Social Signaling and Health Behavior in Low-Income Countries

by

Anne Karing

A dissertation submitted in partial satisfaction of the

requirements for the degree of

Doctor of Philosophy

in

Economics

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Professor Edward Miguel, Co-chair
Professor Stefano DellaVigna, Co-chair
Associate Professor Edward Augenblick

Spring 2019

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Abstract

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This dissertation is comprised of three essays at the intersection of Development and Psychology and Economics. The essays jointly explore how health behavior is influenced by social image concerns. Starting from the empirical fact that individuals care about how they are perceived by others, the essays connect economic theory to real-world settings to experimentally test the strength of these preferences. The objective is to shed light on potential mechanisms that increase the demand for preventative health.

In my first chapter, I introduce Bénabou and Tirole's theory of social signaling that motivates the randomized field experiments discussed in Chapters 2 and 3. I discuss how the theoretical framework maps into a static and dynamic decision making problem. For the static setting, I consider agents' decision to take up a one time deworming treatment. For the dynamic setting, I look at agents' sequential decision making when taking their children for different vaccinations across multiple periods. I investigate how take-up decisions change in the presence of social signaling concerns (i) as actions become more visible, (ii) as the cost of actions increases and (iii) as uncertainty in the form of future cost shocks become relevant. Using simulations I contrast the qualitative predictions of social signaling with and without uncertainty. In a first finding, I show that visibility in actions increases the probability of take up, conditional on individuals perceiving the action as socially desirable and valuing others' perception of their type. Secondly, I show that the effect of cost increases on take-up decisions can be amplified or mitigated as reputational returns change. Third, incorporating uncertainty into decision making leads to less strong bunching predictions at signaling thresholds and more continuous shifts in the distribution of actions. Fourth, I lay out testable predictions for the underlying mechanisms of the model and its assumptions. Chapters 2 and 3 empirically test the qualitative predictions laid out in this first chapter.

In my second chapter, coauthor Karim Naguib and I ask the question: Can social image concerns motivate adults to internalize health externalities? In collaboration with the Kenyan Government, we implement a new community program that offers free deworming treatment to 200,000 adults and emphasizes the public good aspect of deworming. Impor-

tantly, we randomize the introduction of two types of social signals in the form of colorful bracelets and ink applied to the thumb. The bracelets and ink allow adults to signal that they contributed to protecting their community from worms. To separate social signaling preferences from reminder and learning effects, we offer free text messages to a random subset of adults. Further, we exogenously vary the travel distance to treatment locations. We find that (1) bracelets as signals increase deworming take-up by 24 percent, outperforming a material incentive; (2) the effects are not due to pure reminder or learning effects; (3) there is no detectable effect for the ink signal, which we attribute to its lower visibility; (4) adults are highly sensitive to distance and both signaling treatments have a larger impact on take-up at far distances. The latter finding is consistent with the theoretical prediction that signaling returns increase as signals become more informative. Detailed survey data on first and second-order beliefs shed light on the underlying mechanism: signals reduce information asymmetries, and adults are more likely to think that others have information about their deworming decision.

In my third chapter, I investigate social signaling in the context of childhood immunization in Sierra Leone. Despite high initial vaccine take-up, many parents do not complete the five immunizations that are required in a child's first year of life. I introduce a durable signal - in the form of differently colored bracelets - which children receive upon vaccination, and implement a 22-month-long experiment in 120 public clinics. Informed by theory, the experimental design separately identifies social signaling from leading alternative mechanisms. In a first main finding, I show that individuals use signals to learn about others' actions. Second, I find that the impact of signals varies significantly with the social desirability of the action. In particular, the signal has a weak effect when linked to a vaccine with low perceived benefits and a large, positive effect when linked to a vaccine with high perceived benefits. Of substantive policy importance, signals increase timely and complete vaccination at a cost of approximately 1 USD per child, with effects persisting 12 months after the roll out. Finally, I structurally estimate a dynamic discrete-choice model to quantify the value of social signaling.

To my grandparents for their unlimited faith in me, their support and love.

Contents

List of Figures

List of Tables

Acknowledgments

It is difficult to put into words how grateful I am to my advisors for their mentorship and unwavering support over the course of my PhD. I have learned so much from each of them.

Ted Miguel took me under his wing before starting my graduate career at Berkeley. His research in health was a huge inspiration for me. He taught me to focus on topics that are of first order relevance to governments in low-income countries, while at the same time pushing myself to advance my technical skills and be rigorous in the implementation of my research. His class in development economics was one of the most important classes I took at Berkeley. The breadth and depth of Ted's understanding of the field of development economics, its many different topics, as well as methods, created a strong desire in me to become faculty myself. Ted's enthusiasm and unlimited faith in my abilities were crucial for me to complete this PhD and arrive at where I am now.

Stefano DellaVigna changed my view of economics. Through his class in psychology and economics and many office hour meetings, I learned about a new set of economic models and how to connect and test these in the real world. This was incredibly empowering, as it gave me a way forward to study empirical phenomena in contexts like Sierra Leone and Kenya. Stefano consistently encouraged me to formalize my ideas and showed incredible patience with my often non-linear approach to "figuring things out". And maybe most importantly, Stefano helped me focus and with that enabled me to pursue large field projects that I deeply cared about.

Ned Augenblick taught me how to think and how to embrace the things that I am not great at. I consider it one of the most fortunate events in my life to have become one of his students. Ned's commitment to understanding my research and help me figure out how to best execute it, is unique. There were countless times that I came to his office, feeling stuck, at times dumb and frustrated, and left uplifted, with a feeling of clarity and tools in my hand to make progress. Ned taught me by example to be precise and clear in my writing and presentations. I aspire to become an advisor like he was to me.

Rachel Glennerster is a role model both as researcher and practitioner. From her, I have learnt what is at the core of my work: how to do good empirical work in a low-income country setting. Under her guidance I was able to take my first independent steps doing research in the field and working with policy-makers, which ultimately led me to implement what became my job market paper. Her willingness to sit down with me, or pick up the phone, to talk about the nitty-gritty of survey implementation or how to write grant applications, meant the world to me. Rachel shaped my thinking about how to work with local institutions and made me understand the privilege that I have to work in countries like Sierra Leone. Her frank words and timely nudges saved me on multiple occasions from making bad decisions.

Fred Finan gave me some of the best practical advice for my research. Paul Gertler was always insightful and knew how to calm my nerves, when things were not going as planned in the field. I also benefited from helpful comments and feedback from Nava Ashraf, Natalie Bachas, Ceren Baysan, Stephanie Bonds, Zarek Brot-Goldberg, Supreet Kaur, Matthew Rabin, Jonathan Schellenberg, Frank Schilbach, Avner Shlain, Dmitry Taubinsky, Christopher Walters, Noam Yuchtman, and seminar participants at Berkeley.

None of this work would have been possible without the invaluable support of and collaboration with the Ministry of Health and Sanitation Sierra Leone, the International Growth Centre Sierra Leone and Innovations for Poverty Action, and especially Abdulai Bah, Fatu Conteh, Abou Bakarr Kamara, Dennis Marke and Osman Siddiqi. I would also like to thank Arthur Baker, Juliette Finetti, Jonas Guthoff, Anna Stamatiadi and Giang Thai for their immense support as research assistants.

Funding for the dissertation was generously provided by the Center for Effective Global Action, the William and Flora Hewlett Foundation, the International Growth Centre, the National Institutes of Health, the Weiss Family Fund for Research in Development, UNICEF Sierra Leone, and an anonymous donor.

Chapter 1

Social Signaling With and Without Uncertainty

1.1 Introduction

Individuals care about how they are perceived by others and take visible, potentially costly actions to influence others' perceptions of them. Recent field experiments have demonstrated that social image or signaling concerns are relevant across many behaviors, such as individuals' decision to vote, to make campaign contributions, and to the extent to which students exert effort on high school exams. There is a lack of empirical evidence, however, on how social signals can be harnessed to promote good behavior and the effects of these signals in dynamic settings. This dissertation explores social signaling in the context of two health behaviors, adult deworming and childhood immunization. While the decision to deworm is a one time take-up decision, the decision to vaccinate a child requires individuals to take multiple decisions across different time periods.

Both behaviors are imperfectly observable to others in status quo and have large externalities. Individuals lack an understanding of these but agree that "responsible" adults should deworm, and that "good" parents should vaccinate their children. To investigate to what extent social image concerns can motivate individuals to deworm or vaccinate, this dissertation discusses two field experiments that I implemented in Kenya and Sierra Leone. The main treatment, in both experiments, is the introduction of a new signaling mechanism, in the form of colorful bracelets, that allow individuals to show to others that they dewormed or vaccinated their children. Further, individuals are assigned to different walking distances - an important cost factor - to take up deworming treatment or vaccinations. To understand how well the mechanism and outcome predictions of the theory map into a real-life setting, I discuss in this chapter Bénabou and Tirole's (2006; 2011) theory of social signaling applied to individuals' decision to deworm and take up vaccinations.

There are a number of theoretical papers that provide a micro-foundation for social image concerns (Bénabou and Tirole, 2006; Andreoni and Bernheim, 2009; Ali and Lin, 2013). They propose different ways of modeling the social signaling parameters and how signaling returns are linked to the distribution of preferences in society. I will be working with Bénabou and Tirole's framework (2006; 2011) as it provides a clear mapping of model components to my empirical settings. This enables me to link the model's predictions to my experimental treatments and measure relevant intermediary outcomes to verify the mechanisms of the theory.

In this first chapter, I introduce the theoretical framework of social signaling, and investigate how take-up decisions change in the presence of social signaling concerns (i) as actions become more visible, (ii) as the cost of actions increases and (iii) as uncertainty in the form of future cost shocks become relevant. Using simulations I contrast the qualitative predictions of social signaling with and without uncertainty. In a first finding, I show that visibility in actions increases the probability of take up, conditional on individuals perceiving the action as socially desirable and valuing others' perception of their type. Secondly, I show that the effect of cost increases on take-up decisions can be amplified or mitigated as reputational returns change (∼ social multiplier). Third, incorporating uncertainty into decision making leads to less strong bunching predictions at signaling thresholds and more continuous shifts in the distribution of actions. Fourth, I lay out testable predictions for the underlying mechanisms of the model and its assumptions. In Chapters 2 and 3 of this dissertation, I will empirically test the qualitative predictions laid out in this first chapter. I will further estimate a structural model, that is built on the theoretical framework, to quantify the strength of social signaling preferences.

The remainder of this chapter is organized as follows. Section 2 introduces the theoretical framework of social signaling, applied to the decision problems of deworming and child vaccination, and discusses the predictions of the model without uncertainty. Section 3 augments the model to include uncertainty and discusses how that changes the predictions outlined in Section 2. Section 4 discusses potentially alternative mechanisms that are not included in the theoretical model.

1.2 Social Signaling without Uncertainty

In this section, I will map Bénabou and Tirole's (2006; 2011) theory of social signaling into the empirical decision problems of deworming and child vaccination. I will discuss the main predictions of the model abstracting from any uncertainty about future states of the world. I assume that individuals have perfect information about the cost of deworming and vaccination, and ex-ante decide on whether to take up deworming treatment and on the optimal number of vaccines. I separately discuss the model's prediction for deworming, as single action, and vaccination, as an example for multiple actions.

Single Action: Adult Deworming

Preferences are described by:[1]

$$U(a_i; v_i, d, x, r, \lambda, \omega) = B(a_i; v_i) - C(a_i; d) \quad + \quad x\lambda\omega E_{-i}(v|a_i; d_i) \tag{1.1}$$

Individuals, indexed by i, make a decision to take deworming treatment or not $a_i \in \{0, 1\}$. Individuals differ in their intrinsic motivation v_i, which is their valuation to contribute to a worm-free community or more generally a community's health. v_i is drawn from the continuous type distribution of v, $F(v)$, which is common knowledge to all individuals. v_i is known to individual i but not observable to others. $B(a_i; v_i)$ denotes the private benefit of deworming, which is a function of i's choice a_i and i's type. $C(a_i; d)$ denotes individual i's cost of deworming, defined in terms of travel distance d to the deworming treatment location.[2] Ignoring the third term of the model, we have a simple maximization problem where individual i chooses to take up deworming treatment a_i^*, by maximizing $U(a_i; v_i) = B(a_i; v_i) - C(a_i; d)$. Assuming that $B(a_i; v_i)$ is increasing and concave, and $C(a_i; d)$ is weakly convex, there is a unique function that maps for each individual i her type v_i to her optimal action: $a_i^* = a(v_i; d)$. Without loss of generality, assume that $\frac{\partial B(a_i; v_i)}{\partial v_i} > 0$, such that higher types receive greater utility from deworming and therefore will be more likely to take up deworming treatment.[3]

The key part of the model is the third term, the reputational benefits and costs associated with the expectations that others, indexed by $-i$, will form about i's type as actions become visible. Others can either observe that i chose to deworm, that is $a_i = 1$, or that i chose to not take treatment, that is $a_i = 0$. Let $x \in [0, 1]$ denote the probability that others observe i's choice. The parameter λ measures how much individual i cares about the expectations that others form about her, and ω corresponds to the social desirability of being seen as a type who chooses $a_i = 1$. Following the literature, I assume that $\lambda \geq 0$ and $\omega \geq 0$ given that the action $a_i = 1$ is desirable. In equilibrium, different types choose different actions, leading others to form expectations about i's type conditional on the action observed, that is, $E_{-i}(v|a_i = 1)$ or $E_{-i}(v|a_i = 0)$. Importantly, the expectations of others enter directly into i's utility as expressed in 12. Following the logic of Bénabou and Tirole (2006, 2011) there exists a unique set of actions under visibility such that each individual chooses an action a_i^{s*}, given the equilibrium actions of all other individuals. This equilibrium is characterized by the cut-off type $\hat{v}$ (who is indifferent between choosing the optimal $a_i^* = 0$ without visibility and deviating to $a_i^{s*} = 1$) and the reputational returns which solve the fixed-point equation:

$$U(a_i^{s*} = 1) - U(a_i^* = 0) = \underbrace{B(a_i^{s*} = 1; \hat{v}) - C(a_i^{s*} = 1; d) - B(a_i^* = 0; \hat{v})}_{\text{Difference in direct benefits}} + \underbrace{\lambda\omega\triangle(\hat{v})}_{\text{Reputational returns}} = 0 \tag{1.2}$$

[1]I follow, Bénabou and Tirole's (2006; 2011) and Bursztyn and Jensen (2017) here.

[2]There is no direct treatment cost since deworming medication is offered for free.

[3]Formally $a > a^{'}$ if $v > v^{'}$, $\forall\ v, v^{'}$.

where[4] $\triangle(\hat{v}) = \underbrace{E(v|a_i^{s*}=1) - E(v|a_i^*=0)}_{\text{Difference in the average type based on observed actions}}$

Given our previous assumption $\frac{\partial B(a_i;v_i)}{\partial v_i} > 0$, in equilibrium individuals with higher types will be more likely to take up deworming treatment than those with lower types.[5] An empirical object of consistent interest in Chapter 2 of this disseration will be the probability of deworming take-up $Pr(a_i(v) = 1)$.[6]

Theoretical Predictions

I here lay out the effects of x and d on the probability of deworming $Pr(a_i(v) = 1)$ and the empirical predictions that follow from the underlying mechanisms and assumptions of the model.

Main outcome

Prediction 1. $\frac{\partial Pr(a_i(v)=1))}{\partial x} > 0$ the probability of individuals choosing to deworm increases with visibility, if the action is perceived as socially desirable ($\omega > 0$) and individuals value others' perceptions of their type ($\lambda > 0$).

Following Bénabou and Tirole's (2011), I assume that the type distribution $F(v)$ has finite support $V \equiv [v_{\max}, v_{\min}]$ and a continuously differentiable density $f(v) > 0$. For simplicity, define the equilibrium take-up level of deworming as a function of distance d, assuming that distance enters utility linearly such that $C(a_i;d) = d \equiv c$ if $a_i = 1$ and $Pr(a_i(v) = 1) = 1 - F(\hat{v}(c))$, with its derivative $\frac{dPr(a_i(v)=1)}{dc} = -f(\hat{v})\frac{\partial \hat{v}}{\partial c}$. Further, assume that the type distribution is unimodal.[7] In the absence of visibility ($x = 0$), a one unit increase in c will reduce take-up by $f(\hat{v})$ since $\frac{\partial \hat{v}}{\partial c} = 1$. As actions become visible (assume $x = 1$), there is an additional indirect effect through reputational returns:

$$\frac{\partial \hat{v}}{\partial c} = \frac{1}{1 + \lambda\omega\triangle'(\hat{v})} \lesseqgtr 1 \tag{1.3}$$

where $\triangle'(\hat{v})$ is the change in reputational returns, caused by a shift in the cut-off type $\hat{v}$ through the change in cost. Reputational returns change as the observed action a_i becomes

[4] To make the link between types and actions more transparent, note that $E(v|a_i^{s*}=1) - E(v|a_i^*=0) = E(v|v \geq \hat{v}) - E(v|v < \hat{v})$.

[5] It is relatively straight-forward: Suppose, for the sake of contradiction, that there exists an equilibrium in which the action taken by v, v' with $v > v'$ is $a < a'$. By definition the third term concerning other people's inferences, given actions, is the same for all types v. Consequently, if a lower type v' prefers to take the action a' instead of a, then it must be that a higher type must also prefer the action. That contradicts the initial supposition that the higher type prefers a to a'.

