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

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Abstract

We investigate the impact of incentives and their withdrawal on parents' decisions to vaccinate subsequent children. We follow up with parents three years after exposure to a bracelet incentive given to children for timely vaccination in Sierra Leone. Our analysis leverages the design of an experiment in which clinics were randomly assigned to offer incentives or not. Since only parents with a newborn at the time of the experiment were eligible for the incentive, we can exploit individual variation in exposure within clinics. First, we find that eligibility for an incentive for an earlier child reduces parents' motivation to vaccinate their subsequent child on time, with reductions of 5 to 11 percent in the number of timely visits compared to unexposed parents. There are no effects on vaccination rates by 15 months of age, suggesting that parents delay vaccination rather than abstaining altogether. Second, parents living in communities where incentives were offered but who were ineligible for them show no effects, ruling out the possibility that changes in community norms or clinic practices drive the results. Third, incentives that signaled being a caring parent do not lead to adverse effects. Using causal forest analysis and testing for differences in knowledge and practices around immunization, we rule out that negative effects are due to the removal of incentives. Instead, we conclude that the exposure to incentives crowded out parents' intrinsic motivation by altering their self-perception towards or their relationship with vaccination.

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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 in 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). 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 extrinsic rewards do not crowd out prosocial motivation during implementation of the incentives (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). However, 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 as incentives for timely vaccination (Karing 2024). Three years after study, we revisit all 597 study communities, collecting vaccination data for more than 10,000 children born subsequently. Our study seeks to answer the following questions: Does providing incentives for parents to vaccinate their children lead to lower take-up among future children? To what extent do incentives influence parents' intrinsic motivation? When do incentives undermine intrinsic motivation?

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 majority who are 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 bracelet treatments or a Control group where no bracelets 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 program's effects when incentives are withdrawn are attributable to individual experience factors (e.g., motivation crowd-out) versus community- or clinic-level effects (e.g., changes in provider behavior or social norms). Third, by comparing treatments that incentivized different actions, we can test whether the perceived costs and benefits of an action, as well as whether it signals being a good parent, affect post-removal behavior.

The bracelet incentives were designed as follows: All treatments included an initiation bracelet 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 treatment consisted solely of the initiation bracelet and we refer to it as Initiation Reward. The second treatment included an additional incentive: the initiation bracelet was exchanged for 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. Owning the second bracelet is considered a significant indicator of a parent's 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 the incentives influence parents' behavior post-removal, we examine whether direct exposure to the incentives during the experiment affects parents' vaccination decisions for their next child, compared to parents who were only indirectly exposed to the incentives. As incentives were tied to timely vaccination, our primary outcome is the total number of vaccines completed on time. This captures the effort parents make to adhere to their child's vaccination schedule rather than choosing a convenient time. We examine the effects of incentives on the timeliness of specific vaccines to understand the pattern of behaviors across the vaccination schedule. Our final outcome, completion by 15 months, reflects changes in vaccination attitudes leading parents to skip vaccinations altogether.

Our analysis shows that direct exposure to incentives diminishes parents' motivation to vaccinate subsequent children on time. These effects are common across all three incentives treatment, but vary in magnitude and significance. Specifically, parents who had a child during the experiment and were therefore directly exposed to bracelet incentives in the Initiation and Double Reward treatments complete 5% (p<0.05) and 11% (p<0.01) 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). In contrast, parents who were only indirectly exposed to incentives do not show this decline, suggesting that adverse effects are driven by the direct experience of receiving the incentive, rather than community-level factors such as changes in clinic staff's behavior or shifts in community vaccination norms. We find significant heterogeneity in treatment effects by type of exposure to the Initiation and Double Reward incentives (p=0.001-0.07), which supports the hypothesis that prior experience of incentives drives this negative response.

The observed reduction in timely vaccination behavior appears to be specifically related to the experience of receiving the bracelet incentive. Examining the timely completion of specific vaccines, we find that the negative effects of the Initiation Reward are concentrated on the timely completion of the first and second vaccines with effects of -3.8 and 6.3 percentage points (p<0.01) but shows no adverse effects on subsequent visits, where no additional reward was provided (p=0.12-0.44). The Double Reward treatment exhibits a similar negative pattern on the first two vaccines, as well as consistently negative effects throughout the rest of the vaccination schedule. Most strikingly, exposure to this incentive results in a 12.1 and 19.2 percentage points (p<0.05) decrease in the timely completion of the fourth and fifth visits, consistent with the bracelet exchange occurring at the former.

In contrast, the Signaling Reward does not result in strong negative effects: we detect a significant negative effect on the second vaccine (-4.7 percentage points, p<0.01), consistent with the presence of the initial bracelet, but it is not robust to changes in the definition of timeliness. Importantly, we find no effects on later vaccines (p=0.47-0.7).

To better understand the mechanisms behind the reduction in timely vaccination behavior, we apply a causal forest method to identify sources of heterogeneity in treatment effects. We find significant heterogeneity in treatment effects in the Initiation and Double

Reward treatments, with parents facing the highest costs being most likely to be affected by the negative treatment effects. In contrast, we observe no heterogeneity in the Signaling Reward treatment, indicating that the null effect does not conceal negative behavior changes in subgroups of parents. This allows us to use the Signaling Reward as a comparison group, ruling out the possibility 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. We also investigate and rule out potential misinterpretation of the phase-out of bracelets, as well as disappointment from perceived unfair distribution. Together, our results suggest that experience of incentives crowded out parents intrinsic motivation, either by shifting their perception of their motivation for vaccinatingfrom intrinsic to extrinsicor by altering their relationship to the action, making it more transactional.

Lastly, we examine whether the effects are due to parents exerting less effort to complete vaccinations on time or a change in parents' attitudes toward vaccinating at all. We find that the magnitude of effects decreases with children's age and is no longer significant by 15 months, the time at which children are due for the second dose of the measles vaccine.

This paper makes three contributions. First, we contribute to 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 term, 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 have not 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). There are 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 on the mechanisms behind motivation crowd-out and when they can undermine behavior. Our finding is consistent with the Bénabou and Tirole (2003)'s theory that incentives can lead individuals to re-interpret their relationship to an action and crowd-out their intrinsic motivation, making it less likely for them to adopt it when the incentives are removed. We show that these effects can occur in settings where individuals have set 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 long enough to establish expectations (Campos-Mercade et al. 2023). While recent work shows no evidence of crowd-out of one-off financial incentive for COVID-19 vaccination (Schneider et al. 2023), our study provides evidence of crowd-out in the context of vaccination incentives implemented for two years by government health workers.

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 that they are caring parents, it does not diminish their motivation, unlike purely extrinsic rewards. 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 child care 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 reports of the average initiation rates in GAVI's 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 complete

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 they came timely for immunization. A total of 120 government clinics were randomized into four groups. In the Control group, children did not receive any bracelets at vaccinations. All three treatment groups included an initiation bracelet given to children upon the first vaccine visit. Since 99.7% of parents in this context initiate vaccination, the first bracelet does not carry any signaling value and primarily functions as a small material reward. The first treatment consisted solely of the initiation bracelet: children received a bracelet of their chosen color between yellow and green at their first vaccination visit, replaced by a same identical bracelet at vaccines four and five. We refer to this treatment as the "Initiation Reward". The second treatment included an additional incentive: the initiation bracelet set for all as the yellow bracelet was exchanged for a green one 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 the "Double Reward". The third treatment consisted in exchanging the yellow initiation bracelet for a green one at the last vaccine if all vaccines are completed on time. The green bracelet received at the fifth visit, perceived by parents as one of the most important vaccine, was a strong signal of parents' care for their child's health. We refer to it as the "Signaling Reward". If the child came late in either the Double or Signaling Reward treatments, the bracelet was replaced by the same identical yellow bracelet.

The bracelets were implemented for two years from July 2016 to August 2018. Health workers were trained to give bracelets to children when they came to the clinic according to the design rules of each treatment. Field staff held information meeting in all study

series.

communities to create a common understanding of the meaning of the bracelets. The quality of implementation was similar across arms, and provided parents with a strong signal of when a child had completed four or five vaccines in Double and Signaling Reward communities.

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

The study found that the bracelet that allowed parents to signal timely completion of all five vaccines was the most effective, increasing the share of children who completed all vaccines in time by 13.3 percentage points ($p = 0.001$) and increasing the number of timely vaccinations received on average by 9% (p=0.001). It also had large positive impacts on vaccine completion at 12 and 24 months, with increases in the percentage of fully vaccinated children of 9.4 and 5.1 percentage points, respectively (p=0.005 and p=0.09). In contrast, the Initiation Reward and Double Reward led to 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.

We assess heterogeneity in treatment effects in a subsample of 3,040 children who were visited at follow-up and find no significant differences across groups, as shown by Figures A3, A4 and A5. Our results suggest that the null effects in the Initiation and Double Reward treatments do not mask negative or positive effects in some subgroups of parents.

This empirical setting is ideal for investigating our research questions for four reasons. First, it allows us to follow up with communities that experienced the introduction and discontinuation of an incentive program.

Second, this is a setting in which we do not expect incentives to positively influence behavior in the long run through learning by doing or habit formation, making motivation crowd-out potentially relevant. In fact, the overwhelming majority of parents believe that vaccination is beneficial to their child, timely completion rates for the first three vaccines are higher than 80%, and perceptions around the importance of different vaccines did not change as a result of 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–signaling intrinsic motivation or not– by exploiting the original random assignment.

Fourth, the incentive implementation mimicked that of a real program at scale: government health workers distributed the bracelets as part of routine immunization days at clinics, and the incentive was implemented at a large scale, reaching more than 35,000 children and for a prolonged time of two years.

III Methods

III.A Data and Measurement

We conducted a follow-up survey in the 597 communities of the original study between December 2020 and August 2021. In this survey, we list all children born and living in these communities since the experiment ended. Our methodology mirrored that of the original study. The survey included questions about each child's vaccination history, ownership of a bracelet, awareness of the bracelet program, and exposure to other incentives for immunization. To confirm whether parents had previously been exposed to the program, we asked how long they had lived in the community and if 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 by being born after December 1, 2017 and attending one of the 119 study clinics for immunizations.[3] Of these, 11,235 children were alive at the time of the survey, old enough to have completed all vaccinations, and their primary caregiver resided 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 end of the program being announced at the end of the experiment (August 2018), we find that health workers continued to distribute bracelets for two subsequent years (Figure I).[4] We did not anticipate that this would occur, and this makes detecting the persistent effects of incentives throughout the post-experiment period challenging. We identify a threshold point of one year and a half after the end of the experiment (May 2019), after which the children have a 15% chance of receiving any bracelet. We focus our analysis on this period to test for the crowding-out of motivation.

Tables I, II and III displays characteristics and attrition 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 that parents have moved (14%), the child is deceased (8%), and that parents were traveling at the time of the survey (7.9%). Notably, the attrition rates and their cause do not differ significantly between the control and incentive groups.

Tables II and III detail socio-demographic characteristics of parents who were directly exposed to incentives and those who were not directly exposed, respectively. The former sample is well balanced across all indicators with the exception of a significant but small imbalance on the birth order of children. Double Reward parents have 5% (p=0.08) and

[3]This date was chosen as it ensured that children would have had at least one vaccine due when the bracelet program ended.

