Incentives and Motivation Crowd-Out: Experimental Evidence from Childhood Immunization

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Abstract

We investigate the effects of incentives and their removal on parents' decisions to vaccinate subsequent children. We follow up three years after parents were exposed to an incentive designed to promote timely childhood vaccination in Sierra Leone. This study builds on a field experiment where clinics were randomly assigned to either provide an incentive or not. Since only parents with a newborn at the time were eligible, we exploit individual-level variation in incentive exposure within clinics to explore drivers of persistent behavior change. Our findings reveal three main results: First, parents exposed to an incentive for an earlier child are 5 to 11 percent less likely to vaccinate their subsequent child on time. There are no effects on overall vaccination rates by 15 months, indicating that parents delay vaccination rather than abstaining altogether. Second, parents in incentive communities who were ineligible exhibit no change in behavior, suggesting that shifts in community norms or clinic practices do not drive behavior change. Third, incentives that signal being a caring parent do not lead to adverse effects. We argue that mechanisms such as learning and belief updating from the incentives' removal cannot account for our results. Instead, our findings suggest that being rewarded for taking an action can reduce intrinsic motivation to take such action in the future.

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I Introduction

A key concern among policy makers is the potential for incentives to undermine intrinsic motivation, leading to a negative impact on behavior. A common application of material and financial incentives in the field is the promotion of health behaviors, such as deworming or water treatment, where individual actions contribute to public goods. While a large literature has examined the efficacy of these incentives during their implementation (Ahmed et al. 2022), the effects of their withdrawal are unclear. This presents a challenge for policy makers, who need to weigh the immediate benefits of a program against its potential longer-term impacts, especially since most incentive programs are not intended to be permanent.

The crowding out of motivation, widely discussed in the psychology and economics literature, describes situations in which individuals are less motivated to perform an action for its own sake once external incentives are introduced (Deci 1971; Frey and Oberholzer-Gee 1997). This could occur because the incentive changes individuals' future expectations of the action, or sends a negative signal about the cost and benefit of the action (Bénabou and Tirole 2003). Isolating crowding out in a real-world setting is challenging, as it may exist contemporaneously with the positive effects of incentives. If behavior remains positive overall, it is difficult to quantify the negative effects on intrinsic motivation without observing behavior after incentives are removed.

There is limited evidence showing that extrinsic incentives can reduce intrinsic motivation and negatively impact behaviors once withdrawn in real world settings (Gneezy and Rustichini 2000a; Meier 2007). Additionally, there is field evidence suggesting that non-financial incentives do not crowd out prosocial motivation during implementation of the incentives (Lacetera et al. 2012; Ashraf et al. 2014; Khan 2024). This could point to such effects not being relevant outside of a more controlled environment (Esteves-Sorenson and Broce 2022). Much of the field literature on the effects of incentives post-removal has focused on settings where incentives create persistent positive changes through learning (Dupas 2014; Bryan et al. 2014), habit formation (Acland and Levy 2015; Ito et al. 2018), or capital investment (Allcott and Rogers 2014; Costa and Gerard 2021; Levitt et al. 2016). The net impact of incentives, in the absence of these positive mechanisms, therefore remains uncertain.

In this paper, we investigate the impact of incentives and their withdrawal on parents' decisions to vaccinate their child. We do so by following up on a large-scale experiment in Sierra Leone, where color-coded bracelets were used to incentivize timely vaccination (Karing 2024). Three years after the experiment, we revisit all 597 study communities, collecting vaccination data from over 10,000 children who were born subsequently. This paper seeks to answer the following questions: Does providing incentives to parents to vaccinate their child change vaccine behavior for their future child? Do incentives change parents' intrinsic motivation; and if so, under what conditions?

The context of childhood immunization in Sierra Leone offers a relevant policy application to the question of motivation crowd-out. With vaccination rates nearing the levels necessary to reach herd immunity, policy makers face the challenge of increasing take-up among the "last mile" parents without undermining the motivation of the majority who is already vaccinating. Further, in this setting, routine vaccination is a well-established health behavior, with over 80% of parents completing the first three vaccines on time. As such, persistent mechanisms such as learning by doing or habit formation are unlikely to influence behavior when incentives are removed.

We take advantage of three unique features of this empirical setting. First, the original experiment randomly assigned 120 public clinics to one of three incentive treatments and a control group where no incentives were distributed. We use this to compare children's vaccination outcomes across former incentives and control groups and establish the effects of the program once the incentives are removed. Second, we use variation in whether parents had a child during the experiment to distinguish between parents who were directly exposed to the incentive and parents who were indirectly exposed (i.e., were not eligible at the time but resided in treated communities). By analyzing immunization decisions across these two groups, we examine the extent to which the incentives' effects when withdrawn are attributable to individuals' experience (e.g., motivation crowd-out) or community- and clinic-level effects (e.g., changes in provider behavior or social norms). Third, by comparing incentive treatments that encouraged different vaccinations, we can test whether the perceived costs and benefits of an action, as well as its signaling value of being a good parent, affect post-removal behavior.

The bracelet incentives were designed as follows: each incentive arm included an initiation bracelet that was given to children at the first vaccination visit. Since 99.7% of parents in this context initiate vaccination, the first bracelet does not carry any signaling value and mainly functions as a small material reward. The first bracelet treatment consisted solely of the initiation bracelet and we refer to it as Initiation Reward. The second treatment included an additional bracelet incentive: the initiation bracelet was exchanged for a bracelet of a new color at the fourth vaccine if completed on time. While informative about one's vaccine status, parents assign little importance to the fourth vaccine and do not gain social benefit from signaling its timely completion. Instead, its value comes from clinic staff potentially praising the parent and the novelty of receiving a different bracelet color. We refer to it as Double Reward. The third treatment consisted of the exchange of the initiation bracelet at the last vaccine, if all vaccines were completed on time. Having the second bracelet is seen as a meaningful signal of a parent's motivation to care for their child's health because parents regard the last visit as one of the most important ones. We refer to it as Signaling Reward.

To assess the extent to which incentives influence parents' behavior after removal, we examine whether direct exposure to the incentives during the experiment affects parents' vaccination decisions for their next child differently than indirect exposure. Since incentives were tied to timely vaccination, our primary outcome is the total number of vaccines completed on time. This captures parents' effort to adhere to the vaccination schedule rather than choosing a time that is convenient for them to vaccinate their child. We further examine the effects of incentives on the timeliness of specific vaccines to understand the pattern of behavioral responses across the immunization schedule and its link to the original incentive structure. Our second outcome, vaccine completion by 15 months, tests whether parents skip vaccinations altogether, which would point towards changes in vaccination attitudes.

Our results show that direct exposure to incentives reduces timely vaccination of future children. These effects are common across all three incentive treatments, but vary in magnitude and significance. Specifically, parents who had a child during the experiment and were therefore directly exposed to the incentives complete 5% ($p<0.05$) and 11% ($p<0.01$) fewer vaccines on time in the Initiation and Double Reward treatments, respectively, compared to parents in the Control group. The Signaling Reward treatment shows less pronounced and insignificant negative effects (-3%, $p=0.29$). In contrast, parents who were only indirectly exposed to incentives do not exhibit a decline, and we detect significant differences in their treatment effects compared to those of directly exposed parents (p = 0.001-0.07). These results suggest that adverse effects are driven by the experience of receiving an incentive, rather than community-wide factors such as changes in clinic staff behavior or shifts in vaccination norms.

Examining the timely completion of specific vaccines, we find negative effects of the Initiation Reward on the timely completion of the first (-3.8 percentage points, $p<0.01$) and second vaccines (6.3 percentage points, $p<0.01$) but no adverse effects on later vaccines (between -3.9 to -5.7 percentage points, p=0.12-0.44), where no incentive was provided. The Double Reward treatment exhibits a similar pattern for the first two vaccines, and consistently negative effects for later vaccines. Most strikingly, exposure to this incentive results in a 12.1 and 19.2 percentage points ($p<0.05$) decline in the timely completion of the fourth and fifth vaccines, consistent with an incentive being given at the former.

In contrast, the Signaling Reward does not lead to robust negative effects on any of the vaccine visits. While we observe a small negative effect on the second vaccine (-4.7 percentage points, $p<0.01$), it is sensitive to how timeliness is defined.

We apply the causal forest method developed by Wager and Athey (2018) to investigate individual- and community-level characteristics that are associated with negative effects on behavior. The method constructs data-driven subgroups to assess treatment effect heterogeneity and understand how covariates relate to the distribution of treatment effects. We find significant heterogeneity in treatment effects in the Initiation and Double Reward treatments, with parents facing the highest costs experiencing the most negative outcomes. In contrast, we observe no heterogeneity in the Signaling Reward treatment, indicating that the null effect does not mask persistent positive change among some parents and negative effects for others. This allows us to use the Signaling Reward as a comparison group and rule out that the negative effects observed in the other two groups are driven by the removal of bracelets sending a negative signal to parents about the importance of timely vaccination. Instead, our results suggest that the experience of incentives can reduce parents' intrinsic motivation, either by attributing their decision to timely vaccinate to the external reward, or by creating an expectation of being rewarded when taking the action. We also detect heterogeneity in treatment effects among indirectly exposed parents, masking positive and negative treatment effects. We interpret these results as parents with less knowledge of the bracelets misinterpreting their meaning during the phase-out and forming incorrect beliefs about vaccination.

Lastly, we examine whether negative effects are due to parents exerting less effort to complete vaccinations on time or a change in parents' attitudes toward vaccination. We find that the negative effects on vaccinations disappear with children's age and are no longer detectable at 15 months of age.

This paper makes three contributions. First, we contribute to the empirical evidence on incentives crowding out intrinsic motivation. To our knowledge, this is the first study to find evidence of crowd-out persisting up to 2-3 years post-incentives in a policy-relevant low-income country context. By following up with individuals who did not respond to an incentive in the short run, we also uncover that crowding out of intrinsic motivation can be hidden as long as the incentives are in place. Several field studies have documented crowding-out effects during incentive exposure (Gneezy and Rustichini 2000a; Kerr et al. 2012; Gneezy and Rustichini 2000b; Berry et al. 2022), while others found crowding-out during exposure (Ashraf et al. 2014; Khan 2024). However, evidence on a potential decline in motivation after incentives are removed is limited (Gneezy and Rustichini 2000a; Meier 2007). These findings carry important implications: while policy makers commonly focus on moving individuals to adopt a desirable behavior, our findings show that incentives may significantly impact those already performing the desired action.

Second, we contribute to the literature aiming to understand when motivation crowd-out can occur and undermine behavior. Our findings are consistent with the Bénabou and Tirole (2003)'s theory that incentives can lead individuals to re-interpret their relationship to an action and reduce their intrinsic motivation to take the action, making it less likely for them to adopt it when the incentives are removed. We show that this can occur in settings where individuals have informed beliefs about the costs and benefits of the action, contrasting with context where there is scope for positive learning (Dupas 2014; Bryan et al. 2014). We also address a gap in understanding incentives' impact when they are implemented for long enough to create expectations around them (Campos-Mercade et al. 2023). While recent work shows no evidence of crowding-out from one-time financial incentives for COVID-19 vaccination (Schneider et al. 2023), our study provides evidence of crowding-out from vaccination incentives that were implemented for over two years.

Third, our study provides insights into when and how to use incentives to promote prosocial behavior, contrasting incentives that leverage intrinsic motivation with extrinsic incentives of varying short-term efficacy. We find that when an incentive allows recipients to showcase their intrinsic valuation for an action, it does not diminish their motivation. This suggests that signaling incentives could offer a more favorable cost-benefit balance for repeated behaviors over the long term. Although our research suggests that extrinsic rewards have no negative impact on those indirectly exposed, many decisions are recurrent rather than one-time. Numerous incentive programs, especially in areas like early childhood investments or environmental efforts (e.g., purchasing electric vehicles), necessitate ongoing motivation. In the domain of childhood vaccination specifically, there is a growing interest in financial and in-kind incentives to reduce zero-dose children (UNICEF 2023). Given that around 80% of the population in priority countries initiate vaccination in the absence of the reward, the risk of motivation crowd-out warrants careful consideration.[1] Our results underscore the importance of collecting data on behaviors after incentives stop, to monitor potential declines in take-up, improve our understanding of these issues, and find strategies to mitigate them.

The paper is organized as follows: Section 2 provides background on the original experiment, Section 3 details our hypotheses and methods, Section 4 discusses the findings, and Section 5 concludes.

II Background

II.A Childhood Vaccination

Vaccination in a low-income context is a life-saving behavior due to the high rates of mortality from infectious diseases like diphtheria, pertussis, and measles (UNICEF 2019). Timely vaccination is particularly important as the risk of infection and death from diseases is the highest among children under the age of one (CDC 2022). However, despite improvements in the availability and reliability of immunization services (UNICEF and WHO 2016), only 56% of the children complete the first year series of vaccinations and a significant number are vaccinated late (Sierra Leone DHS 2020). This pattern is common in many low-income countries, and a large public health literature documents that insufficient demand, due to parents forgetting appointments or lack of awareness about the benefits of vaccines, explains the low take-up.[2]

[1] This is based on UNICEF (2023)s report of the average initiation rates in GAVIs top 57 priority countries.

[2] E.g., in India (India DHS 2020), Peru (Peru DHS 2015), and Indonesia (Indonesia DHS 2017), 98%, 91%, and 91% of children, respectively, begin vaccinations, but only 78%, 62%, and 65% complete the

Parents in Sierra Leone must take their child for five routine vaccinations under the age of one. The first vaccine is due after birth, followed by a three-dose series of vaccines that protect against diphtheria, tetanus, and pertussis (DTP). The three doses must be given one month apart, with the first administered at 1.5 months and the last dose given at 3.5 months of age. The fifth vaccine is the first of a two-dose series that protects against measles and is due at nine months of age. The schedule is therefore particularly demanding, highlighting the potential value of incentives. At the same time, women living in rural Sierra Leone have 5.1 children on average (DHS 2019). This means that parents have to go through the vaccination schedule many times, and the effects of incentives once they are removed on subsequent generations need to be carefully considered.

II.B Bracelets as Incentives for Childhood Immunization

Karing (2024) tested the effects of social signaling incentives–in the form of color-coded bracelets–given to children when coming for timely immunization. A total of 120 government clinics were randomized into four groups. In the Control group, children did not receive any bracelets. All three treatment groups included an initiation bracelet given to children upon their first vaccine visit. Since 99.7% of parents in this context initiate vaccination, the first bracelet primarily acts as a small material reward. The first treatment consisted solely of the initiation bracelet: children received a bracelet of their chosen color, either yellow or green, at their first vaccination, replaced by an identical bracelet at the fourth and fifth vaccine visits. We refer to this treatment as the "Initiation Reward". The second treatment included an additional incentive: children received a green bracelet, in exchange for the yellow initiation bracelet, when they completed the first four vaccinations on time. While the green bracelet was informative about the completion of later vaccines, it was not valued as a signal since parents assign little importance to the fourth vaccine. Instead, its value comes from clinic staff praising a parent for her compliance with the schedule, or from the novelty of receiving a different bracelet color. We refer to this treatment as the "Double Reward". In the third bracelet treatment, the yellow initiation bracelet is exchanged for a green bracelet when a child completed all vaccinations on time. Parents perceive the fifth vaccine as one of the most important vaccines, and the green bracelet provided a strong signal of a parent caring for their child's health. We refer to it as the "Signaling Reward".

The incentives were implemented over two years from July 2016 to August 2018. Health workers were trained to distribute bracelets to children according to the design rules of each treatment when they visited the clinic. The quality of incentive implementation was consistent across treatments, and parents in Double and Signaling reward communities had significantly more accurate beliefs about other children's vaccination status in their

full series.

community.

II.C Effects of Social Signals on Timely and Complete Vaccinations

The Signaling Reward was the most effective incentive, increasing the share of children who completed all vaccines in time by 13.3 percentage points ($p = 0.001$) and raising the average number of timely vaccinations by 9% (p=0.001). The incentive also had large positive effects on vaccine completion at 12 and 24 months, increasing the percentage of fully vaccinated children by 9.4 and 5.1 percentage points, respectively (p=0.005 and p=0.09). In contrast, the Initiation and Double Reward produced small and insignificant increases of 2.5% and 0.8% in the number of timely vaccinations received (p=0.34 and p=0.79), respectively.

This empirical setting is ideal for investigating our research questions for four reasons. First, it allows us to follow up with communities that experienced both the introduction and discontinuation of an incentive program. Second, this setting provides a context in which we do not expect incentives to influence long-term behavior through learning by doing, habit formation, or negatively updating from the incentive (Bénabou and Tirole 2003). This is supported by several key observations: most parents already believe vaccination is beneficial for their child, timely completion rates for the first three vaccines are consistently above 80%, and perceptions regarding the importance of different vaccines remained unchanged despite the incentives. Third, we can measure the long-term effects of the incentives on subsequent cohorts' vaccination outcomes and study whether parental behavior varies with the type of action incentivized–whether signaling intrinsic motivation or not– by leveraging the original random assignment. Finally, the incentive implementation mirrored that of a large-scale, real-world program: government health workers distributed the bracelets during routine immunization, and the incentive reached more than 35,000 children over a prolonged period of two years.

III Research Design

III.A Identifying Motivation Crowd-Out

Our research design relies on the comparison between directly and indirectly exposed parents. We define directly exposed parents as those who had a newborn child during the experiment and were therefore eligible for the incentive, experiencing its associated costs and benefits. Indirectly exposed parents lived in incentive treatment communities during the experiment but did not have a newborn at that time. Thus, they observed the bracelets but did not directly experience the associated costs and benefits.

HYPOTHESIS 1: *The experience of receiving an incentive reduces parents' intrinsic motivation to vaccinate their subsequent child.*

This hypothesis predicts that by providing an extrinsic reward for vaccination, parents shift their motivation from the intrinsic "joy" of vaccinating to a focus on the external reward (Deci 1971; Frey and Oberholzer-Gee 1997). One possible explanation is that parents misattribute their past timely vaccination behavior to the bracelet rather than to their own intrinsic motivation (Bénabou and Tirole 2003). We conclude that crowding-out is relevant if directly exposed parents vaccinate their subsequent child at lower rates once the incentives are removed, compared to those in the Control group and parents indirectly exposed to the incentives. These effects may vary by the type of incentive, with signaling incentives, helping to maintain intrinsic motivation and mitigate crowd-out.

HYPOTHESIS 2: *The removal of the incentive negatively affects parents' beliefs about the benefits and costs of vaccination, leading to lower vaccination rates.*

The hypothesis predicts that the removal of the incentive will cause both directly and indirectly exposed parents to update their beliefs about vaccination, interpreting the removal as a signal that vaccination is less important. The greater salience of the incentive to directly exposed parents may result in differences in beliefs updating and vaccination behavior between directly and indirectly exposed parents.

HYPOTHESIS 3: *The implementation and removal of the incentive leads to changes at the community or clinic level that affect vaccination behavior.*

The third hypothesis predicts that the introduction and removal of incentives will generate broader changes at the community or clinic level, affecting vaccination rates regardless of individual parents direct or indirect exposure to the incentive. Such changes could occur through shifts in social norms or changes in the behavior of health workers, such as reducing their efforts to promote vaccination. If significant differences in vaccination rates emerge between treatment and Control groups, with no significant heterogeneity by exposure, we would attribute the effects to community- or clinic-level mechanisms.[3]

III.B Data and Sample Definition

We conducted a follow-up survey in the 597 communities from the original study between December 2020 and August 2021. In this survey, we listed all children born and living in these communities since the experiment ended. Our methodology mirrored that of

[3] We assume that clinic health workers cannot systematically distinguish between parents who had a child during the experiment and those who did not. In this context, there is no digital system allowing health workers to easily access past birth records. Hence, if health workers respond to the removal of the incentives, we will observe similar patterns of treatment effects among parents with and without direct exposure.

the original study. The survey included questions about each childs vaccination history, bracelet ownership, knowledge of the bracelet program, and exposure to other immunization incentives. To confirm whether parents had previously been exposed to the program, we asked how long they had lived in the community and whether they had a newborn between 2016 and 2018. A total of 19,318 children were identified as eligible for inclusion in our study sample. Eligibility was defined as being born after December 1, 2017, and attending one of the 119 study clinics for immunizations.[4] Of these, 11,235 children were alive at the time of the survey, were old enough to have completed all vaccinations, and had their primary caregiver residing in one of the study communities, making it possible for us to collect their immunization data.