[6] I am dropping excess parameters here, since in the empirical part of the analysis these are unobservable.

[7] If the type distribution is uniform, reputational returns will be constant, such that a change in c has the same effect on take-up as in the no visibility case.

more or less informative about i's type, that is, the difference between $E(v|v \geq \hat{v})$ and $E(v|v < \hat{v})$ increases or decreases:

- If $\triangle^{'}(\hat{v}) < 0$ i.e., $\frac{\partial E(v|v\geq\hat{v})}{\partial\hat{v}} < \frac{\partial E(v|v<\hat{v})}{\partial\hat{v}}$ the effect of an increase in cost on take-up will be amplified by a decrease in reputational returns, as signals become less informative. This is the case if take-up of deworming is high ("everyone but the worst people do it") and increases in c lower the pressure on those that do not deworm as the share of non-dewormers increases.
- If $\triangle^{'}(\hat{v}) > 0$ i.e., $\frac{\partial E(v|v\geq\hat{v})}{\partial\hat{v}} > \frac{\partial E(v|v<\hat{v})}{\partial\hat{v}}$ the effect of an increase in cost will be mitigated by an increase in reputational returns, as signals become more informative. This is the case if take-up is low and increases in c increase the praise for those individuals that deworm.[8]

Prediction 2. The effect of an increase in the cost of deworming on the probability of take-up is reduced (increased), if reputational returns increase (decrease) due to changes in the cut-off type $\hat{v}$.

Mechanisms

i. Individuals observe others' actions more often than not: $\Pr_{-i}(a_i = 1|a_i = 1)$ - $\Pr_{-i}(a_i = 1|a_i = 0) > 0$.
ii. Individuals form expectations about others' types conditional on the actions observed: $E_{-i}(v|a_i = 1)$ - $E_{-i}(v|a_i = 0) > 0$.

Assumption

Individuals have imperfect information about others' actions, so that visibility in actions provides new information about others' actions (and subsequently types).

Multiple Actions: Childhood Vaccination

Preferences are described by:

$$U(a_i; v_i, d, x, r, \lambda, \omega) = B(a_i; v_i) - C(a_i; d) \quad + \quad x\lambda\omega \begin{Bmatrix} E_{-i}(v|a_i \geq r) & \text{if } a_i \geq r \\ E_{-i}(v|a_i < r) & \text{if } a_i < r \end{Bmatrix} \tag{1.4}$$

The same assumptions about preferences, model parameters and information structure as stated in Section 2.1 hold for the case of multiple actions.

[8]For their to be a unique equilibrium, we follow Bénabou and Tirole's (2011) assuming that $1 + \lambda\omega\triangle^{'}(\hat{v}) > 0$, which holds for $\lambda\omega$ not too large.

Individuals, indexed by i, make a decision to take their child for zero, one, two, three, four or five vaccinations $a_i \in \{0, 1, 2, 3, 4, 5\}$. Individuals differ in their intrinsic motivation v_i to look after their child's health. $B(a_i; v_i)$ denotes the private benefit of vaccination. $C(a_i; d)$ denotes the cost of vaccination, defined in terms of distance d to the clinic. Let $r \in \{1, 2, 3, 4, 5\}$ denote the threshold number of vaccines, that partitions the six possible actions a_i into two groups of observable vaccine decisions: others can either observe that i chose to vaccinate her child for at least r vaccines, that is $a_i \geq r$, or that i chose to vaccinate her child for fewer than r vaccines, that is $a_i < r$. In equilibrium, different types choose different actions, leading others to form expectations about i's type conditional on the action observed, that is, $E_{-i}(v|a_i \geq r)$ or $E_{-i}(v|a_i < r)$. The equilibrium is characterized by the cut-off type $\hat{v}_r$ (who is indifferent between choosing the optimal a_i^* without visibility and deviating to $a_i^{s*} = r$) and the reputational returns which solve the fixed-point equation:

$$U(a_i^{s*}) - U(a_i^*) = \underbrace{B(a_i^{s*}; \hat{v}_r) - C(a_i^{s*}) - B(a_i^*; \hat{v}_r) + C(a_i^*)}_{\text{Difference in direct benefits}} + \underbrace{\lambda\omega\triangle(\hat{v}_r)}_{\text{Reputational returns}} = 0 \tag{1.5}$$

where[9] $$\triangle(\hat{v}_r) = \underbrace{E(v|a_i^{s*} \geq r) - E(v|a_i^* < r)}_{\text{Difference in the average type based on } \textit{observed} \text{ actions}}$$

An empirical object of consistent interest in Chapter 3 of this dissertation will be the discrete probability density function $g(a) = Pr(a_i(v) = a)$, with the associated discrete cumulative distribution function $G(a) = Pr(a_i(v) \leq a)$.[10] I will use the cumulative distribution function to specify the share of children that completed at least a vaccines, that is, $Pr(a_i(v) \geq a)$.

Theoretical Predictions

In Chapter 3 of this dissertation, I will experimentally manipulate the visibility of vaccines x and threshold number r to test their effects on the share of children vaccinated. I here lay out the theoretical predictions of the effect of x on the distribution $G(a)$ and the empirical predictions that follow from the underlying mechanisms and assumptions of the model.

Main outcome

1. $\frac{\partial Pr(a_i(v) \geq r))}{\partial x} > 0$ the probability of individuals choosing to vaccinate at at least r increases with visibility, if the action is perceived as socially desirable ($\omega > 0$) and individuals value others' perceptions of their type ($\lambda > 0$).

[9]To make the link between types and actions more transparent, note that $E(v|a_i^{s*} \geq r) - E(v|a_i^* < r) = E(v|v \geq \hat{v}_r) - E(v|v < \hat{v}_r)$.

[10]I am dropping excess parameters here, since in the empirical part of the analysis these are unobservable.

2. $\frac{\partial Pr(a_i(v) \geq r-\tau))}{\partial x} \geq 0$ the probability of individuals vaccinating at at least $r-\tau$ remains constant, unless all individuals who previously vaccinated at $r-\tau$ moved to r, such that $Pr(a_i(v) \geq r)) = Pr(a_i(v) \geq r-\tau))$ $\forall \tau \in \{1, 2...r-1\}$.

3. $\frac{\partial Pr(a_i(v) \geq r+\tau))}{\partial x} \geq 0$ the probability of individuals choosing to vaccinate at at least $r+\tau$ depends on the cost-benefit structure of vaccination. The probability remains constant if the marginal net benefits are constant or declining ($\frac{B(a_i;v_i)-C(a_i)}{a_i} \leq 0$), and it increases if marginal net benefits are increasing $\frac{B(a_i;v_i)-C(a_i)}{a_i} > 0$ $\forall \tau \in \{1, 2...S-1\}$.

4. $\frac{\partial^2 Pr(a_i(v) \geq r))}{\partial x \partial \lambda} > 0$ the effect of an increase in x is increasing in the value individuals assign to their social image.

5. $\frac{\partial^2 Pr(a_i(v) \geq r))}{\partial x \partial \omega} > 0$ the effect of an increase in x is increasing in the social desirability of being seen as type who chooses $a \geq r$. If there are no concerns of social approval or disapproval ($\omega = 0$), changing x should have no effect on vaccine outcomes.

Mechanisms

i. Individuals observe others' actions more often than not: $\Pr_{-i}(a_i \geq r | a_i \geq r))$ - $\Pr_{-i}(a_i \geq r | a_i < r) > 0$.
ii. Individuals form expectations about others' types conditional on the actions observed: $E_{-i}(v|a_i \geq r)$ - $E_{-i}(v|a_i < r) > 0$.

Equilibrium Simulations with Signaling

Figure 1.1 presents results from two calibrated simulations, first assuming x=0 (no visibility of actions) and second x=1 (full visibility of actions), to illustrate the equilibrium effects of visibility on the cut-off type, $\hat{v}_r$, and type expectations. Using the empirical rates of vaccination for vaccine one, two, three, four and five from the control group data from my experiment, I calibrate the moments of a normal type distribution $v \sim N(\mu_v, \sigma_v)$ and the parameters of the utility function:

$$U(a_i; v_i) = (v_i - \kappa D)a_i - \sum_{a=1}^{a_i} \alpha a + x\lambda\omega \mathbb{1}(a_i = r)[E(v|a_i \geq r) - E(v|a_i < r)] \tag{1.6}$$

where I assume that the marginal cost of vaccination κ is constant, and the marginal benefit is declining. $D = 2$ is set to the mean walking distance to the clinic. The calibrated parameters are $\mu_v = 1.48$, $\sigma_v = 0.64$, $\kappa = -0.1$, $\alpha = -0.3$. I assume that individuals can signal that they took their child for five vaccinations, with $r = 5$ and that $\lambda\omega = 0.2$. I solve for the cut-off type $\hat{v}_5$ and $\triangle(\hat{v}_5)$ using the fixed-point equation 1.5. Visibility, as indicated by "Signal at 5" in Figure 1.1, leads to a shift in the cut-off, v_5, to the left, meaning that individuals with lower types are now choosing $a_i^{s*} = 5$. However, given the magnitude of reputational returns $\lambda\omega\triangle(\hat{v}_5)$, only some individuals who previously chose $a_i^* = 4$ now vaccinate further, while everyone who chose $a_i^* < 4$ in the absence of visibility, will continue to choose the same number of vaccines. As v_5 shifts to the left, and lower types start to vaccinate further, $E(v|a_i^{s*} = 5) < E(v|a_i^* = 5)$ and $E(v|a_i^{s*} < 5) < E(v|a_i^* < 5)$, meaning that visibility lowers the average type expectations for those who vaccinate at 5 (since some low type individuals moved in) and for those who vaccinate at less than 5 (since some high type individuals moved out).

1.3 Social Signaling with Uncertainty

The above model assumes that individuals have perfect information about the future. However, uncertainty is a common feature of many decision-making processes in low-income countries. Individuals are exposed to cost or preference shocks, in the form of sickness of household members or unforeseen work obligations, that make it difficult to travel to the clinic. Instead of assuming that individual i has perfect information and ex-ante decides on the optimal number of vaccinations, I now consider the case where she decides in each period t whether to take her child for the next vaccine, or stop. The flow utility of a vaccine at time $t \in \{1, 2, 3, 4, 5\}$ is given by:[11]

$$u_{it} = b(t; v_i) - c(t) \quad + \quad \lambda\omega\triangle(\hat{v}_r)\mathbb{1}\{t = r\} + \epsilon_{it}$$

and the utility of stopping vaccination is normalized to zero. This gives the value function at t:

$$V_{it} = \max\{0, u_{it} + \underbrace{E[V_{it+1}|v_i]}_{\text{Continuation value}}\} \quad \text{for t} < 5$$

$$V_{i5} = \max\{0, u_{i5}\} \quad \text{for t} = 5$$

where $b(t; v_i)$ and $c(t)$ denote the marginal benefit and cost of vaccine $t \in \{1, 2, 3, 4, 5\}$, $\lambda\omega\triangle(\hat{v}_r)$ the reputational return from vaccinating up to $t = r$, and ϵ_{it} a new, second source of unobserved individual heterogeneity in the form of iid logistically distributed shocks.[12] Individuals are assumed to know the distribution of shocks, but only learn in period t about the realization of their shock. Individuals therefore maximize the *expected* future value of vaccines. This decision-problem is solved by backward recursion, with individuals optimizing according to the decision-rule: vaccinate if $V_{it} > 0$, stop otherwise.

Comparing individual decision-making under uncertainty to that without, theoretical predictions 2 and 3 change. As individuals plan dynamically, individuals' decision to deviate

[11] For simplicity I am dropping parameters here and denoting $u_t(t; v_i, x, r, \lambda, \omega)$ as u_{it}.

[12] Relating back to the static model without uncertainty in , the action a in the dynamic model is denoted by t since the decision to take a certain number of vaccines (e.g. $a_i = 2$, two vaccines) coincides with the time period (e.g. $t = 2$). The marginal benefit and cost in the dynamic model are therefore equivalent to $b(t; v_i) = B(t; v_i) - B(t-1; v_i)$ and $c(t; v_i) = C(t; v_i) - C(t-1; v_i)$. In the dynamic model, I am assuming we are in equilibrium, with individuals taking reputational returns as given.

from the optimal action chosen in the absence of visibility, is now partly decoupled from their decision to vaccinate up to r. Individuals choose to vaccinate further if the option value of signaling is sufficiently large (for them to expect to vaccinate up to r), and will stop vaccinating before reaching r if they receive a too negative cost draw. As a result, individuals are more likely to complete earlier vaccines ($r - \tau \ \forall \tau \in \{1, 2...r-1\}$), even if not making it to r, where the signaling benefit occurs (formally $\frac{\partial Pr(a_i(v) \geq r - \tau)}{\partial x} \geq 0$, without the condition $Pr(a_i(v) \geq r) = Pr(a_i(v) \geq r - \tau)$). Further, individuals are more likely to vaccinate for $r + \tau$ vaccines even if the marginal net benefit of vaccination is declining. Some of the individuals who vaccinate up to r, receive a positive cost shock in $t = r + \tau$ making it optimal for them to vaccinate further.

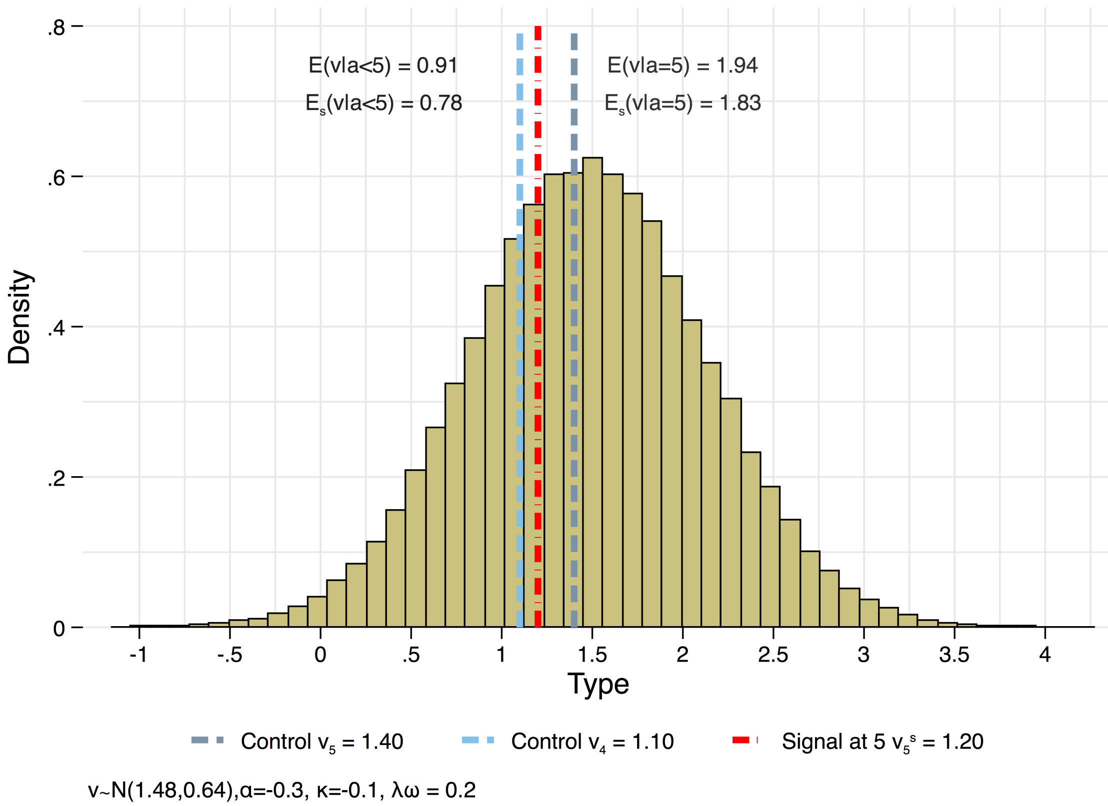
Figure 1.2 shows how augmenting the social signaling model to include uncertainty changes the qualitative predictions of the model, by comparing the simulated effects of visibility at vaccine four and five, for the cases with and without uncertainty. Extending the signaling model to include uncertainty produces less stark bunching predictions at thresholds $r \in \{4, 5\}$ and more continuous shifts in the distribution $G(a)$.

1.4 Discussion

The model laid out in Sections 2 and 3 abstracts from the externality benefits of deworming. I do so for two reasons: (1) individuals in the context of deworming and child vaccination predominantly think of these behaviors as private goods and lack an understanding of the externalities; (2) formally including the level of take-up (of deworming treatment or specific vaccinations) as part of individuals' preferences would not change the qualitative predictions of the model. I provide empirical evidence from survey questions on individuals' lack of understanding of externalities in Chapters 2 and 3. If individuals understood the externality effects of these health behaviors, they might have an incentive to free-ride on the disease protection that others provide. As more children get vaccinated and adults dewormed because of social image concerns, the private benefits of vaccinations decline, which would mitigate the positive effect that reputational returns have on take-up decisions.