[4]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.

2% (p=0.09) fewer children on average than those in the Control group and Signaling Reward, respectively.

We observe two additional imbalances in the latter sample of parents who were not directly exposed to the incentives. First, children in the Double Reward are on average 18, 16 and 16 days younger than the Control group, Initiation Reward and Signaling Reward, respectively. These imbalances are not meaningful enough to affect outcomes, representing differences in age of between 2% and 3%. Second, parents in the Double Reward are less likely to be farmers by 17.4 (p=0.02), 13 (p=0.04), and 13.6 (p=0.05) percentage points compared to those in the Control group, Initiation Reward, and Signaling Reward, respectively. This is a noteworthy imbalance which warrants a more thorough analysis. Examining the distribution of this variables suggests that imbalances are driven by the presence of two outlier clinics assigned to the Double Reward treatment. When running balance checks without those clinics, significant differences on this variable disappear (Tables B6 and B7). To understand if our results are driven by outlier clinics, we present the estimates of our main specifications when those outliers are removed from the analysis.

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

III.B Empirical Strategy

III.B.1 Main Specification

We examine the effects of exposure to incentives during the experiment on parents' vaccination decisions for their subsequent child, compared to the Control group. Specifically, we conduct a heterogeneity analysis of the effects of the different incentive treatments on timely vaccination after the incentives are removed, categorized by type of exposure. We consider any parent who had a newborn during the experiment as eligible and thus directly exposed to the incentive, while parents who lived in treated communities but did not have a newborn at the time as indirectly exposed. We are interested in the effects of incentives compared to the Control group within each sample of parents (those had a newborn versus not during the experiment), as well as whether there is heterogeneity in treatment effects across those groups. Lastly, we are interested in comparing treatment effects by type of incentive (with or without a focus on signaling care for the child).

Our specification is:

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

in which Vaccine_i denotes our primary vaccine outcome which is the total number of vaccines a child has received on time [5]; $T_{j(i)}$ 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 order of the child compared to potential siblings, 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.

We use this same specification on another set of outcomes. First, we estimate treatment effects on the binary indicators for a child i being vaccinated for each vaccine $a \in \{1, 2, 3, 4, 5\}$ in a timely manner, that is by the age of 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. This captures the effort parents make to adhere to their child's vaccination schedule rather than choosing a convenient time. Second, we examine the completion of all vaccines by 15 months, which, as the due date for the second dose of the measles vaccine, represents the next milestone in the vaccination schedule. This reflects changes in vaccination attitudes leading parents to skip vaccinations altogether.

III.B.2 Supporting Analyses

To shed light on mechanisms, we perform a number of additional analyses. Firstly, we look at the treatment effects over time by estimating cohort-specific treatment effects throughout the experiment and post-experiment periods. This allows us to understand how parents overall responded to the removal of bracelets in each of the three treatments.

Secondly, we use survey data from parent and health worker surveys to understand changes in knowledge, beliefs and practices related to immunization and bracelets. Specifically, we examine the observability, knowledge and receipt of bracelets over time; reminders and talks about vaccination, and whether parents give or receive something from the nurse when visiting the clinic for immunization.

Lastly, we test whether treatment effects are concentrated among particular subgroups using a causal forest algorithm à la Wager and Athey (2018). We rank and group individual observations by predicted treatment effect and estimate Conditional Average Treatment Effects (CATEs) by quantiles. Using this approach, we can test for treatment effect heterogeneity by comparing data-driven subgroups. We can further uncover differences in covariates associated with higher and lower treatment effects by comparing covariate distributions within our constructed subgroups. The results of this analysis are

[5]Timely vaccination is defined in the same way as in the paper Karing (2024). The timeliness cutoffs are informed by WHO guidelines stating that the DTP series should be completed by six months of age (WHO 2018). There is no strict guidelines for the measles vaccine, but we use the same definition allowing for a 2.5 months buffer window for each vaccine.

presented in Section IV.B.

Once we establish predicted CATE quantiles, we estimate CATE by quantile using both OLS and Augmented Inverse Probability Weighting (AIPW). However, we report the AIPW estimates, standard errors, and p-values, because it is consistent (and efficient, giving us the most statistical power) under certain conditions of non-random assignment, which may occur with any differential attrition/participation in the follow-up since there have been several years between original randomization and follow-up collection.

For each treatment, we use a causal forest algorithm to estimate CATEs on total number of timely vaccines, compared to the Control group. We do so on three different sample of parents: the experiment period, to rule out any heterogeneity that occurred during the implementation of the bracelets, and the directly and indirectly exposed parents in the post-experiment period.

For each of our causal forest applications, we employ the same set of covariates: caregiver age, number of children (also referred to as "birth number"), clinic population, community population, whether the caregiver has no education, whether the caregiver came to the clinic well dressed, and the distance of the community to the clinic.

These covariates were chosen in part to shed light on mechanisms by highlighting different aspects of the take-up decision for caregivers. Distance to clinic, community population, and number of children feed directly into the cost of the action, because of travel distance, smaller social network, and opportunity cost. However, this also may change the value of the signal, as a bracelet for timeliness reveals a higher incurred cost for a caregiver who lives far from the clinic for example.

Other covariates, such as caregiver age and whether the caregiver has no education, shed light on the amount of experience and scope for learning that a caregiver has with regards to raising a child during the first year. Caregivers with less experience or knowledge surrounding vaccination processes and benefits may be more likely to update on the removal of bracelets as a signal from the government that vaccines are no longer as important, causing caregivers to negatively update on instrumental payoff beliefs.

Lastly, we include clinic population, which highlights important features of implementation of health policy, such as social relationships with nurses, and potential for scolding. The bracelet emphasis on timeliness may have led to stronger scolding or scrutiny surrounding vaccination decisions in small clinics where nurses are more likely to recognize caregivers, leading to negative effects.

An additional benefit of our chosen set of covariates is that these are characteristics of clinics, communities, and individuals which are easily observable to policy-makers and stakeholders. Identifying clear trends in heterogeneous treatment effects can help inform governments of which types of caregivers are at risk of negative effects in the event of incentive removal, aiding in the targeting of efforts to mitigate them.

IV Results

IV.A The Effects of Incentives Post-removal on Timely and Complete Vaccination

IV.A.1 Timely Vaccination

We start by examining the impact of incentives on parents' decisions to vaccinate their subsequent children on time after incentives are removed, by type of exposure.

Our analysis shows that direct exposure to extrinsic incentives diminishes parents' motivation to vaccinate subsequent children on time. Table IV (Panel A, column 1) shows a common trend across all three incentives treatment, 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).

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 community-level factors such as changes in clinic staff's behavior or shifts in community vaccination norms. 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 heterogeneity 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 (p=0.12-0.44). The Double Reward treatment exhibits a similarly negative pattern on 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.

We interpret these results as evidence that the experience of receiving rewards for initiating vaccination and completing the first four visits on time lead to reduced parental effort to vaccinate when the incentive is discontinued.

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).

IV.A.2 Completion by 15 Months

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 ($p=0.55$). 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 heterogeneity in treatment effects between directly exposed parents and those with indirect exposure.

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.A.3 Robustness Checks

In this section, we test the sensitivity of our results to alternative outcome definitions, samples and specifications.

Alternative outcome definitions: We examine the post-experiment impact of incentives on different measures of timely vaccination. Specifically, we test how adjusting the age thresholds for considering vaccinations as timely affects our findings. Table B3 displays treatment effects on the share of children receiving the first three vaccines within 1.5, 3.5, and 5.5 months of the recommended vaccination dates, instead of the 2.5-month criterion employed in Table IV. This analysis yields two insights. First, the Double and Initiation Rewards effects on one, two, and three vaccines are robust to both stricter and more lenient timeliness definitions, albeit diminishing with the child's age. The effect sizes for the Double and Initiation Rewards decrease by an average of 37% and 53%, respectively, for every two-month increment in age. Notably, by six months, neither treatment shows discernible negative impacts on the completion of the first vaccine. By seven months, the Initiation Reward's effect becomes indiscernible (-2.2 percentage points, p=0.11), unlike the Double Reward (-5.2 percentage points, p=0.003), indicating a convergence of parents in incentive groups to the vaccination rates of the Control group as the child ages. Heterogeneity by exposure aligns with comparisons to the Control group, with effects being significantly more negative for parents with direct exposure to Double Reward on two vaccines (p=0.009-0.02) and for parents with direct exposure to the Initiation Reward on one (p=0.01-0.08) and two vaccines (p=0.04-0.29).

Secondly, we do not observe negative impacts with adjusted timeliness criteria for the Signaling Reward. Parents directly exposed to the Signaling Reward show similar likelihoods of completing the first two vaccines within three, five, and seven months as the Control group, with no observed heterogeneity based on direct exposure (p=0.21-0.91). We interpret these results as evidence that the negative effects of the Signaling Reward is not robust as it is only significant for a specific definition of timeliness.

Alternative samples: We now examine the stability of our estimates when removing two types of outliers from the sample. First, we identify outliers on vaccination rates at the clinic level. A potential concern is that clinic-level endogenous responses may be particularly strong at a small number of clinics, driving the effects in one or more treatment arms. In Figure A1, we present the distribution of clinic-level timely completion rates for each vaccine by treatment arm. In Table B4, we estimate our main specification (Equation 1) after removing clinics identified as outliers for the first vaccine.[6] The results show negative treatment effects in all three arms, similar to previous analyses. This suggests they are a general feature of bracelet clinics across the outcome distribution in the post-experiment period, not driven by extreme effects in a few clinics.

Second, we turn our attention to outliers observed on the share of parents working as farmers. We identify two clinics in the Double Reward treatments that have significantly higher share of parents working in non-farming jobs, driving an imbalance in population across arms. This could influence parents' ability to go to the clinic and bias our results. We run our main specification without those two outlier clinics, which leads to dropping 269 observations. Table B8 shows that our results are stable to this modified sample: our coefficients are broadly similar and their significance levels identical.

[6]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.

Alternative specification: Lastly, we present estimates for specifications without controls on timely vaccination in Table B1 and vaccination by 15 months in B2, and find very similar results.

IV.B Mechanisms

In this section, we explore potential mechanisms driving the negative effects on timely vaccination.

IV.B.1 Heterogeneity Results

We observe significant heterogeneity in treatment effects among 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 ($p = 0.070$).[7]

We observe a similar pattern of heterogeneity in the Double Reward treatment, with stronger effects. Specifically, the Conditional Average Treatment Effect (CATE) for the first tercile is estimated at 0.96 fewer timely vaccinations ($p < 0.001$), and this estimate is again significantly different from that of the third tercile ($p < 0.001$). In contrast, we detect no significant heterogeneity for the Signaling Reward treatment, as indicated by the absence of distinct or monotonic CATE estimates across terciles, as illustrated in Figure A10. This finding suggests that the overall null effect observed in this treatment does not obscure any notable positive or negative effects within specific subgroups of parents.