To estimate the persistent effects of incentives on subsequent generations' vaccinations, we need to observe behavior when bracelets are no longer distributed. However, despite the programs official end at the conclusion of the experiment (August 2018), we find that health workers continued to distribute bracelets for two additional years (Figure I).[5] This unanticipated continuation complicates the detection of persistent effects throughout the post-experiment period. We identify a threshold of one and a half years after the experiment ended (May 2019), after which children have only a 15% chance of receiving any bracelet. We focus our analysis on this period to test our hypotheses.

III.C Balance Checks

Tables I, II, and III display attrition rates and socio-demographic characteristics for our main analysis sample. Table I shows consistent survey success rates across control and treatment groups, ranging from 66.3% to 72.9% ($F = 1.09$, $p = 0.36$). The primary reasons for attrition include parents having moved (14%), the child being deceased (8%), and parents traveling at the time of the survey (7.9%). Notably, the attrition rates and their causes do not differ significantly between the control and incentive groups.

Table II shows that the sample of directly exposed parents is well balanced across all indicators, with the exception of a significant but small imbalance in the birth order of children. Double Reward parents have 5% fewer children ($p = 0.08$) and 2% fewer children ($p = 0.09$) on average than those in the Control group and Signaling Reward, respectively.

Table III shows two imbalances in the sample of indirectly exposed parents. First, children in the Double Reward group are, on average, 18, 16, and 16 days younger than those in the Control, Initiation Reward, and Signaling Reward groups, respectively. These imbalances are not substantial enough to affect outcomes, representing age differences of

[4] This date was chosen to ensure that children would have had at least one vaccine due when the bracelet program ended.

[5] The probability of receiving a bracelet is calculated as the share of children within a birth cohort and treatment arm who received a bracelet for immunization.

between 2% and 3%. Second, parents in the Double Reward group are less likely to be farmers by 17.4 percentage points ($p = 0.02$), 13 percentage points ($p = 0.04$), and 13.6 percentage points ($p = 0.05$), compared to those in the Control, Initiation Reward, and Signaling Reward groups, respectively. This is a noteworthy imbalance that warrants further analysis. Examining the distribution of this variable suggests that the imbalances are driven by two outlier clinics assigned to the Double Reward treatment. Tables B6 and B7 present balance checks excluding these clinics, and the significant differences on this variable disappear. To confirm that results are not influenced by the outlier clinics, we show estimates of our main specifications with these clinics removed.

Across all tables, 9.7% of comparisons are statistically significant at the 10% level, and 4.8% are significant at the 5% level, with F-tests for joint significance consistently yielding p-values above 0.10. We control for all variables exhibiting imbalances in more than one comparison, so they should not affect our results.

IV Do Incentives Affect Vaccination Choices Post-Removal?

IV.A Empirical Strategy

We examine how different types of incentive treatments impact parents vaccination decisions for their subsequent child, focusing on timely vaccination after the incentives are removed. Our analysis compares these effects to those in the Control group, based on whether parents were directly or indirectly exposed to the incentives. We further assess whether effects differ significantly between the two subgroups. Finally, we compare the impact of different types of incentives, distinguishing between those that emphasize signaling care for a child's health and those that do not. Our specification is as follows:

$$\text{Vaccine}_i = \alpha + \beta T_{j(i)} + \gamma Z_i + \theta (T_{j(i)} \times Z_i) + \delta X_i + \rho_{s(i)} + \varepsilon_i \tag{1}$$

where Vaccine_i denotes our primary outcome, the total number of vaccines a child has received on time.[6] $T_{j(i)}$ are treatment indicators for Double Reward, Signaling Reward, and the Initiation Reward assigned at the clinic level (j); Z_i indicates whether a child i's parent had a newborn during the experiment; X_i denotes the control variables of the distance from the community to the clinic, the clinic population size, the age of the child at the end of the data collection, the birth order of the child, and whether the parent is a farmer; and $\rho_{s(i)}$ denotes the strata fixed effects. Standard errors are cluster bootstrapped at the clinic level.

[6]Timely vaccination is defined as in Karing (2024). The timeliness definition is informed by WHO guidelines, which recommend that the DTP series be completed by six months of age (WHO 2018). While there are no strict guidelines for the measles vaccine, we apply the same definition, allowing for a 2.5-month buffer window.

We apply this same specification to additional outcomes. First, we estimate treatment effects on binary indicators for child i being vaccinated for each vaccine $a \in \{1, 2, 3, 4, 5\}$ in a timely manner–by three months for vaccine one, four months for vaccine two, five months for vaccine three, six months for vaccine four, and 11.5 months for vaccine five. Both timely vaccination outcomes capture parents' efforts to adhere to the vaccination schedule rather than choosing a more convenient time. In contrast, the second outcome examines the completion of all vaccines by 15 months, which is the due date for the second dose of the measles vaccine and the last in the schedule. This outcome captures potential shifts in attitudes, leading to parents skipping vaccinations altogether.

IV.B Persistent Effect of Incentives on Timely Vaccination

This section provides evidence that direct exposure to incentives decreases timely vaccination of subsequent children.

Table IV (Panel A, column 1) shows a common trend across all three incentive treatments, with effects varying in magnitude and significance. Specifically, parents who had a child during the experiment and were thus directly exposed to the bracelet incentives in the Initiation and Double Reward treatments complete 5% (p=0.05) and 11% (p<0.001) fewer vaccines on time, respectively, compared to parents in the Control group who had a child during the experiment. The Signaling Reward shows less pronounced and insignificant negative effects (-3%, p=0.29).[7]

Conversely, parents who were only indirectly exposed to the incentives do not show this decline, suggesting that the adverse effects are driven by the direct experience of receiving the incentive, rather than clinic- or community-level factors. Table IV (Panel B, column 1) displays the results for indirectly exposed parents, indicating minimal effects of incentive treatments on the total number of vaccines received on time: coefficients hover around zero (-0.14 to 0.002) and are insignificant (p=0.14-0.98). Further, we detect significant differences in treatment effects for parents with direct exposure to the Initiation and Double Reward incentives compared to those only indirectly exposed (p<0.001 and p=0.03, respectively).

The observed reduction in timely vaccination appears to be specifically linked to the use of bracelet incentives as extrinsic rewards. Examining the timely completion of specific vaccines in columns 2 to 6 of Table IV, we find that the Initiation Reward's negative effects are concentrated on the timely completion of the first and second vaccines with effects of -3.8 and 4.9 percentage points (p<0.01) but shows no adverse effects on later visits, where no additional reward was provided (between -3.9 to -5.7 percentage points, p=0.12-0.44). The Double Reward treatment exhibits a similarly negative pattern on

[7] We present estimates for specifications without controls on timely vaccination in Table B1, and find very similar results.

the first and second vaccines, with effects of -2.6 ($p<0.05$) and -7.2 ($p<0.01$) percentage points, respectively. The magnitude of these declines in timeliness for early vaccines in both of these treatments is particularly noteworthy, given the universal uptake in the Control group for the first vaccine (100%) and nearly universal uptake for the second vaccine (99%). We also find consistently negative effects of the Double Reward treatment throughout the rest of the vaccination schedule with a striking reduction of 12.1 ($p<0.05$) and 19.2 ($p<0.01$) percentage points in the timely completion of the fourth and fifth vaccines, respectively. This is consistent with the bracelet exchange occurring at the fourth visit in this treatment.

In contrast, the Signaling Reward does not result in strong negative effects: we detect marginally significant negative effects on the second vaccine (-4.7 percentage points, $p<0.01$), consistent with the presence of the initiation bracelet. However, we find no effects on later vaccines ($p=0.47$ for the fourth and $p=0.7$ for the fifth).

To verify the robustness of our results, we test whether alternative outcome definitions and samples affect our findings. Table B3 assesses treatment effects under either more lenient or stricter timeliness definitions.[8] The Double and Initiation Rewards' effects on vaccines one through three are robust to different timeliness definitions, albeit diminishing with the child's age.[9] We do not detect significant effects in timely vaccination outcomes in the Signaling Reward, suggesting that its negative impact is not robust to different definitions of timeliness.

We assess the sensitivity of our estimates to removing clinic-level outliers, identified based on vaccination rates for the first vaccine.[10] The results presented in Table B4 show negative treatment effects across all treatment arms, indicating that these effects are not driven by a few extreme clinics but are a general feature of incentive clinics in the post-experiment period. We also test removing two outlier clinics with a higher share of non-farming parents to address potential imbalance across treatment arms. The results remain stable with coefficients and significance levels consistent with the original analysis (Table B8).

We interpret these results as evidence that the experience of receiving rewards for initiating vaccination and completing the first four visits on time leads to reduced parental effort to vaccinate when the incentive is discontinued. Our results further rule out the hypothesis that changes in clinic staff behavior or shifts in community vaccination norms are driving effects on vaccination behavior.[11]

[8] We show treatment effects on the share of children completing the first three vaccines within 1.5, 3.5, and 5.5 months of the vaccines due dates.

[9] We observe an average reduction of of 37% and 53% for the Double and Initiation Reward, respectively, for every two-month increment in age.

[10] Figure A1 shows the distribution of clinic-level timely completion rates for each vaccine by treatment arm. Within treatment arms, outliers are determined as clinics with vaccination rates 1.5 times the inter-quartile range below or above the first or third quartiles, respectively.

[11] One possibility is that there were supply-side responses but they only influenced vaccination deci-

IV.C Persistent Effect of Incentives on Vaccine Completion

We then examine the completion of all vaccines by 15 months, which is the due date for the second measles dose and the next milestone in the immunization schedule. Panel A of Table V reveals that direct exposure to incentives does not have a pronounced effect on the completion of vaccinations by 15 months. The coefficients on the total number of vaccines received in the Initiation and Double Reward treatments are negative but insignificant at -0.12 (p=0.22) and -0.075 (p=0.55).[12]It is worth noting the magnitude of the effect on the fifth vaccine for parents directly exposed to the Double Reward treatment is high at -8.6 percentage points (p=0.15), which is likely due to less time having passed since that vaccine's due date (six months compared to over a year for the other vaccines). In line with these coefficients, we no longer detect significant differences in treatment effects between directly and indirectly exposed parents.

We interpret these results as evidence that the Initiation and Double Reward incentives lead parents to exert less efforts to vaccinate on time, but did not change vaccination attitudes and deter them from completing these vaccines altogether.

IV.D Heterogeneity and Subgroup Analysis

We test for treatment effect heterogeneity using a causal forest algorithm following the method of Wager and Athey (2018). We rank and group individual observations by predicted treatment effect and estimate Conditional Average Treatment Effects (CATEs) by quantiles. This approach allows us to identify data-driven subgroups and compare treatment effects across them. Furthermore, by examining covariate distributions within the subgroups, we can uncover differences in characteristics associated with higher and lower treatment effects. For each treatment, we estimate CATEs on the total number of timely vaccinations compared to the Control group, focusing on three samples: parents during the experiment period, directly exposed parents, and indirectly exposed parents in the post-experiment period. We use both OLS and Augmented Inverse Probability Weighting (AIPW) to estimate the effects.[13]

In each causal forest application, we use the same set of covariates, grouped into three categories based on their relevance to different behavioral mechanisms. First, distance

sions for parents directly exposed to the incentive. We examine differences in supply-side behaviors based on parents and clinic staff self-reports across control and incentive arms in Table B5. These surveys reveal few differences in parents' communication about vaccines, and the likelihood of receiving something from the nurse, which we see as unlikely to explain the negative effects on vaccination behavior.

[12]We present estimates for specifications without controls on timely vaccination in Table B2, and find very similar results.

[13]We report the AIPW estimates, standard errors, and p-values because they are consistent and efficient under conditions of non-random assignment, which may arise due to differential attrition between the original randomization and the follow-up survey. The AIPW method is doubly-robust, providing consistent estimates as long as either the outcome or treatment probability model is correctly specified.

to the clinic, the number of children living in the community, and the number of children a parent has affected the cost of vaccination through travel cost, the number of other parents with similarly aged children each caregiver might interact with and the opportunity cost of vaccination. Second, caregiver characteristics, namely age, education and number of children, reflect their experience and knowledge of childhood vaccination. Less experienced or knowledgeable caregivers may be more likely to update their beliefs about the value of vaccination when bracelets are removed. Third, we use how well a mother is dressed and clinic population size as indicators of potential differences in social interactions between caregivers and clinic staff. Anecdotal evidence suggests that nurses may treat less well-dressed mothers worse, being less kind and making them wait longer before their child is seen. This type of discrimination may also affect who nurses give a bracelet to. Clinic population size may influence the level of scrutiny mothers face. In smaller clinics, where nurses are more likely to recognize caregivers, the incentive's emphasis on timely vaccination may have led to increased scolding or scrutiny regarding vaccination decisions.

These covariates are observable by policymakers, making the identification of treatment heterogeneity particularly useful for guiding policy interventions. Understanding which types of caregivers are most at risk of negative outcomes when incentives are removed can help target efforts to mitigate these effects.

IV.D.1 Heterogeneity Results

We start by examining heterogeneity in treatment effects during the experiment and find no significant differences across clinic-, community-, or individual-level characteristics, as shown in Figures A3, A4, and A5. This suggests that the null short-term results are not masking detectable subgroups with positive and negative effects.

We find significant heterogeneity in treatment effects during the post-experiment period among directly exposed parents in both the Initiation and Double Reward treatments. Figures A6 and A8 present the CATE estimates across three terciles, highlighting notable differences between the first and third terciles in both treatments. For caregivers exposed to the Initiation Reward, the CATE for the first tercile is estimated to result in 0.41 fewer timely vaccinations compared to the Control group (p=0.012). This estimate is significantly different from that of the third tercile ($CATE = 0.06,\ p$=0.07).[14] We observe a similar pattern of heterogeneity in the Double Reward treatment, with stronger effects: the first tercile's CATE is estimated at 0.82 fewer timely vaccinations (p<0.001), and is significantly different from that of the third tercile ($CATE = -0.17,\ p$=0.001). In contrast, we detect no significant heterogeneity for the Signaling Reward treatment, as indicated by the lack of distinct or monotonic CATE estimates across terciles in Figure

[14] p-values are based on AIPW estimates, with adjustments for multiple hypothesis testing using the step-down correction procedure (Romano and Wolf 2005).

A10. This suggests that the null effect observed in this treatment does not mask persistent positive change among *marginal takers* –parents influenced by the bracelet during its implementation– combined with negative effects among *always takers* –parents who would have vaccinated regardless of the bracelet during its implementation.

We identify two covariates that vary monotonically with the Double and Initiation Reward CATEs and exhibit distinct levels between terciles in both treatments: number of children in the community and distance to the clinic (Figures A7 and A9). We observe the strongest negative treatment effects among directly exposed parents in the Initiation and Double Reward groups who live in small communities (mean size: $\mu_{IR} = 14$, $\mu_{DR} = 18$) and those located further away from clinics (mean distance: $\mu_{IR} = 2.3$ miles, $\mu_{DR} = 2.2$ miles). In contrast, parents in the third terciles, live in larger communities (mean population: $\mu_{IR} = 34$, $\mu_{DR} = 47$) that are closer to clinics (mean distance: $\mu_{IR} = 1.1$ miles, $\mu_{DR} = 1.1$ miles), with differences being significant at the 1% level. The Double Reward treatment also shows heterogeneity, with a higher number of children in the first tercile compared the third tercile (3.9 versus 3.5, $p < 0.001$). Additionally, nearly twice as many parents in the first tercile have no education compared to the third tercile ($\mu_{DR} = 0.58$ in T1 compared to $\mu_{DR} = 0.38$ in T3, $p < 0.001$).

We find significant heterogeneity in treatment effects among indirectly exposed parents for all three incentive treatments, with differences at the 1% level between the first and third terciles (Figures A12, A14 and A16). We estimate negative CATEs of 0.4 to 0.7 fewer timely vaccines for the first tercile and positive increases in the total number of timely vaccinations of 0.2 and 0.5 in the third tercile.[15]

There are several variables which covary with differences in CATEs across terciles for indirectly exposed parents (Figures A13, A15 and A17). Similar to the group of directly exposed parents, population size is tightly related to treatment effect heterogeneity: we find the strongest negative treatment effects for parents who reside in communities with fewer children ($\mu_{IR} = 18$, $\mu_{SR} = 14$) and attending smaller clinics ($\mu_{IR} = 59$, $\mu_{SR} = 57$) for the Initiation and Signaling Reward. These are significantly different from the third-tercile estimates for number of children in a community ($\mu_{IR} = 33$, $\mu_{SR} = 27$) and clinic population size ($\mu_{IR} = 90$, $\mu_{SR} = 95$) at the 1% level. In the Double Reward treatment, caregivers in the first tercile have, on average, more children ($\mu_{DR} = 4.2$), are older ($\mu_{DR} = 32$), and are more likely to have no education ($\mu_{DR} = 0.52$). This contrasts strongly with third-tercile caregivers, who have half as many children ($\mu_{DR} = 2.1$), are

[15]We have poor overlap of treatment and control over the span of our specified set of covariates for the Double and Signaling Reward treatments, driven by imbalances in clinic population size. To correctly assess heterogeneity in these cases, we estimate a causal forest on a subset of observations which have treatment propensity bounded away from 0 and 1. To estimate a propensity score, we separately implement a regression forest using our set of covariates to predict treatment status, and then drop observations with treatment propensity outside of the range $[0.02, 0.98]$. Monotonicity of CATEs and covariate trends are robust to changes in cutoffs or alternate procedures which drop observations at the clinic (treatment assignment) level rather than the individual level.

much younger ($\mu_{DR} = 24$) and are less likely to have no education ($\mu_{DR} = 0.35$). Differences are significant at the 1% level.

These results offer four key takeaways. First, there is significant heterogeneity in treatment effects among parents directly exposed to the Double and Initiation Reward incentives, with the strongest negative effects found in subgroups which face higher costs to get vaccinated. Second, there is no evidence of heterogeneity among parents directly exposed to the Signaling Reward treatment, suggesting there is no negative effects hidden by a persistent positive change for some parents in this group. Third, the absence of effects among indirectly exposed parents conceals both negative and positive treatment effects, indicating a shift in the composition of who vaccinates on time. Fourth, the covariates associated with negative effects differ by exposure, suggesting different mechanisms are at play.

V Do Incentives Crowd Out Intrinsic Motivation?

We explore potential mechanisms driving the negative effects on timely vaccination, using additional survey data and exploring results from the CATEs analysis.

V.A Experience of Incentives and Motivation Crowd-Out

Self-perception and memory: Exposure to incentives may have altered parents' perceptions of *why* they vaccinate their child. Bénabou and Tirole (2003)'s theory highlights individuals not always recalling the reason for taking an action as one of the main possible causes of motivation crowd-out. This is particularly relevant when there are intervals between instances of taking an action, which is the case for childhood immunization.

To examine this hypothesis, we first assess whether parents accurately remember their child's vaccination and whether they received anything for it–two important assumptions of the model. Table B12 shows that 83% of parents in the Control group surveyed during the follow-up recall the last vaccine their child received, and 94% of parents in the treatment groups report receiving a bracelet for their child's immunization. We detect no significant differences across arms. This demonstrates a high level of knowledge about prior actions and incentives, given that these questions pertain to children last due for a vaccine 3.5 years before the survey. Parents may misattribute their motivation to the reward rather than to concern for their child's health. Consequently, when the reward is no longer available for their next child, they may be less motivated to vaccinate on time.

Although we lack data on the specific reasons parents report for coming to immunize, our heterogeneity analysis suggests that this mechanism is relevant. We would expect parents with higher costs or lower valuation of vaccination to be more likely to deliberate at each decision point, and hence be more sensitive to changes in their memory of why they took an action. Figures A7 and A9 show that living in communities with fewer children and communities that are further away from the clinic is highly correlated with negative treatment effects. Parents living farther from the clinic face higher travel costs, and those in smaller communities have fewer other mothers to remind them of vaccinations or accompany them to the clinic, indicating higher effort costs. In the Double Reward treatment, having more children and less education is also correlated with more negative treatment effects. The former suggests a higher opportunity costs, in line with the distance and communities with fewer children. Education is more indicative of a parent's knowledge of vaccination. Parents with no education are less likely to understand the benefits of timely vaccination and might therefore be more likely to deliberate for a vaccine perceived as unimportant.