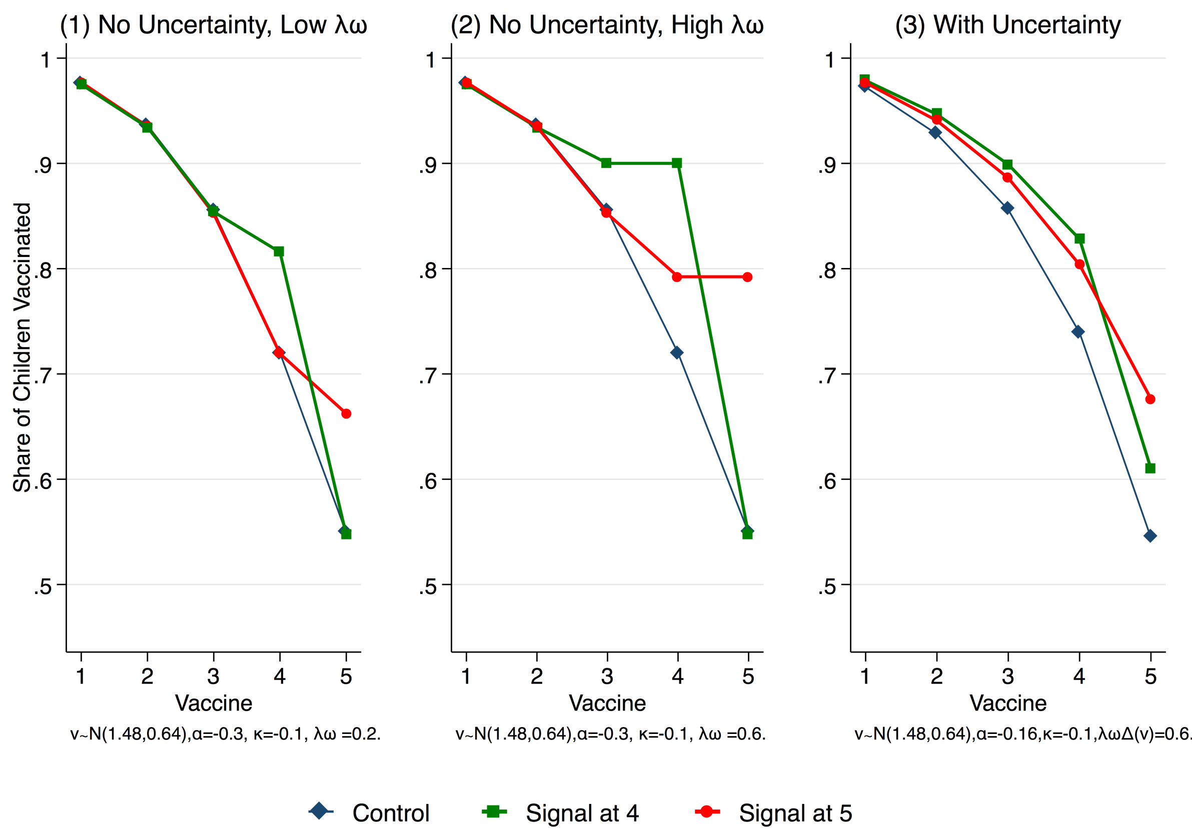
The model further assumes that visibility in actions is only going to affect take-up decisions through social image concerns. However, if individuals are uncertain about the private benefits of deworming or vaccinations, and have incorrect priors over the share of individuals taking up these health behavior, then observing others' actions could lead to updates in beliefs and the usefulness of deworming or vaccinations. The experimental designs laid out in Chapters 2 and 3 aim to separately identify social signaling preferences, by either holding constant potential learning effects or by quantifying them.

Figure 1.1: Simulation of the Effect of Signaling at Vaccine 5 on Cut-off Type and Expectations



This figure shows a simulated type distribution, calibrated based on the observed levels of vaccine take-up in the Control Group. I assume that the type distribution is normal, the marginal cost of vaccination is constant (captured by the parameter κ interacted with D miles walking distance to the clinic) and the marginal benefit is declining (captured by the parameter α), with individual i's utility being given by: $U(a_i; v_i) = (v_i - \kappa D)a_i - \sum_{a=1}^{a=a_i} \alpha a + x\lambda\omega\mathbb{1}(a_i = r)[E(v|a_i \geq r) - E(v|a_i < r)]$ with one signaling threshold $r \in \{5\}$ and $D = 2$ set to the mean walking distance. The calibrated parameters are $\mu_v = 1.48$, $\sigma_v = 0.64$, $\kappa = -0.1$, $\alpha = -0.3$. I assume $\lambda\omega = 0.2$, i.e. the weight assigned to social image is equivalent to 2 miles walking. Control v_5 and v_4 are cut-off types for vaccine 5 and 4, in the absence of signaling (x=0). I solve for v_5^s under signaling (x=1), solving the fixed-point equation 1.5. E and E_s define the expectations formed about types conditional on actions. The cut-off type v_5^s pins down the new equilibrium type expectations $E_s(v|a_i < 5) = E_s(v|v < v_5^s)$ and $E_s(v|a_i = 5) = E_s(v|v = v_5^s)$. $v_4 < v_5^s < v_5$ implies that some individuals who previously chose $a_i^* = 4$ now choose $a_i^{s*} = 5$, while anyone who chose $a_i^* = 3$ will still choose $a_i^{s*} = 3$, given parameters.

Figure 1.2: Simulation of the Effect of Signaling at Vaccine 4 and 5 on the Cumulative Distribution of Vaccinations, With and Without Uncertainty



This figure shows the simulated cumulative distributions of vaccine take-up for the case without signaling (x=0) - calibrated based of the observed levels of vaccine take-up from the Control Group - and with signaling at Vaccine 4 and 5, with and without uncertainty over future cost or preference shocks. Individual i's utility is given by: $U(a_i; v_i) = (v_i - \kappa D)a_i - \sum_{a=1}^{a=a_i} \alpha a + x\lambda\omega\mathbb{1}(a_i = r)[E(v|a_i \geq r) - E(v|a_i < r)]$ with two signaling thresholds $r \in \{4, 5\}$ and $D = 2$ set to the mean walking distance. The parameter values used are indicated under each graph, with $\lambda\omega$ being set to 0.2 in graph (1) and to 0.6 in (2). For the no uncertainty case, displayed in graphs (1) and (2) I solve the fixed-point equation 1.5, to obtain v_4 and v_5 and the corresponding equilibrium type expectations $\triangle(\hat{v}) = E(v|a_i \geq r)$ - $E(v|a_i < r)$. For the case of uncertainty, I assume that signaling utility $\lambda\omega\triangle(\hat{v})$ is the same as under certainty with $\lambda\omega\triangle(\hat{v}) = 0.6$ and simulate vaccine take-up, assuming that shocks ϵ_{it}, which are iid logistically distributed, enter i's utility function.

Chapter 2

Social Signaling and Prosocial Behavior: Experimental Evidence in Community Deworming in Kenya

2.1 Introduction

Externalities play an important role in many health behaviors. Prominent examples are deworming, smoking, open defecation and vaccination. Individuals frequently undervalue the social costs and benefits of their actions and under-invest in public goods like deworming. There is an extensive theoretical literature on the role of social signaling and image concerns (Bernheim 1994; Bénabou and Tirole 2006, 2012). Recent field experiments show that social image concerns affect important behaviors, such as consumption, the decision to vote, and student effort (DellaVigna et al. 2017; Bursztyn et al. 2017; Bursztyn and Jensen 2015). However, empirical evidence from field settings in low-income countries is scarce. Developing countries are a particularly important context, since formal enforcement mechanisms are often missing to produce efficient contributions to public goods and signaling incentives could be an inexpensive substitute.[1] We chose community deworming, as an empirical setting, to manipulate social signaling concerns. Deworming is a public good for which most of its benefits accrue through reduced disease transmission (Miguel and Kremer 2004). Worm infections in adults present a large disease burden as they lead to continuous reinfections among children (Anderson et al. 2013; Anderson, Truscott, and Hollingsworth 2014; Truscott et al. 2014; Njenga et al. 2011). In this paper, we ask two questions: Can social signals increase adults' willingness to take up deworming treatment? To what extent do differences in peer take-up affect the reputational returns of signals?

[1]For example, the United States and Germany have laws that require children to be immunized in order to attend daycare. There is an extensive literature showing the effectiveness of material and financial incentives in encouraging positive health behaviors (Thornton 2008; Banerjee et al. 2010; Sato and Takasaki 2017) However, governments often raise concerns about the scalability and financial sustainability of these incentives.

We answer these questions by implementing a large-scale field experiment that is closely tied to Bénabou and Tirole's theory of social signaling (2006, 2012). In collaboration with the Kenyan Government, we launch a new community deworming program where health volunteers offer free deworming treatment to over 200,000 adults at central locations (instead of through a traditional door-to-door campaign) over a period of 12 days. We inform adults, prior to the program, about the health benefits of deworming with an explicit emphasis on the public good aspect of treatment. Our main experimental manipulations are (i) to increase the observability of deworming decisions and (ii) to vary the distance that adults have to travel to receive treatment. Specifically, we introduce two social signals in the form of a colorful bracelet and ink applied to adults' thumbs, both of which are given to adults upon coming for deworming treatment. As a third treatment, we introduce a private material incentive in the form of a one-page wall calendar that allows us to hold constant the consumption value of the bracelet. We randomize 144 treatment locations and their surrounding communities into the ink, bracelet and calendar treatments or a control group where no incentive is given. The cluster randomization identifies the combined effect of social signaling, salience and social learning effects. To separately identify the extent to which deworming decisions are driven by a desire to signal, we offer free text messages to a small random subset of adults in each of the study communities. The text messages (a) remind individuals of the availability of deworming and (b) provide information about the share of adults that have come for deworming in their community. Lastly, to exogenously vary the cost of deworming, we randomly assign communities to close (less than 1.25 kilometers) or far (between 1.25 and 2.5 kilometers) walking distances to treatment locations. The distance randomization allows us to exogenously shift the level of deworming take-up at community level.

We monitor deworming decision of 38,000 adults at the points of treatment, avoiding to rely on self-reported data on take-up. In addition, we collect detailed survey data on adults' knowledge about deworming, social norm concerns, and first- and second-order beliefs to speak to the mechanisms underlying social signaling. We show that bracelets as signals increased the visibility of deworming decisions, compared to the ink and calendar treatments, and reduced perceived information asymmetries. Adults were significantly more likely to think that others had knowledge about their own deworming decision. The survey data further reveals that adults have a limited understanding of the externalities from deworming, and think of it as "the right thing to do" for a person that looks after their own health. We then build a static and dynamic hierarchical Bayesian model to impute the counterfactuals necessary to estimate the partial effect of social signaling on deworming take-up decisions. Our Bayesian analysis allowed for the flexible construction of statistical models accounting for the complexity of the experiment in terms of the population studied and experimental treatment.[2] This allowed us to manage the large number of treatment comparisons, using

[2]These are,

(i) The population studied: treatment was stratified over counties and clustered over villages, and household sampling was stratified over phone-ownership.
(ii) The experimental treatments: four village level incentivization/signaling treatment arms, two village

regularization, while allowing us to efficiently learn from the data using partial pooling (Imbens and Rubin 2015; Gelman et al. 2013; Carpenter et al. 2017).[3] In the static model, we show that bracelets as signals meaningfully increase individuals' probability to deworm, and that these effects persist when also providing text message reminders and information about others' take-up decisions. Bracelets increase deworming take-up by 8.4 percentage points, a 24 percent increase compared to the control group. We find no detectable effect for the ink incentive, which we attribute to the lower visibility of ink and its strong association with voting in Kenya. Our experiment took place shortly before the presidential election in Kenya, which might have created suspicion among adults. The calendar incentive increased take-up only marginally by 2.7 percentage points. Its effect on take-up is one-third of that of bracelets, despite adults preferring the calendar as consumption item. Building a dynamic model, we explicitly account for potential salience and social learning effects that were introduced as individuals could observe other people's deworming decision. Same as in the static model with the text message treatment, we find no evidence that visibility of actions, through ink or bracelets, affected take-up decisions through reminders or social learning. Lastly, our distance treatment shows that adults are highly sensitive to increases in cost. An increase in the mean walking distance by 1 kilometer leads to a decline in take-up from 44 to 28 percent in the control group. We find that both signals have a larger impact on take-up at far distances, which is consistent with a model of social signaling where returns to signaling increase as signals are more informative about individuals' types.

Our work contributes to the literature of social signaling in four ways. First, we provide direct evidence of the effectiveness of making actions observable through a low-cost signal. Recent empirical studies have highlighted the potential negative effects of visibility like in the case of student effort (Bursztyn and Jensen 2015) and career ambitions (Bursztyn et al. 2017). This study shows how social image concerns can be leveraged to increase public goods. Even in the absence of an understanding of externalities, social signals can increase the direct benefits of deworming - as individuals care to be perceived as doing the "right thing" - and as a result by acting in their own best interest, internalize the interests of others (namely reduce the reinfection risk of children whose health is most affected by worms).

Second, this study makes a contribution by exogenously varying distance and with that deworming take-up at the community level. Such is in most settings impossible and allows us to investigate the relationship between equilibrium take-up levels, the informativeness of signals and subsequent differences in the impact of signals on individuals' decision to deworm. By generating large exogenous differences in take-up levels, we show that signals have much larger effects at far distances and can undo the negative demand response observed in the control group. We observe no such effect for the material incentive. This result has the potential to inform policy decisions about optimal treatment locations: as reputational returns are higher at far compared to close distances, increasing individuals' willingness to

level distance assignment arms, and three individual level SMS treatment arms.

[3]Reduced form OLS regression analysis plots are included in Appendix A for comparison.

walk, optimal treatment locations can be set up further apart and with the same number of locations larger geographic areas can be covered.

Third, we show that the type of signal offered can drastically influence its effectiveness. To our knowledge, this is the first study that simultaneously tests two different signals. Our findings suggest that differences in the perceived salience of signals can translate into large differences in their effectiveness to influence behavior. While ink is a known, well-established signal in Kenya - commonly used for voting - our study highlights that such familiarity does not imply it is transferable as signal to another domain.

Fourth, this paper is one of two papers (in addition to Karing (2018)) that provides the first evidence on social signaling in health, and therefore contributes to a large literature on incentives to increase the use of health services and public goods in low-income settings (Thornton 2008; Banerjee et al. 2010; Ashraf, Bandiera, and Jack 2014; Sato and Takasaki 2017). Complementing Karing's (2018) findings, our paper demonstrates the potential effectiveness of social signals in an environment with low take-up levels and where a new technology is introduced.[4]

Lastly, this is one of few studies that directly compares the effect of a material to a social incentive. In line with Ashraf, Bandiera, and Jack (2014), we show that social signals can outperform a more costly material incentive. Moving beyond existing evidence, our experimental design sheds light on the underlying mechanism and provides convincing evidence for social signaling concerns.

The remainder of this paper is organized as follows. In Section 2.2, we describe the setting of the intervention. In Section 2.3, we describe the experimental design to identify social signaling concerns. Section 2.4, provides descriptive statistics from the experiment. In Section 2.5, we present the empirical Bayesian model. In Section 2.6 we discuss the results and section 2.7 concludes.

2.2 Empirical Setting

Intestinal worms are a development burden to children and adults in many developing countries. According to the World Health Organization approximately 1.5 billion people are infected with soil-transmitted helminths worldwide.[5] While mild infections are asymptomatic, more severe infections lead to abdominal pain, iron-deficiency, anemia, malnutrition, and stunting. Epidemiologists postulate that it might be feasible to eliminate worms using mass drug administration covering the entire population, including children and adults. While significant progress has been made in deworming children through school-based deworming programs, the remaining infectious reservoir among adult populations fosters reinfection. An

[4] In our study, take-up of deworming treatment is 36 percent in the control group, compared to 73 percent take-up of vaccine four in the control groups of Karing's study.

[5] World Health Organization, Fact Sheet Soil-transmitted helminth infections, September 2017 http://www.who.int/mediacentre/factsheets/fs366/en/.

important empirical question is therefore, how high take-up of deworming treatment among adults can be achieved cost-effectively and within a short time.

Community deworming and the context of Western Kenya provide an empirically relevant and suitable setting to study prosocial behavior and the potential of social signaling. Firstly, deworming is a public good. Most of its benefits come through reduced disease transmission to others, while private health benefits are low for many individuals. Secondly, deworming is an established health technology in Kenya. In 2009 the Government of Kenya launched a National School-Based Deworming Programme (NSBDP) through which between 2012 and 2017 over 5 million children got dewormed. Trained teachers administered deworming tablets to all enrolled and non-enrolled children aged 2-14 years in all primary schools in areas endemic to parasitic worms, including our study area. Most likely as a results of that, 78 percent of adults in our baseline survey sample know about deworming treatment and 61 percent are aware that treatment should be taken regularly, every three to twelve months. When asked who is at risk of worm infections, 94 percent of adults answer children and 67 percent answer that adults are at risk too. Only 4 percent say that deworming treatment is for sick people only. Third, there is a strong prescriptive norm around deworming. 95 percent of adults at baseline say they would praise someone who would come for free deworming treatment, while 69 percent said they would look down on a person who did not come. Figure 2.1 shows that image concerns for deworming are comparable to those for open defecation and child immunization. Individuals consider deworming as the “right thing to do” to protect one’s health and not spread worms, while those who do not deworm are considered as careless and ignorant. While there could be concerns about adults’ interpreting others’ decision to deworm as a sign of them having worms or “being dirty” (i.e. revealing negative health characteristic) the baseline data suggests that this is not the case. Instead, deworming is seen as a preventative health behavior for everyone to take, regardless of whether someone believes to have worms or is feeling healthy. However, adults have a limited understanding of externalities. Less than half (41 percent) of adults know that worms can spread between people.[6] Lastly, adults under-invest in deworming despite treatment being readily available at a low cost.[7] While 68 percent of adults at baseline report to have taken treatment before, only 38 percent say they dewormed in the past 12 month. Adults in endemic areas are advised to deworm every 6 to 12 months but there is currently no formal program that provides free treatment to adults. In collaboration with the Kenyan Government, we implemented a new community deworming program that offered free deworming treatment to over 200,000 adults in Western Kenya. The program was implemented across three counties, Busia, Siaya and

[6] When asked if a person sick with worms can spread works to others, only 31 percent answered yes, 56 percent said know and 13 percent were uncertain. When asked if “If you have worms, does that affect your neighbors? or relatives? health?” 27 percent and “If your neighbors or relatives have worms, does that affect your health?” 25 percent answered yes. Only 18 percent answered yes to all three questions, had full externality knowledge. 41 percent answered yes to one of the three questions, had partial understanding of externalities.