Turning to observable characteristics of parents, Figures A7 and A9 present the average values for all covariates and the results of difference-in-means tests between terciles for the Initiation and Double Reward treatments, respectively. We identify two covariates that vary monotonically with the CATE estimates and exhibit distinct levels between terciles in both treatments: community population and distance to the clinic. Among caregivers directly exposed to either the Initiation Reward or Double Reward treatments, the strongest negative effects are observed in small communities (mean population: $\mu_{IR} = 14$, $\mu_{DR} = 19$) and in communities located farther from their assigned clinics (mean distance: $\mu_{IR} = 2.3$ miles, $\mu_{DR} = 2.2$ miles). These values contrast sharply with those in the third tercile, where caregivers come from much larger communities (mean population: $\mu_{IR} = 34$, $\mu_{DR} = 47$) and from communities much closer to their assigned clinics (mean

[7]These p-values are based on Augmented Inverse Probability Weighting estimates, with adjustments for multiple hypothesis testing using the step-down correction procedure (Romano and Wolf 2005).

distance: $\mu_{IR} = 1.1$ miles, $\mu_{DR} = 0.98$ miles), with all these differences significant at the 1% level. The Double Reward treatment also shows heterogeneity on individual level characteristics. The mean number of children in the first tercile is 4, compared to 3.3 in the third tercile ($p < 0.001$). Additionally, nearly twice as many parents in the first tercile have no education compared to the third tercile ($\mu_{DR} = 0.59$ in T1 compared to $\mu_{DR} = 0.32$ in T3, $p < 0.001$).[8]

IV.B.2 Supply-side Response

One possible explanation for the negative effects on timely vaccination is that health workers, who distributed the bracelets during the experiment, changed their behavior in response to its discontinuation, leading to changes in how immunization services were implemented. The absence of clinic-wide negative effects allows us to rule out this possibility. We assume that clinic health workers cannot systematically distinguish between parents who had a child during the experiment and those who did not. Hence, if health workers had responded to the removal of the incentives, e.g., by exerting less effort to remind parents of the importance of timely vaccination, we would observe similar negative effects among parents without direct exposure who live in treatment communities. An alternative explanation could be social norm change as a result of incentive exposure. For example, communities could increase efforts to enact fines or bylaws for defaulters, or members could discuss childhood vaccines more or less. We expect these responses to similarly affect both indirectly and directly exposed parents.

We can rule out the possibility that these supply-side responses occurred but only influenced the future vaccination decisions of parents directly exposed to the incentive. We examine self-reports of these behaviors from both parents and clinic staff, testing for balance across control and incentive arms in Table B5. These surveys reveal few differences in parental communication about vaccines and the likelihood of receiving medicines, a bednet, or food from clinic staff. However, parents in the Double Reward treatment were less likely to report receiving something from clinics compared to all other treatment arms.

IV.B.3 Incentives' Removal

In this section, we examine various behavioral explanations that may have influenced parental behavior specifically in response to the removal of incentives.

[8] Figures A12 through A17 illustrate the heterogeneity in CATEs among indirectly exposed parents. We observe significant heterogeneity between the first, second, and third terciles, with effects of large magnitudes across all three treatments, suggesting that parents indirectly exposed to the incentives may also experience negative outcomes. Unlike the group of directly exposed parents, there is no clear pattern in covariates that vary with CATE estimates.

Learning about the importance of timely vaccination: The removal of bracelets may have sent a negative signal to parents about the value of timely vaccination, potentially altering their beliefs about its benefits. This is consistent with Bénabou and Tirole (2003)s theory on the impact of incentives on intrinsic motivation, where an incentive signals the payoff of an action when information asymmetries exist between the agent (parents) and the principal (health workers). Although the perceived importance of different vaccines remained consistent across treatment and control groups post-experiment (Karing 2024), the gradual discontinuation of bracelets may have shifted parents beliefs. While we lack direct data on vaccination beliefs from the follow-up survey, we have data on bracelet observability and awareness, which provide insights into how directly and indirectly exposed parents might have responded to the removal. We find that 70-80% 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). If learning from the removal played a significant role, we would expect some negative effects even among unexposed parents.

One limitation of this analysis is the distinction between having a bracelet on ones own child versus merely observing it in the community, suggesting that only directly exposed parents may have learned from its removal. To further test if learning played an important role, we examine differences vaccination behavior across treatments. If the removal led parents to believe that timely vaccination was unimportant, we would expect to observe similar effects across all treatment groups. However, we find no significant negative effects in the Signaling Reward treatment. One possible explanation is that demand for the vaccine associated with the rewardMeasles 1was inelastic, as it is considered the second most important vaccine, and its timeliness is well understood among parents due to its colloquial name, the '9-month marklet.' Nonetheless, the reduction in the timely completion of Measles 1 in the Double Reward treatment indicates that demand for this vaccine remained responsive to external incentives.

Composition shift in the Signaling Reward: The absence of negative effects in the Signaling Reward treatment could obscure a composition shift in which different groups of parents vaccinate on time. This would suggest that the earlier updating of beliefs about the importance of timely vaccination may still occur in this treatment but is masked by persistent positive treatment effects. We calculate the difference in timely vaccinations between directly exposed parents children born during the experiment and their younger children born in the post-experiment period. We find a similar reduction in timely vaccinations of 0.3 to 0.5 compared to the Control group, with no significant differences across treatments (Table B10). This trend either reflects a return to Control levels as bracelets are phased out in the Signaling Reward treatment or a composition shift. Such a shift could occur if *marginal takers*parents influenced by the bracelet during its implementationcontinue vaccinating on time after the bracelets are removed, while *always takers*parents who would have vaccinated regardless of the braceletstart vaccinating late once the bracelets are removed. We detect no heterogeneity in CATEs in the Signaling Reward treatment among directly exposed parents (Figure A10), ruling out the possibility that the removal of incentives affected marginal takers and always takers differently.

Misinterpretation of decreased bracelet distribution: 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 could have led to incorrect inferences about lower vaccination uptake in their community. To investigate how treatment effects impact evolved as the bracelets were phased out, we examine responses in the Signaling Reward over time. Figure A2 presents the average treatment effects for each four-month cohort since the experiments start, compared to the Control group. The effects of the Signaling Reward progressively decline in line with the programs reduced implementation, reaching near zero 15 months post-experiment. If parents had interpreted the reduced bracelet frequency as a sign that fewer other children were being vaccinated, rather than recognizing it as the program winding down, we would not expect a drop in vaccination uptake. Fewer parents vaccinating should have increased the informativeness of the signaling reward for Measles 1, and parents would have had an even stronger incentive to vaccinate on time. The observed decrease in treatment effects suggests that parents understood the program was being phased out.

An alternative explanation is that parents recognized that the program was winding down, 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.

IV.B.4 Implementation-related Mechanisms

In this section, we explore plausible mechanisms of motivation crowd-out that may have arisen as a result of individuals receiving incentives during the experiment.

Self-perception and memory: Exposure to incentives may have altered parents perceptions of *why* they vaccinate their children. 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 repeated instances of taking an action, which is the case for childhood immunization.

To examine this hypothesis, we first assess whether parents accurately remember their childs vaccination and whether they received anything for it–two important assumptions of the model. Table B13 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 childs immunization. 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 childs 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 facing higher costs to be more sensitive to changes in their memory of why they took an action, as they are more likely to deliberate at each decision point. Figures A7 and A9 show that community population and distance to the clinic are 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, the number of children and parents education levels are also correlated with more negative treatment effects, with the former suggesting higher opportunity costs as well.

Changes in expectations from the nurse: Another possible mechanism as incentives are implemented is a shift in parents relationship to the action and their expectations of it. In this context, parents may receive various gifts when visiting clinics, such as bed nets, medicine, and food. Of these, food is the only item specifically tied to bringing their child to the clinic for vaccination. Table B12 shows that in the Control group, just over 10% of parents reported receiving food for their child during an immunization visit. This suggests there is potential for a reward to influence parents perceptions of what they can expect from vaccinating. Since incentives were implemented over a two-year period, they were likely not perceived as a temporary program and may have established a transactional norm around vaccination. When examining differences across treatment arms and by type of exposure to the bracelets, we find significant differences in exchanges with the nurse in the Double Reward treatment between directly and indirectly exposed parents, with the former being significantly more likely to either give ($p = 0.16$) or receive ($p = 0.06$) something from the nurse.

Parents in the Initiation and Double Reward treatments did not respond to the incentives during their implementation, raising the question of whether they valued the braceletsa necessary assumption for a change in expectations to be a relevant mechanism. A plausible explanation for the lack of effect from the initiation bracelet is the presence of a ceiling effect on initiation rates. In other words, the majority of parents were already highly motivated to bring their child for the first two vaccines on time, leaving little room for improvement. To explore this hypothesis, we analyze the short-run effects of incentives by distance to the clinic. Given the lower initiation rates in communities located farther from the clinic, if parents assigned some consumption value to the bracelet, we would expect to observe stronger effects in this subsample. Table B9 presents the results, showing larger and significant coefficients for the timely completion of vaccines two and three in distant communities, with coefficients of 4.3 and 5.8 percentage points ($p < 0.1$), respectively. We interpret this as suggestive evidence that parents positively valued the bracelets, despite close to universal take-up of the first vaccinations.[9]

We do not observe a positive effect for the fourth vaccine, indicating that the bracelet exchange at the fourth visit neither drives the increases seen in earlier vaccine visits nor serves as a valued signal. This aligns with the fact that there is sufficient scope to observe movement for this action in both distant and nearby communities, as evidenced by the large effects of the Signaling Reward on the timeliness of the fourth vaccine. A plausible explanation is that while parents may not initially have valued the bracelet exchange at the fourth vaccine enough to alter their behavior, they may have come to value it after receiving the new color bracelet and praise from the nurse. In other words, the experience of receiving the reward may have increased its perceived value through an endowment effect.

We do not observe such a positive effect for the fourth vaccine, demonstrating that the bracelet exchange at the fourth visit is neither driving these increases in earlier vaccine visits nor valued as a signal. This is consistent with the fact that there is sufficient room to detect movement on this margin in both far and close communities, as shown by the large effects of the Signaling Reward on timeliness for vaccine four. A plausible explanation is that while parents may not have valued the bracelet exchange on the fourth vaccine enough to change their behavior, they valued it after experiencing receiving the new color and praise from the nurse. In other words, the experience of receiving the reward increases parents' valuation for it through an endowment effect.

The absence of negative effects in the Signaling Reward treatmentwhere bracelets served as a signal of being a caring parentis consistent with the memory and change in expectation hypotheses. If parents exposed to the Signaling Reward misattributed

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

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, such as by informing other caregivers about their actions.

Reminder effort: Another plausible mechanism is that parents may have substituted their personal efforts to remind themselves of their childs 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 earlier habits and, as a result, failed to remember their childs vaccination due date after the bracelets were removed. Table B11 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. One limitation of this analysis is that it does not capture the frequency or extent to which parents reminded themselves, such as looking at their child's vaccination card, which indicates the vaccines' due dates. We interpret the results as evidence that the treatments did not affect the extensive margin of reminders.