Future expectations: Another possible mechanism occurring while incentives are implemented is a shift in parents' expectations of it. Several pieces of evidence suggest that parents in our context are susceptible to changing their expectations of immunization visits as a result of the incentive. Table B11 shows that there is an established norm around caregivers giving something to the nurse when coming for immunization in our study context, with between 48% and 59% of caregivers report giving something across all treatment and exposure groups. However, there is no established norm for receiving gifts or incentives from nurses. Nurses may provide health-related items (e.g., bed nets, medicine) during clinic visits, though they do so regardless of immunization and unreliably, given that 27% of parents report not receiving them. Food, the only item given alongside immunization, is given inconsistently, with fewer than 20% of caregivers reporting having received it.[16]

The implementation of the bracelets disrupted this status quo by introducing a valuable gift, clearly tied to immunization and that is reliably given out for over two years. While parents in the Initiation and Double Reward treatments did not respond to the incentives during their implementation, there is some evidence that they valued the bracelets. We analyze the short-run effects of incentives by clinic distance to test whether the lack of treatment effect stems from a ceiling effect on earlier vaccines. Since initiation rates are lower in communities farther from the clinic, any consumption value assigned to the bracelet may reveal effects in this subsample. Table B9 shows significant coefficients for timely completion of vaccines two and three in distant communities, with increases of 4.3 and 5.8 percentage points ($p<0.1$). This suggests that parents valued the initiation bracelet, despite near-universal take-up of the first vaccines.[17]

[16]Food distribution is not concentrated in some clinics, rather given out to a small share of caregivers in the majority of clinics.

[17]The larger and significant effect on vaccine three (5.8 percentage points, $p < 0.1$) suggests positive spillover effects across vaccine visits. Parents who bring their child on time for the first dose of the

Turning to data on health workers' behavior, we observe that they continued implementation of the bracelet over two years beyond the official end of the program and monitoring visits. Given that the program was anecdotally reported as effortful to sustain, and that health workers behavior did not change in any way that would suggest they perceived it as beneficial to their work, the most plausible explanation for their continued effort is caregivers demanding bracelets. We therefore interpret the continued implementation as suggestive evidence of the change in caregivers' expectations, putting pressure on the health workers to continue handing them out.[18]

The absence of negative effects in the Signaling Reward treatment, where bracelets served as a signal of being a caring parent, is consistent with hypotheses about the memory and change in expectation. If parents exposed to the Signaling Reward misattributed their timely vaccination behavior to the signaling benefits or came to expect these benefits as part of the vaccination payoff, they may have remained motivated by the broader reputational benefits of vaccination, which persist even without the bracelets. For example, parents could have continued signaling their vaccination efforts to the community, by informing other caregivers about their actions for instance.

V.B Removal of Incentives and Beliefs Updating

Belief updating about the intrinsic benefits of vaccination: The removal of bracelets may have sent a negative signal to parents about the value of timely vaccination, altering their beliefs about its benefits.[19] Several pieces of evidence contradict this explanation. First, parents' beliefs about the costs and benefits of vaccination do not seem to be very sensitive to external incentives: at the end of two years of incentives exposure, we observe no difference in the perceived importance of different vaccines as well as the costs and benefits of vaccination in general in bracelets treatments compared to the control (Karing 2024). Second, in order for this to explain the strong effects among directly exposed parents compared to the null effects among the indirectly exposed parents it would have to be the case that there was very limited awareness of the bracelet program among indirectly exposed parents. Survey data however, suggests the contrary: between 70 and 80% of indirectly exposed parents had seen or heard of the bracelets, and over 40% knew the bracelets were related to vaccination (see Tables VII and VI). Third, if parents saw timely vaccination as less important after removal, we would expect

Pentavalent series (vaccine two) are more likely to do so for the next dose, due one month later.

[18]The slightly higher hand-out rate in the Signaling Reward treatment compared to the Double Reward treatment further supports this hypothesis, as it was more highly valued.

[19]The assumption here is that parents correctly interpret the progressive decline in the number of bracelets distributed as the incentive program being discontinued.

similar effects across all groups, but no significant negative effects appear in the Signaling Reward treatment. This may be because Measles 1, known as the "9-month marklet", has inelastic demand, though the Double Reward treatment shows that external incentives can still influence its timely completion.

Belief updating about the extrinsic benefits of vaccination: Another plausible explanation is that parents may have been uncertain about whether the program was still active, as the distribution of bracelets was gradually phased out (Figure I). This confusion may be particularly relevant to indirectly exposed parents who do not know the number of children expected to have a bracelet when the program is active. A negative effect could occur if parents have wrong inferences from the bracelets about the lower share of parents vaccinating their child on time, reducing social pressure to do so. A positive effect may be the result of forming wrong beliefs about the likelihood that a bracelet will be given when vaccinating their child on time. While the Initiation and Double Reward bracelets did not yield positive effects during the implementation, they may have been valued more highly when fewer of them were handed out. We do observe both positive and negative effects across all treatment in the results of the CATEs analysis for indirectly exposed parents (Figures A12, A14 and A16).

V.C Alternative Mechanisms

Reminder efforts: Parents may have substituted their personal efforts to remind themselves or seek out information from others about their child's vaccination dates with the use of bracelets. While this substitution would be undetectable during the experiment, parents who adopted this behavior for their previous child may not have reverted to their prior habits and, as a result, failed to remember their child's vaccination due date after the bracelets were removed. Table B10 presents an analysis of treatment effects on whether a parent reports having discussed or been reminded of their childs vaccination by anyone. We observe no treatment effects among either directly or indirectly exposed parents regarding whether they were reminded or by whom. However, parents in the Signaling and Double Reward treatments are more likely to report having discussed or been reminded by a broader range of sources. This suggests that parents are not less likely to be reminded, and if anything, are receiving reminders from a wider variety of sources.

Discrimination in bracelet distribution: An alternative explanation is that parents recognized that the program was ending, and that they were discouraged to take their child for vaccination as only certain parents received a bracelet for their child. Since only 15% of children received a bracelet post-experiment (Figure I), parents may have suspected favoritism from nurses, leading to disappointment. However, bracelet receipt data show that both directly and indirectly exposed parents had equal chances of receiving a bracelet (Table VII, column 1), making this explanation unlikely.

The results of our mechanism exploration suggest that the experience of receiving an incentive drives the negative effects on vaccination behavior, through a crowd-out of intrinsic motivation. We also find positive and negative effects among indirectly exposed parents, suggesting the progressive phase-out of the program may have led parents to form incorrect beliefs about the bracelets or vaccination behaviors.

VI Conclusion

This study investigates the impact of introducing and subsequently removing incentives, specifically color-coded bracelets, on parents vaccination decisions in Sierra Leone. We find that extrinsic incentives can undermine parental motivation to vaccinate their children on time. This is demonstrated by the fact that parents who received bracelets during the experiment are less likely to vaccinate their subsequent child on time compared to those in the Control Group, once the bracelets are no longer in use. To our knowledge, this is the first study to provide evidence of motivation crowd-out from incentives after their removal in a highly policy-relevant setting.

The effects are large in magnitude, comparable to the impact of social signals during their active phase, and persist for up to two to three years after exposure. Importantly, we find no evidence of negative impacts on vaccination completion by 15 months, indicating that incentives primarily affected parents' motivation to vaccinate their child in a timely manner. Parents who live in communities where incentives were implemented but were not eligible to receive them appear to be largely unaffected, suggesting that merely observing or hearing about an incentive does not affect motivation.

These findings are particularly striking given that two of the bracelet incentives did not produce an effect during their implementation. Our results suggest that changes in parents' self-perception or expectations of vaccination are likely driving the reduction in motivation. This presents a challenge for policymakers, as it implies that crowding-out of intrinsic motivation may not be immediately evident during the implementation of incentives but can persist long after their discontinuation. Our findings highlight the need to assess how incentives designed to increase the uptake of positive behaviors may impact those who are already performing the desired action.

The observed reduction in parental motivation seems to be linked to extrinsic incentives that carry no signaling value. By contrast, incentives that allowed parents to signal their concern for their childs health did not undermine intrinsic motivation or produce pronounced negative effects. This suggests that social image-based incentives may mitigate the crowding-out of motivation and offer a promising strategy for promoting desirable behaviors.

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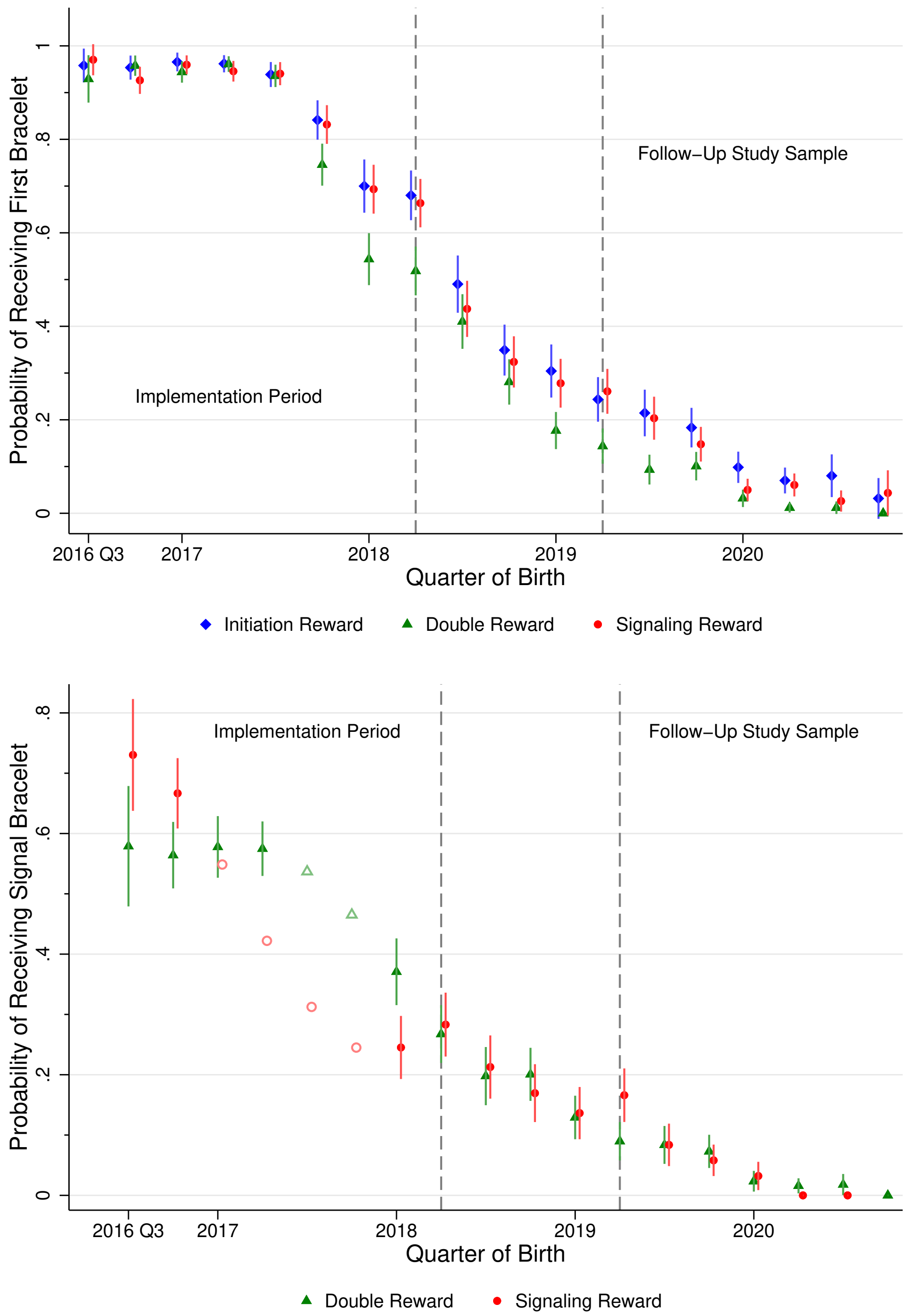

Figure I: Share of Children Receiving a First and Signaling Bracelet over Time by Treatment

Notes: For the figures above we restricted the sample to children that were born after the launch of the project, that have received at least one vaccine and primarily attend a project clinic for immunization. The figures plot the share of children who received their first (top) or signal bracelet (bottom), provided that they received the corresponding vaccines in a timely manner. In the second plot, hollow data points represent quarter observations which are estimated via cubic spline interpolation. The dashed lines represent endline data collection from the original experiment in Karing (2024) and the beginning of the post-experiment sample period t=2, respectively.

Table I: Sample Characteristics and Attrition

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
					t-test differences			
	Control				**[p-value]**			
Variable	**Mean/(SE)**	**(C)-(IN)**	**(C)-(D)**	**(C)-(SL)**	**(IN)-(D)**	**(IN)-(SL)**	**(D)-(SL)**	**F-Test**
Panel A: Sample Definition								
Regular listed child	0.729	0.027	0.066	0.013	0.039	-0.013	-0.052	1.093
	(0.016)	[0.351]	[0.084]	[0.811]	[0.342]	[0.320]	[0.109]	[0.355]
Moved child	0.125	-0.013	-0.032	-0.012	-0.019	0.000	0.020	0.380
	(0.013)	[0.569]	[0.250]	[0.547]	[0.600]	[0.847]	[0.384]	[0.767]
Traveled child	0.071	-0.003	-0.022	-0.001	-0.019	0.003	0.021	0.629
	(0.007)	[0.773]	[0.325]	[0.832]	[0.220]	[0.662]	[0.144]	[0.598]
Deceased child	0.075	-0.011	-0.012	-0.000	-0.001	0.011	0.012	0.870
	(0.006)	[0.291]	[0.202]	[0.880]	[0.940]	[0.161]	[0.259]	[0.459]
Clinic population	176.914	7.683	-107.984	-10.568	-115.667	-18.250	97.416	1.635
	(19.961)	[0.701]	[0.054]	[0.579]	[0.028]	[0.384]	[0.074]	[0.185]
Number of observations	4252	8737	10065	9020	10298	9253	10581	19318
Number of clusters	30	60	60	59	60	59	59	119

Notes: This table summarizes attrition indicators between treatment arms for the full sample of parents eligible for the follow-up study. It displays the share of parents who we were able to capture immunization data for, and the share of parents we were not able to survey for each of the following reasons: the parents moved to a different community, the parents are currently traveling and unavailable to respond to the survey or the child is deceased.

Table II: Balance Checks on Characteristics of Parents with Direct Exposure

Variable	(1) Control Mean/(SE)	(2) (C)-(IN)	(3) (C)-(D)	(4) t-test differences [p-value] (C)-(SL)	(5) (IN)-(D)	(6) (IN)-(SL)	(7) (D)-(SL)	(8) F-Test
Panel B: Characteristics of Parents With Direct Exposure								
Child's age (end of follow-up)	662.640	12.406	8.001	9.247	-4.405	-3.160	1.245	0.843
	(8.013)	[0.202]	[0.177]	[0.112]	[0.472]	[0.664]	[0.762]	[0.473]
Good vaccine data source	0.907	0.048	0.058	0.015	0.010	-0.033	-0.043	1.030
	(0.022)	[0.224]	[0.133]	[0.648]	[0.881]	[0.469]	[0.254]	[0.382]
Temne Ethnicity	0.654	0.009	0.055	-0.016	0.046	-0.026	-0.071	0.508
	(0.091)	[0.925]	[0.257]	[0.803]	[0.265]	[0.915]	[0.589]	[0.678]
Limba Ethnicity	0.234	0.097	0.035	0.077	-0.061	-0.020	0.042	0.462
	(0.086)	[0.480]	[0.754]	[0.590]	[0.317]	[0.701]	[0.918]	[0.710]
Mothers' age (in years)	28.346	-0.445	0.173	0.278	0.617	0.722	0.105	0.905
	(0.375)	[0.328]	[0.754]	[0.681]	[0.279]	[0.052]	[0.669]	[0.441]
Number of children	3.743	0.066	0.187	0.104	0.121	0.039	-0.083	1.662
	(0.079)	[0.533]	[0.076]	[0.271]	[0.176]	[0.527]	[0.094]	[0.179]
Well dressed	0.621	-0.044	-0.075	-0.093	-0.031	-0.050	-0.018	0.270
	(0.084)	[0.596]	[0.691]	[0.639]	[0.887]	[0.800]	[0.804]	[0.847]
No education	0.458	-0.034	-0.004	0.012	0.030	0.046	0.016	0.475
	(0.049)	[0.136]	[0.421]	[0.713]	[0.580]	[0.464]	[0.876]	[0.700]
Some primary education	0.355	0.004	0.023	0.038	0.019	0.034	0.015	0.397
	(0.040)	[0.781]	[0.595]	[0.135]	[0.654]	[0.210]	[0.608]	[0.755]
At least secondary education	0.187	0.030	-0.019	-0.050	-0.049	-0.080	-0.031	1.906
	(0.033)	[0.114]	[0.582]	[0.229]	[0.209]	[0.016]	[0.423]	[0.133]
Caregiver is a farmer	0.795	0.019	0.103	0.025	0.084	0.006	-0.078	1.097
	(0.050)	[0.966]	[0.140]	[0.351]	[0.134]	[0.323]	[0.276]	[0.353]
Travels outside community	0.551	0.013	-0.098	-0.080	-0.110	-0.093	0.018	0.416
	(0.082)	[0.935]	[0.639]	[0.440]	[0.413]	[0.387]	[0.974]	[0.742]
Num. children in community	25.042	2.736	-13.092	1.175	-15.827	-1.561	14.266	1.850
	(3.042)	[0.148]	[0.082]	[0.563]	[0.037]	[0.164]	[0.058]	[0.142]
Number of observations	214	462	491	463	525	497	526	988
Number of clusters	29	59	59	57	60	58	58	117

Notes: This table summarizes relevant sample characteristics between treatment arms for parents with direct exposure to incentives. The table shows balance for the sample used in the main specification of Table IV, Panel A. Number of children in the community is calculated using data for all children born starting 60 days before the original implementation start date and January 2018.

Table III: Balance Checks on Characteristics of Parents with Indirect Exposure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				t-test differences				
	Control			[p-value]				
Variable	**Mean/(SE)**	**(C)-(IN)**	**(C)-(D)**	**(C)-(SL)**	**(IN)-(D)**	**(IN)-(SL)**	**(D)-(SL)**	**F-Test**
Panel C: Characteristics of Parents With Indirect Exposure								
Child's age (end of follow-up)	685.599	2.042	18.016	2.173	15.974	0.131	-15.843	1.988
	(6.698)	[0.531]	[0.023]	[0.507]	[0.008]	[0.890]	[0.029]	[0.120]
Good vaccine data source	0.877	-0.018	-0.009	0.005	0.010	0.024	0.014	0.178
	(0.016)	[0.738]	[0.601]	[0.565]	[0.904]	[0.533]	[0.642]	[0.911]
Temne Ethnicity	0.631	0.029	0.111	0.015	0.082	-0.014	-0.095	0.626
	(0.081)	[0.765]	[0.255]	[0.769]	[0.082]	[0.826]	[0.486]	[0.599]
Limba Ethnicity	0.211	0.032	-0.040	0.031	-0.072	-0.001	0.071	0.392
	(0.067)	[0.618]	[0.770]	[0.821]	[0.226]	[0.907]	[0.500]	[0.759]
Mothers' age (in years)	27.553	-0.150	0.478	0.058	0.628	0.208	-0.420	0.613
	(0.349)	[0.743]	[0.407]	[0.809]	[0.199]	[0.540]	[0.535]	[0.608]
Number of children	3.096	0.044	0.169	0.082	0.125	0.038	-0.087	1.225
	(0.062)	[0.673]	[0.064]	[0.212]	[0.159]	[0.368]	[0.534]	[0.304]
Well dressed	0.683	-0.054	-0.095	-0.031	-0.041	0.023	0.064	0.542
	(0.052)	[0.356]	[0.320]	[0.479]	[0.640]	[0.882]	[0.590]	[0.654]
Stayed in community < 2 yr.	0.079	-0.013	-0.053	-0.019	-0.039	-0.006	0.034	1.740
	(0.013)	[0.649]	[0.056]	[0.233]	[0.058]	[0.342]	[0.258]	[0.163]
No education	0.471	0.030	0.082	0.053	0.052	0.023	-0.029	1.668
	(0.025)	[0.421]	[0.045]	[0.191]	[0.340]	[0.549]	[0.516]	[0.178]
Some primary education	0.273	-0.000	-0.013	-0.004	-0.013	-0.004	0.009	0.312
	(0.021)	[0.819]	[0.349]	[0.908]	[0.768]	[0.819]	[0.365]	[0.816]
At least secondary education	0.256	-0.030	-0.069	-0.049	-0.039	-0.019	0.020	0.959
	(0.027)	[0.552]	[0.184]	[0.136]	[0.546]	[0.454]	[0.863]	[0.415]
Caregiver is a farmer	0.726	0.044	0.174	0.038	0.130	-0.006	-0.136	2.128
	(0.038)	[0.705]	[0.017]	[0.189]	[0.038]	[0.523]	[0.048]	[0.100]
Travels outside community	0.586	0.072	-0.047	-0.025	-0.118	-0.097	0.021	0.613
	(0.070)	[0.360]	[0.754]	[0.774]	[0.251]	[0.234]	[0.862]	[0.608]
Num. children in community	23.850	-0.943	-22.587	1.491	-21.644	2.435	24.078	1.559
	(2.806)	[0.837]	[0.048]	[0.493]	[0.047]	[0.868]	[0.029]	[0.203]
Number of observations	845	1686	1912	1771	1908	1767	1993	3679
Number of clusters	30	60	60	59	60	59	59	119

Notes: This table summarizes relevant sample characteristics between treatment arms for parents with indirect exposure to incentives. The table shows balance for the sample used in the main specification of Table IV, Panel B. Number of children in the community is calculated using data for all children born starting 60 days before the original implementation start date and January 2018.