[7] Adults can purchase deworming treatment at pharmacies and clinics for a price of about 50-200 Kenyan shillings ($0.50-2)

Kakamega, where soil-transmitted helminths are endemic. We implemented the program and experiment in two waves: wave one of deworming was implemented from early to mid-October in Busia and Siaya County, and wave two was implemented from late October until early November in Kakamega County. In both waves, deworming started on a Monday and was offered for twelve consecutive days, each day from 8am until 5pm.

2.3 Experimental Design

The first part of this section introduces the different experimental treatments and discusses the identification of signaling preferences. Next, we describe the selection and randomization of treatment points and communities. We then provide an overview of the different data collected and the relevant outcomes.

Treatments

To create visibility in actions, we experimentally introduce two signals - in the form of a bracelet and ink applied to adults' thumbs. The bracelet and ink create an opportunity for adults to publicly signal that they took deworming treatment. Figure 2.2 displays the experimental design which we discuss in the following.

Social Signals: Ink and Bracelet

We introduce two different types of signals to increase the visibility of deworming decisions x: a green silicone bracelet (Figure 2.3) and green ink that is applied to a person's thumb. Bracelets and ink were randomized at the cluster level: at 39 treatment locations individuals received a bracelet when coming for deworming and at 36 locations they received ink. The color green was chosen as it is not associated with any political parties and was liked by most individuals during piloting. We test two different signals since it was unclear upfront which one could be more effective.[8]

[8]Ink and bracelets vary as signals across important dimensions:

- Ink is known for its use during elections. Individuals get their thumb inked after they cast their vote to avoid double voting. Bracelets are not commonly worn among adults in Kenya.
- Ink has zero or negative consumption utility if individuals perceive it as messy or distrust it due to its link to voting. Bracelets could provide positive consumption value but cannot cause disutility since it is a voluntary signal.
- Bracelets have a high visibility as they are worn around the wrist. Ink's visibility is lower as it is applied to the thumb and only lasts for about 3 days to 2 weeks (on the skin/on the nail).
- The cost of ink is close to zero while a bracelets cost $0.20. Our research partner, a non-profit, had a strong interest in testing ink.

Material Incentive: Calendar

To control for the consumption value z of the bracelet, we introduced a material incentive in the form of a simple one-page wall calendar (Figure 2.4). At 35 treatment locations individuals received the calendar when coming for deworming. The cost of the calendar is 50 Kenyan Shillings (50 Cents). Wall calendars are popular in Kenya as people use them to decorate the walls of their homes and often have many calendars for the same year put up. Due to its durability and visibility inside the home, the calendar would also act as a self-signal to individuals, reminding them of their participation in deworming. 34 treatment locations were randomized into a control arm where no incentives were provided. Signal/incentives were randomized at the cluster level for them to be informative about actions as opposed to adults' preferences for different incentives.

Cost of Deworming: Close and Far

We vary the cost of deworming and with that the marginal person by varying the distance that individuals have to walk to treatment locations. We randomly assigned communities to either a "close" (0-1.25 kilometers) or "far" (1.25-2. kilometers) deworming location. Due to small changes in the actual location of treatment and the dispersion of households within targeted areas, actual distances to points of treatments were distributed as shown in Figure 2.5. While there does appear to be some slight overlap between close and far clusters (i.e. non-compliance with assigned treatment), this does not affect the intention-to-treat analysis. Table 1 shows that individuals from far communities had to travel more than twice the distance to treatment locations compared to individuals from close communities: the randomization shifted the mean distance from 0.84 kilometers to 1.86 kilometers. Figure 2.6 shows the distribution of targeted communities' distances (in meters) to their own treatment locations and to the closest other treatment locations. The distance to the assigned treatment location was for all clusters, except for two, shorter than the distance to the closest other treatment location.

Reminder and Social Info Text Messages

We introduced a separate treatment at the individual level to hold constant salience and learning effects, and isolate the effect of social signaling. We recruited a random sample of adults in each signal/incentive treatment and the control group to receive text messages with reminders and information about aggregate take-up in their community. The text messages included two statements: "Free deworming now at [Central Location]." (=Reminder) "[No/few/almost half/half/more than half/almost all/all] of your village came, that is X in 10 adults." (=Social Info). We recruited 30 individuals per cluster in the control arm and 25 individuals per cluster in the signal/incentive treatments. In the control arm, we divided the sample into two groups of 15: one group received reminder messages only, and a second group received reminders and social information. In the signal/incentive treatments, individuals always received both. The text message sample was drawn from the population of phone owners, from which we also drew a comparison sample, that did not receive any text messages and also no recruitment visit. The majority of adults (65 percent to 80 percent) in our study sample own a phone. Text messages were sent the day before the deworming program started and after that every other day (on the second, fourth, sixth, eight and tenth deworming day). The first text message, on the day before treatment started, only included the Reminder message in the Social Info treatment since deworming had not yet started. The recruitment for treatment took place one to three weeks before the start of the deworming program. 387 adults were recruited for the reminder treatment in the control group and 2,637 adults across all signal/incentive and control groups for the Social Info treatment.

SMS Airtime Reward

To verify that adults were reading the text messages, we offered an airtime reward of 50 Kenyan Shillings (50 US Cents) to a random subset of adults conditional on texting back and confirming the receipt of the text message.[9] In each control group cluster four adults were randomly selected (two from Reminder and two from Social Info treatment) and in each signal/incentive treatment cluster two adults got selected. The following reward text message was sent to adults on day 2 and 6 of deworming treatment: "Thank you for signing up for this text message from Evidence Action. To receive your 50Ksh airtime reward, message [1234] to [XXX]. The text is free." We sent a reminder text message to adults that had not taken up the reward two days after the original reward text was sent[10]. The reminder messages were sent on day 4 (for reward messages sent on day 2) and on day 8 (for reward messages sent on day 6). The SMS airtime reward treatment gives us a lower bound for information take-up for our text message treatment.

Information Campaign Prior to Deworming

One week before the launch of the community deworming program Community Health Volunteers (CHVs) together with field researchers visited *each* of the selected 144 communities to inform adults about the upcoming program.[11] CHVs are trusted community members who are known for their involvement in health campaigns and are part of the school-based deworming program in Kenya. Figure 2.7 shows the information script that CHVs used. The objective was to send a strong message (i) that regular deworming, even in the absence of symptoms, is not only important for children but also for adults and (ii) that deworming is a public good. CHVs further informed community members that ink, calendars and bracelets would be given when coming for deworming, and distributed flyers (see Appendix, Figure

[9] We only implemented the reward scheme during the second wave of deworming treatment. The reason being, we only decided to add the treatment shortly before we started deworming in wave one communities and had to wait for ethical approval for this added component before we could implement it.

[10] "Thank you for signing up for text messages from Evidence Action. Don't forget to reclaim your 50Ksh airtime. Message [1234] to [XXX] to receive the reward. The text is free."

[11] Deworming treatment started on a Monday. The information campaign ended on the Friday before the program started to not avoid "nudging" adults too close to treatment.

24-26) that displayed the incentives. The objective was to create common knowledge about the meaning of the signals/incentives among community members before the start of the program.

Site Selection and Randomization

We randomly selected 158 clusters in the three study counties, of which 144 were used in the study.[12] Each cluster was defined as a treatment location and targeted community pair. We used the location of primary schools as proxies to (i) identify acceptable locations to set up our treatment locations and (ii) to find villages to target with our informational campaign and data collection. We relied on the high geographic density of primary schools in the study counties to select both treatment locations and targeted communities.[13] To select our clusters from the pool of a total of 1,451 primary schools in our study area, we used an acceptance-rejection method whereby we randomly picked schools, checked their acceptability based on their overlap with already selected clusters, and if accepted added them to our selected sample. This process was repeated until we had selected the requisite number of clusters. If no acceptable schools remained before completion, the whole process was restarted. Each cluster, centered on its treatment location, had a 2.5 kilometer radius catchment circle and 3-4 kilometer radius buffer circle. A cluster was considered acceptable if its buffer circle did not leave any of the already selected clusters' non-overlapping catchment circles smaller than an a pre-specified size. Figure 2.8 shows the final cluster selection. After all clusters were selected, we randomly assigned[14] each cluster to be either a "close" or "far" cluster. We then selected for each cluster, from its non-overlapping catchment circle and according to its assigned distance treatment, a primary school as an anchor for us to locate its targeted community.[15] Clusters were then randomly assigned, stratified over counties and distance treatment, to the different signal/incentive treatments: control, ink, calendar and bracelet. To finalize the cluster selection process, we surveyed the treatment location and target community anchor schools. For the treatment locations we confirmed that treatment would be feasible there and identified alternative treatment locations, close to the selected schools, as potential backups. For the anchor schools, we identified all the communities near them and randomly selected one community to target.[16]

[12] We only intended to use 150 clusters, and only included eight extra clusters as fallback clusters. For various practical reasons, implementation was only possible in 144 clusters.

[13] Geographic coordinates for primary schools were retrieved from the Kenya Open Data Portal (`http://www.opendata.go.ke/`).

[14] Randomization was stratified within counties to increase statistical power.

[15] For further details on the cluster selection algorithm refer to the study's pre-analysis plan.

[16] In some cases because the initial village was too small, we added a second village.

Data and Outcomes

Our analysis uses several data sources, including administrative data on deworming take-up and survey data that was collected before and after the intervention.

(1) *Census data:* We conducted a census of all adults (18 years age or older) residing in the 144 selected communities: surveyors visited each household, captured their geographic coordinates, and collected basic information of each household member that would allow us to follow-up with individuals and to stratify over relevant characteristics (e.g., phone ownership). In total we listed 38,019 adults. Using the census lists we randomly sampled individuals to be surveyed at base- and/or endline and to be part of the text messaging intervention. The sampling of individuals who did not receive any text messages was stratified over phone ownership.

(2) *Baseline survey data:* From each of the 144 communities we randomly sampled 15 households and from each household one adult was randomly picked to respond to the baseline survey. We surveyed 4,823 adults about their knowledge about private and social benefits, prior experience and beliefs about deworming take-up and social norms. We reported outcomes under in Section 2. The baseline survey and census were implemented in August and September 2016, eight to five weeks before the start of the deworming program.

(3) *Point of treatment administrative data:* For all individuals listed in the census, we monitored the decision to take-up deworming treatment directly at the point of treatment. This allowed us to avoid using self-reported data and work with a large sample of 38,019 adults. While CHVs distributed the deworming drugs and incentives, field researchers recorded personal information on electronic devices.

(4) *Endline survey data:* We surveyed 5,664 adults to verify the correct implementation of all treatments (i.e. information visits by CHVs, receipt and understanding of signals/incentives and text messages), the visibility of signals/incentives, first and second order beliefs and to conduct a separate choice experiment to elicit preferences for calendar and bracelets. The survey was conducted three days to two weeks after the end of the deworming program and the sample included 4,436 respondents that were not part of the text message intervention, and 223 and 1,005 adults who had received the Reminder and Social Info treatment respectively.

Our main outcomes are deworming take-up and beliefs about individual and aggregate level take-up. In our analysis we work with two different samples to estimate the effect of treatments on deworming take-up: i) individuals whose deworming take-up was directly monitored at the point of treatment (N = 12,827) and ii) individuals who were not monitored (N = 25,192). For individuals whose deworming take-up was monitored at the point of treatment personal information was uploaded on tablets so that surveyors could directly verify their attendance. For the larger non-monitored sample of individuals, surveyors manually recorded identifying information at the point of treatment which we matched (through an algorithm) with the census data. We were conservative in the acceptance of names matches, such that deworming levels for the non-monitored sample are significantly lower. When reporting deworming take-up levels we therefore only use data from the smaller, randomly drawn monitored sample. When estimating treatment effects we use the full sample of monitored and non-monitored individuals (N=38,017). In the analysis of beliefs we work with the (base- and) endline survey data.

Compliance with Implementation Protocol

Figure 2.9 shows how well adults were informed about the deworming program. At endline (2-14 days after the end of treatment), 89 percent of individuals knew that deworming treatment was offered to adults in their community. 74 percent reported to have received an information visit from a CHV before the start of the program. 69 percent of adults were able to recall the number of days that treatment was offered. Almost all individuals that came for deworming treatment, reported that they had received the assigned signal/incentive at the point of treatment (95 percent for ink, 97 percent for bracelet and 95 percent for calendar). All adults who were recruited for the text message treatment, consented to receiving the messages. 82 percent of endline respondents that had been assigned to the text message treatment reported to have received messages. When asked about the content of the messages, 98 percent said that the message was a reminder for deworming and 47 percent said that the messages told them about how many people had dewormed. The average number of reported messages received was 4, with 75 percent of people reporting to have received between 3 and 6 text messages. It is possible that individuals do not precisely recall the number of messages received, given that the endline survey took place 2-14 days after deworming treatment had ended.

2.4 Descriptive Statistics

In this section we will present descriptive statistics pertinent to implementation of the community deworming program and its outcomes.

Study sample

Table 2.1 shows the total number of clusters by treatment arm and county as well as the number of adults that were surveyed during the census. Across 144 cluster, we surveyed 39,301 individuals who formed the sampling frame for different surveys and text messaging intervention.

Figure 2.10 shows socio-economic characteristics of individuals in the census, base- and endline survey. The mean age is 35 and 40 years respectively. Over half of respondents have not completed primary education and a large share (80 percent) live in houses with floors made of earth. Both is indicative of low-income status, yet 65 percent to 80 percent of respondents report to have a mobile phone across the different surveys.

Knowledge, beliefs and perceptions of deworming

Figure 2.11 presents individuals' responses to base- and endline questions about worms and deworming treatment. The majority of respondents knew about deworming treatment (78 percent). However, less than one third of adults had knowledge of the negative externalities of worm infection. Comparing reported knowledge and beliefs of baseline and endline surveyed individuals, the most significant change is in people's understanding of who is at risk of worm infections. More people are reporting after the intervention the belief that everyone is at risk of worm infection.

Since our study's main research questions focus on social influence in deworming decisions, we tried to elicit individuals' perceptions and beliefs of others' deworming choices. Figure 2.12 shows individuals reporting that their own deworming decisions would not be influenced by others' decisions, as well as having a strong *intention* to getting dewormed.

In addition, as shown in Figure 2.13, people predicted similar deworming among peers. As we will see below, most people overestimated their own and others' deworming take-up. Finally, when individuals were asked about how others would respond to the use of indelible ink as a signal of deworming treatment, they predicted a positive effect, contrary to what we actually observed.

Prior experience with deworming

Figure 2.14 presents individuals' reported experience with deworming, prior to the intervention. The majority of respondents said that children in their families had been previously dewormed, and the most common location for treatment for family members had been during the annual school-based deworming program. We also see that almost two third of adults had been previously dewormed. However, more than half of those reporting prior treatment, were treated over two years ago. Furthermore, most adults reported getting dewormed at a hospital/clinic or to have purchased medication.

Preferences for incentives

In order to distinguish between the private and social motivation of incentives, we asked endline respondents for their preferences for the calendar and bracelet incentives, and separately conducted a willingness-to-pay survey in the control arm. The identification challenge for us is to separate

a) *private valuation*, individuals like the bracelets for their consumption value, and

b) *social valuation*, individuals are influenced by bracelets in their deworming decision because they want to broadcast that their deworming or because they are reminded or learn about deworming by observing others' decisions.

Figure 2.15 reports which incentive (bracelet or calendar) individuals preferred as a gift during the endline survey. Calendars are typically preferred by all except for those already dewormed. Similarly, in the willingness-to-pay survey, we find that the majority (72 percent) of individuals preferred calendars to bracelets. This provides evidence that individuals assign a higher private value to calendars and that by comparing deworming treatment take-up across calendar and bracelet arms we are able to isolate the social component.

Distance to treatment location

As described in the Experiment Design section, communities were randomly assigned to be either located *close* or *far* from the treatment locations. The distance assignment is key for us to estimate individuals' response to changes in treatment (travel) cost and quantify the utility from social signaling. Due to small changes in the actual location of treatment and the dispersion of households within targeted areas, actual distances to points of treatments were distributed as shown in Figure 2.5. While there does appear to be some slight overlap between *close* and *far* clusters (i.e. non-compliance with assigned treatment), this does not affect the intention-to-treat analysis carried out in this report.