V 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 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 relationship to the vaccination action 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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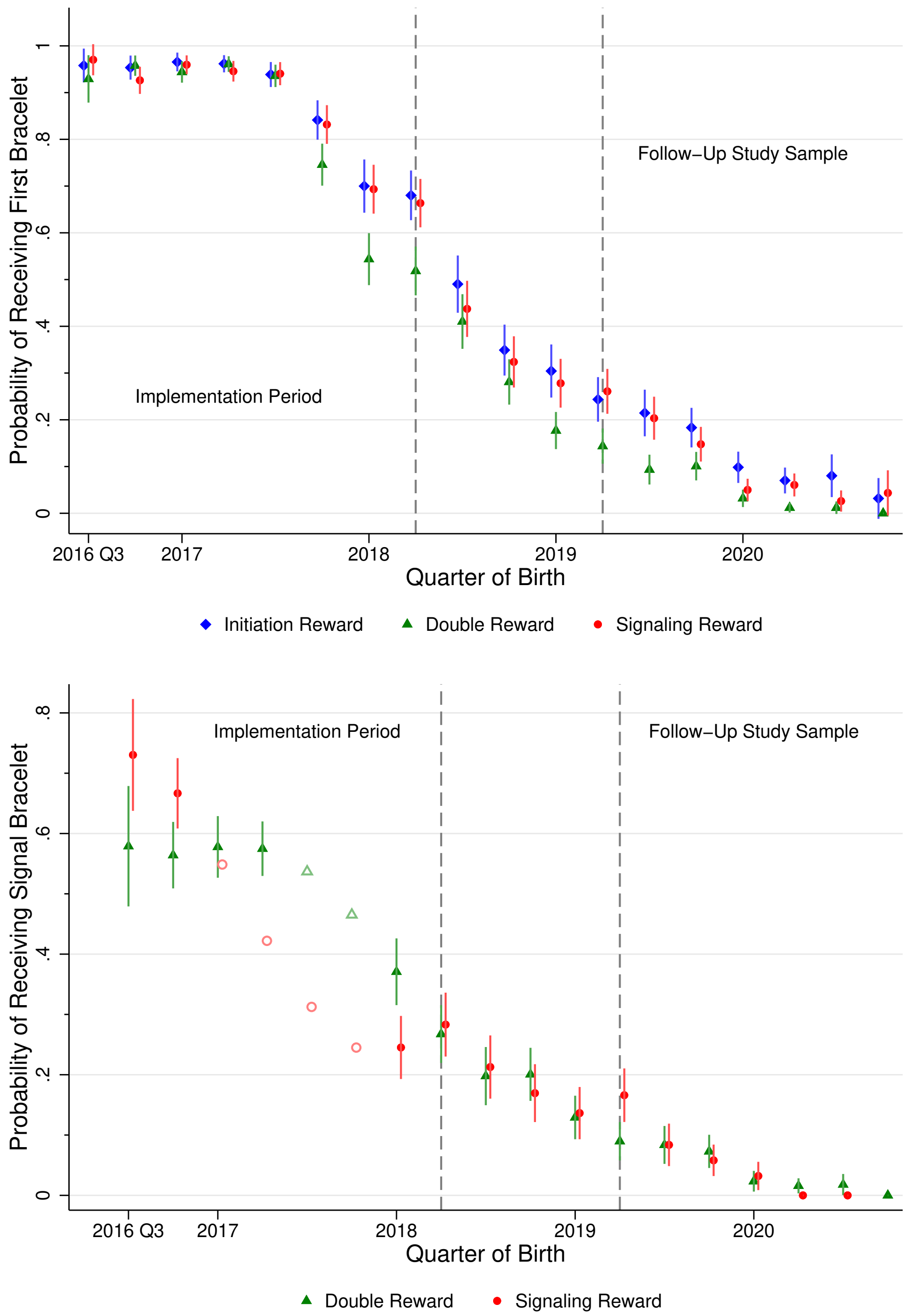

Figure I: Share of Children Receiving a First and Signaling Bracelet over Time by Treatment

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 [p-value]				
Variable	**Control 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

	(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 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]
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.

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]
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.

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

September 2024

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A Online Only Supplementary Figures

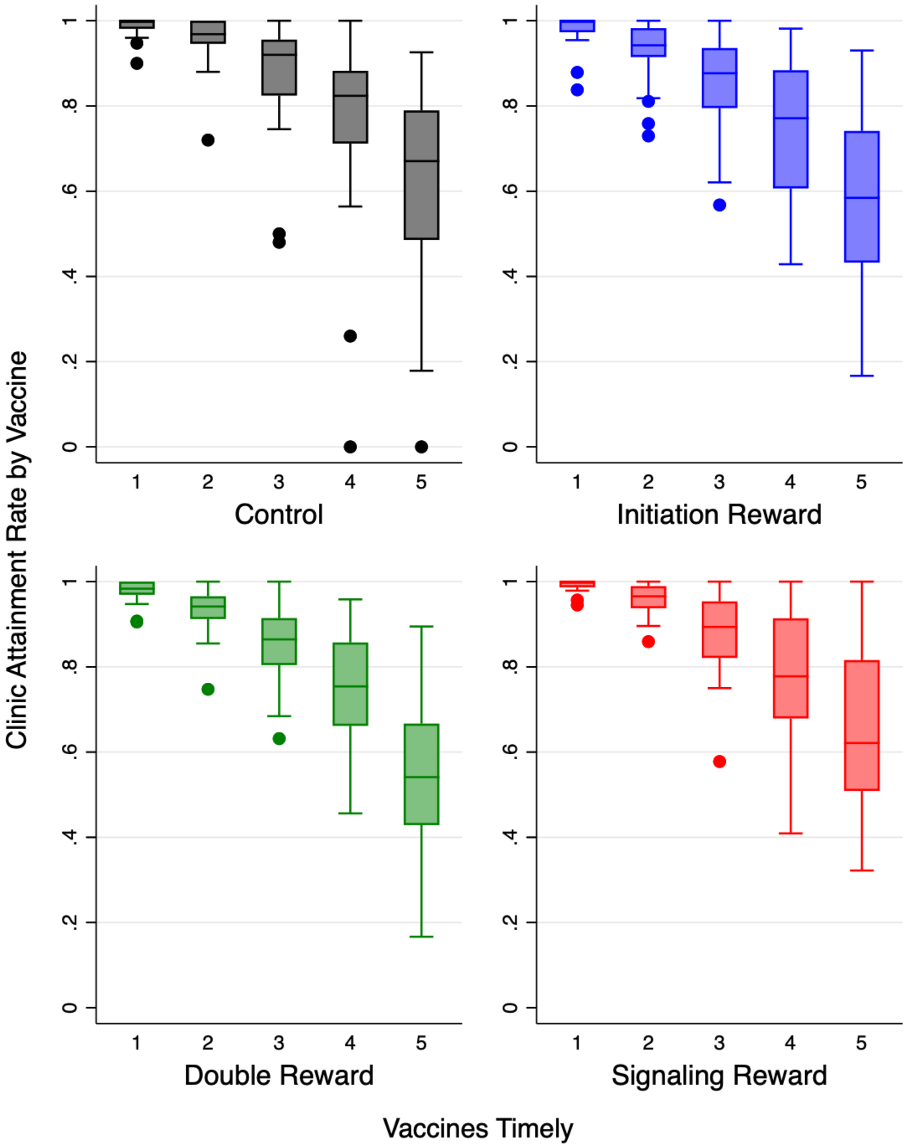

Figure A1: Clinic-Level Timely Vaccination Rate by Vaccine

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.

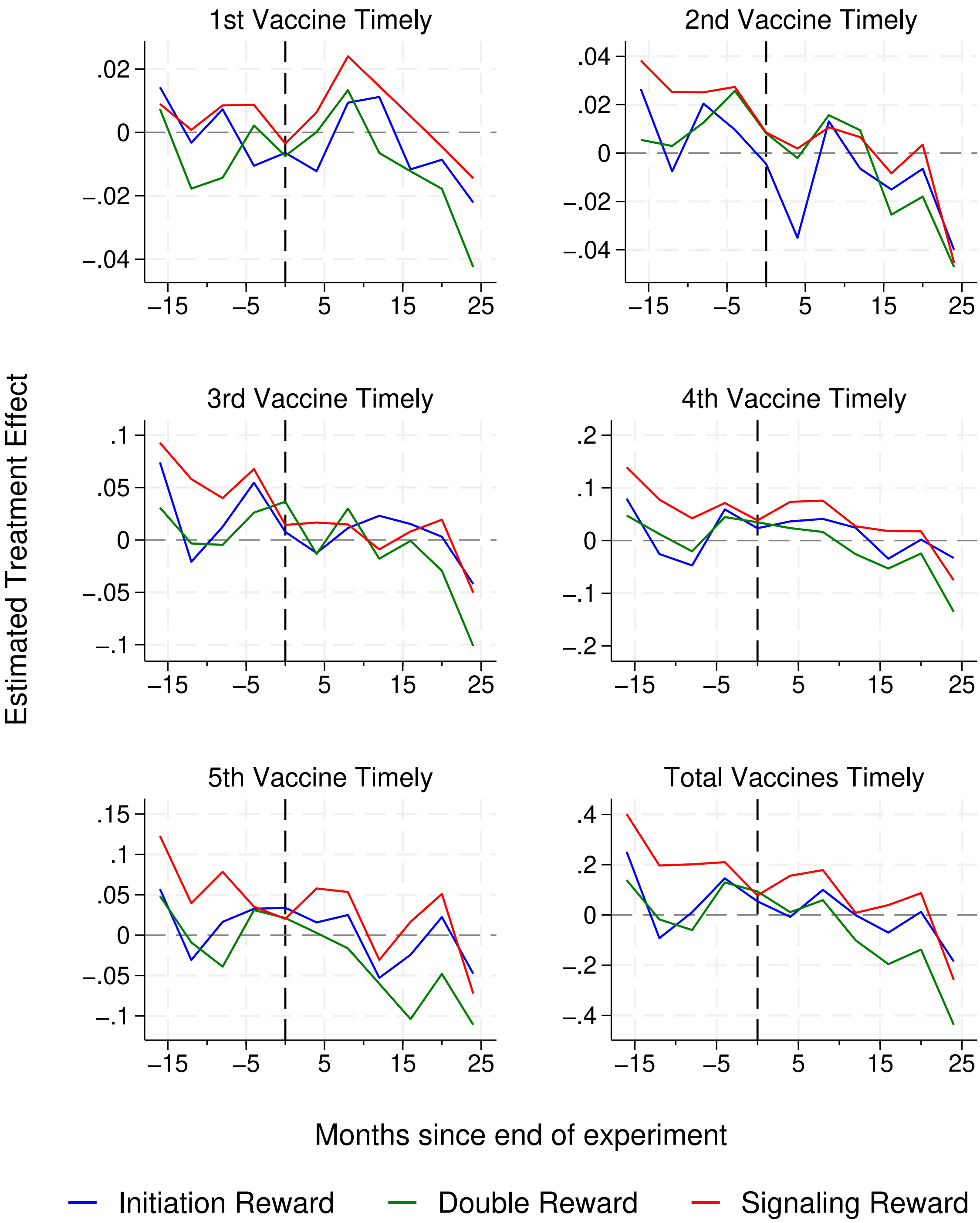

Figure A2: Estimated Treatment Effects by Birth Cohort

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).

