment-arm coefficients relative to pure control; this is numerically equivalent to the pooled active-message effect up to the small imbalance in treatment-arm counts.

Figure 8: GenericML Group Average Treatment Effects



Notes: The figure reports group average treatment effects (GATES) estimated 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, yielding a sample of 4,922 observations. Groups are quintiles ranked from the lowest (G1) to the highest (G5) predicted treatment gains. For each sample split, one half is used to construct the predicted-gain score and the other to estimate group effects; estimates are aggregated across 100 splits using medians. The shaded region shows 90% confidence intervals at each quintile. The upper dashed horizontal line reports the average treatment effect from the best linear predictor; the dashed line at zero marks no effect. Effects are expressed in percentage points.