We can see in Table 2.2 that individuals from *far* communities had to travel more than twice the distance to treatment locations compared to individuals from *close* communities.

Absolute deworming treatment take-up

By the end of the intervention, we had recorded the deworming treatment of approximately 97,000 individuals. Figure 2.16 shows how much absolute take-up there was, split by assigned incentive treatment, assigned distance to treatment location, and county. While the pattern of take-up is similar to what we will find in our formal analysis, we are not making any causal interpretations based on these findings. The aim of this study is to learn about impact on the *proportion* of take-up in targeted communities, and therefore an analysis based on absolute figures would be misleading as we do not have information of what communities all treated individuals are from.

2.5 Empirical Models

Model subscripts

- i: individuals
- j: clusters (villages)
- k: strata (counties)

- $m \in \{1, \dots, 12\}$, deworming day index.

Variables

- $Y_i \in \{0, 1\}$, is observed take-up over the whole intervention duration. $Y_{im} \in \{0, 1\}$, is observed take-up on day m.
- $\mathbf{Z}_i$, assigned treatment: incentive treatments, SMS treatment, distance treatment
- $\mathbf{X}_i$, covariates: phone-ownership, and name-matching indicators.

Static Model

Likelihood Model

This is the model's likelihood, the data generating process for observed outcomes:

$$\begin{aligned} \Pr[Y_i = 1 | \mathbf{Z}_i, \mathbf{X}_i, \theta_{j[i]}] &= g^{-1}(\eta_i | \theta_{j[i]}) \\ \eta_i &= f(\mathbf{Z}_i, \mathbf{X}_i | \theta_{j[i]}) \end{aligned}$$

where

- g is a binary link function (e.g., logit or probit link functions).
- f is a linear function of $\mathbf{Z}$ and $\mathbf{X}$.[17]
- $\theta_j = (\alpha_j, \beta_j)'$, where α_j and β_j are the model parameters for the village-level intercept and the village-level vector of slope coefficients, respectively.

Parameters Model

This section describe the hierarchical model for parameters. Distribution that have `Multi` prefix are multivariate distributions and distributions with `Half` prefix are restricted to have non-negative values.

The vector of village-level parameters has the following multivariate distribution:

$$\forall j : \theta_j \sim \texttt{MultiStudentT}(\nu^{\text{village}}, \theta_{k[j]}, \Sigma^{\text{village}}_{k[j]})$$

while the county-level parameters have the following distribution

$$\forall k : \theta_k \sim \texttt{MultiStudentT}(\nu^{\text{county}}, \theta^o, \Sigma^{\text{county}})$$

where $\theta^o = (\alpha^o, \beta^o)$ are the hyperparameters of the linear model:

$$\begin{aligned} \alpha^o &\sim \texttt{Normal}(0, 5) \\ \beta^o &\sim \texttt{MultiNormal}(0, \text{diag}(1)) \end{aligned}$$

[17] I'm writing it this way so I don't have to write the whole linear model with treatment indicators, covariates and, their interactions. I can specify it separately, it would just distract from the overall model here.

(diag(x) is a matrix with the diagonal vector x.)

The covariance matrices for θ_j and θ_k are defined as

$$
\begin{aligned}
\forall k : \Sigma_k^{\text{village}} &= \text{diag}(\sigma^{\text{village}})\mathbf{\Omega}_k^{\text{village}}\text{diag}(\sigma^{\text{village}}) \\
\Sigma^{\text{county}} &= \text{diag}(\sigma^{\text{county}})\mathbf{\Omega}^{\text{county}}\text{diag}(\sigma^{\text{county}})
\end{aligned}
$$

where $\mathbf{\Omega}_k^{\text{village}}$ and $\mathbf{\Omega}^{\text{county}}$ are correlation matrices with their own prior distributions.

Other hyper parameters have the following prior distributions

$$
\begin{aligned}
\nu^{\text{village}} &\sim \Gamma(2, 0.1) \\
\nu^{\text{county}} &\sim \Gamma(2, 0.1) \\
\sigma^{\text{village}} &\sim \texttt{HalfMultiNormal}(0, \text{diag}(1)) \\
\sigma^{\text{county}} &\sim \texttt{HalfMultiNormal}(0, \text{diag}(1))
\end{aligned}
$$

Posterior Distribution of Model Parameters

Let θ be a vector of all linear model parameters at all levels and ϕ is a vector of all hyper-parameters, and $\mathbf{Y}$ is a matrix of all observed outcomes. To conduct our inference we need to calculate the distribution

$$
p(\theta, \phi | \mathbf{Y}^{\text{observed}}, \mathbf{Z}^{\text{observed}}, \mathbf{X}) = p(\mathbf{Y}^{\text{observed}} | \mathbf{Z}^{\text{observed}}, \mathbf{X}, \theta, \phi) p(\theta|\phi) p(\phi)
$$

Inference

After calculating the posterior distribution of the model's parameters we want to be able to calculate, for any two treatments z and z', the distribution of the average treatment effect

$$
\frac{1}{N} \Sigma_i Y_i(z) - Y_i(z')
$$

Since we only observe at most one outcome of $Y_i(\cdot)$, we need to quantify the distribution of *missing* counterfactuals:

$$
p(\mathbf{Y}^{\text{missing}} | \mathbf{Y}^{\text{observed}}, \mathbf{Z}^{\text{observed}}, \mathbf{Z}^{\text{missing}}, \mathbf{X}) =
$$

$$
\int p(\mathbf{Y}^{\text{missing}} | \mathbf{Z}^{\text{missing}}, \mathbf{X}, \theta, \phi) \cdot p(\theta, \phi | \mathbf{Y}^{\text{observed}}, \mathbf{Z}^{\text{observed}}, \mathbf{X}) \, d\theta d\phi
$$

Dynamic Model

The text message treatment may only imperfectly capture the potential salience and information effects of the bracelet and ink signals. Individuals might become inattentive to text messages over time, due to the repetitiveness of the reminder messages, or not pay sufficient attention to information about peer take-up as it is communicated in an abstract way (e.g. "few" versus "half" of people dewormed). The possible difference in salience and information effects of text messages, and bracelets or ink is a valid concern. Individuals might become inattentive to text messages as the reminder content is the same each time, and information about peers' take-up might appear abstract (e.g., "few" versus "half" of people dewormed). Signals on the other hand, allow adults to observe on a daily basis who dewormed, providing information about the take-up decisions of specific friends, neighbors or family members. Further, despite adults' low private valuation of bracelets, bracelets could gain popularity and a fad could emerge as more adults start wearing the item. To separately identify these dynamic effects, we estimate a piecewise-constant proportional hazard model (Wooldridge 2010) that uses the full information about changes in take-up levels across the twelve days of deworming. We model the probability to deworm, conditional on not having already done so, extending the static model to twelve deworming days $m \in \{1, ..., 12\}$.[18]

Likelihood Model

The dynamic model is extends the static model to the 12 deworming days.

$$\begin{aligned}\Pr[Y_{im} = 1 | Y_{i,m-1} = 0, \mathbf{Z}_i, \mathbf{X}_i, \theta_{j[i]}] &= g^{-1}(\eta_{im} | \theta_{j[i]}) \\ \eta_{im} &= \log(\lambda_{k[i],m}) + f(\mathbf{Z}_i, \mathbf{X}_i | \theta^{\text{static}}_{j[i]}) + f_m(\mathbf{Z}_i, \mathbf{X}_i | \theta^{\text{dynamic}}_{k[i]})\end{aligned}$$

$$\begin{aligned}\Pr[Y_{im} = 1 | Y_{i,m-1} = 1, \mathbf{Z}_i, \mathbf{X}_i, \theta_{j[i]}] &= 1 \\ \Pr[Y_{im} = 0 | Y_{i,m-1} = 1, \mathbf{Z}_i, \mathbf{X}_i, \theta_{j[i]}] &= 0\end{aligned}$$

The link function g here is a complementary log-log.[19] λ_{km} the is the piecewise-constant proportional hazard for deworming day m, that is, the the probability of deworming on day m conditional on not having dewormed on day $m-1$. The term captures individuals' preferences for deworming on particular days, e.g., given individuals' different work arrangements some might prefer to come for deworming on weekends than weekdays. The linear function f is the same as in the static model. Indicators for treatments (ink, calendar, bracelet; far distance; reminder and social info) enter linearly and are interacted. Dynamic treatment effect are captured by the f_m quadratic function, where m is interacted with treatment indicators.

Willingness-to-Pay Model

Let

- V_i^c and V_i^b be an individual's monetary valuation of calendars and bracelets, respectively.

[18] The estimation is restricted to the sample of name-matched individuals.

[19] Logit and probit work just as well.

- $G_i \in \{0, 1\}$ indicates initial gift choice of calendar (vs bracelet). Also, let $\widetilde{G}_i \equiv 2 \cdot G_i - 1 \in \{-1, 1\}$.
- $M_i \in \{0, 10, 20, \dots, 100\}$ be the randomly assigned offer (in Ksh) to switch gifts choice (calendar instead of bracelet and v.v.)
- $W_i(m) \in \{0, 1\}$ indicates accepting m Ksh to switch gift choice.

$$
\begin{aligned}
V_i \equiv V_i^c - V_i^b &\sim \texttt{StudentT}(\nu_v, \mu_{k[i]}, \sigma) \\
\mu_k &\sim \texttt{StudentT}(\nu_\mu, \mu, \tau_\mu) \\
\mu &\sim \texttt{StudentT}(\nu_\mu, 0, \tau_\mu) \\
\sigma &\sim \texttt{HalfStudentT}(\nu_\sigma, 0, \tau_\sigma)
\end{aligned}
$$

with fixed hyper parameters: $\nu_v, \nu_\mu, \nu_\sigma, \tau_\mu, \tau_\sigma$.
We estimate these model parameters using the likelihood probability:

$$
\mathcal{L}_i = \left\{\widetilde{G}_i \cdot [F_V(\widetilde{G}_i \cdot M_i | \mu, \mu_{k[i]}, \sigma) - F_V(0 | \mu, \mu_{k[i]}, \sigma)]\right\}^{W_i} \cdot \left\{F_V(\widetilde{G}_i \cdot M_i | \mu, \mu_{k[i]}, \sigma)\right\}^{(1-W_i)}
$$

Beliefs and Knowledge Model

Model subscripts

- i: individuals
- j: clusters (villages)

Variables

- $Y_i^{\text{rec}} \in \{0, 1, \dots, 10\}$, the number of of individuals recognized (from a list of 10 randomly selected community members).
- $Y_i^{\text{2ord}} \in \{0, 1, \dots, Y_i^{\text{rec}}\}(z)$, second-order beliefs about the Y_i^{rec} recognized community members. $\overline{Y}^{\text{2ord,fp}}(z)$ is the estimated proportion of X_i^{degree}.
- $Y_i^{\text{1ord}} \in \{0, 1, \dots, Y_i^{\text{rec}}\}$, first-order beliefs about the Y_i^{rec} recognized community members. $\overline{Y}^{\text{1ord,fp}}(z)$ is the estimated proportion of X_i^{degree}.
- $N_j \in \mathcal{Z}^+$, the population size of village j (observed from the census).
- $X_i^{\text{degree}} \in \{Y_i^{\text{rec}}, \dots, N_{j[i]}\}$, the estimated number of community members i recognizes.
- $\mathbf{Z}_i$, assigned treatment: incentive treatments, SMS treatment, distance treatment

Likelihood Model

Social connectness or recognition The first stage is to impute each respondents' recognition degree by estimating a parametric binomial model. g is a link function (e.g. logit).

One thing doesn't makes sense here: δ_j^{rec} is the coefficient for a village level independent variable, N_j. It seems like δ_j^{rec} should just be a village level random effect or make it a county level parameter.

$$\begin{aligned}
Y_i^{\text{rec}} &\sim \texttt{Binomial}(10, p_i^{\text{rec}}) \\
p_i^{\text{rec}} &= g^{-1}(\alpha_i^{\text{rec}} + \delta_{j[i]}^{\text{rec}} \cdot N_{j[i]}) \\
X_i^{\text{degree}} &= Y_i^{\text{rec}} + p_i^{\text{rec}} \cdot (N_{j[i]} - 10)
\end{aligned}$$

X_i^{degree} is imputed by adding the known Y_i^{rec} with the estimated number of community members (and not asked about in the survey) recognized.

Second-order beliefs Same as above but with treatment assignment potentially having a causal effect. Same comment as above about parameter levels.

$$\begin{aligned}
Y_i^{\text{2ord}}(\mathbf{Z}_i) &\sim \texttt{Binomial}(Y_i^{\text{rec}}, p_i^{\text{2ord}}(\mathbf{Z}_i)) \\
p_i^{\text{2ord}}(\mathbf{z}) &= g^{-1}(\alpha_i^{\text{2ord}} + \mathbf{z} \cdot \beta_i^{\text{2ord}} + \delta_i^{\text{2ord}} \cdot N_{j[i]}) \\
\overline{Y}^{\text{2ord,fp}}(\mathbf{Z}_i) &= \frac{Y_i^{\text{2ord}} + p_i^{\text{2ord}}(\mathbf{Z}_i) \cdot p_i^{\text{rec}} \cdot (N_{j[i]} - 10)}{X_i^{\text{degree}}}
\end{aligned}$$

The number of community members i has second-order beliefs[20] is the sum of the observed sampled Y_i^{2ord} and the estimated beliefs about the remaining population not asked about, $p_i^{\text{rec}} \cdot (N_{j[i]} - 10)$.

For unobserved (missing) treatment $\mathbf{z}^{\text{missing}}$ we impute $\overline{Y}^{\text{2ord,fp}}(\mathbf{z}^{\text{missing}})$ as

$$\overline{Y}^{\text{2ord,fp}}(\mathbf{z}^{\text{missing}}) = \frac{p_i^{\text{2ord}}(\mathbf{z}^{\text{missing}}) \cdot p_i^{\text{rec}} \cdot N_{j[i]}}{X_i^{\text{degree}}}$$

First-order beliefs The first-order beliefs model is the same as the second-order model.

2.6 Results

Assumptions and Mechanisms

In our experiment, we assume that the bracelets and ink as signals increase the visibility of actions and lead adults to have more accurate beliefs about others' deworming decisions. At endline, we collected detailed data on the visibility of signals and adults' belief to verify these. We first provide descriptive data on visibility. Second we estimate the impact of signals on individuals' beliefs.

[20]In the current model $Y_i^{\text{2ord}} = 1 \iff i$ reports yes/no.

Visibility of Signals

Each respondent was asked if they had seen people with a bracelet, or ink on their finger, or a calendar.[21] 66 and 73 percent of respondents in the ink and calendar treatments respectively reported to have seen someone with the signal/incentive. For bracelets, the visibility was significantly higher with 95 percent of respondents reporting to have seen someone with a bracelet. Bracelets were also more durable as signals, increasing the length of time during which deworming decisions could be observed. At endline, 46 percent of dewormed adults in the bracelet treatment were still wearing the bracelet and 90 percent still had the bracelet.[22] Only 14 percent of dewormed adults in ink treatment still had ink on their finger. 95 percent of dewormed respondents in the calendar treatment, still had the calendar in their possession. Taken together, this shows that bracelets were significantly more visible than the ink and a more durable signal. The social visibility of ink was no different than that of a private incentive. Given the high retention and private visibility of calendars, the calendar incentive also provides an adequate control for any potential self-signaling value of bracelets.

Observability of Actions

To measure (perceived) information asymmetries in deworming decisions, we asked each endline respondent about a random subsample of ten other adults in their community. The objective was to learn about the information that adults have about other adults within their (potential) reference group (instead of only about close contacts e.g. household members, family). Conditional on recognizing the person's name, we asked i) "Do you think this person came for deworming?", ii) "Do you think this person thinks you came for deworming, or do you think they think you did not come for deworming?", iii) "Why do you think they would think that?" and iv) "What is your relationship to the person?". Question i) measures first-order beliefs, capturing the information that respondents have about others' deworming status, and ii) measures second-order beliefs, capturing the information that respondents believe that others have about their deworming status. Both questions could be answered with Yes, No or Don't know.

Figure 2.17 shows that the majority of recognized adults are neighbors (49 percent) and extended family (29 percent). Figures 2.18 and 2.19 show that adults generally (believe to) have more information about the deworming status of household members and extended family, than of neighbors or community members.

Figure 2.20 shows the proportion of recognized peers that respondents believe have knowledge of their own deworming status. In the control *and* calendar group, adults believe that

[21] Respondents that said they were not aware that deworming was offered to their community, i.e. answered "No" to the question "Did you know that deworming was offered to adults in your community?" were not asked about the incentives. Respondents who knew about the signals/incentives were then asked "Have you seen people wearing these bracelets?" "Have you seen this mark of ink people's fingers?" "Have you seen this calendar?". By "people", respondents were asked to think of people outside their household.

[22] Surveyors from their own observation verified the retention of signals/incentives, answering the questions: "Is the person still wearing the bracelet?" "Is the ink still visible?" "Do you still have the calendar?"