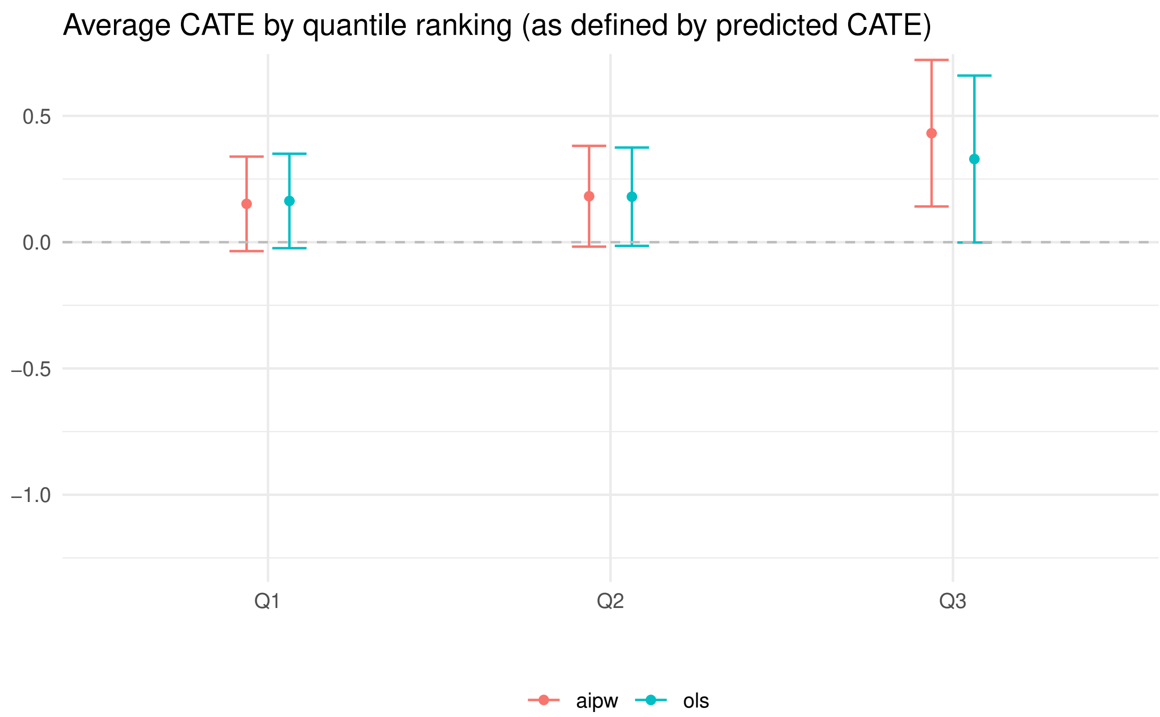

Figure A3: CATE Terciles for Signaling Bracelet Reward

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.

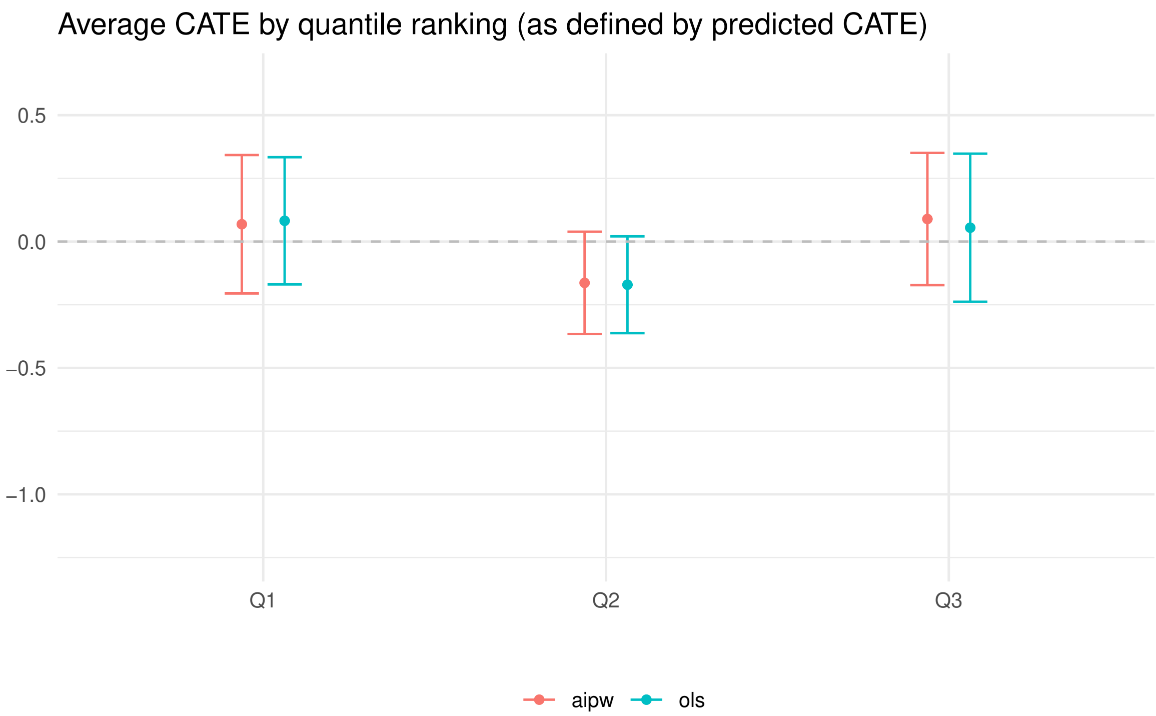

Figure A4: CATE Terciles for Double Bracelet Reward

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.

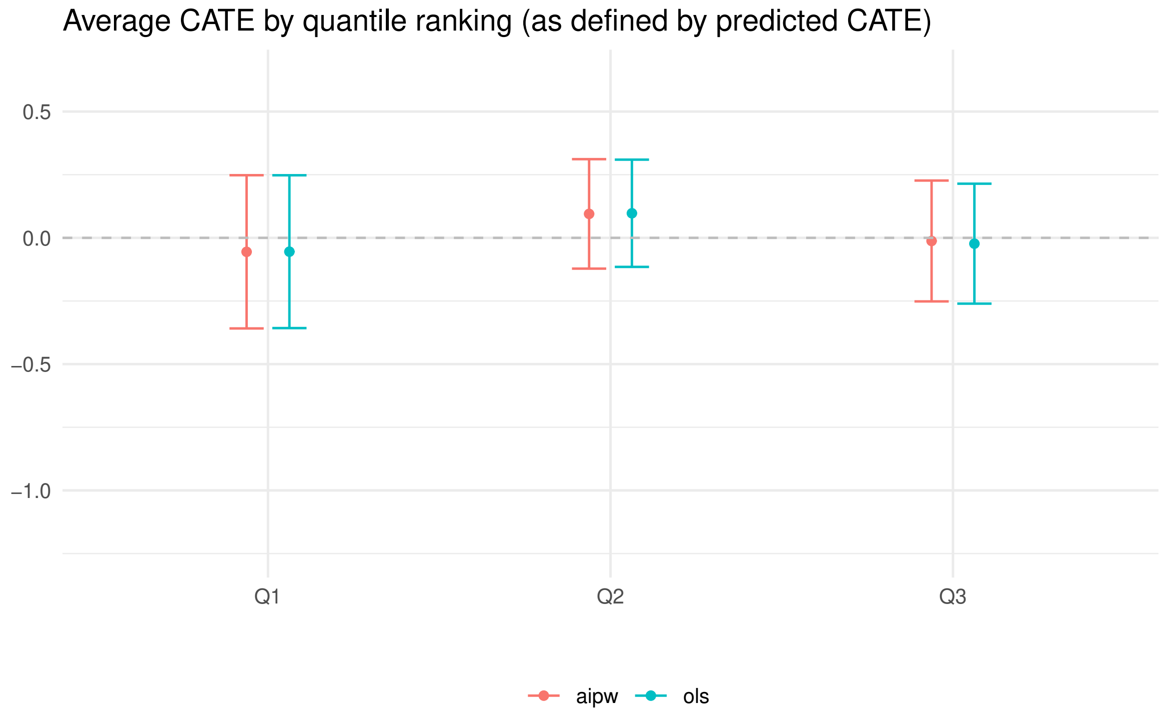

Figure A5: CATE Terciles for Initiation Bracelet Reward

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

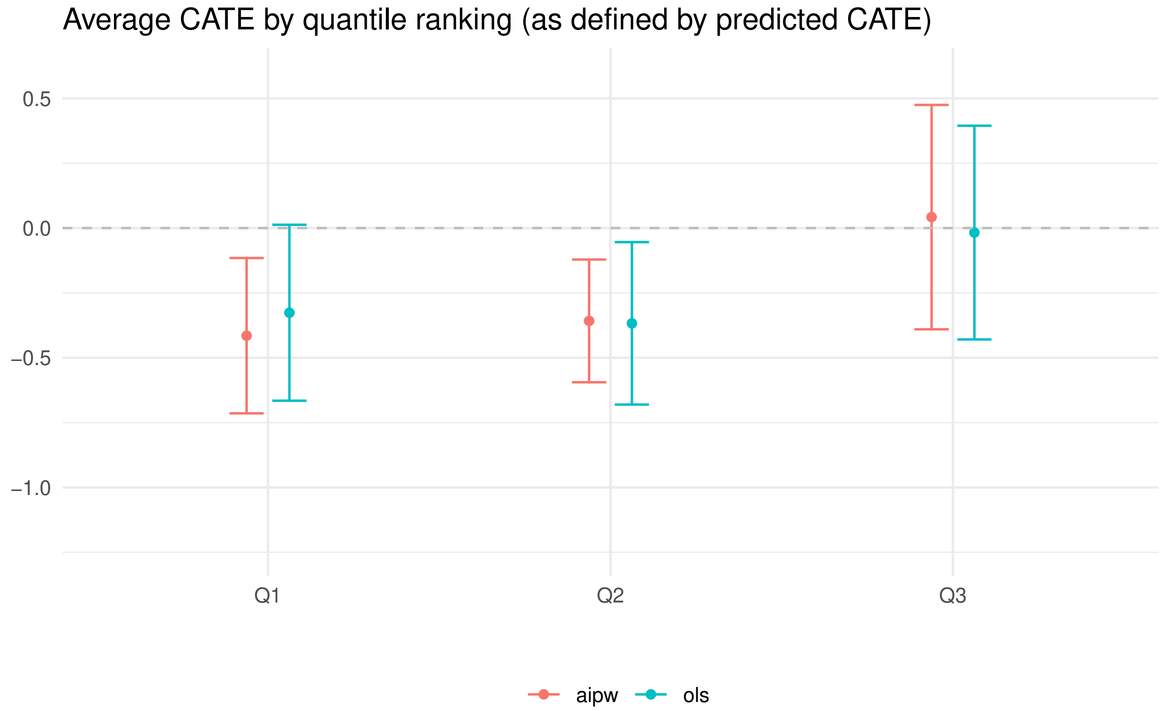

Figure A6: Conditional Average Treatment Effects of Direct Exposure to Initiation Reward on Number of Vaccines Timely

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).