Table IV: Effects of Removing Incentives on Timely Vaccination, by Type of Exposure

Dependent variable:	**Total # of vaccines timely**	**1st Vaccine**	**2nd Vaccine**	**3rd Vaccine**	**4th Vaccine**	**5th Vaccine**
Panel A: Direct Exposure (DE)						
Signaling Reward	-0.140	-0.011	-0.047***	-0.022	-0.040	-0.021
	(0.136)	(0.008)	(0.017)	(0.031)	(0.057)	(0.057)
Double Reward	-0.505***	-0.026**	-0.072***	-0.094**	-0.121**	-0.192***
	(0.136)	(0.011)	(0.019)	(0.039)	(0.059)	(0.060)
Initiation Reward	-0.232*	-0.038***	-0.049***	-0.049	-0.039	-0.057
	(0.123)	(0.011)	(0.014)	(0.031)	(0.053)	(0.052)
Control Group Mean	4.131	0.980	0.938	0.837	0.714	0.662
Panel Obs.	988	988	988	988	988	988
Panel Num. Clinics	117	117	117	117	117	117
Panel B: Indiriect Exposure (IE)						
Signaling Reward	0.002	0.002	0.002	0.001	-0.006	0.004
	(0.103)	(0.006)	(0.012)	(0.026)	(0.034)	(0.041)
Double Reward	-0.144	-0.018**	-0.015	-0.025	-0.042	-0.045
	(0.099)	(0.008)	(0.014)	(0.025)	(0.032)	(0.040)
Initiation Reward	-0.020	-0.002	-0.007	0.016	-0.015	-0.013
	(0.091)	(0.006)	(0.012)	(0.022)	(0.033)	(0.037)
Control Group Mean	4.294	0.985	0.954	0.884	0.797	0.674
Panel Obs.	3679	3679	3679	3679	3679	3679
Panel Num. Clinics	119	119	119	119	119	119
p(Signaling × DE = 0)	0.142	0.109	0.004	0.409	0.492	0.564
p(Double × DE = 0)	<0.001	0.398	0.003	0.015	0.086	0.003
p(Initiation × DE = 0)	0.028	<0.001	0.005	0.018	0.615	0.324
p(Initiation × DE = Signaling × DE)	0.455	0.044	0.915	0.375	0.995	0.496
p(Double × DE = Signaling × DE)	0.006	0.260	0.280	0.040	0.115	0.005
p(Initiation × DE = Double × DE)	0.015	0.399	0.283	0.230	0.068	0.017
Observations	4667	4667	4667	4667	4667	4667
Number of Clinics	119	119	119	119	119	119
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table displays the heterogeneity of incentives treatment effects on timely vaccination in the post-experiment period from Equation 1, by type of exposure. Exposure is direct if the child has an older sibling born during the experiment and indirect if they live in former incentive communities but do not have an older sibling born during the experiment. The outcome in each column is the difference in timely vaccination by the age of 3, 4, 5, 6 and 11.5 months, respectively *compared to the Control group within the same panel.* For a child to be coded as timely for a given number of vaccines, they need to have been timely for only the indicated vaccine, regardless of timeliness for earlier vaccines. We include children born after May 1, 2019, who were at least 12 months old by the time last observed.

We include children born after May 1, 2019, who were at least 12 months old by the time last observed. The p-values are reported below Panel B for interaction coefficients for each treatment arm with direct exposure, followed by p values for difference in means between directly exposed children between treatment arms.

We control for the distance from the community to the clinic, the clinic population size, the age of the child at the end of the data collection, the order of the child compared to potential siblings, and whether the parent is a farmer. All regressions include strata-fixed effects. Standard errors are cluster bootstrapped (500 repetitions) at the clinic level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table V: Effects of Removing Incentives on Vaccination at 15 months, by Type of Exposure

Dependent variable:	Total # of vaccines by 15 months	1st Vaccine	2nd Vaccine	3rd Vaccine	4th Vaccine	5th Vaccine
Panel A: Direct Exposure (DE)						
Signaling Reward	0.053	0.001	0.013	-0.003	0.020	0.022
	(0.098)	(0.001)	(0.013)	(0.020)	(0.034)	(0.051)
Double Reward	-0.119	-0.001	-0.002	-0.022	-0.009	-0.086
	(0.112)	(0.001)	(0.014)	(0.023)	(0.034)	(0.060)
Initiation Reward	-0.075	-0.007	-0.007	-0.009	-0.027	-0.025
	(0.112)	(0.007)	(0.019)	(0.024)	(0.036)	(0.050)
Control Group Mean	4.603	0.998	0.980	0.950	0.905	0.771
Panel Obs.	597	597	597	597	597	597
Panel Num. Clinics	113	113	113	113	113	113
Panel B: Indirect Exposure (IE)						
Signaling Reward	0.055	0.004	0.006	0.002	0.018	0.026
	(0.068)	(0.002)	(0.005)	(0.013)	(0.023)	(0.037)
Double Reward	-0.035	0.002	-0.007	-0.004	0.001	-0.028
	(0.071)	(0.003)	(0.007)	(0.011)	(0.021)	(0.041)
Initiation Reward	-0.006	0.001	0.002	0.007	0.008	-0.023
	(0.061)	(0.003)	(0.005)	(0.011)	(0.020)	(0.034)
Control Group Mean	4.653	0.998	0.989	0.964	0.921	0.782
Panel Obs.	2529	2529	2529	2529	2529	2529
Panel Num. Clinics	119	119	119	119	119	119
p(Signaling × DE = 0)	0.982	0.145	0.600	0.745	0.941	0.940
p(Double × DE = 0)	0.406	0.429	0.765	0.376	0.732	0.297
p(Initiation × DE = 0)	0.487	0.243	0.647	0.431	0.297	0.972
p(Initiation × DE = Signaling × DE)	0.219	0.273	0.253	0.800	0.165	0.323
p(Double × DE = Signaling × DE)	0.099	0.465	0.229	0.422	0.391	0.055
p(Initiation × DE = Double × DE)	0.710	0.331	0.789	0.647	0.617	0.285
Observations	3126	3126	3126	3126	3126	3126
Number of Clinics	119	119	119	119	119	119
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table displays the heterogeneity of incentives treatment effects on vaccine completion by 15 months of age in the post-experiment period from Equation 1, by type of exposure. Exposure is direct if the child has an older sibling born during the experiment and indirect if they live in former incentive communities but do not have an older sibling born during the experiment. The outcome in each column is the difference in vaccination by the age of 15 months *compared to the Control group within the same panel.*

We include children born after May 1, 2019, who were at least 12 months old by the time last observed. The p-values are reported below Panel B for interaction coefficients for each treatment arm with direct exposure, followed by p values for difference in means between directly exposed children between treatment arms.

We control for the distance from the community to the clinic, the clinic population size, the age of the child at the end of the data collection, the order of the child compared to potential siblings, and whether the parent is a farmer. All regressions include strata-fixed effects. Standard errors are cluster bootstrapped (500 repetitions) at the clinic level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table VI: Knowledge of Bracelet Meaning in the Post-Experiment Period, by Treatment

Dependent variable:	**Seen or heard of Bracelets** (1)	**Vaccine Related Meaning** (2)	**Don't know or Remember** (3)	**Incorrect Meaning** (4)
Panel A: Direct Exposure				
Signaling Reward	-0.038	-0.018	-0.013	0.028
	(0.025)	(0.065)	(0.041)	(0.039)
Double Reward	-0.021	0.014	-0.013	-0.023
	(0.025)	(0.059)	(0.048)	(0.034)
Initiation Reward Mean	0.950	0.629	0.194	0.163
Panel B: Indirect Exposure				
Signaling Reward	-0.041	0.021	-0.072	0.049
	(0.038)	(0.044)	(0.045)	(0.034)
Double Reward	-0.080**	-0.011	-0.009	0.030
	(0.039)	(0.046)	(0.039)	(0.035)
Initiation Reward Mean	0.815	0.408	0.348	0.208
p(Signaling × Direct Exposure = 0)	0.920	0.428	0.189	0.559
p(Double × Prior Exposure = 0)	0.056	0.609	0.933	0.077
p(Initiation × Direct Exposure = 0)	<0.001	<0.001	<0.001	0.036
Observations	3608	2908	2908	2908
Number of Clinics	89	89	89	89
Controls	Yes	Yes	Yes	Yes

Notes: This table shows estimated treatment effects of intervention arms and direct exposure on the perceived bracelet meanings for children born in the post-experiment period, *compared to the Control group within the same panel.* We include children who were born on and after May 1st, 2019 and were at least 12 months old by the time last observed, and has either seen or heard of the bracelets.

We include children born after May 1, 2019, who were at least 12 months old by the time last observed. The p-values are reported below Panel B for interaction coefficients for each treatment arm with direct exposure, followed by p values for difference in means between directly exposed children between treatment arms.

We control for the distance from the community to the clinic, the clinic population size, the age of the child at the end of the data collection, the order of the child compared to potential siblings, and whether the parent is a farmer. All regressions include strata-fixed effects. Standard errors are cluster bootstrapped (500 repetitions) at the clinic level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table VII: Bracelet Retention and Awareness in the Post-Experiment Period

Dependent variable:	Child received a bracelet (1)	Child received a yellow bracelet (2)	Child received a green bracelet (3)	Seen or heard of bracelets (4)	Child still has bracelet (5)
Panel A: Prior Exposure					
Signal at 5	-0.001	0.034	-0.001	-0.042	0.225**
	(0.047)	(0.043)	(0.031)	(0.026)	(0.104)
Signal at 4	-0.066*	-0.035	-0.008	-0.017	-0.079
	(0.036)	(0.031)	(0.028)	(0.025)	(0.100)
Uninformative Group Mean	0.203	0.135	0.087	0.926	0.281
Panel B: No Prior Exposure					
Signal at 5	0.010	0.043	-0.008	-0.036	0.053
	(0.041)	(0.034)	(0.018)	(0.037)	(0.080)
Signal at 4	-0.069**	-0.038	-0.014	-0.077**	-0.107
	(0.033)	(0.024)	(0.020)	(0.037)	(0.070)
Uninformative Group Mean	0.169	0.108	0.070	0.818	0.352
p(S5 × Prior Exposure = 0)	0.774	0.822	0.818	0.859	0.107
p(S4 × Prior Exposure = 0)	0.923	0.950	0.822	0.050	0.808
p(UI × Prior Exposure = 0)	0.271	0.346	0.490	<0.001	0.325
p(S4 × No Prior Exposure = S5 × No Prior Exposure)	0.014	0.011	0.636	0.272	0.026
p(S4 × Prior Exposure = S5 × Prior Exposure)	0.118	0.086	0.753	0.334	0.006
Observations	3608	3608	3608	3608	553
Number of Clinics	89	89	89	89	82
Controls	Yes	Yes	Yes	Yes	Yes

Notes: This table displays the heterogeneity of incentives treatment effects on bracelet retention and observability in the post-experiment period, by whether or not the child has an older sibling born during the experiment. The outcome in each column is whether a child still has a bracelet or a caregiver has heard of or seen bracelets, *compared to the Control group within the same panel.*

We include children born after May 1, 2019, who were at least 12 months old by the time last observed. The *p*-values are reported below Panel B for interaction coefficients for each treatment arm with direct exposure, followed by *p* values for difference in means between directly exposed children between treatment arms.

We control for the distance from the community to the clinic, the clinic population size, the age of the child at the end of the data collection, the order of the child compared to potential siblings, and whether the parent is a farmer. All regressions include strata-fixed effects. Standard errors are cluster bootstrapped (500 repetitions) at the clinic level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Incentives and Motivation Crowd-Out:
Experimental Evidence from Childhood Immunization
Online Appendix

Anne Karing, Juliette Finetti and Zachary Kuloszewski

October 2024

Contents

A Online Only Supplementary Figures

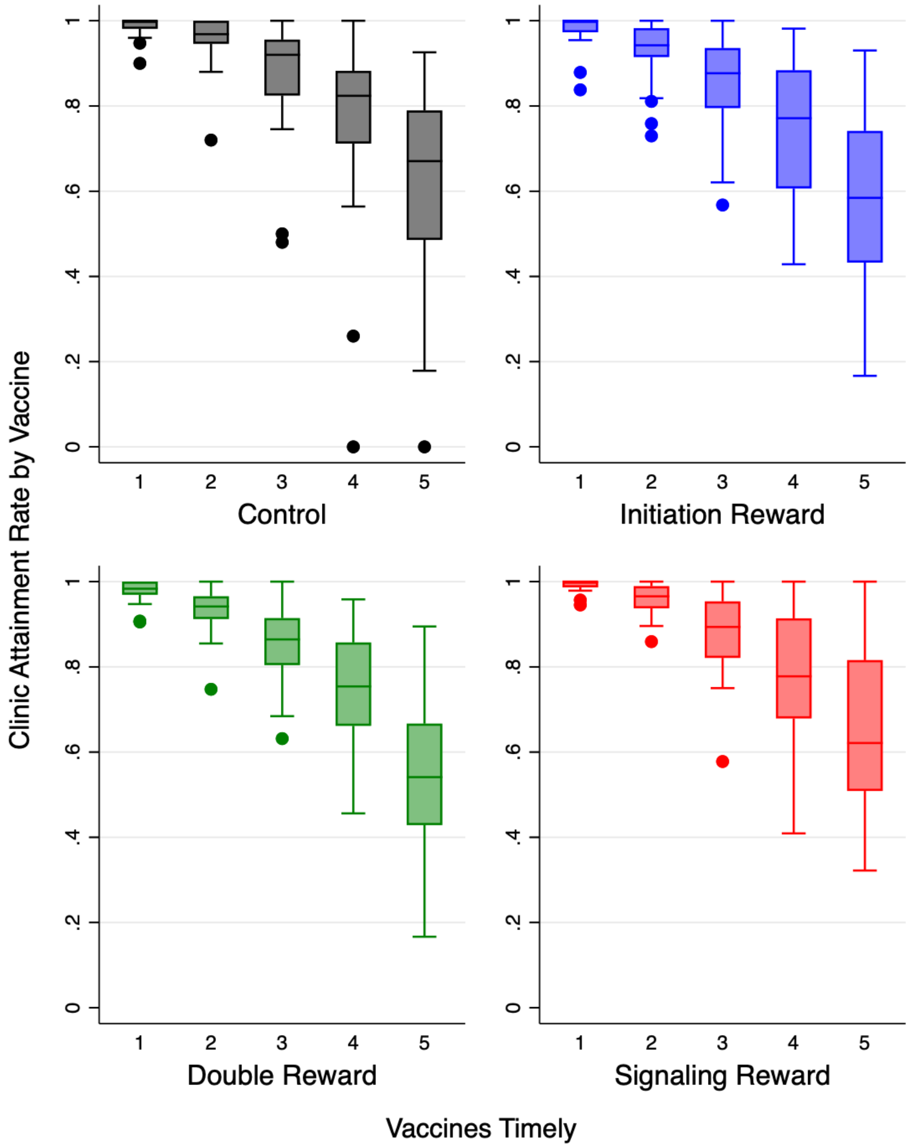

Figure A1: Clinic-Level Timely Vaccination Rate by Vaccine

Notes: This figure shows the distribution of clinic-level proportion of children receiving each vaccine timely by treatment arm in the period more than 1 year post experiment. Clinics are considered outliers if they are more than 1.5 times the IQR below the 1st quartile value for vaccination rates within the same treatment arm. The outlier clinic in the control arm which attains 0% timely vaccinations for 4 and 5 vaccines is a small clinic for which we observe only two children. This selection rule removes 10 clinics: three each from Double Reward, Signaling Reward, and Control, and one from Initiation Reward. We also assess the presence of outliers based on the distribution across control and treatment arms. This alternative definition results in eight outlier clinics: five in Double Reward and three in Initiation Reward. Estimated treatment effects when dropping those are similar to the analysis we present in Table B4.

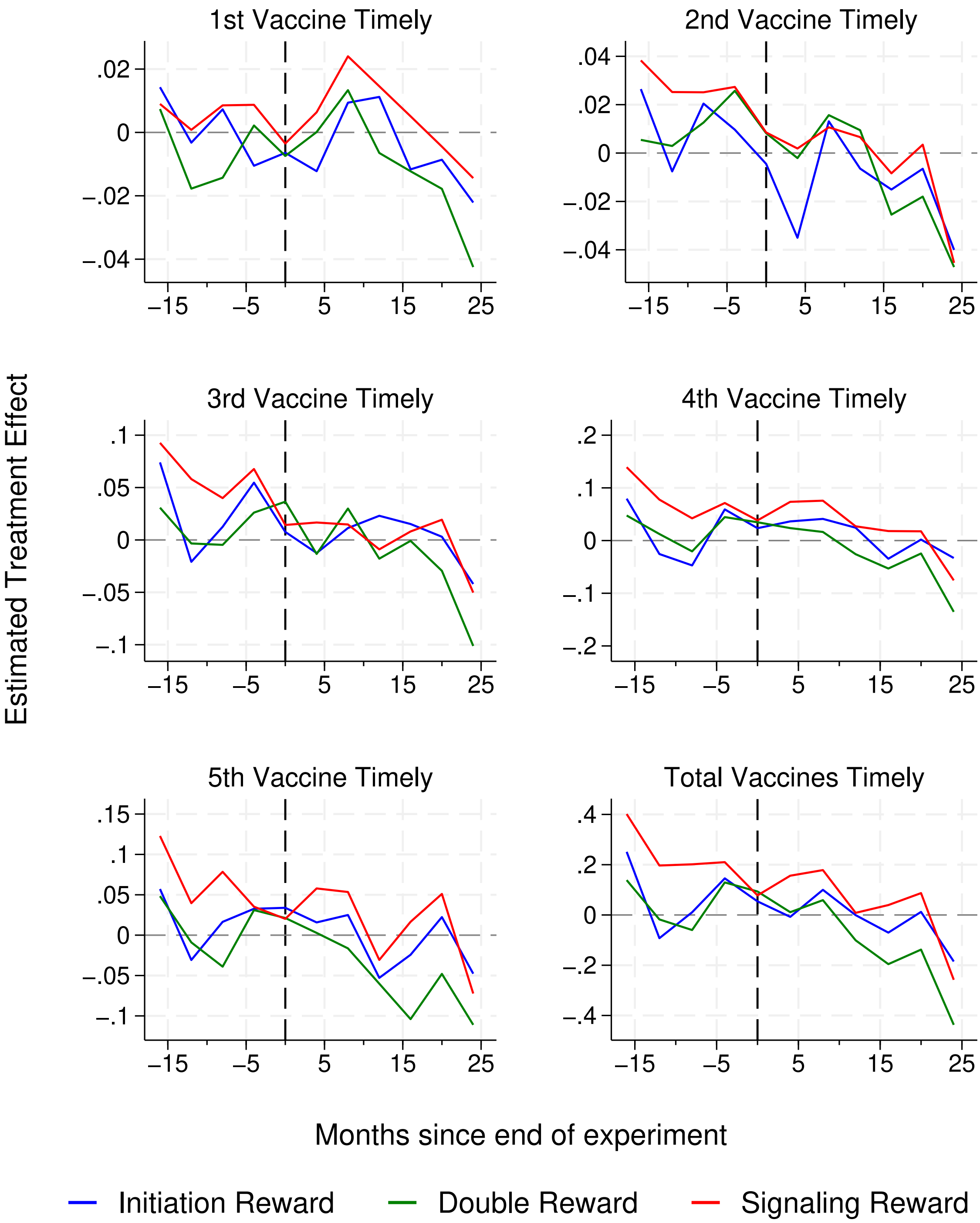

Figure A2: Estimated Treatment Effects by Birth Cohort

Notes: Each of the figures above reports treatment for all children in the original experiment and post-implementation period, grouped into 4-month cohorts. The dashed line represents endline data collection from the original experiment in Karing (2024).