66 percent of peers have information about their deworming status, illustrating i) that there is significant scope for reducing information asymmetries and ii) that a material incentive, in spite of its perceived visibility, does not necessarily increase the (perceived) visibility of actions. Bracelets significantly shifted beliefs: adults reported that 74 percent (a 7.9 percentage point increase) of peers have information about their deworming status. We find a smaller (2.7 percentage points) and insignificant shift of second-order beliefs in the ink treatment. Figure 2.21 shows a similar movement for first-order beliefs. Adults in the bracelet treatment are 10.5 (over the control) and 8.8 percentage points (over the calendar group) respectively are more likely to say that they have information about someone's deworming status. For the ink, we find smaller (5.8 over the control group) shifts in first-order beliefs. When asking respondents "Why do you think they would think [Yes/No/Don't know]?" 35 percent of respondent mention that others had "seen the bracelet" in their answer, while only 6 percent and 7 percent of respondents respectively mentioned ink or calendar as a reason.

Isolating Social Signaling

Controlling for the Consumption Value of Bracelets

To test the assumption that the consumption value of the bracelet is equal to that of the calendar, we conducted a separate willingness-to-pay experiment with a random subsample of adults in the control group as part of the endline survey. Adults had no prior exposure to the bracelet or calendar incentives. The experiment was conducted in two stages. First, as a gift for completing the survey, adults were offered a bracelet or calendar. The majority of people (75 percent) chose the calendar, 23 percent chose the bracelet and 2 percent wanted neither of the items. After choosing their preferred item, respondents were offered to exchange it for the not chosen item, plus a randomly assigned Kenyan Shilling (KSh) value between 0 and 100 (0 and $1). We estimate the average utility difference between the two items. Table 2.3 shows the summary of the posterior distribution of the willingness-to-pay model. On average, individuals value the calendar between 42-53 KSh more than the bracelet (see μ_1, μ_2 and μ_3 as separately estimated for each strata i.e., county). The calendar is therefore a valid control for the consumption value of the bracelet. In the following estimation we assume that the private value of bracelets is equal to calendars, which is a conservative assumption given the greater preference for calendars.

Reminders and Information about Community Take-up Levels

Given that signals increased the visibility of deworming (see First stage results), signals could *also* have affected take-up decisions by acting as reminders, or causing individuals to update their beliefs about the overall level of take-up $\bar{y}$ and the benefits of deworming. To control for these effects, an ideal experiment would i) provide the same take-up information observed by individuals in the bracelet (ink) treatment to individuals in the calendar (control) treatment and ii) saturate individuals with reminders. Figure 2.22 shows the actual information adults were texted. Texted take-up levels between bracelet and calendar, and control and ink for close communities are comparable.[23] For individuals located in far communities, texted take-up levels differed significantly between bracelet and calendar, and ink and control. We find that the text message treatment had no effect on adults' beliefs about community level take-up.

Main Results: Deworming Take-Up

Result 1. The Effect of Visibility x on Take-up Decisions

Figure 2.23 shows deworming take-up levels for the four main treatments, excluding individuals that received the text message treatment. Take-up levels are generally low, with only 36 percent of individuals coming for deworming treatment in the control group. Figure 2.24 presents evidence on the impact of signals/incentives on take-up. In black we show the effects of treatments compared to the control. In blue, we estimate the effect of the bracelet holding constant the consumption value at the level of the calendar. Bracelets have a strong positive effect (8.4 percentage points / 5.6 percentage points) over the control and calendar group, increasing take-up by 24 percent and 13 percent respectively. Despite calendars being privately preferred to bracelets, the calendar increases take-up only marginally by 2.9 percentage points (one-third of bracelet effect). The mean average treatment effect of ink is positive but small and with greater uncertainty in its direction.

Figure 2.25 compares the average treatment effects of signals/incentives for individuals in the control group without text messages to individuals that received the Social Info/Reminder text messages. Signal/incentive treatment effects remain stable and we find no evidence for substitution or complementarity effects between signaling treatments and reminders/social information. This suggests that our signaling treatments are not confounded and identify the partial effect of social signaling on deworming take-up. We interpret the difference in treatment effects between bracelets and ink as (partly) due to differences in the first stage. The ink treatment was much less visible and durable as signal than the bracelets. In addition, anecdotally some people expressed concerns about having their finger inked given its link to elections. It is therefore possible that the ink had a negative private consumption value which would have further lowered its effectiveness.

The Persuasion Effect of Text Messages

Figure 2.26 shows that the text message treatment had a large effect on deworming take-up. Text messages increased the average take-up level by 13 percentage points in the control, ink and calendar treatment and by 16 percentage points in the bracelet treatment. However, it is difficult to interpret these effects since the text message treatment was bundled with a recruitment visit. Only individuals in the Social Info/Reminder treatments were visited by a surveyor who explained the text messages, obtained individuals' consent to participate in the intervention and reminded them about the upcoming community deworming program.

[23] The information was texted in whole numbers such that 2.6 and 3.4 would be 3 out of 10.

Individuals in the SMS control group did not receive a visit. To shed further light on the potential mechanism we can look at the dynamic effect of Social Info/Reminders across the twelve days of deworming. Figure 15 shows that text messages had the largest effect on day one and two, as a result of the reminder that was sent the day before treatment started. The treatment effects become negligible on later days, suggesting that text messages sent on days 4, 6, 8 and 10 and the social information contained in them had no relevant effect on deworming decisions. It is therefore likely that the initial "nudge" and/or the private recruitment visit is what most influenced individuals' decision to take up treatment.

Result 2: The Effect of Cost on Deworming Take-up

Similar to previous findings in the literature (Kremer and Miguel 2007) a small increase in cost leads to a dramatic reduction in the demand for deworming treatment. Figure 2.27 shows that an increase in the mean walking distance by 1 kilometer (from 0.86km to 1.86km) led to a drop in deworming take-up from 44 percent to 28 percent in the control group. This is a decrease of 64 percent. Since we randomly assigned individuals to far and close treatment locations, we were able to hold the underlying preference distribution $\boldsymbol{G}(v)$ and the private valuation that individuals assign to incentives constant, while *only* shifting the marginal person $\hat{v}$. Figure 2.28 shows the treatment effects by distance. The effect of calendar, a consumption incentive, is constant for far and close communities, increasing take-up by 3 percentage points. The average treatment effects of ink and bracelet, however, sizably increased at far distances where take-up is lower. The average treatment effect of bracelets doubled from 5.5 percentage points to 11 percentage points. As a result, the take-up level for far communities in the bracelet treatment is 41 percent which is close to the take-up level for close communities in the control group. In other words, the signaling effect almost fully compensated for the increase in deworming cost. This is provides suggestive evidence for that changes in $\hat{v}$ can lead to significant changes in reputational returns.

Dynamic Take-up Model Results

Figure 2.31 repeats the analysis done in the static model (Figure 2.24), estimating average treatment effects for incentives/signals compared to the control arm with no SMS treatment. There are some differences from the static model results, but not surprisingly so, considering the stronger model assumptions we place in the dynamic model on how the utility for deworming evolves over the twelve days.

The main purpose of analyzing this dynamic model is to impute normally unobservable dynamic outcomes, namely, what would deworming take-up look like were we able to shut down any treatment dynamics beyond what is normally observed in the control arm (as mention in section 2.5, the dynamic model's piecewise-constant proportional hazard parameters represent individual's baseline deworming day preference). The dynamic model has two sets of treatment effect parameters, static and dynamic: Static treatment effects capture the utility of deworming on day one, while the dynamic parameters of a quadratic model capture the time-varying utility of deworming. The static component thus captures both the private utility of deworming (assuming it is static over twelve days) *and* individuals' estimated reputational returns of signaling using their prior beliefs about the take-up of others. The dynamic component of the model captures how individuals' estimated reputational returns evolve in response to what they observe about their peers' deworming take-up, *as well as potentially other unmodeled channels of social learning distinct from prosocial signaling.* Hence, in other to focus on signaling returns, we impute the treatment effect of social signals but restricting the dynamic component to be that of the control arm.

Figure 2.32, reports the average treatment effect of bracelets and ink, net their dynamic effects. While out estimates are imprecise, we continue to observe some potential static effect to the bracelets treatment. Keeping in mind that this calculation of the static effect is very likely a downward biased estimate, this presents compelling evidence that reputational concerns are motivating individuals to seek treatment.

2.7 Conclusion

This paper provides field experimental evidence on the effects of social signaling in the context of deworming. Working with the Kenyan Government, we introduced a new community deworming program where adults were offered free deworming treatment at central locations. We show that a social signal can lead to meaningful increases in deworming take-up and outperform material incentives. Our experiment overcomes a main identification challenge when introducing visibility in actions in setting where individuals make decisions sequentially. Further, we show that the impact of signals varies with the cost of the action and therefore average take-up levels. Signals have large effects at low take-up levels, consistent with signaling models where reputational returns are higher as signals become more informative about types. We also provide evidence of the powerful nature of private nudges. The SMS treatment had the unintended consequence of being most effective in increasing deworming take-up. We believe that this research can be extended in several directions. First, it will be important to test whether social signals can have similar effects on take-up in future rounds of community deworming. Once community deworming is no longer novel and the signals are repeatedly implemented, will treatment effects disappear or could they increase as signals could contribute to forming stronger norms around community deworming. Second, given the large effects of private text message nudges it will be important to understand if the effects are dependent on the initial in-person recruitment that was part of this experiment, or if we could send simple text message reminders to all households in Kenya and thereby increase deworming take-up.

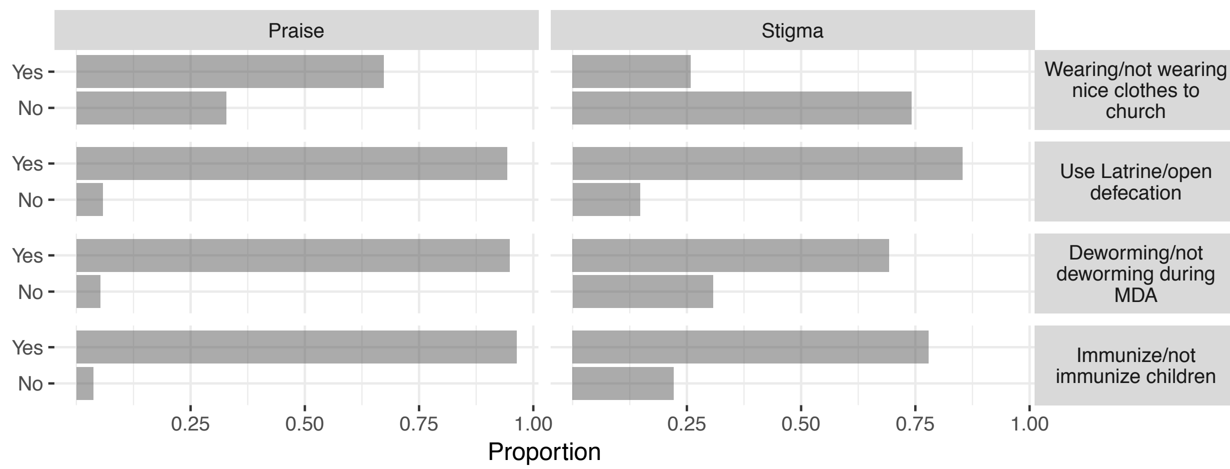
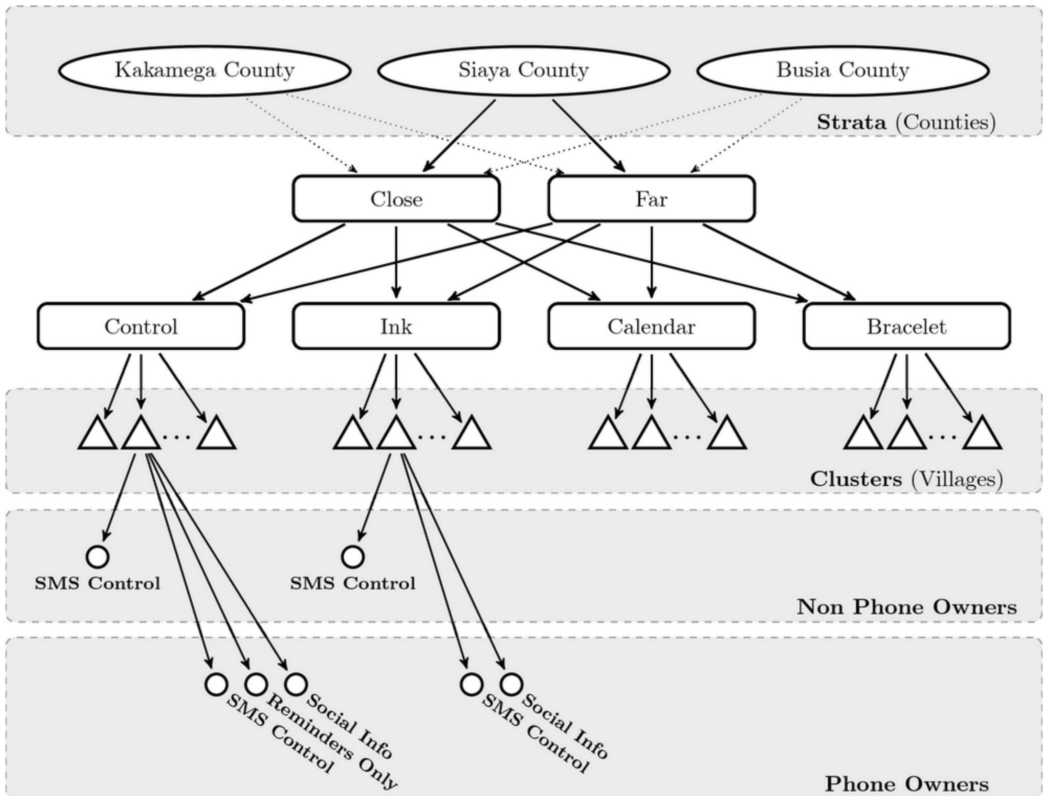


Figure 2.1: Reported social perception of some observable activities.

Figure 2.2: Experimental Treatment Groups



Grey boxes identity the types of population units over which treatment was assigned. The study was stratified over counties (ellipses) and clustered over villages (triangles). Boxes identify cluster (village) level treatments while circles identify individual level treatments.

Figure 2.3: Green Bracelets

In Swahili it says on the bracelet "Treat worms improve the health of your community".

Figure 2.4: Calendar

The calendar made no reference to deworming to minimize its social signaling value.

Figure 2.5: Distribution of actual distance from targeted villages and their assigned treatment location