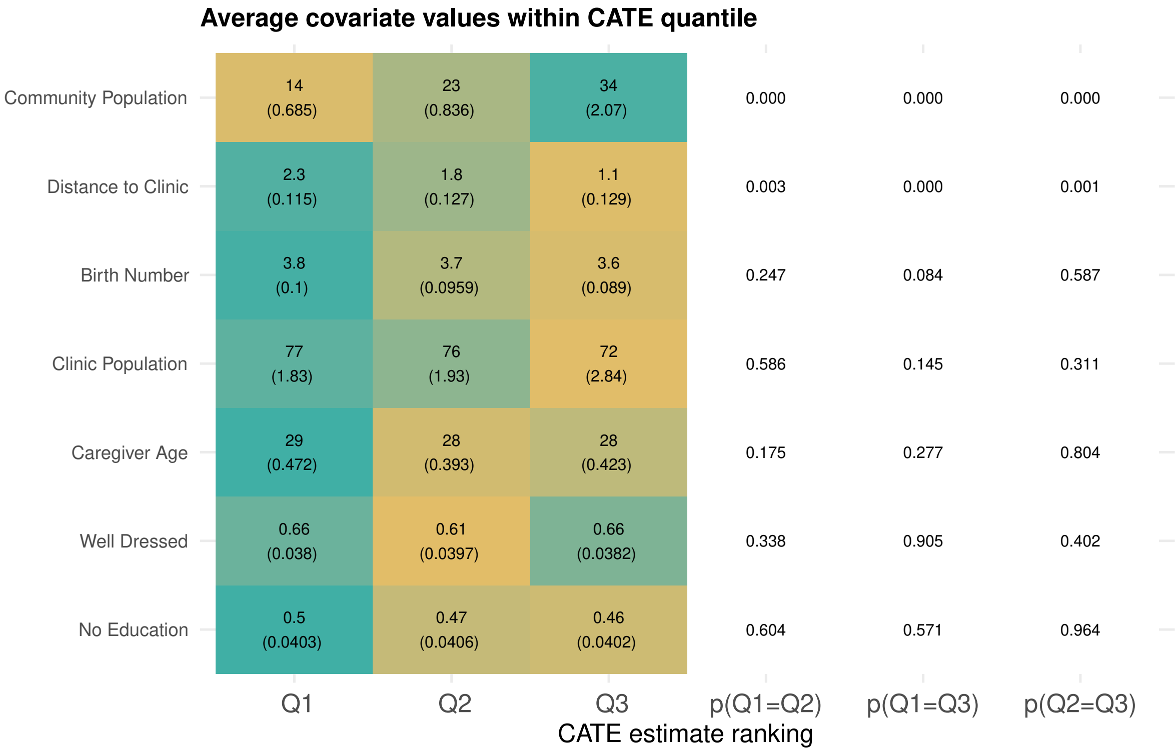

Figure A7: Caregiver Characteristics by Direct Exposure Initiation Reward CATE Tercile

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.

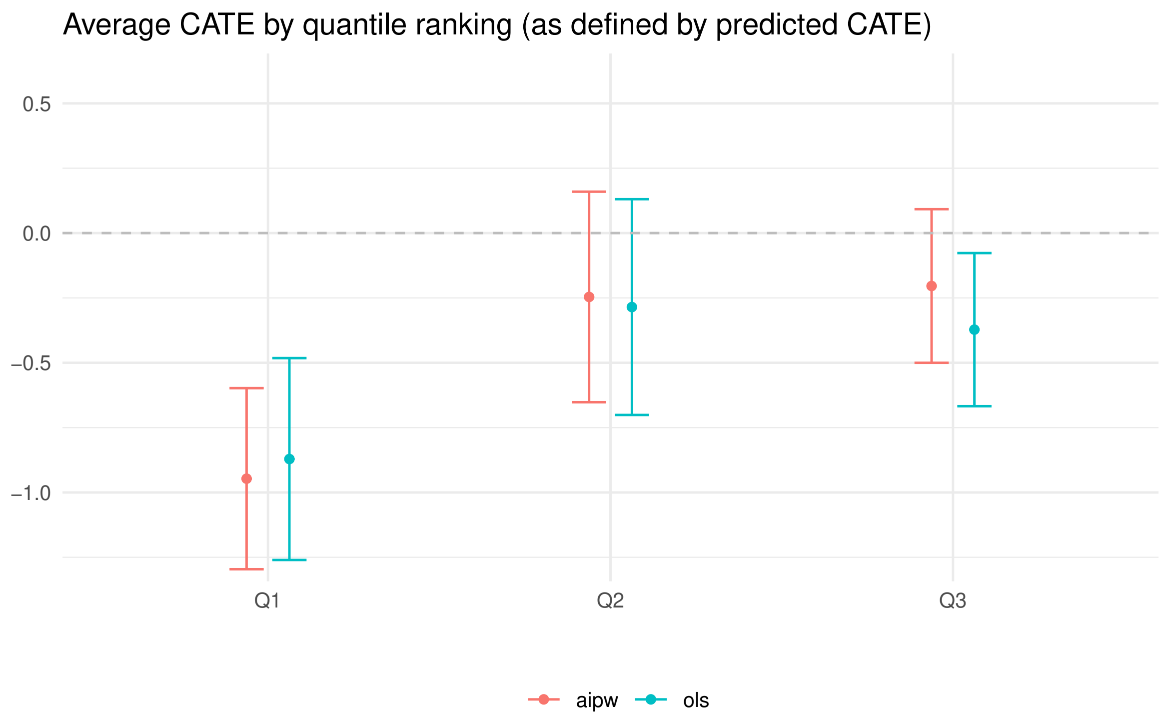

Figure A8: Conditional Average Treatment Effects of Direct Exposure to Double Reward on Number of Vaccines Timely

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).

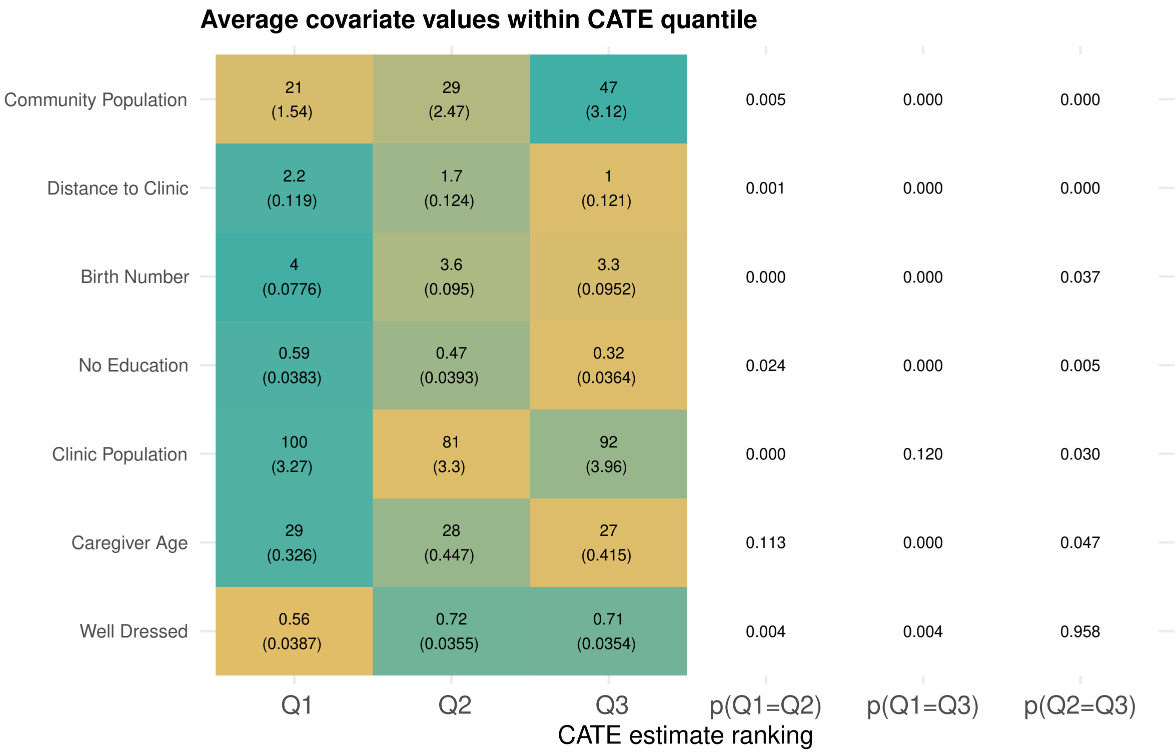

Figure A9: Caregiver Characteristics by Direct Exposure Double Reward CATE Tercile

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.

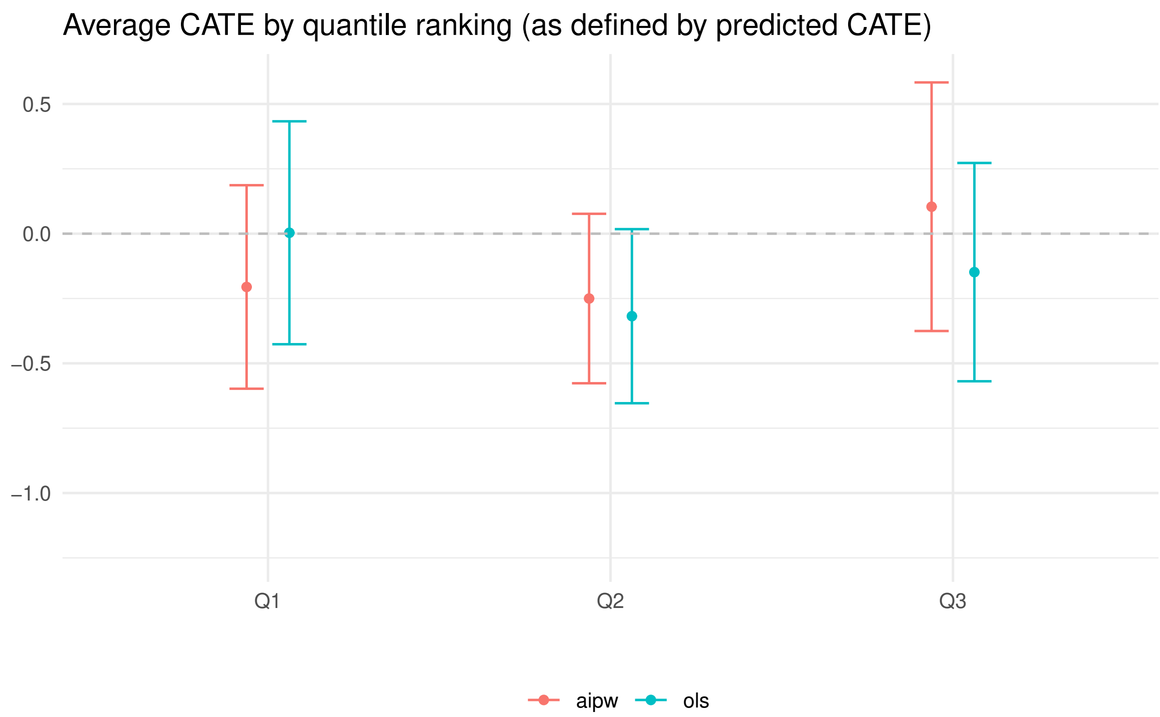

Figure A10: Conditional Average Treatment Effects of Direct Exposure to Signaling Reward on Number of Vaccines Timely

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).