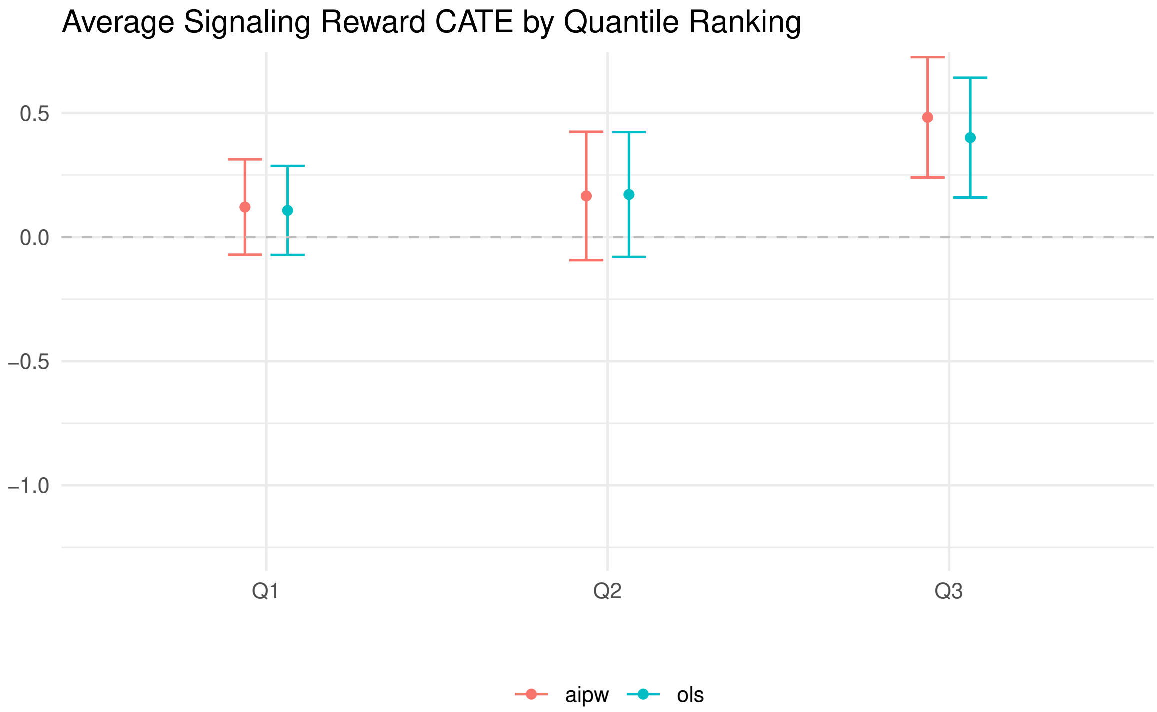

Figure A3: CATE Estimates for Signaling Bracelet Reward

Notes: This figure shows average Conditional Average Treatment Effects (CATEs) estimated by a causal forest where treatment defined as receiving Signaling Reward and outcome defined as total number of vaccines timely.

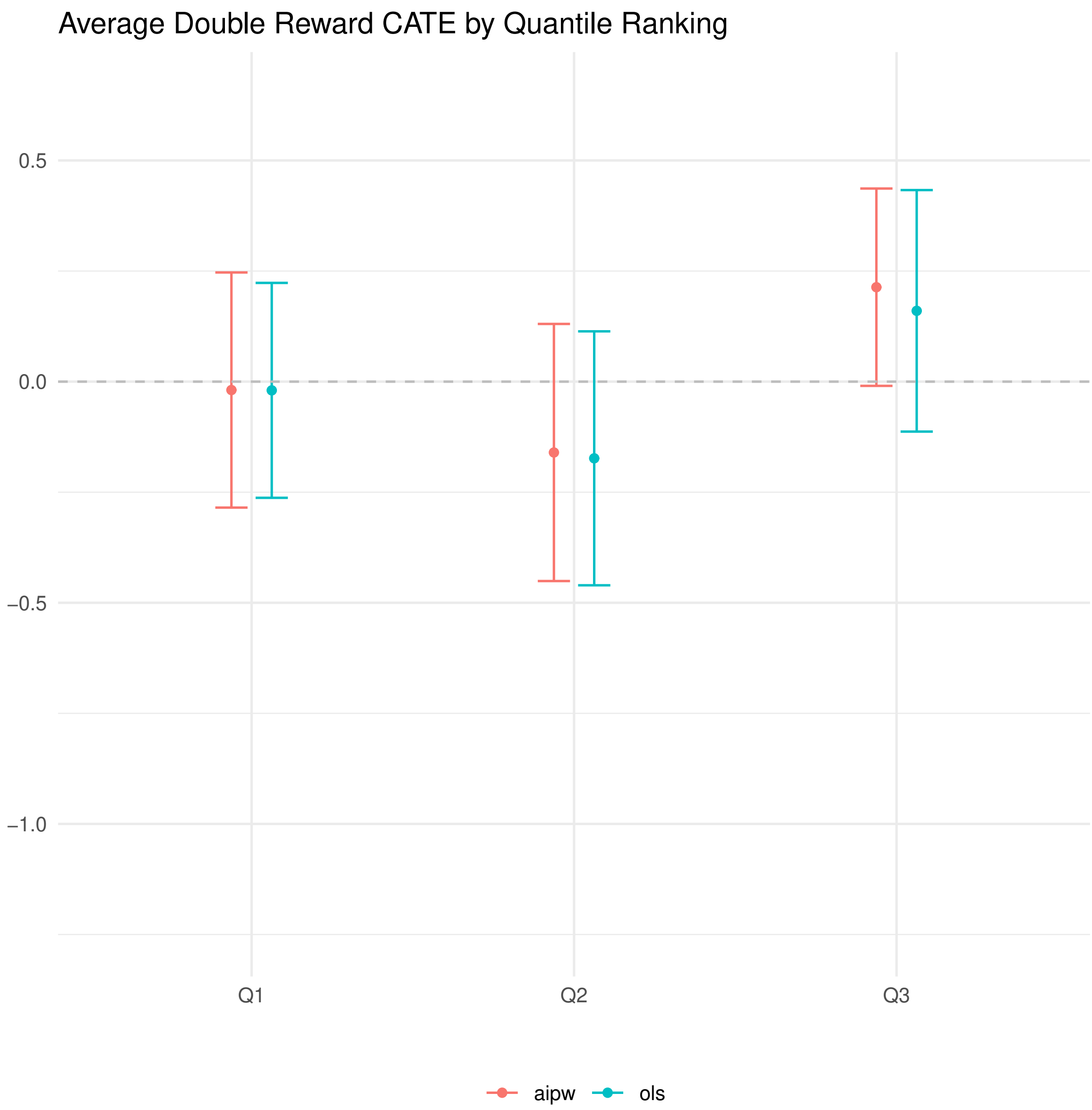

Figure A4: CATE Estimates for Double Bracelet Reward

Notes: This figure shows average Conditional Average Treatment Effects (CATEs) estimated by a causal forest where treatment defined as receiving Double Reward and outcome defined as total number of vaccines timely.

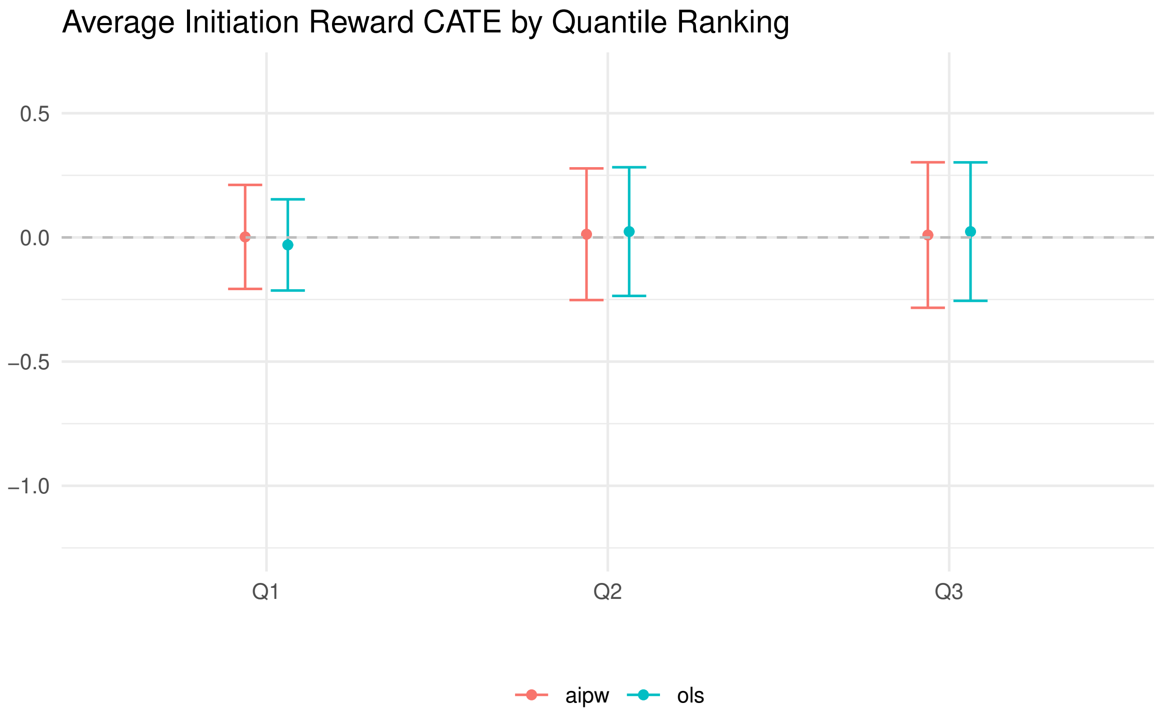

Figure A5: CATE Estimates for Initiation Bracelet Reward

Notes: This figure shows average Conditional Average Treatment Effects (CATEs) estimated by a Initiation Reward and outcome defined as total number of vaccines timely.

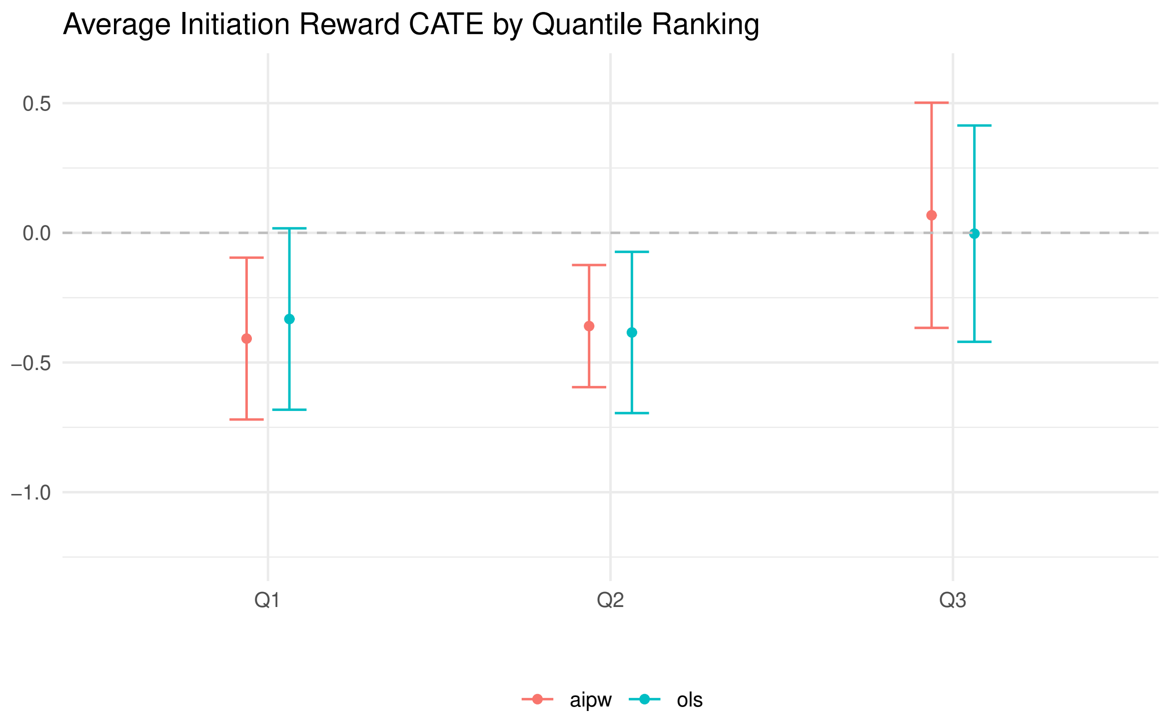

Figure A6: CATE Estimates for Direct Exposure to Initiation Reward

Notes: This figure shows estimated Conditional Average Treatment Effects (CATEs) for caregivers in the post-experimental period with Direct Exposure to the Initiation Reward. The outcome variable is number of vaccines received timely. Caregivers are grouped into terciles using predicted CATE values estimated via a causal forest. Confidence intervals are reported at the 95% level and are calculated with clustering at the clinic level. CATEs in each tercile are estimated using a regression of outcome on treatment status interacted with tercile placement using both unweighted ordinary least-squares (blue) and augmented inverse probability weighting (red).

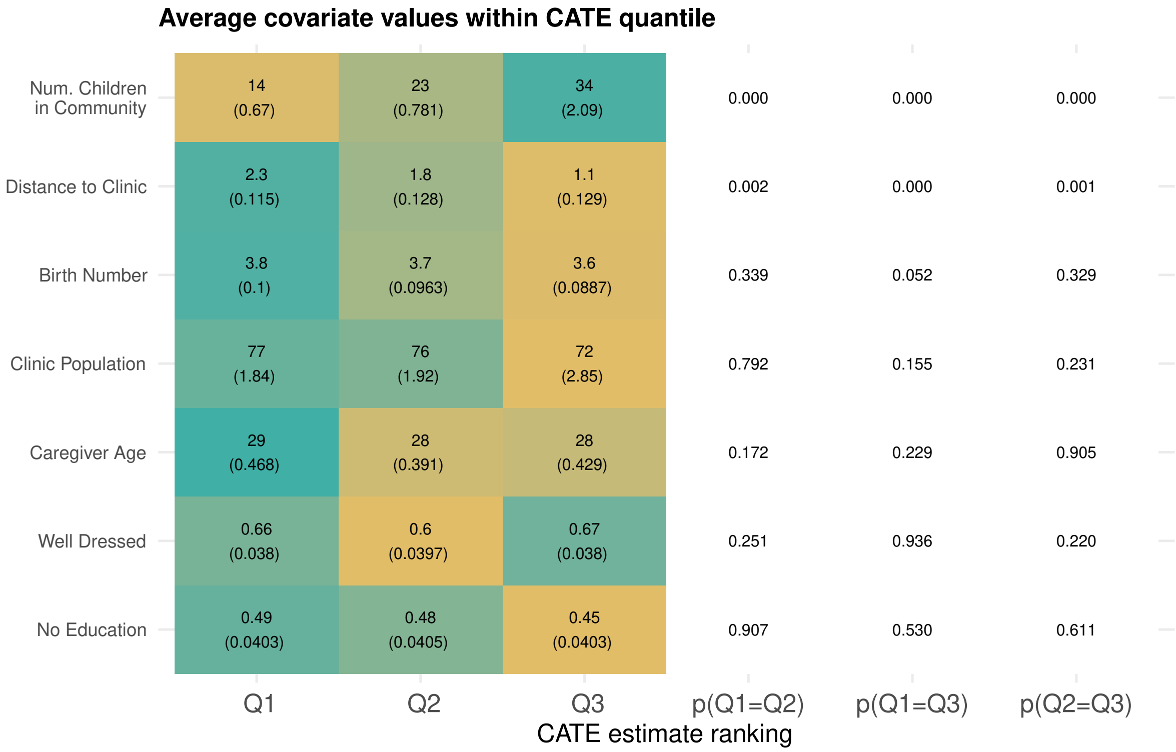

Figure A7: Covariate Values by Direct Exposure Initiation Reward CATE Tercile

Notes: This figure shows average covariate values for caregivers in each CATE tercile reported in Figure A6. The reported variables represent the complete set used in the estimate of CATEs, and are sorted by decreasing explanatory power of tercile rankings for total variation of the covariate in the sample. Standard deviations for coviariate values are reported in parentheses for each covariate × tercile cell. P-values reported are for difference in means tests comparing terciles within covariate.

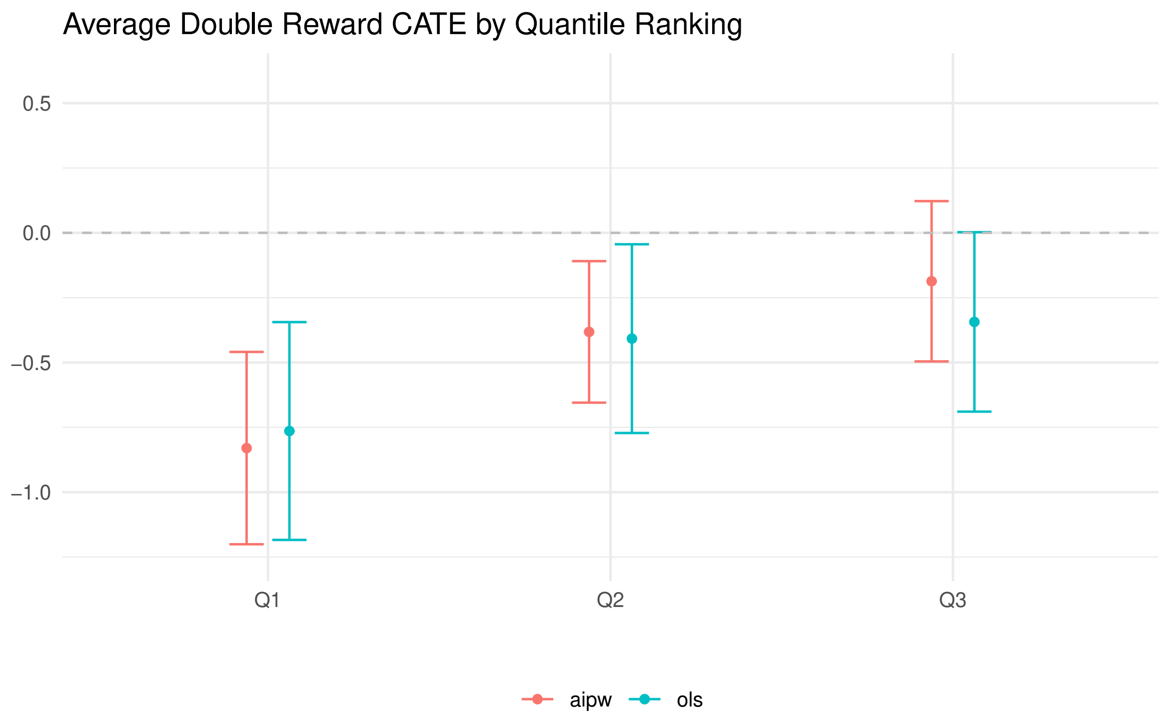

Figure A8: CATE Estimates for Direct Exposure to Double Reward

Notes: This figure shows estimated Conditional Average Treatment Effects (CATEs) for caregivers in the post-experimental period with Direct Exposure to the Double Reward. The outcome variable is number of vaccines received timely. Caregivers are grouped into terciles using predicted CATE values estimated via a causal forest. Confidence intervals are reported at the 95% level and are calculated with clustering at the clinic level. CATEs in each tercile are estimated using a regression of outcome on treatment status interacted with tercile placement using both unweighted ordinary least-squares (blue) and augmented inverse probability weighting (red).