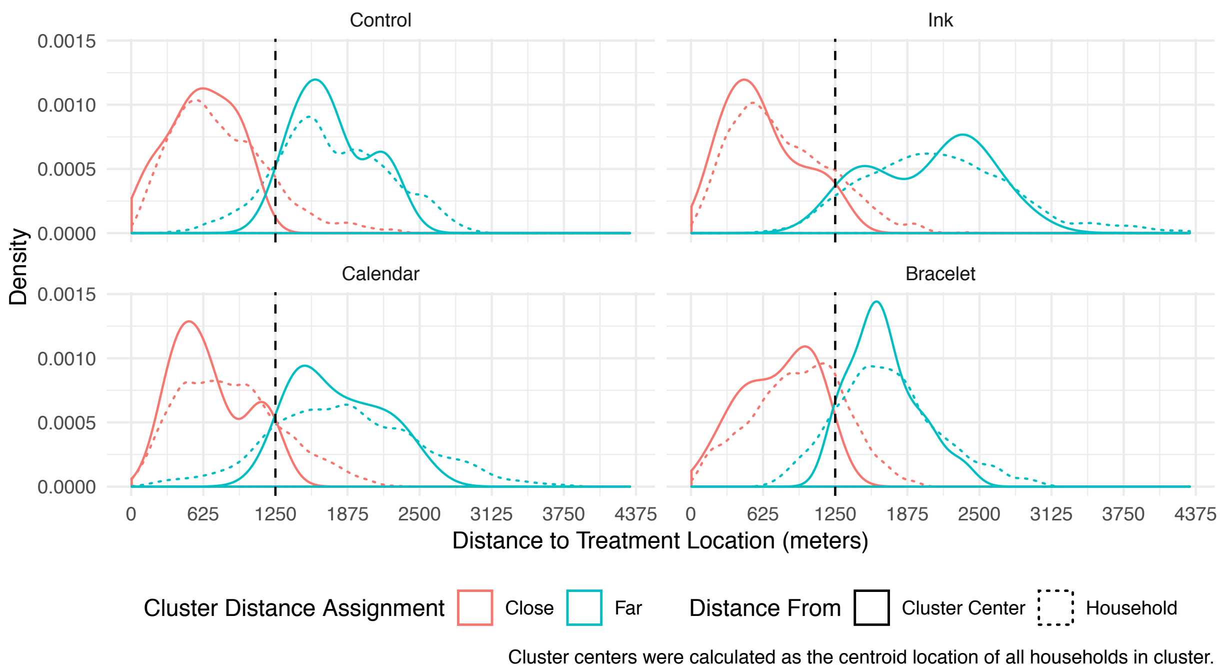


Figure 2.6: Distance to Treatment Locations

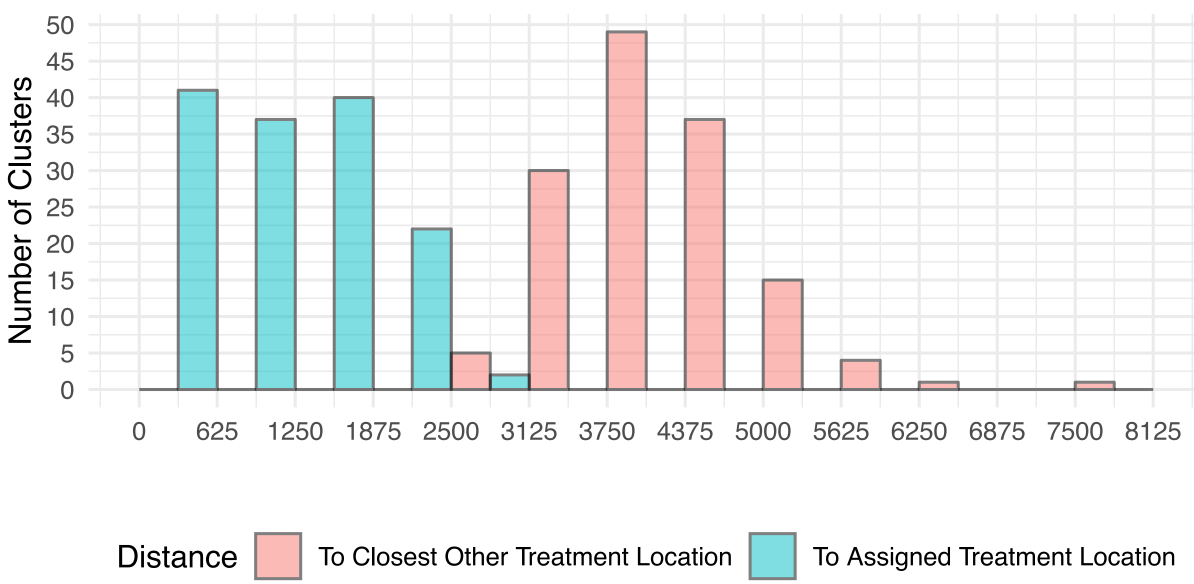
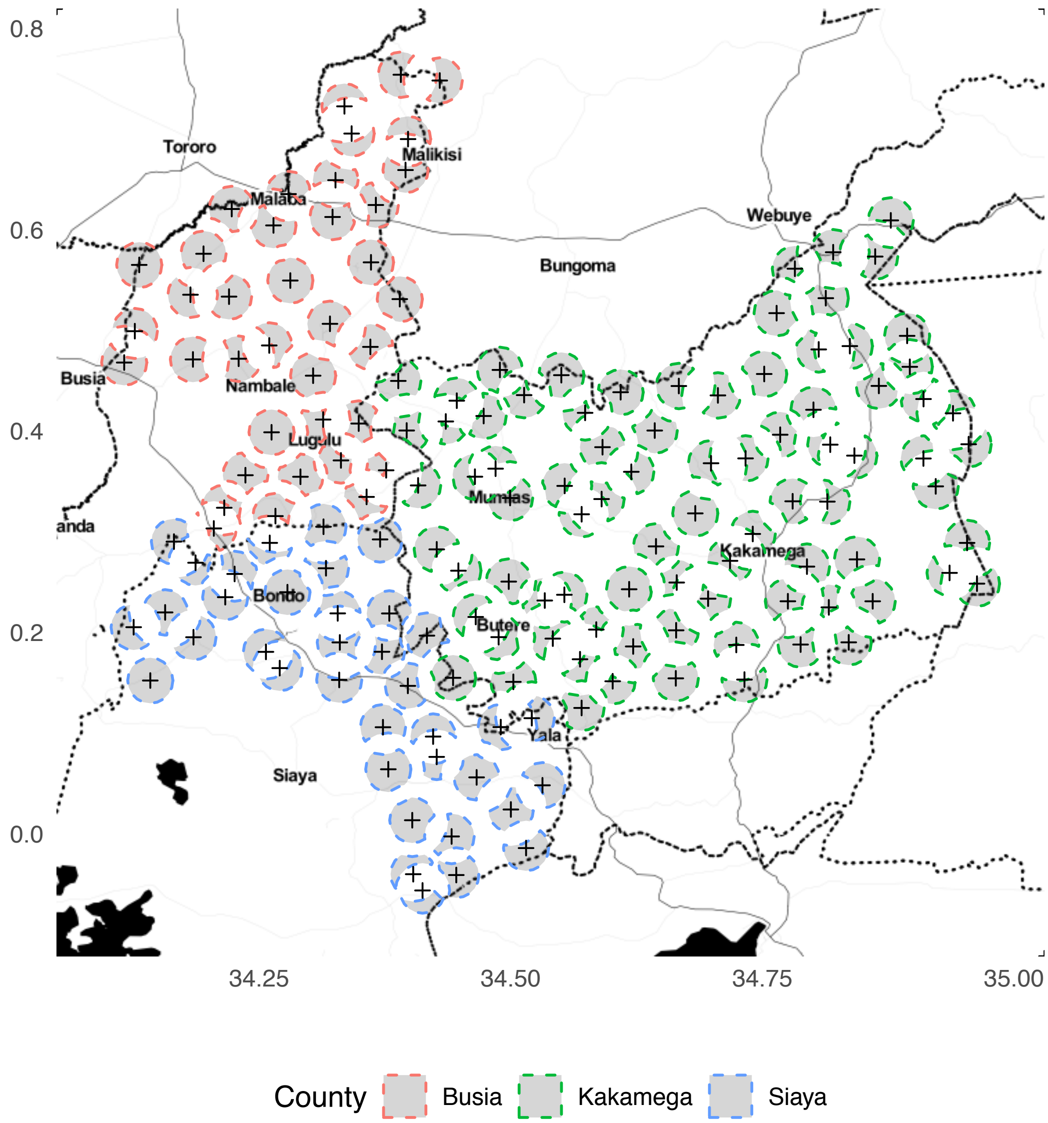


Figure 2.7: Information Script

1. Deworming is not only for children because everyone is at risk of being infected by worms or is infected but does not know.
2. Taking deworming tablets is like using a mosquito net to prevent Malaria or washing hands before eating to avoid diarrhea. You do not have to be sick or experience symptoms in order for you to get dewormed.
3. It is important to take deworming tablets every six 6 months to ensure that your body is always free of worms.
4. The government is providing free deworming tablets and all adults are encouraged to deworm themselves.
5. Deworming all adults will keep our community free from worms and those who do not deworm themselves shall put the entire community at risk, especially towards our children.
6. Remind your family members and neighbors to turn up for the free deworming medication on ______________ at _____________.
7. You will receive __________ for deworming yourself as a symbol of your passion towards improving the health of the members in your family and the community.

Community Health Volunteers informed all households in study communities one week prior to the start of the deworming treatment about the social benefits of deworming, when and where free deworming treatment will be available and if applicable what type of incentive will be given to adults when coming for treatment.

Figure 2.8: Map of Initial Cluster Selection



Black crosses (+) indicate the selected treatment locations, while the grey regions indicate the non-overlapped catchment circles from which we selected village(s) to target.