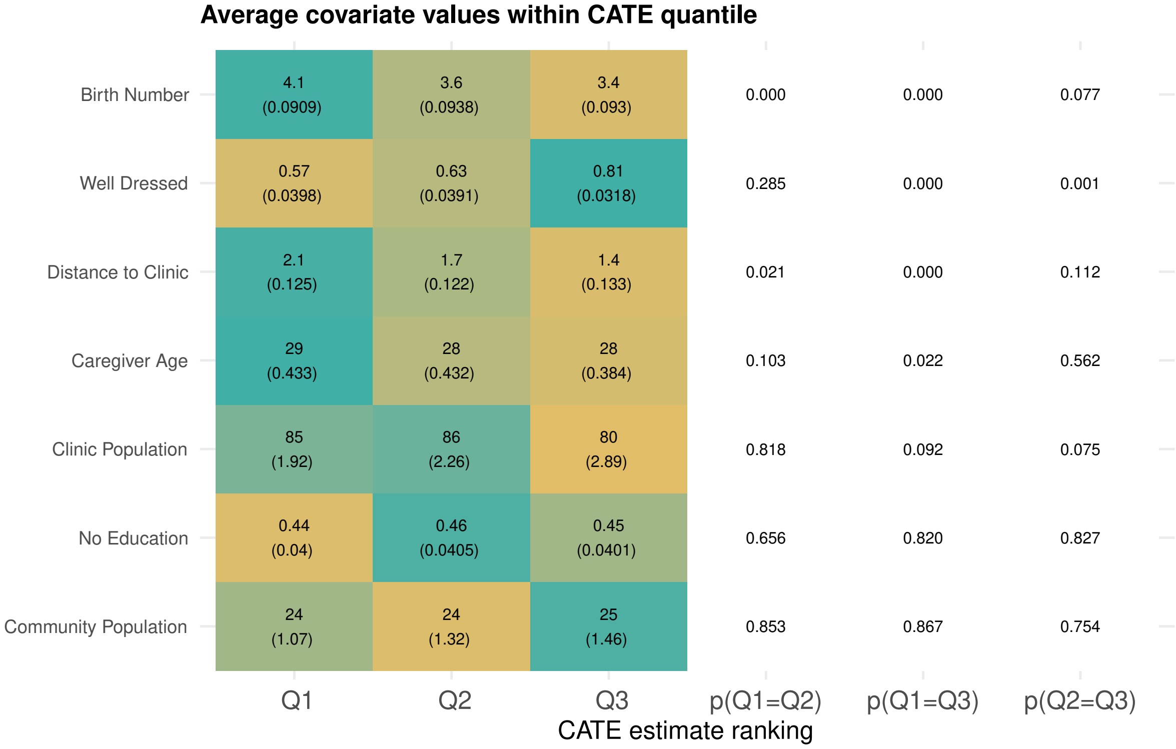

Figure A11: Caregiver Characteristics by Direct Exposure Signaling Reward CATE Tercile

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.

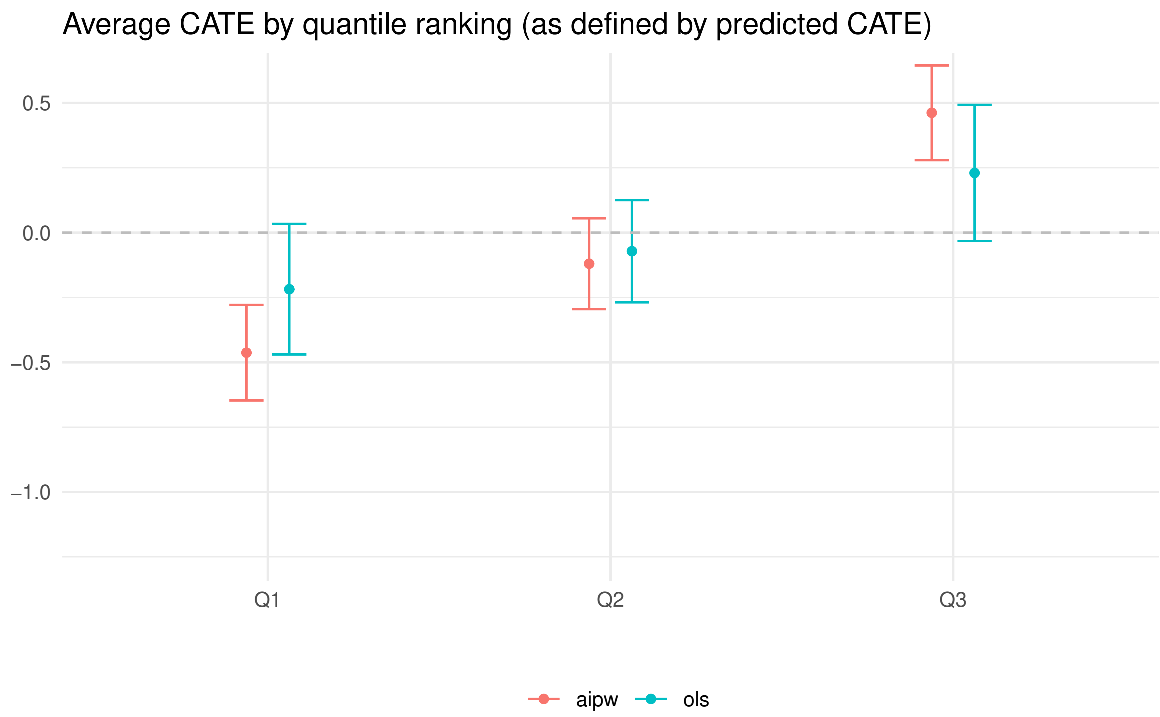

Figure A12: CATE Terciles for Initiation Bracelet Reward

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.

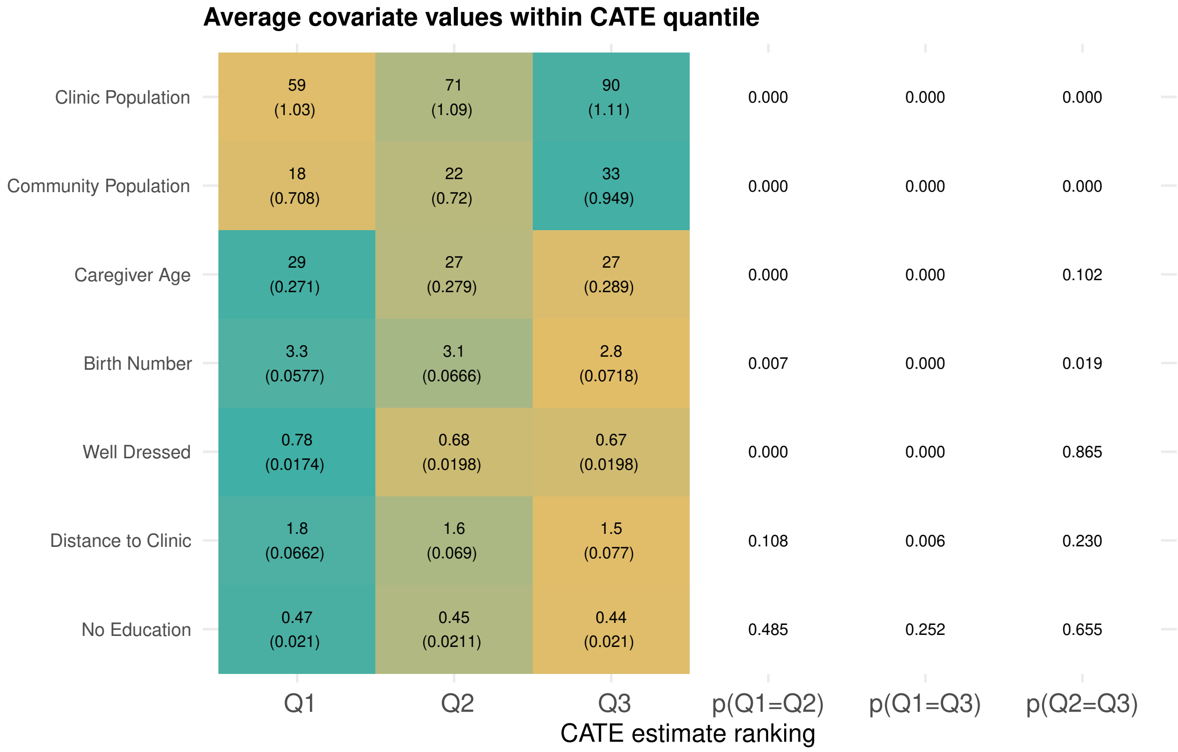

Figure A13: Average Covariate Values by Initiation Bracelet Reward CATE Quantile

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.

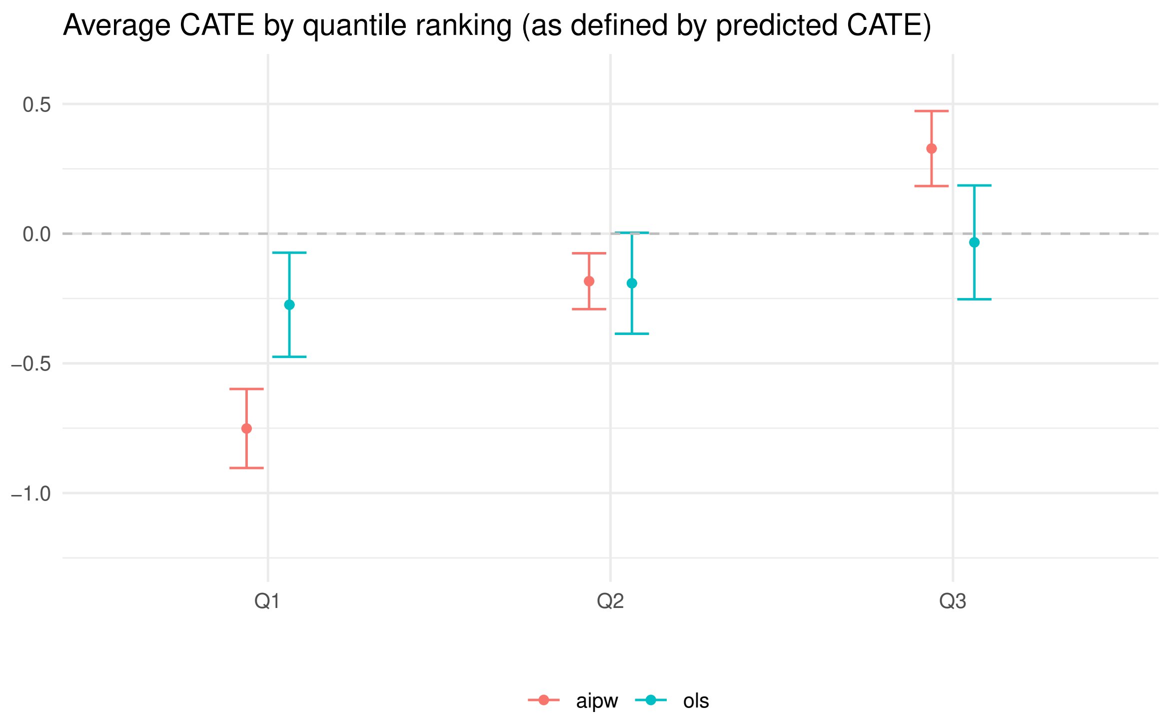

Figure A14: CATE Terciles for Double Bracelet Reward

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.