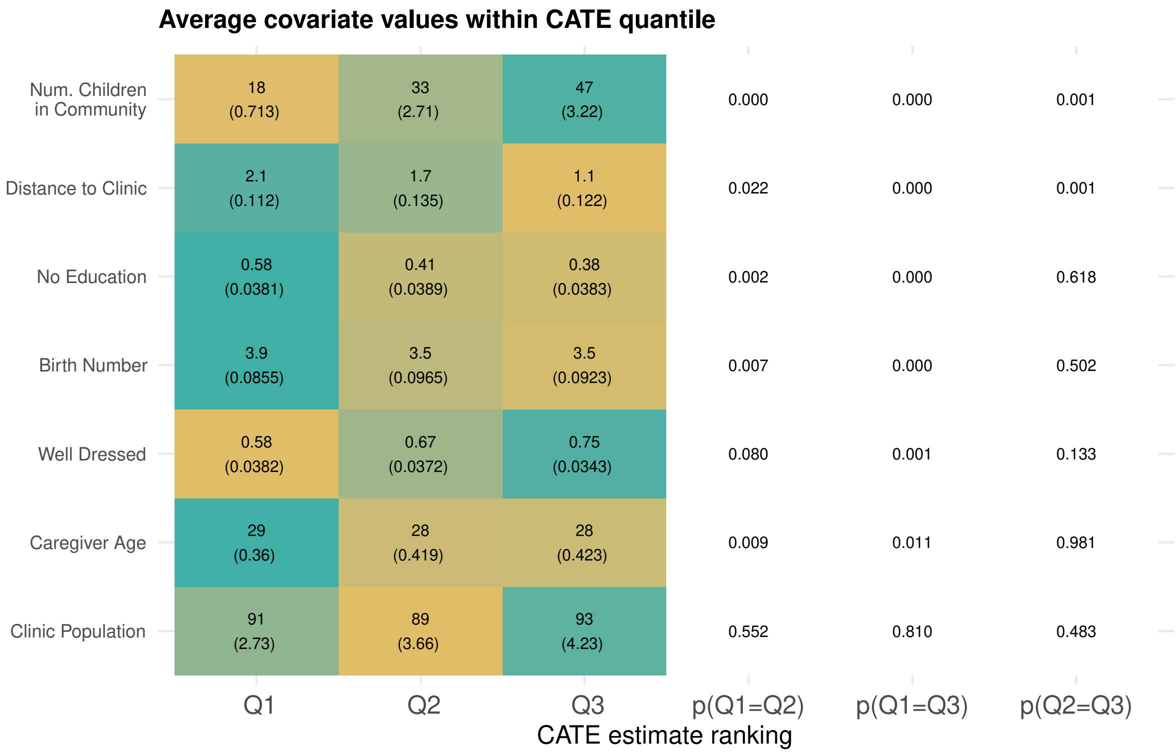

Figure A9: Covariate Values by Direct Exposure Double Reward CATE Tercile

Notes: This figure shows average covariate values for caregivers in each CATE tercile reported in Figure A8. The reported variables represent the complete set used in the estimate of CATEs, and are sorted by decreasing explanatory power of tercile rankings for total variation of the covariate in the sample. Standard deviations for coviariate values are reported in parentheses for each covariate × tercile cell. P-values reported are for difference in means tests comparing terciles within covariate.

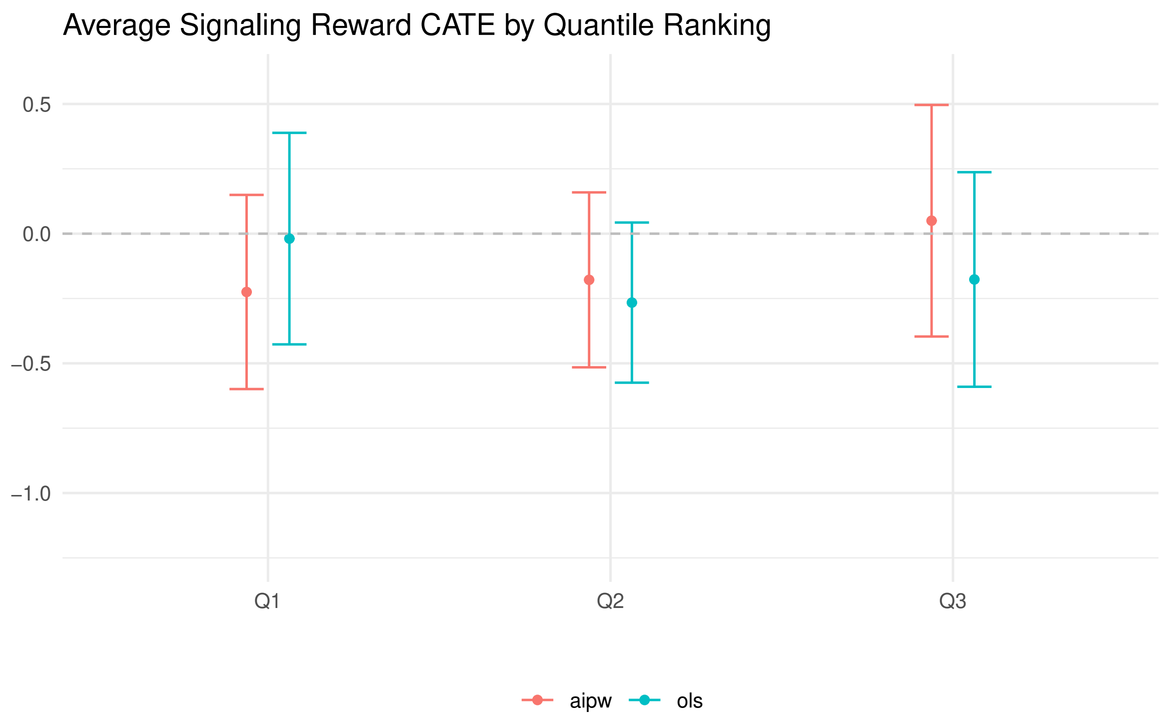

Figure A10: CATE Estimates for Direct Exposure to Signaling Bracelet Rewards

Notes: This figure shows estimated Conditional Average Treatment Effects (CATEs) for caregivers in the post-experimental period with Direct Exposure to the Signaling Reward. The outcome variable is number of vaccines received timely. Caregivers are grouped into terciles using predicted CATE values estimated via a causal forest. Confidence intervals are reported at the 95% level and are calculated with clustering at the clinic level. CATEs in each tercile are estimated using a regression of outcome on treatment status interacted with tercile placement using both unweighted ordinary least-squares (blue) and augmented inverse probability weighting (red).

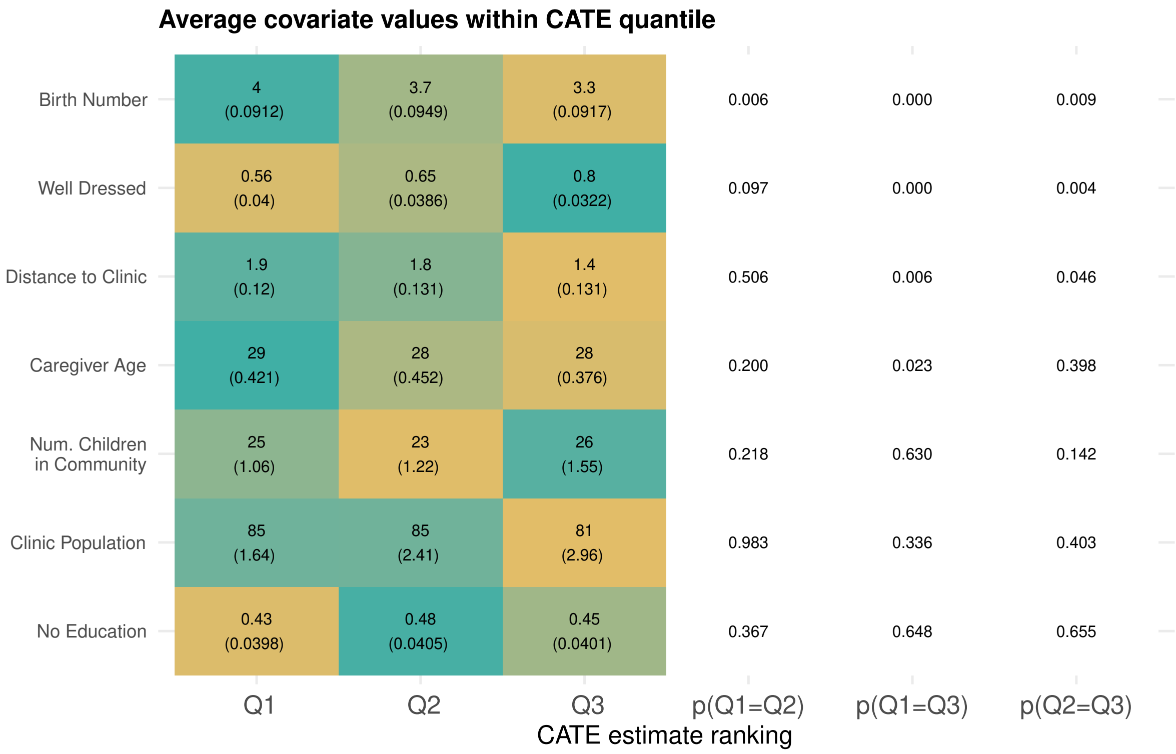

Figure A11: Covariate Values by Direct Exposure Signaling Reward CATE Tercile

Notes: This figure shows average covariate values for caregivers in each CATE tercile reported in Figure A10. The reported variables represent the complete set used in the estimate of CATEs, and are sorted by decreasing explanatory power of tercile rankings for total variation of the covariate in the sample. Standard deviations for coviariate values are reported in parentheses for each covariate × tercile cell. P-values reported are for difference in means tests comparing terciles within covariate.

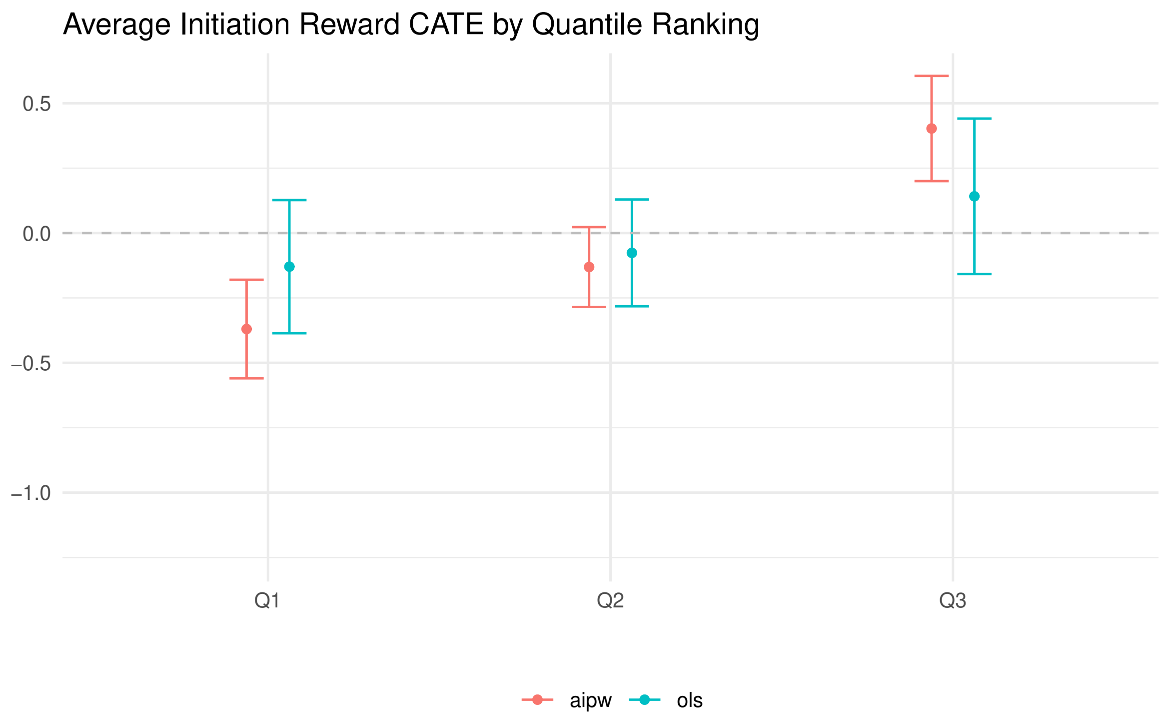

Figure A12: CATE Estimates for Indirect Exposure to Initiation Bracelet Reward

Notes: This figure shows average Conditional Average Treatment Effects (CATEs) estimated by a causal forest where treatment defined as belonging to a clinic catchment area originally assigned to the Initiation Reward and outcome defined as number of vaccines timely.

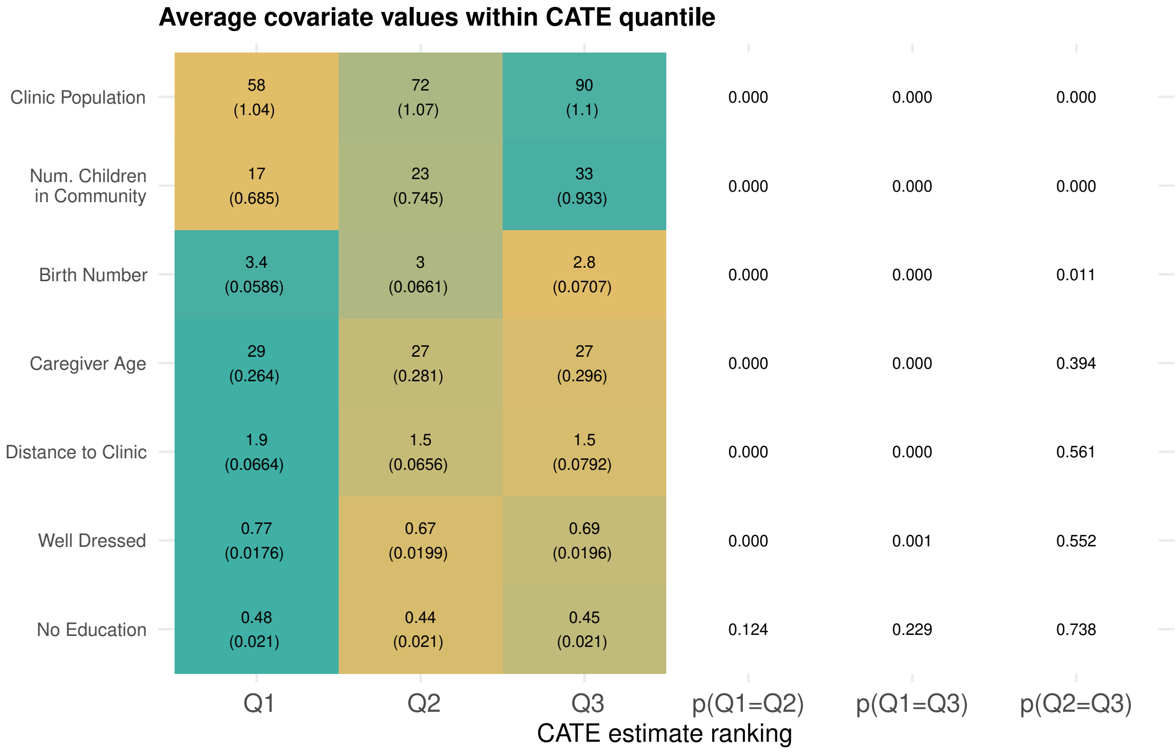

Figure A13: Covariate Values by Indirect Exposure Initiation Reward CATE Quantile

Notes: This figure shows average covariate values for individuals in CATE quantiles as defined by estimating a causal forest with treatment defined as belonging to a clinic catchment area originally assigned to the Initiation Reward and outcome defined as number of vaccines timely.

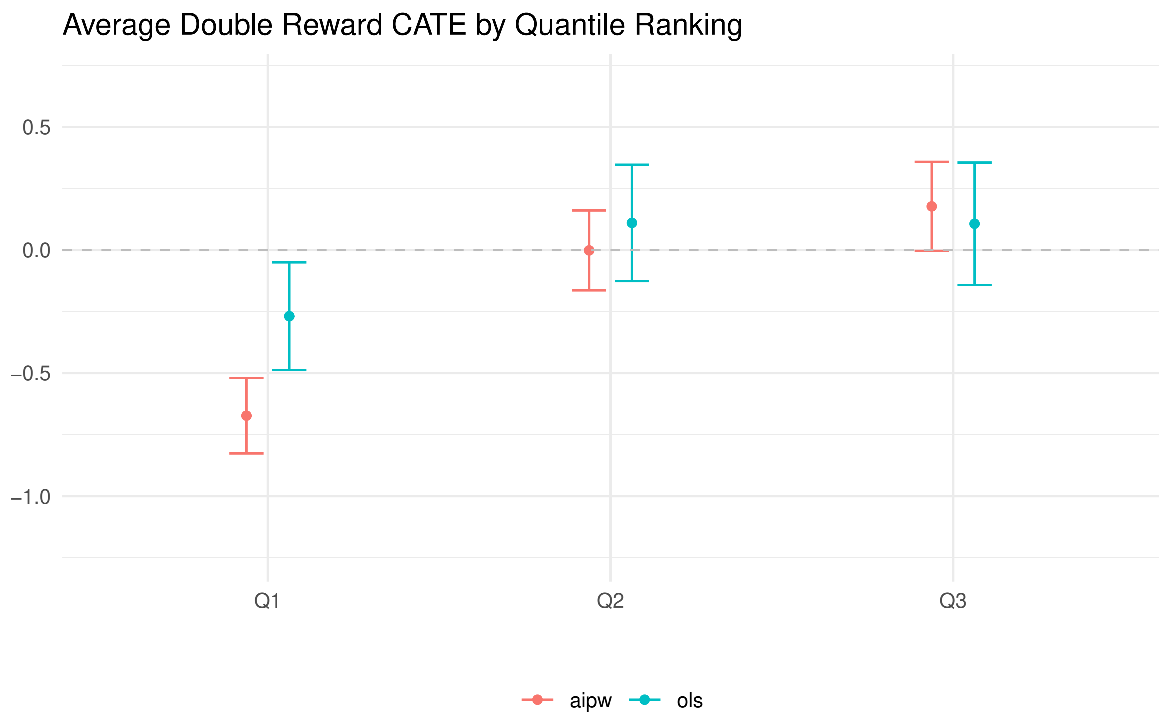

Figure A14: CATE Estimates for Indirect Exposure to Double Bracelet Reward

Notes: This figure shows average Conditional Average Treatment Effects (CATEs) estimated by a causal forest where treatment defined as as belonging to a clinic catchment area originally assigned to the Double Reward and outcome defined as number of vaccines timely.

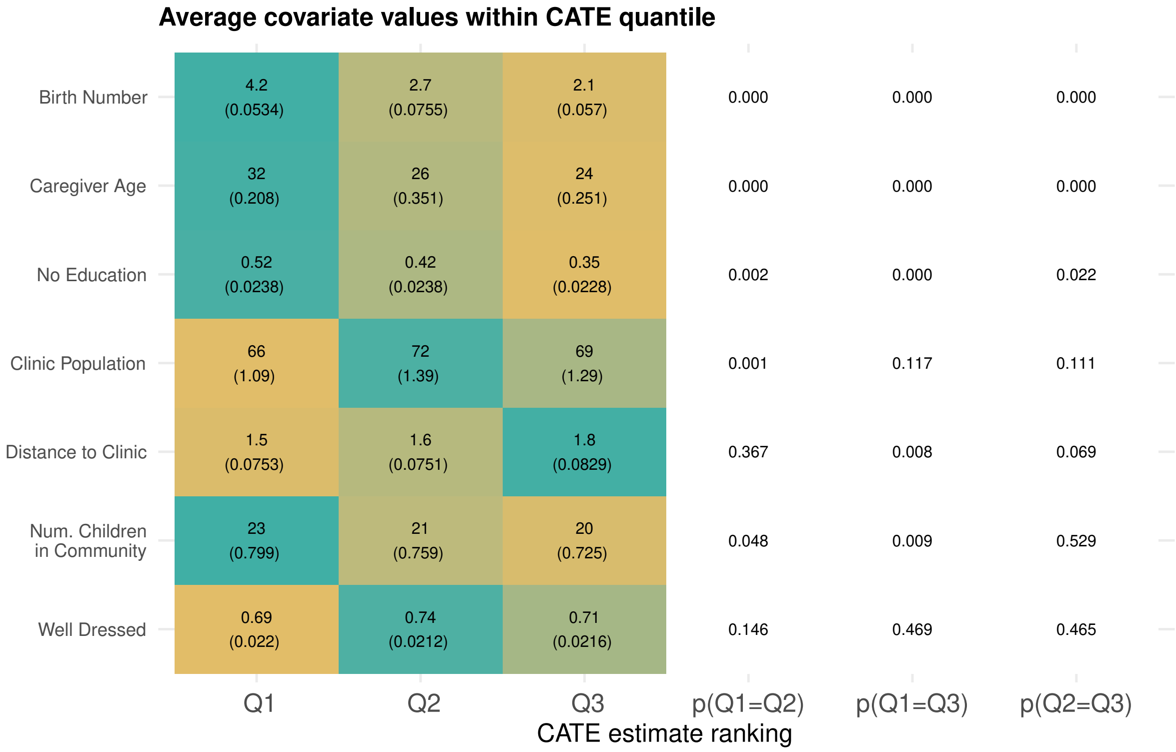

Figure A15: Covariate Values by Indirect Exposure Double Reward CATE Quantile

Notes: This figure shows average covariate values for individuals in CATE quantiles as defined by estimating a causal forest with treatment defined as belonging to a clinic catchment area originally assigned to the Double Reward and outcome defined as number of vaccines timely.

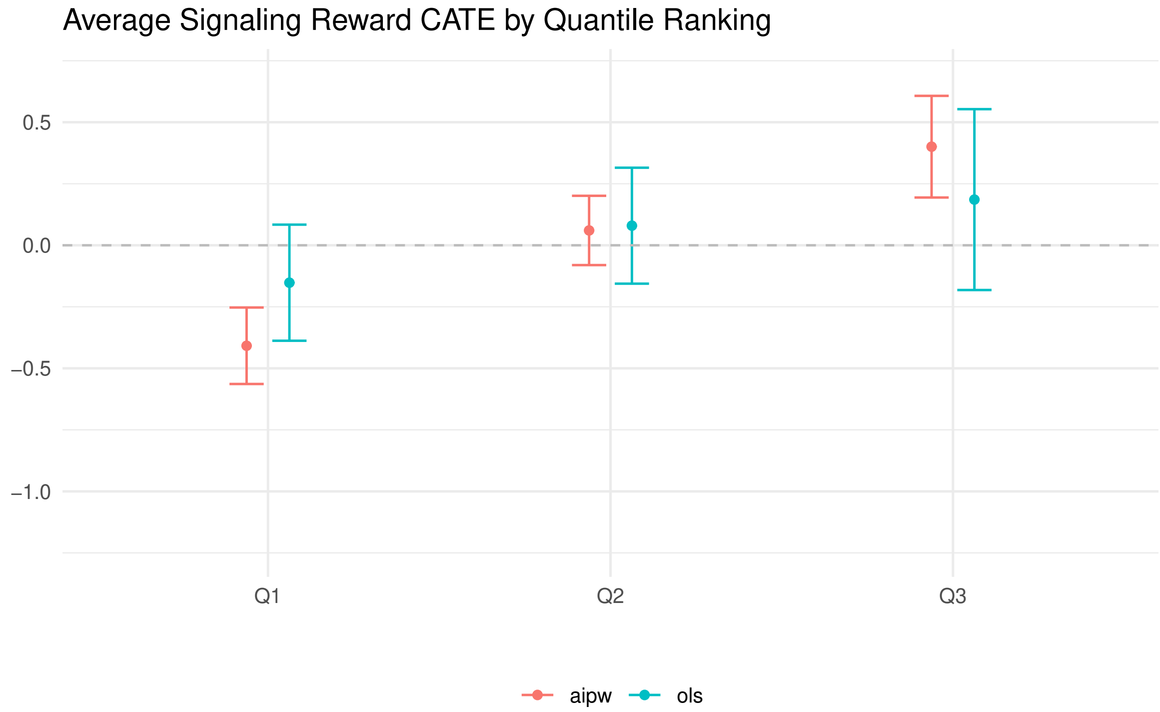

Figure A16: CATE Estimates for Indirect Exposure to Signaling Bracelet Reward

Notes: This figure shows average Conditional Average Treatment Effects (CATEs) estimated by a causal forest where treatment defined as belonging to a clinic catchment area originally assigned to the Signaling Reward and outcome defined as number of vaccines timely.

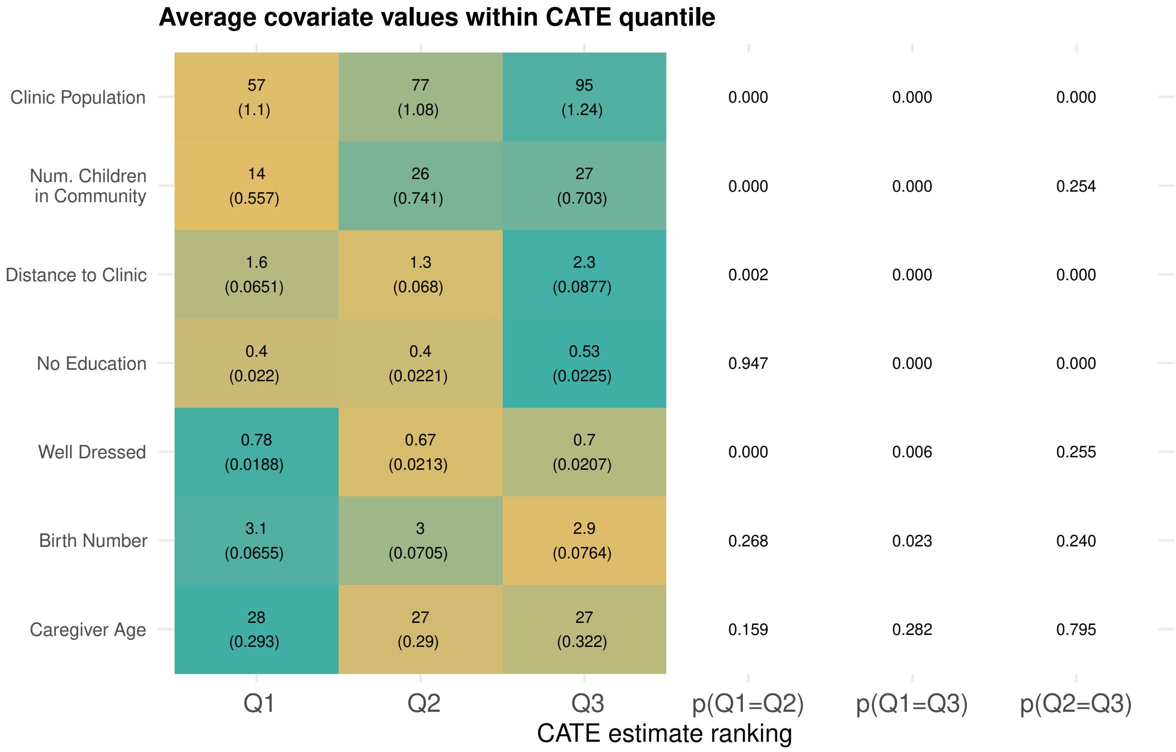

Figure A17: Covariate Values by Indirect Exposure Signaling Reward CATE Quantile

Notes: This figure shows average covariate values for individuals in CATE quantiles as defined by estimating a causal forest with treatment defined as belonging to a clinic catchment area originally assigned to the Signaling Reward and outcome defined as number of vaccines timely.

B Online Only Supplementary Tables

Table B1: Effects of Removing Incentives on Timely Vaccination, by Type of Exposure, Without Controls

Dependent variable:	Total # of vaccines timely	1st Vaccine	2nd Vaccine	3rd Vaccine	4th Vaccine	5th Vaccine
Panel A: Direct Exposure DE						
Signaling Reward	-0.160	-0.013	-0.047***	-0.028	-0.049	-0.023
	(0.140)	(0.008)	(0.018)	(0.033)	(0.058)	(0.057)
Double Reward	-0.512***	-0.027**	-0.069***	-0.096**	-0.125**	-0.194***
	(0.136)	(0.011)	(0.019)	(0.039)	(0.059)	(0.060)
Initiation Reward	-0.239*	-0.041***	-0.050***	-0.052*	-0.045	-0.051
	(0.124)	(0.011)	(0.014)	(0.031)	(0.054)	(0.052)
Control Group Mean	4.131	0.980	0.938	0.837	0.714	0.662
Panel Obs.	988	988	988	988	988	988
Panel Num. Clinics	117	117	117	117	117	117
Panel B: Indiriect Exposure (IE)						
Signaling Reward	-0.014	0.002	0.002	-0.005	-0.012	-0.001
	(0.101)	(0.006)	(0.012)	(0.025)	(0.033)	(0.041)
Double Reward	-0.154	-0.016**	-0.010	-0.027	-0.045	-0.056
	(0.096)	(0.008)	(0.015)	(0.024)	(0.031)	(0.038)
Initiation Reward	-0.018	-0.002	-0.006	0.016	-0.015	-0.011
	(0.093)	(0.006)	(0.012)	(0.022)	(0.033)	(0.038)
Control Group Mean	4.294	0.985	0.954	0.884	0.797	0.674
Panel Obs.	3679	3679	3679	3679	3679	3679
Panel Num. Clinics	119	119	119	119	119	119
p(Signaling × DE = 0)	0.130	0.070	0.004	0.378	0.439	0.616
p(Double × DE = 0)	<0.001	0.260	0.002	0.015	0.082	0.005
p(Initiation × DE = 0)	0.027	<0.001	0.002	0.014	0.545	0.381
p(Initiation × DE = Signaling × DE)	0.517	0.033	0.863	0.436	0.927	0.579
p(Double × DE = Signaling × DE)	0.008	0.271	0.345	0.054	0.138	0.004
p(Initiation × DE = Double × DE)	0.010	0.323	0.359	0.217	0.059	0.010
Observations	4667	4667	4667	4667	4667	4667
Number of Clinics	119	119	119	119	119	119
Controls	No	No	No	No	No	No

Notes: This table displays the results for the same analysis as Table IV without controls. All regressions include strata-fixed effects. Standard errors are cluster bootstrapped (500 repetitions) at the clinic level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B2: Effects of Removing Incentives on Vaccination at 15 months, by Type of Exposure, Without Controls

Dependent variable:	**Total # of vaccines by 15 months**	**1st Vaccine**	**2nd Vaccine**	**3rd Vaccine**	**4th Vaccine**	**5th Vaccine**
Panel A: Direct Exposure (DE)						
Signaling Reward	0.043	0.001	0.013	-0.003	0.016	0.017
	(0.099)	(0.001)	(0.013)	(0.021)	(0.033)	(0.051)
Double Reward	-0.138	0.001	-0.003	-0.023	-0.014	-0.098
	(0.112)	(0.001)	(0.014)	(0.023)	(0.034)	(0.061)
Initiation Reward	-0.067	-0.007	-0.007	-0.008	-0.026	-0.019
	(0.112)	(0.007)	(0.019)	(0.023)	(0.036)	(0.051)
Control Group Mean	4.603	0.998	0.980	0.950	0.905	0.771
Panel Obs.	597	597	597	597	597	597
Panel Num. Clinics	113	113	113	113	113	113
Panel B: Indirect Exposure (IE)						
Signaling Reward	0.038	0.003	0.005	-0.001	0.013	0.018
	(0.067)	(0.002)	(0.005)	(0.013)	(0.023)	(0.035)
Double Reward	-0.061	0.003	-0.008	-0.009	-0.002	-0.045
	(0.075)	(0.003)	(0.007)	(0.013)	(0.023)	(0.040)
Initiation Reward	0.001	0.001	0.002	0.009	0.009	-0.019
	(0.060)	(0.003)	(0.005)	(0.010)	(0.021)	(0.033)
Control Group Mean	4.653	0.998	0.989	0.964	0.921	0.782
Panel Obs.	2529	2529	2529	2529	2529	2529
Panel Num. Clinics	119	119	119	119	119	119
p(Signaling × DE = 0)	0.956	0.138	0.578	0.872	0.917	0.985
p(Double × DE = 0)	0.454	0.325	0.754	0.505	0.718	0.343
p(Initiation × DE = 0)	0.496	0.239	0.643	0.417	0.289	0.989
p(Initiation × DE = Signaling × DE)	0.268	0.275	0.252	0.840	0.190	0.438
p(Double × DE = Signaling × DE)	0.081	0.968	0.224	0.401	0.366	0.042
p(Initiation × DE = Double × DE)	0.539	0.285	0.830	0.556	0.709	0.163
Observations	3126	3126	3126	3126	3126	3126
Number of Clinics	119	119	119	119	119	119
Controls	No	No	No	No	No	No

Notes: This table displays the results for the same analysis as Table V without controls. All regressions include strata-fixed effects. Standard errors are cluster bootstrapped (500 repetitions) at the clinic level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B3: Effects of Removing Incentives on Timely Vaccination Using Alternative Age Cut-offs, by Type of Exposure

Dependent variable:	**1st Vaccine**			**2nd Vaccine**			**3rd Vaccine**		
Age cut-off:	**2 months**	**4 months**	**6 months**	**3 months**	**5 months**	**7 months**	**4 months**	**6 months**	**8 months**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Direct Exposure (DE)									
Signaling Reward	0.012	-0.005	-0.004	-0.032	-0.021	-0.010	0.002	-0.020	-0.028
	(0.015)	(0.004)	(0.004)	(0.031)	(0.015)	(0.011)	(0.044)	(0.029)	(0.022)
Double Reward	-0.037**	-0.019**	-0.011	-0.073**	-0.059***	-0.044***	-0.094*	-0.092***	-0.075***
	(0.017)	(0.009)	(0.008)	(0.033)	(0.018)	(0.014)	(0.049)	(0.035)	(0.028)
Initiation Reward	-0.038**	-0.024***	-0.008	-0.066**	-0.029*	-0.023	-0.033	-0.033	-0.030
	(0.018)	(0.009)	(0.006)	(0.029)	(0.016)	(0.014)	(0.042)	(0.030)	(0.023)
Control Group Mean	0.978	1.000	1.000	0.934	0.985	0.988	0.797	0.922	0.956
Panel B: Indirect Exposure (IE)									
Signaling Reward	-0.014	0.006	0.003	-0.009	-0.004	-0.004	-0.007	0.002	0.007
	(0.011)	(0.004)	(0.003)	(0.018)	(0.009)	(0.009)	(0.030)	(0.022)	(0.016)
Double Reward	-0.031**	-0.007	-0.002	-0.016	-0.018	-0.013	-0.017	-0.020	-0.018
	(0.015)	(0.005)	(0.004)	(0.022)	(0.011)	(0.008)	(0.028)	(0.020)	(0.015)
Initiation Reward	-0.001	0.001	0.002	-0.009	-0.012	-0.009	0.017	0.001	0.001
	(0.009)	(0.005)	(0.003)	(0.017)	(0.008)	(0.007)	(0.027)	(0.018)	(0.015)
Control Group Mean	0.979	0.992	0.995	0.923	0.976	0.980	0.814	0.919	0.942
p(Signaling × DE = 0)	0.117	0.021	0.045	0.447	0.284	0.539	0.807	0.452	0.110
p(Double × DE = 0)	0.725	0.153	0.211	0.042	0.020	0.019	0.020	0.007	0.018
p(Initiation × DE = 0)	0.041	0.011	0.078	0.066	0.285	0.327	0.203	0.210	0.192
p(Initiation × DE = Signaling × DE)	0.002	0.045	0.615	0.290	0.637	0.391	0.417	0.654	0.941
p(Double × DE = Signaling × DE)	0.002	0.152	0.403	0.198	0.050	0.018	0.040	0.028	0.078
p(Initiation × DE = Double × DE)	0.958	0.704	0.693	0.820	0.155	0.203	0.197	0.084	0.104
Observations	4667	4667	4667	4667	4667	4667	4667	4667	4667
Number of Clinics	119	119	119	119	119	119	119	119	119
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table displays the heterogeneity of incentives treatment effects on timely vaccination in the post-experiment period from Equation 1, by whether or not the child has an older sibling born during the experiment. The outcome in each column is the difference in timely vaccination for vaccines 1, 2 and 3 at different ages, testing the sensitivity of our results to the definition of timely vaccination. For a child to be coded as timely for a given vaccine, they need to have been timely for only the indicated vaccine, regardless of timeliness for earlier vaccines. Column 1, 4, and 7 displays the results when timeliness is more strictly defined as completing vaccines within 1.5 months of their due date. The rest of the columns show results when loosening the definition of timeliness to be within 3.5 months of the vaccine's due date (columns 2, 5, and 8) and within 5.5 months of the vaccine's due date (columns 3, 6, and 9). We include children born after May 1, 2019, who were at least 12 months old by the time last observed.

We include children born after May 1, 2019, who were at least 12 months old by the time last observed. The p-values are reported below Panel B for interaction coefficients for each treatment arm with direct exposure, followed by p values for difference in means between directly exposed children between treatment arms.

We control for the distance from the community to the clinic, the clinic population size, the age of the child at the end of the data collection, the order of the child compared to potential siblings, and whether the parent is a farmer. All regressions include strata-fixed effects. Standard errors are cluster bootstrapped (500 repetitions) at the clinic level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B4: Effects of Removing Incentives on Timely Vaccination, by Type of Exposure, No Vaccination Outliers

Dependent variable:	Total # of vaccines timely	1st Vaccine	2nd Vaccine	3rd Vaccine	4th Vaccine	5th Vaccine
Panel A: Direct Exposure (DE)						
Signaling Reward	-0.116	0.001	-0.047**	-0.020	-0.028	-0.021
	(0.151)	(0.002)	(0.021)	(0.038)	(0.064)	(0.060)
Double Reward	-0.496***	-0.021**	-0.077***	-0.088**	-0.113**	-0.198***
	(0.131)	(0.009)	(0.020)	(0.037)	(0.055)	(0.062)
Initiation Reward	-0.214*	-0.032***	-0.047***	-0.039	-0.037	-0.058
	(0.115)	(0.010)	(0.015)	(0.031)	(0.050)	(0.055)
Control Group Mean	4.168	0.985	0.939	0.845	0.733	0.666
Panel Obs.	858	858	858	858	858	858
Panel Num. Clinics	103	103	103	103	103	103
Panel B: Indirect Exposure (IE)						
Signaling Reward	0.069	0.007**	0.006	0.020	0.011	0.025
	(0.104)	(0.003)	(0.010)	(0.027)	(0.036)	(0.044)
Double Reward	-0.119	-0.011***	-0.013	-0.023	-0.037	-0.034
	(0.089)	(0.004)	(0.010)	(0.023)	(0.031)	(0.045)
Initiation Reward	-0.009	-0.002	-0.006	0.018	-0.013	-0.005
	(0.091)	(0.004)	(0.011)	(0.022)	(0.033)	(0.040)
Control Group Mean	4.344	0.992	0.961	0.895	0.811	0.685
Panel Obs.	3236	3236	3236	3236	3236	3236
Panel Num. Clinics	105	105	105	105	105	105
p(Signaling × DE = 0)	0.083	0.016	0.010	0.229	0.454	0.327
p(Double × DE = 0)	<0.001	0.262	0.002	0.034	0.110	<0.001
p(Initiation × DE = 0)	0.037	0.003	0.011	0.053	0.605	0.273
p(Initiation × DE = Signaling × DE)	0.459	0.001	0.995	0.613	0.854	0.485
p(Double × DE = Signaling × DE)	0.011	0.010	0.267	0.120	0.149	0.003
p(Initiation × DE = Double × DE)	0.019	0.385	0.169	0.170	0.107	0.014
Observations	4094	4094	4094	4094	4094	4094
Number of Clinics	105	105	105	105	105	105
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table displays the results from the same specification as Table IV, without clinics identified as outliers based on vaccination rates in Figure A1. This represents 10 total clinics: 3 from each of Double Reward, Signaling Reward, and Control, 1 from Uninformative. All regressions include strata-fixed effects. Standard errors are cluster bootstrapped (500 repetitions) at the clinic level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B5: Balance Checks for Community- and Clinic-level Indicators