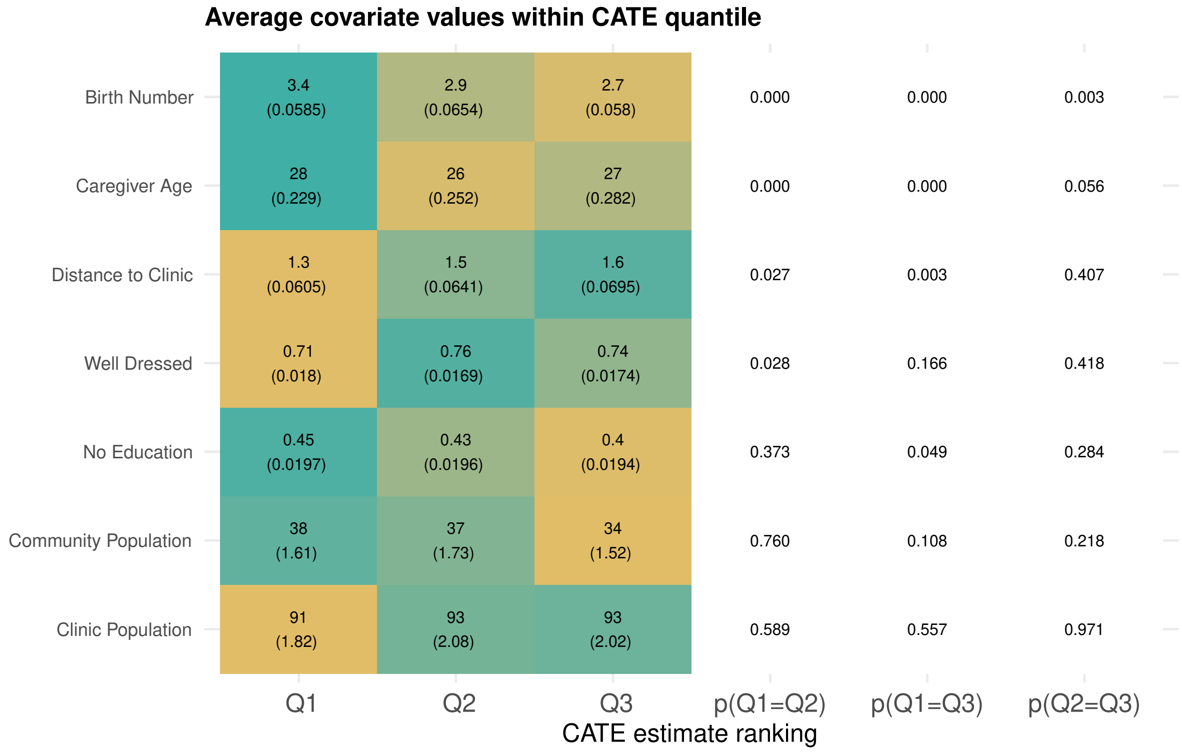

Figure A15: Average Covariate Values by Double Bracelet Reward CATE Quantile

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.

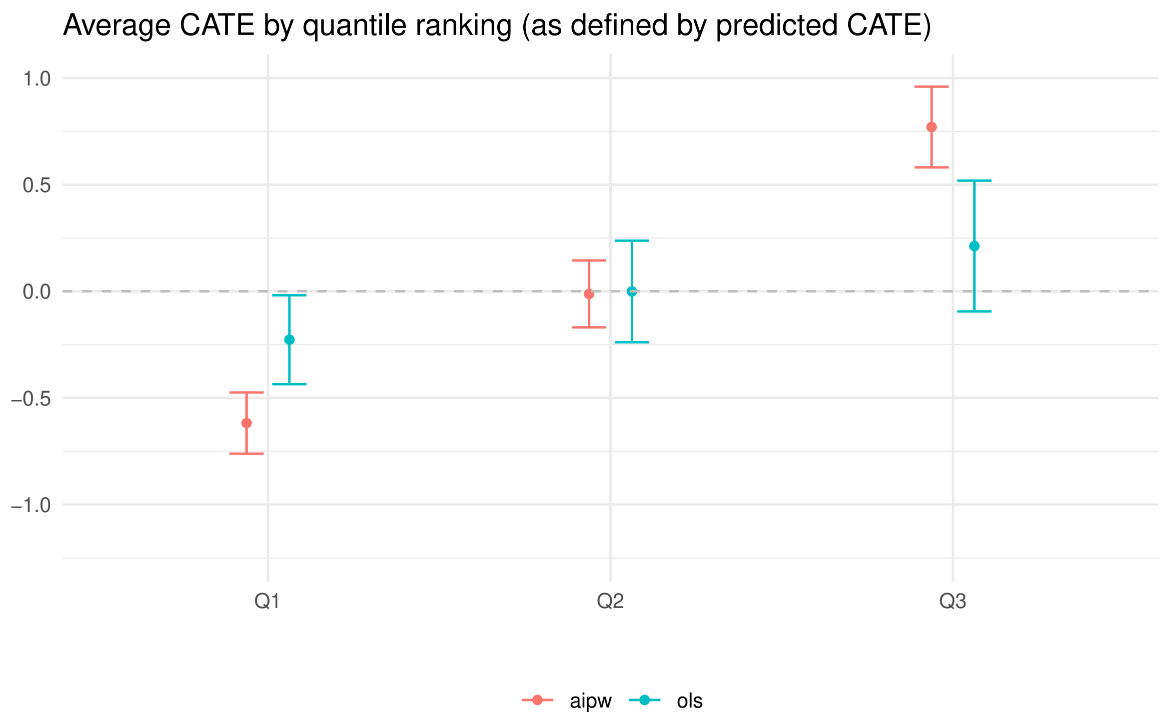

Figure A16: CATE Terciles for Signaling Bracelet Reward

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.

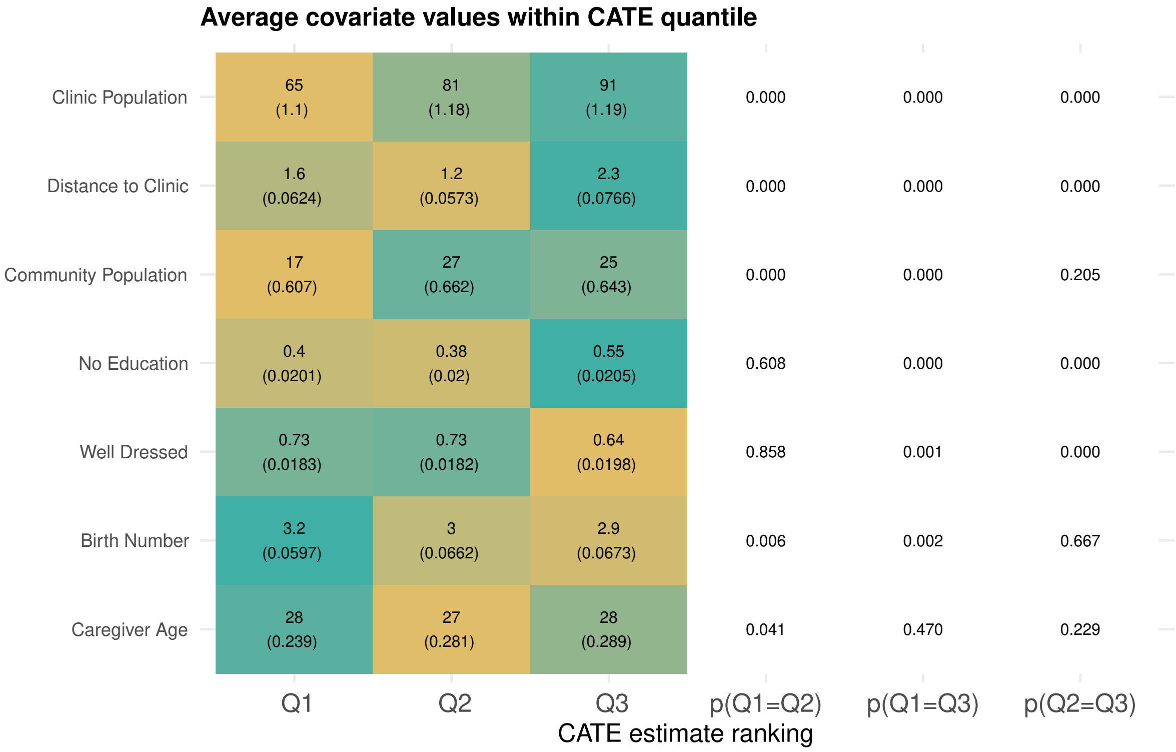

Figure A17: Average Covariate Values by Signaling Bracelet Reward CATE Quantile

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 Vaccine Related Behaviors

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 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.

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 [p-value]				
Variable	**Control 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

Variable	(1) Control Mean/(SE)	(2) (C)-(IN)	(3) (C)-(D)	(4) t-test differences (C)-(SL)	(5) [p-value] (IN)-(D)	(6) (IN)-(SL)	(7) (D)-(SL)	(8) 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 Population 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 population. These represent two clinics in the Double Reward treatment. 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: Effects of Removing Incentives Using Difference-in-Difference

Dependent variable:	**1st Vaccine**	**2nd Vaccine**	**3rd Vaccine**	**4th Vaccine**	**5th Vaccine**	**Total # of vaccines timely**
Signaling Reward	-0.014	-0.048*	-0.039	-0.098	-0.148*	-0.348**
	(0.016)	(0.029)	(0.046)	(0.068)	(0.084)	(0.152)
Double Reward	-0.036**	-0.077**	-0.073	-0.116*	-0.186***	-0.488***
	(0.016)	(0.032)	(0.055)	(0.068)	(0.068)	(0.158)
Initiation Reward	-0.049**	-0.063**	-0.060	-0.036	-0.105	-0.313**
	(0.019)	(0.029)	(0.058)	(0.059)	(0.070)	(0.158)
Control Group Mean	-0.007	-0.020	-0.045	-0.063	-0.105	-0.240
Obs.	1095	1095	1095	1095	1095	1095
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table displays differences-in-differences estimates of bracelet incentives for mothers where we observe all vaccination outcomes for both a child in the original experiment implementation and a child during the follow-up survey. The outcome in each column is timely vaccination by the age of 3, 4, 5, 6 and 11.5 months, respectively. Standard errors are cluster bootstrapped (500 repetitions) at the clinic level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B11: 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 B12: 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 B13: 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.