Variable	(1) Control Mean/(SE)	(2) (C)-(IN)	(3) (C)-(D)	(4) (C)-(SL)	(5) (IN)-(D)	(6) (IN)-(SL)	(7) (D)-(SL)	(8) F-Test
		t-test differences [p-value]						
Panel A: Parent Level Variables								
Talked to someone about vaccines	0.944	0.011	0.026	-0.019	0.015	-0.030	-0.045	1.602
	(0.014)	[0.713]	[0.255]	[0.192]	[0.435]	[0.464]	[0.031]	[0.193]
Received at least one vaccine at outreach	0.093	-0.019	0.016	-0.000	0.035	0.019	-0.016	0.540
	(0.026)	[0.772]	[0.590]	[0.946]	[0.239]	[0.559]	[0.434]	[0.656]
Nurse gave something to caregiver	0.707	-0.029	0.076	-0.047	0.105	-0.017	-0.123	3.766
	(0.038)	[0.643]	[0.021]	[0.352]	[0.007]	[0.784]	[0.001]	[0.013]
Nurse gave something other than a bracelet	0.706	-0.022	0.082	-0.035	0.104	-0.013	-0.118	3.128
	(0.038)	[0.790]	[0.018]	[0.594]	[0.011]	[0.891]	[0.004]	[0.028]
Community bylaws or fines for defaulters	0.599	0.027	0.042	-0.012	0.015	-0.039	-0.054	0.284
	(0.044)	[0.838]	[0.715]	[0.611]	[0.658]	[0.626]	[0.474]	[0.837]
Number of observations	1612	3232	3649	3411	3657	3419	3836	7068
Number of clusters	30	60	60	59	60	59	59	119
Panel B: Clinic Level Variables								
Immunization Days Per Month	2.833	0.233	0.267	-0.063	0.033	-0.297	-0.330	0.469
	(0.254)	[0.468]	[0.393]	[0.958]	[0.965]	[0.412]	[0.390]	[0.705]
Clinic Focus on Health talks	0.200	-0.200	-0.067	-0.076	0.133	0.124	-0.009	1.024
	(0.074)	[0.102]	[0.655]	[0.414]	[0.289]	[0.350]	[0.879]	[0.384]
Clinic Focus on Accurate record-keeping	0.333	0.133	0.033	-0.011	-0.100	-0.145	-0.045	0.676
	(0.088)	[0.221]	[0.739]	[0.993]	[0.332]	[0.264]	[0.747]	[0.568]
Clinic Focus on Procuring vaccines	0.333	0.100	0.133	0.126	0.033	0.026	-0.007	0.700
	(0.088)	[0.347]	[0.172]	[0.260]	[0.744]	[0.802]	[0.946]	[0.554]
Clinic Focus on Administering vaccines	0.267	0.033	0.033	0.094	0.000	0.061	0.061	0.348
	(0.082)	[0.696]	[0.801]	[0.343]	[0.911]	[0.640]	[0.433]	[0.790]
Clinic Focus on Outreach/Home visits	0.167	0.033	-0.067	-0.006	-0.100	-0.039	0.061	0.356
	(0.069)	[0.767]	[0.596]	[0.862]	[0.329]	[0.362]	[0.619]	[0.785]
Clinic Focus on Growth monitoring	0.133	0.033	0.033	-0.005	0.000	-0.038	-0.038	0.144
	(0.063)	[0.696]	[0.696]	[0.930]	[1.000]	[0.643]	[0.627]	[0.933]
Number of observations	30	60	60	59	60	59	59	119
Number of clusters	30	60	60	59	60	59	59	119

Notes: The sample in this table consists of all children eligible to be included in the follow-up study. The panel shows balance on behaviors related to but separate from vaccination behavior. Panel A shows balance on self-reported behaviors related to vaccine behaviors and community norms in the follow-up survey for caregivers for all children born in the post-experiment period (born after May 1st, 2019). Panel B reports immunization day frequency and priorities of clinic staff from a clinic-level survey. Variables in Panel B regarding "Clinic Focus" are indicators for whether clinic staff surveyed at the clinic stated that they exert the most effort on the activity. "Talked to someone about vaccines" refers to caregiver responses to the question "Did you talk to someone about your childs immunizations, or did someone remind you about your childs immunizations?"

Table B6: Balance Checks on Characteristics of Parents with Direct Exposure, No Population Outliers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				t-test differences				
	Control			[p-value]				
Variable	**Mean/(SE)**	**(C)-(IN)**	**(C)-(D)**	**(C)-(SL)**	**(IN)-(D)**	**(IN)-(SL)**	**(D)-(SL)**	**F-Test**
Panel B: Characteristics of Parents With Direct Exposure								
Child's age (end of follow-up)	662.640	12.406	3.220	9.247	-9.186	-3.160	6.027	0.850
	(8.013)	[0.202]	[0.280]	[0.112]	[0.355]	[0.664]	[0.566]	[0.470]
Good vaccine data source	0.907	0.048	0.058	0.015	0.010	-0.033	-0.043	0.887
	(0.022)	[0.224]	[0.190]	[0.648]	[0.819]	[0.469]	[0.264]	[0.450]
Temne Ethnicity	0.654	0.009	0.045	-0.016	0.036	-0.026	-0.061	0.272
	(0.091)	[0.925]	[0.419]	[0.803]	[0.444]	[0.915]	[0.699]	[0.845]
Limba Ethnicity	0.234	0.097	0.066	0.077	-0.031	-0.020	0.011	0.328
	(0.086)	[0.480]	[0.492]	[0.590]	[0.591]	[0.701]	[0.677]	[0.805]
Mothers' age (in years)	28.346	-0.445	0.232	0.278	0.677	0.722	0.045	0.902
	(0.375)	[0.328]	[0.806]	[0.681]	[0.341]	[0.052]	[0.684]	[0.442]
Number of children	3.743	0.066	0.188	0.104	0.123	0.039	-0.084	1.262
	(0.079)	[0.533]	[0.125]	[0.271]	[0.271]	[0.527]	[0.168]	[0.291]
Well dressed	0.621	-0.044	-0.042	-0.093	0.001	-0.050	-0.051	0.285
	(0.084)	[0.596]	[0.907]	[0.639]	[0.772]	[0.800]	[0.591]	[0.836]
No education	0.458	-0.034	-0.013	0.012	0.021	0.046	0.025	0.454
	(0.049)	[0.136]	[0.355]	[0.713]	[0.721]	[0.464]	[0.688]	[0.715]
Some primary education	0.355	0.004	0.015	0.038	0.010	0.034	0.023	0.377
	(0.040)	[0.781]	[0.821]	[0.135]	[0.779]	[0.210]	[0.553]	[0.770]
At least secondary education	0.187	0.030	-0.002	-0.050	-0.032	-0.080	-0.048	1.847
	(0.033)	[0.114]	[0.240]	[0.229]	[0.410]	[0.016]	[0.231]	[0.143]
Caregiver is a farmer	0.795	0.019	0.036	0.025	0.017	0.006	-0.011	0.500
	(0.050)	[0.966]	[0.518]	[0.351]	[0.521]	[0.323]	[0.965]	[0.683]
Travels outside community	0.551	0.013	-0.090	-0.080	-0.103	-0.093	0.011	0.413
	(0.082)	[0.935]	[0.613]	[0.440]	[0.446]	[0.387]	[0.960]	[0.744]
Number of observations	214	462	452	463	486	497	487	949
Number of clusters	29	59	57	57	58	58	56	115

Notes: This table summarizes relevant sample characteristics between treatment arms for parents with direct exposure to incentives as in Table II but without clinics identified as outliers based on population.

Table B7: Balance Checks on Characteristics of Parents with Indirect Exposure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				t-test differences [p-value]				
Variable	**Control Mean/(SE)**	**(C)-(IN)**	**(C)-(D)**	**(C)-(SL)**	**(IN)-(D)**	**(IN)-(SL)**	**(D)-(SL)**	**F-Test**
Panel C: Characteristics of Parents With Indirect Exposure								
Child's age (end of follow-up)	685.599	2.042	11.969	2.173	9.927	0.131	-9.796	1.859
	(6.698)	[0.531]	[0.030]	[0.507]	[0.023]	[0.890]	[0.049]	[0.140]
Good vaccine data source	0.877	-0.018	-0.018	0.005	0.001	0.024	0.023	0.300
	(0.016)	[0.738]	[0.397]	[0.565]	[0.692]	[0.533]	[0.498]	[0.825]
Temne Ethnicity	0.631	0.029	0.098	0.015	0.069	-0.014	-0.083	0.263
	(0.081)	[0.765]	[0.455]	[0.769]	[0.227]	[0.826]	[0.612]	[0.852]
Limba Ethnicity	0.211	0.032	0.011	0.031	-0.021	-0.001	0.020	0.077
	(0.067)	[0.618]	[0.806]	[0.821]	[0.634]	[0.907]	[0.988]	[0.972]
Mothers' age (in years)	27.553	-0.150	0.297	0.058	0.447	0.208	-0.239	0.235
	(0.349)	[0.743]	[0.774]	[0.809]	[0.491]	[0.540]	[0.853]	[0.871]
Number of children	3.096	0.044	0.093	0.082	0.050	0.038	-0.012	0.474
	(0.062)	[0.673]	[0.360]	[0.212]	[0.639]	[0.368]	[0.816]	[0.701]
Well dressed	0.683	-0.054	-0.060	-0.031	-0.006	0.023	0.029	0.383
	(0.052)	[0.356]	[0.559]	[0.479]	[0.955]	[0.882]	[0.872]	[0.765]
Stayed in community < 2 yr.	0.079	-0.013	-0.037	-0.019	-0.023	-0.006	0.018	1.529
	(0.013)	[0.649]	[0.103]	[0.233]	[0.162]	[0.342]	[0.697]	[0.211]
No education	0.471	0.030	0.064	0.053	0.034	0.023	-0.011	1.054
	(0.025)	[0.421]	[0.115]	[0.191]	[0.634]	[0.549]	[0.915]	[0.372]
Some primary education	0.273	-0.000	-0.029	-0.004	-0.029	-0.004	0.025	0.786
	(0.021)	[0.819]	[0.112]	[0.908]	[0.423]	[0.819]	[0.132]	[0.504]
At least secondary education	0.256	-0.030	-0.035	-0.049	-0.005	-0.019	-0.014	0.648
	(0.027)	[0.552]	[0.674]	[0.136]	[0.898]	[0.454]	[0.358]	[0.586]
Caregiver is a farmer	0.726	0.044	0.073	0.038	0.028	-0.006	-0.034	1.009
	(0.038)	[0.705]	[0.124]	[0.189]	[0.341]	[0.523]	[0.486]	[0.391]
Travels outside community	0.586	0.072	-0.106	-0.025	-0.177	-0.097	0.080	1.401
	(0.070)	[0.360]	[0.299]	[0.774]	[0.062]	[0.234]	[0.377]	[0.246]
Number of observations	845	1686	1682	1771	1678	1767	1763	3449
Number of clusters	30	60	58	59	58	59	57	117

Notes: This table summarizes relevant sample characteristics between treatment arms for parents with indirect exposure to incentives as in Table III but without clinics identified as outliers based on population.

Table B8: Effects of Removing Incentives on Timely Vaccination, by Type of Exposure, No Occupation Outliers

Dependent variable:	Total # of vaccines timely	1st Vaccine	2nd Vaccine	3rd Vaccine	4th Vaccine	5th Vaccine
Panel A: Direct Exposure (DE)						
Signaling Reward	-0.143	-0.011	-0.047***	-0.023	-0.040	-0.021
	(0.131)	(0.009)	(0.018)	(0.032)	(0.056)	(0.054)
Double Reward	-0.486***	-0.025**	-0.072***	-0.089**	-0.124**	-0.178***
	(0.130)	(0.011)	(0.021)	(0.037)	(0.058)	(0.060)
Initiation Reward	-0.228*	-0.039***	-0.049***	-0.047	-0.037	-0.056
	(0.121)	(0.011)	(0.015)	(0.033)	(0.054)	(0.052)
Control Group Mean	4.412	1.000	0.986	0.892	0.786	0.748
Panel Obs.	949	949	949	949	949	949
Panel Num. Clinics	115	115	115	115	115	115
Panel B: Indiriect Exposure (IE)						
Signaling Reward	0.001	0.002	0.002	-0.001	-0.006	0.003
	(0.101)	(0.006)	(0.011)	(0.026)	(0.033)	(0.040)
Double Reward	-0.137	-0.018**	-0.014	-0.019	-0.042	-0.044
	(0.094)	(0.009)	(0.014)	(0.022)	(0.030)	(0.041)
Initiation Reward	-0.017	-0.002	-0.006	0.017	-0.014	-0.012
	(0.091)	(0.007)	(0.012)	(0.022)	(0.032)	(0.037)
Control Group Mean	4.328	0.988	0.957	0.885	0.810	0.687
Panel Obs.	3449	3449	3449	3449	3449	3449
Panel Num. Clinics	117	117	117	117	117	117
p(Signaling × DE = 0)	0.122	0.109	0.004	0.436	0.469	0.551
p(Double × DE = 0)	<0.001	0.541	0.012	0.009	0.080	0.013
p(Initiation × DE = 0)	0.026	<0.001	0.007	0.024	0.619	0.326
p(Initiation × DE = Signaling × DE)	0.482	0.047	0.923	0.451	0.950	0.509
p(Double × DE = Signaling × DE)	0.010	0.304	0.288	0.068	0.109	0.008
p(Initiation × DE = Double × DE)	0.032	0.368	0.288	0.264	0.076	0.035
Observations	4398	4398	4398	4398	4398	4398
Number of Clinics	117	117	117	117	117	117
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table displays the results from the same specification as Table IV, without clinics identified as outliers based on occupation. These represent two clinics in the Double Reward treatment or 269 observations. All regressions include strata-fixed effects. Standard errors are cluster bootstrapped (500 repetitions) at the clinic level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B9: Short-run Effects of Incentives on Timely Vaccination, by Distance to the Clinic

Dependent variable:	**1st Vaccine**	**2nd Vaccine**	**3rd Vaccine**	**4th Vaccine**	**5th Vaccine**	**Total # of vaccines timely**
Panel A: Far Clinics (=>3 miles)						
Signaling Reward	0.039**	0.062***	0.126***	0.143***	0.051	0.386***
	(0.017)	(0.023)	(0.035)	(0.048)	(0.045)	(0.134)
Initiation + Double Reward	0.020	0.043*	0.058*	0.034	0.019	0.165
	(0.016)	(0.022)	(0.035)	(0.046)	(0.041)	(0.131)
Panel Obs.	2240	2168	2087	2002	1664	1664
Panel B: Close Clinics (<3 miles)						
Signaling Reward	0.003	0.033***	0.055**	0.085***	0.095***	0.258***
	(0.008)	(0.011)	(0.022)	(0.032)	(0.035)	(0.088)
Initiation + Double Reward	-0.006	0.011	0.005	0.015	0.016	0.051
	(0.007)	(0.010)	(0.020)	(0.031)	(0.033)	(0.083)
Panel Obs.	4524	4381	4251	4102	3454	3454
p(Signaling × far = 0)	0.046	0.203	0.044	0.226	0.311	0.330
p(Initiation + Double × far = 0)	0.124	0.155	0.106	0.636	0.938	0.333
p(Initiation + Double × far = Signaling × far)	0.022	0.114	0.001	0.002	0.418	0.013
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table displays the results of an heterogeneity analysis of the effects of bracelet incentives during the experiment, by distance to the clinic. It uses the variable sample from Karing (2024) and its specification for the analysis of timely completion of each vaccine independently. Children are categorized as living far from the clinic if they live in a community located three miles ore more from the clinic. Children in the close category are living in communities located within less than three miles from the clinic.

The Initiation and Double Rewards are pooled and we use a variable sample of all children who are old enough to have a received a given vaccine as opposed to a constant sample of 11.5-month old children. These two choices increase our power to detect changes in the smaller sample of children located in far communities. The results reported from the original experiment show that the Double and Initiation Reward treatments exhibit similar effects and the variable sample analysis shows effects consistent with the constant sample.

All regressions include strata-fixed effects. Standard errors are cluster bootstrapped (500 repetitions) at the clinic level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B10: Community Vaccination Reminders

Dependent variable:	Talked or Was Reminded	Reminded by CHW	Reminded by Other Caregiver	Number of Reminder Sources
Panel A: Direct Exposure (DE)				
Signaling Reward	0.012	0.007	0.027	0.445**
	(0.025)	(0.061)	(0.045)	(0.218)
Double Reward	0.016	0.025	0.044	0.353*
	(0.022)	(0.056)	(0.041)	(0.185)
Initiation Reward	0.012	-0.021	-0.020	0.118
	(0.022)	(0.059)	(0.039)	(0.189)
Control Group Mean	0.949	0.509	0.108	2.389
Panel Obs.	988	988	988	988
Panel Num. Clinics	117	117	117	117
Panel B: Indirect Exposure (IE)				
Signaling Reward	0.021	-0.026	0.037	0.428*
	(0.017)	(0.055)	(0.047)	(0.228)
Double Reward	-0.002	-0.052	-0.024	-0.039
	(0.020)	(0.058)	(0.038)	(0.194)
Initiation Reward	-0.007	-0.023	-0.046	0.022
	(0.020)	(0.043)	(0.035)	(0.192)
Control Group Mean	0.937	0.451	0.117	2.353
Panel Obs.	3679	3679	3679	3679
Panel Num. Clinics	119	119	119	119
p(Signaling × DE = 0)	0.692	0.419	0.674	0.921
p(Double × DE = 0)	0.455	0.053	0.009	0.008
p(Initiation × DE = 0)	0.324	0.970	0.305	0.515
p(Initiation × DE = Signaling × DE)	0.982	0.658	0.267	0.169
p(Double × DE = Signaling × DE)	0.863	0.750	0.693	0.706
p(Initiation × DE = Double × DE)	0.854	0.448	0.085	0.218
Observations	4667	4667	4667	4667
Number of Clinics	119	119	119	119
Controls	Yes	Yes	Yes	Yes

Notes: This table displays the heterogeneity of incentives treatment effects on community reminder behaviors by incentive treatment and exposure type. The outcome in each column is whether a caregiver reported receiving a reminder for vaccination, and then whether any reminder came from a Community Health Worker (CHW) or another caregiver. All regressions include strata-fixed effects. Standard errors are cluster bootstrapped (500 repetitions) at the clinic level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B11: Transfer to and From the Nurse

Dependent variable:	**Caregiver Gave Something to Nurse**	**Caregiver Received Something from Nurse**	**Caregiver Received Food from Nurse**
Panel A: Direct Exposure (DE)			
Signaling Reward	-0.046	0.074*	0.074*
	(0.059)	(0.043)	(0.039)
Double Reward	-0.082	-0.118**	0.031
	(0.068)	(0.050)	(0.041)
Initiation Reward	-0.061	0.017	0.041
	(0.059)	(0.041)	(0.038)
Control Group Mean	0.590	0.739	0.121
Panel Obs.	988	988	988
Panel Num. Clinics	117	117	117
Panel B: Indirect Exposure (IE)			
Signaling Reward	-0.042	0.035	0.058
	(0.050)	(0.031)	(0.037)
Double Reward	-0.012	-0.035	0.010
	(0.055)	(0.040)	(0.032)
Initiation Reward	-0.060	0.029	-0.003
	(0.048)	(0.041)	(0.027)
Control Group Mean	0.536	0.728	0.104
Panel Obs.	3679	3679	3679
Panel Num. Clinics	119	119	119
p(Signaling × DE = 0)	0.931	0.306	0.614
p(Double × DE = 0)	0.158	0.055	0.530
p(Initiation × DE = 0)	0.982	0.740	0.167
p(Initiation × DE = Signaling × DE)	0.811	0.230	0.447
p(Double × DE = Signaling × DE)	0.592	0.001	0.361
p(Initiation × DE = Double × DE)	0.746	0.010	0.824
Observations	4667	4667	4667
Number of Clinics	119	119	119
Controls	Yes	Yes	Yes

Notes: This table displays the heterogeneity of incentives treatment effects on transfer behavior by incentive treatment and exposure type. The outcome in the first two columns is whether a caregiver reported giving something to the nurse or reported the nurse giving something other than a bracelet at the clinic. The outcome in the last column is whether a caregiver recieved an incentive in the form of food at the clinic. All regressions include strata-fixed effects. Standard errors are cluster bootstrapped (500 repetitions) at the clinic level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B12: Implementation Sample Recall and Bracelet Receipt

Dependent Variable:	**Caregiver can recall last vaccine**	**Caregiver Received a Bracelet**
Double Reward	0.019	-0.008
	(0.031)	(0.021)
Signaling Reward	-0.011	-0.003
	(0.039)	(0.022)
Initiation Reward	-0.048	
	(0.037)	
Control Group Mean	0.834	
Initiation Reward Mean		0.943
Observations	3040	2217
Controls	Yes	Yes

Notes: This table displays the heterogeneity of incentives treatment effects on caregiver recall and bracelet hand out rates during the original implementation period. The outcome in the first two columns is whether a caregiver correctly reported the last vaccine their child received, and whether or not the caregiver received a bracelet. All regressions include strata-fixed effects. Standard errors are cluster bootstrapped (500 repetitions) at the clinic level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.