Figure 2.9: Knowledge of Deworming Intervention

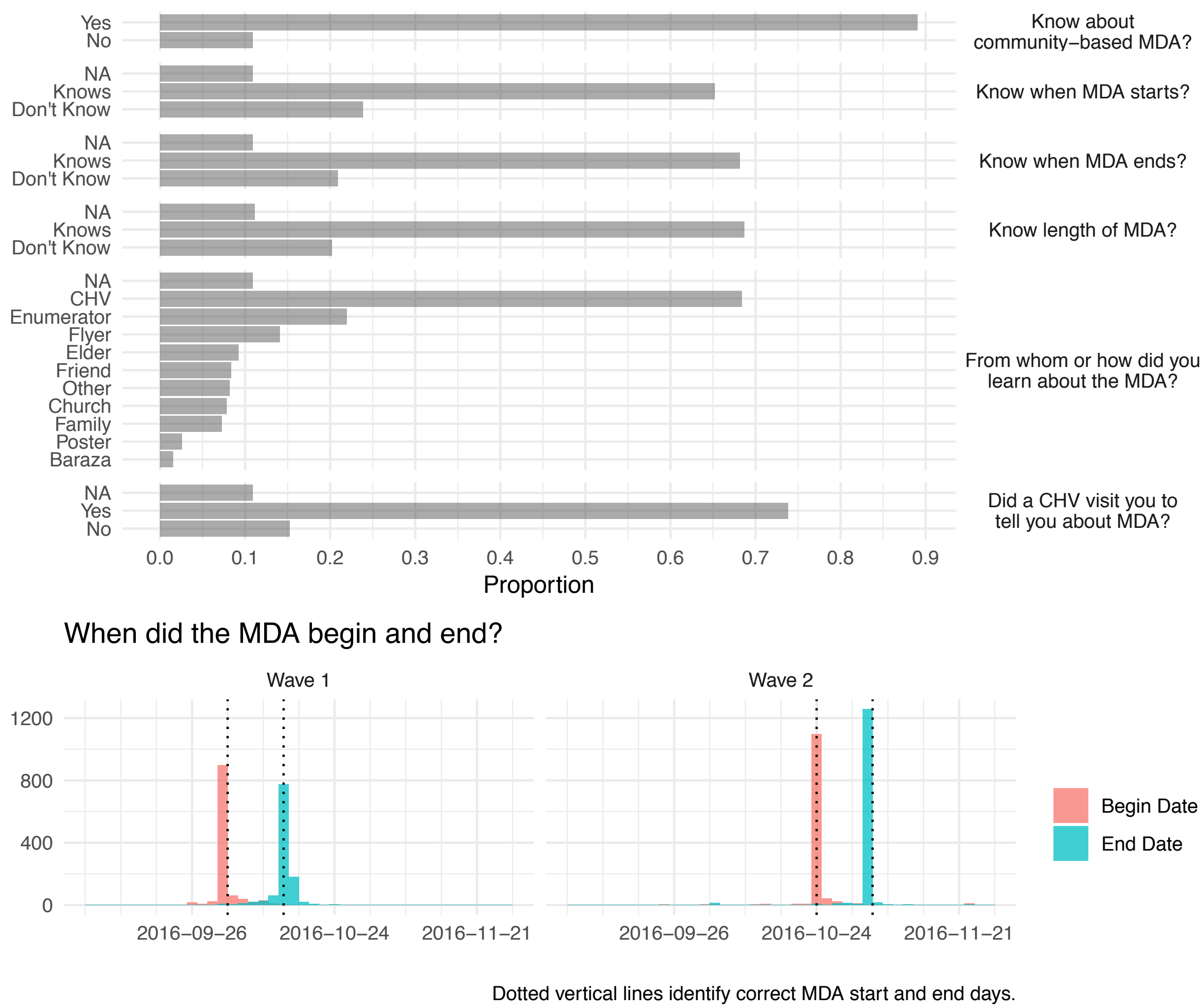


Figure 2.10: Study Population Summary Statistics

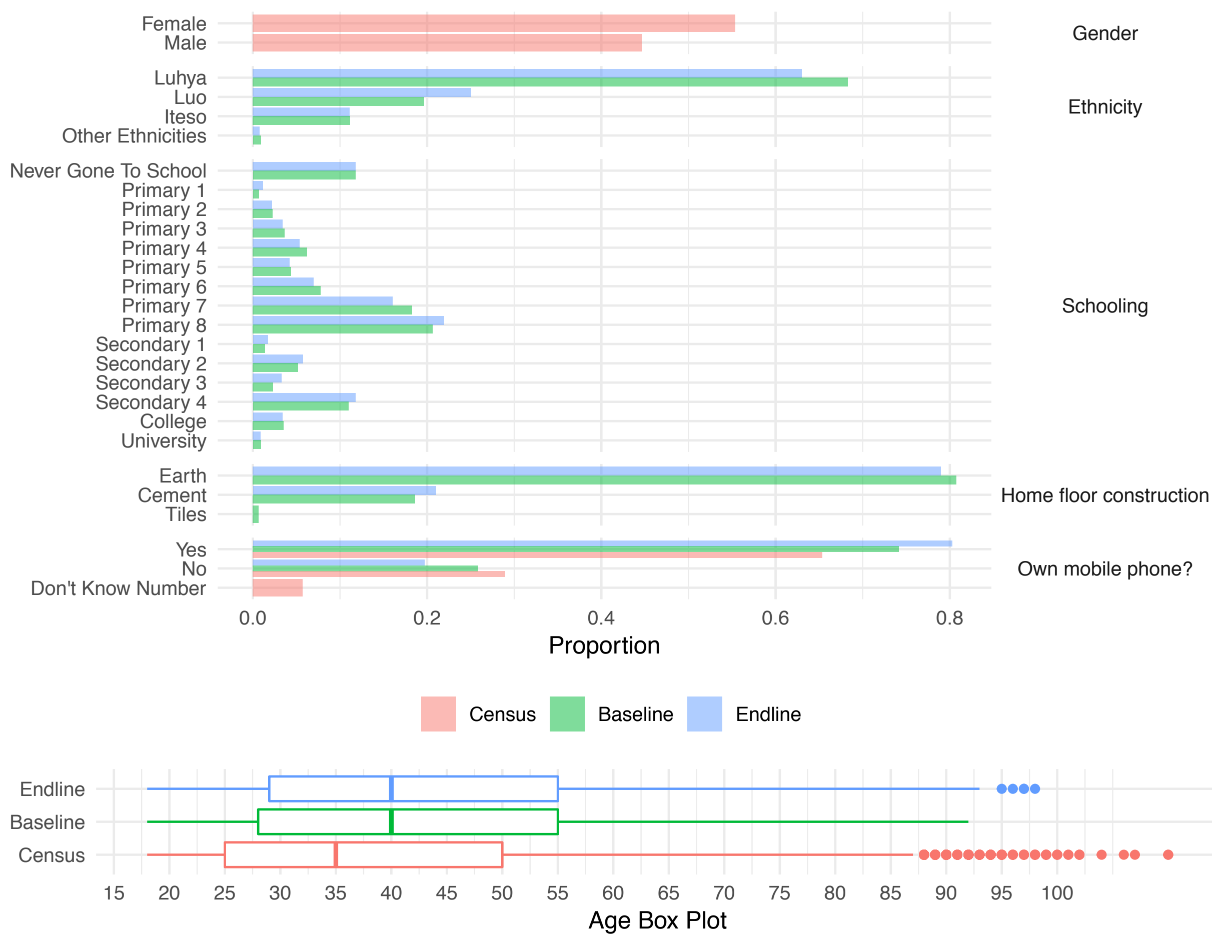


Figure 2.11: Knowledge and beliefs baseline and endline survey responses

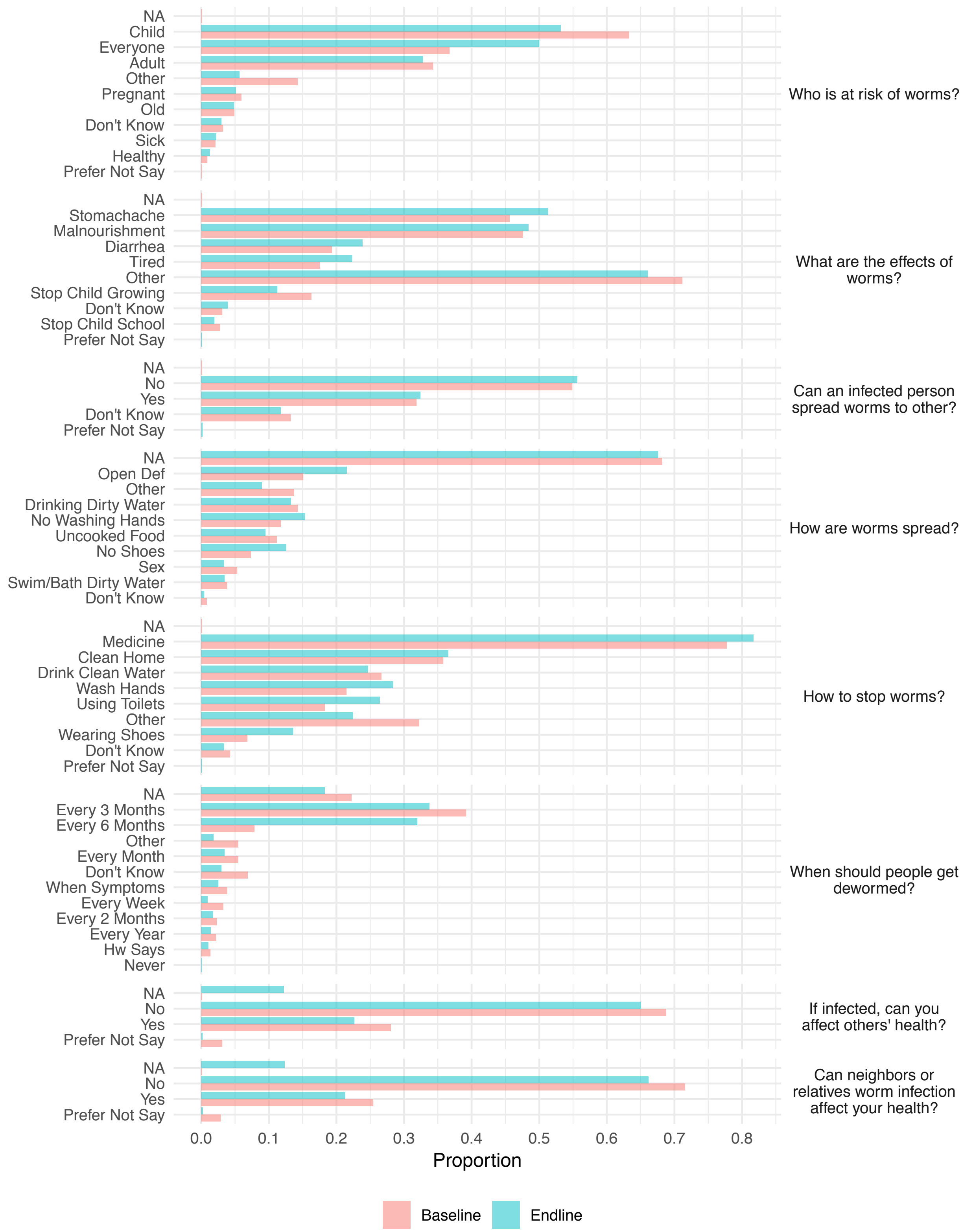


Figure 2.12: Reported response to others' deworming choices

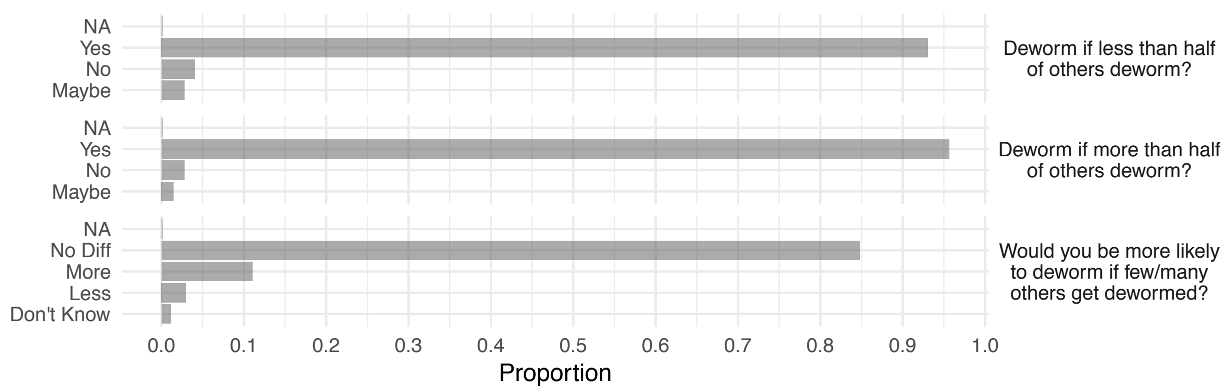


Figure 2.13: Baseline beliefs/predictions about others' deworming choices

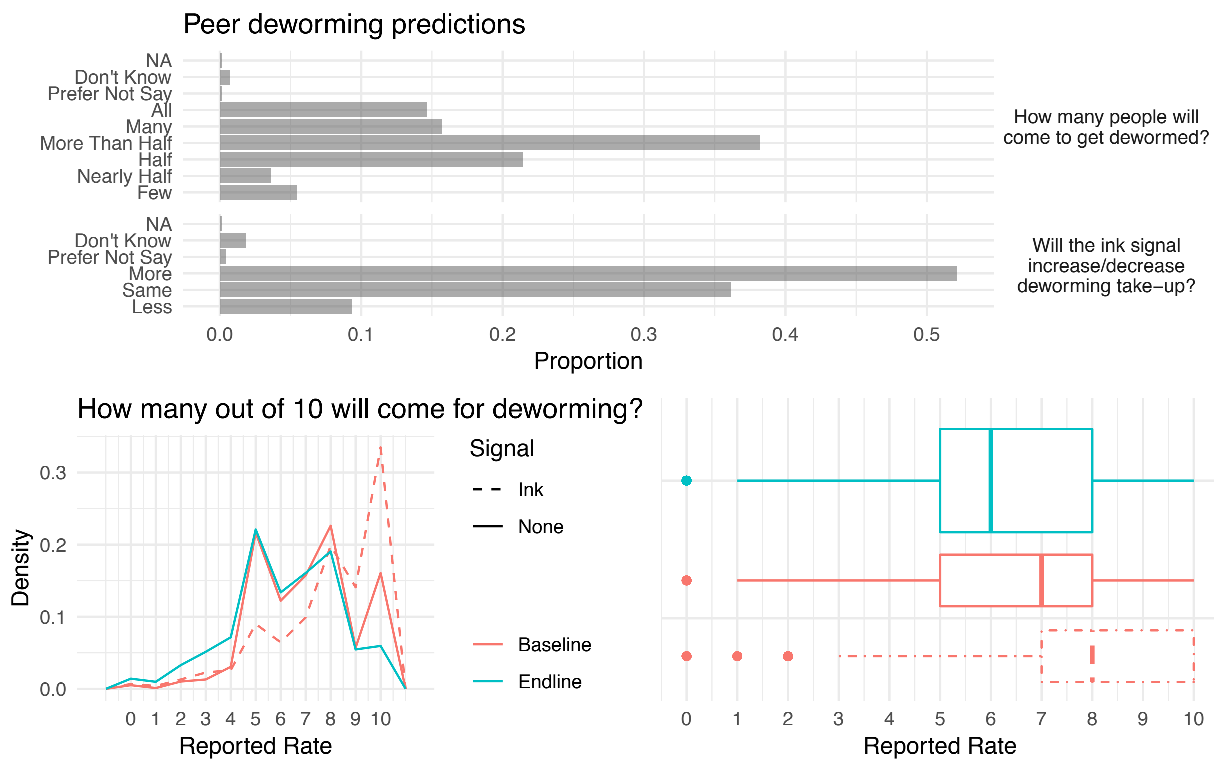


Figure 2.14: Prior Experience with Deworming Treatment

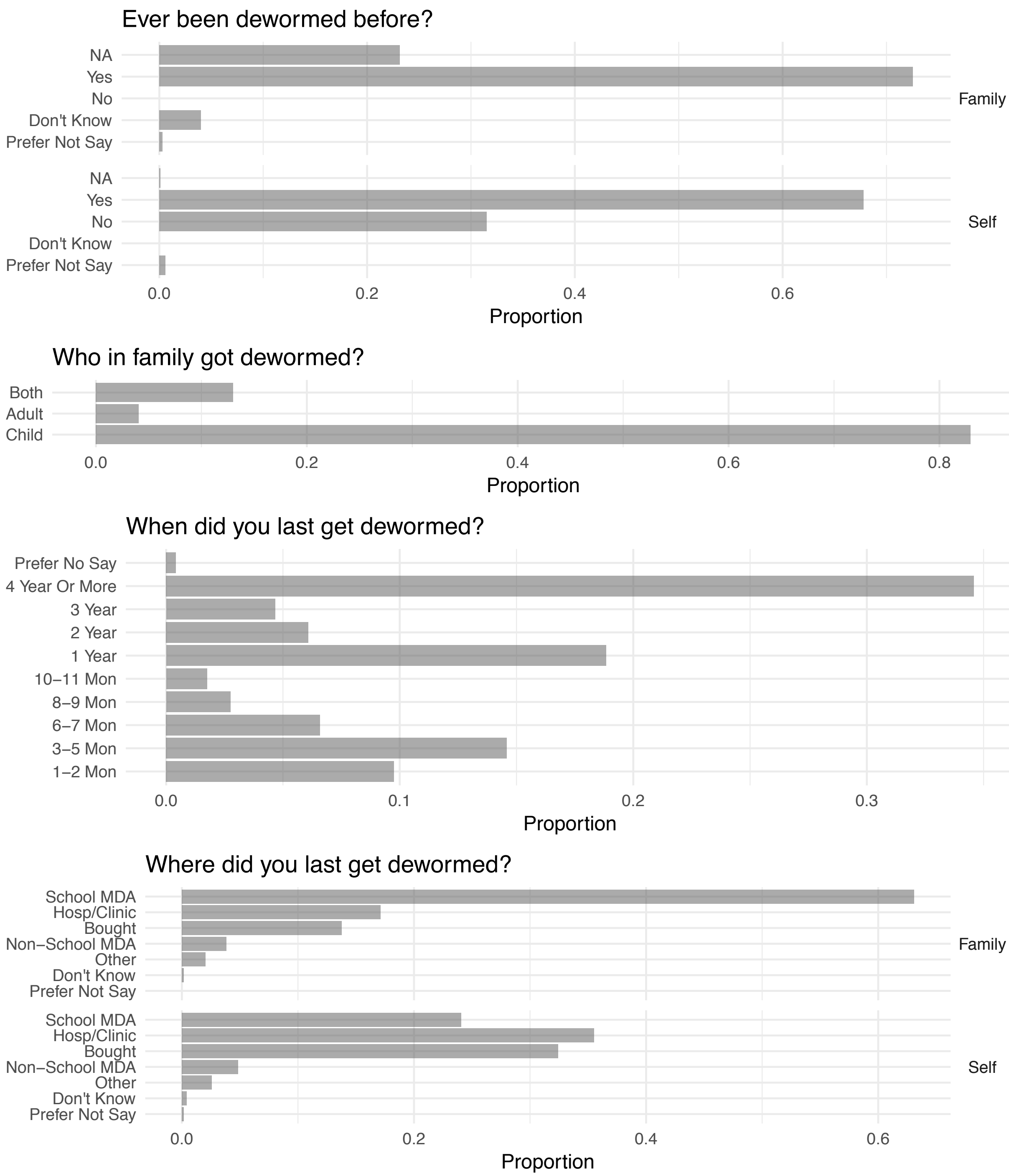
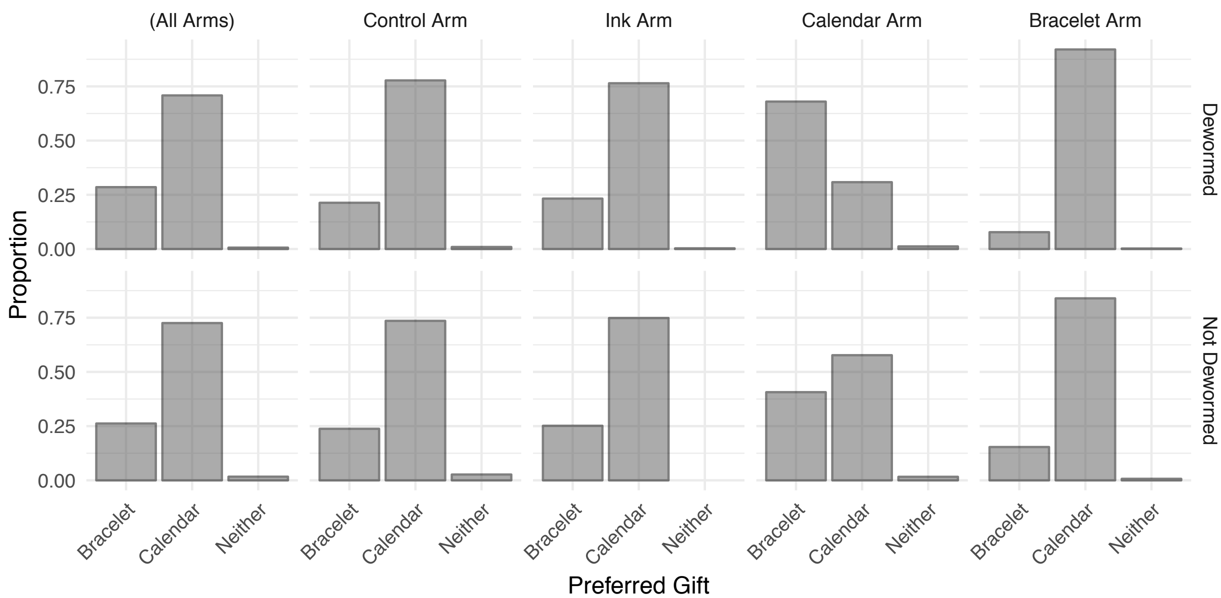


Figure 2.15: Reported Calendar/Bracelet Preferences



Results are split by experiment arm and respondents' deworming take-up.

Figure 2.16: Absolute Deworming Take-up

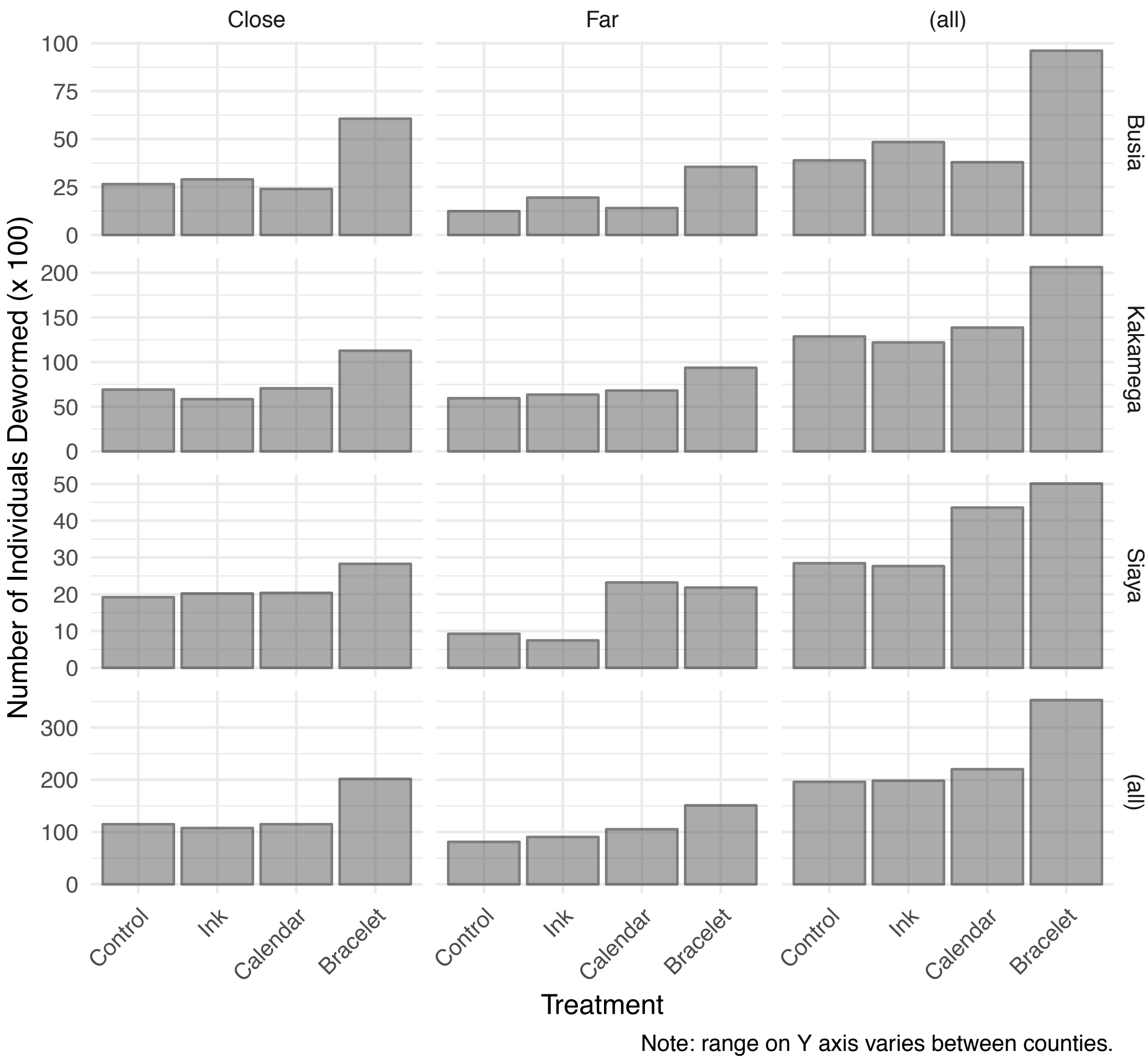
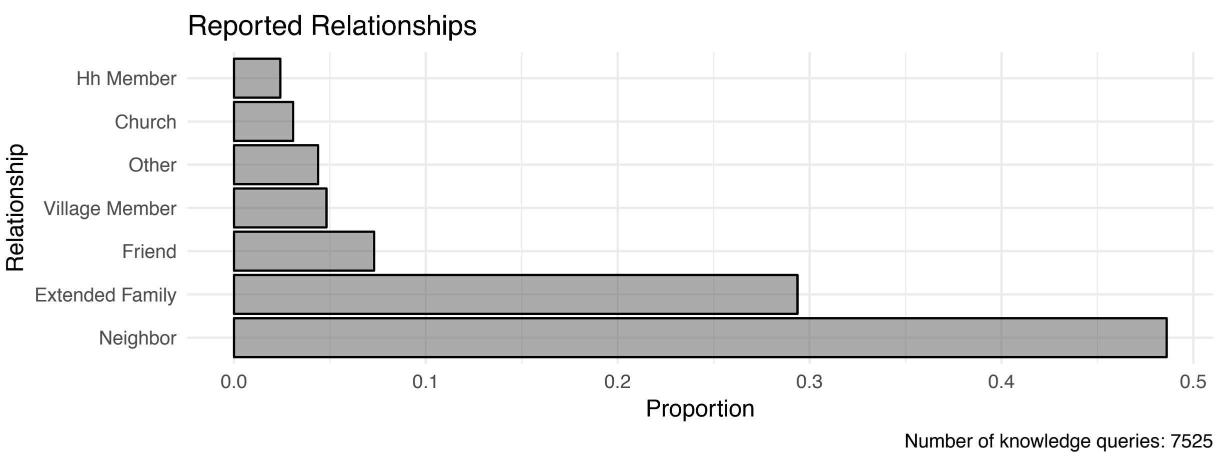
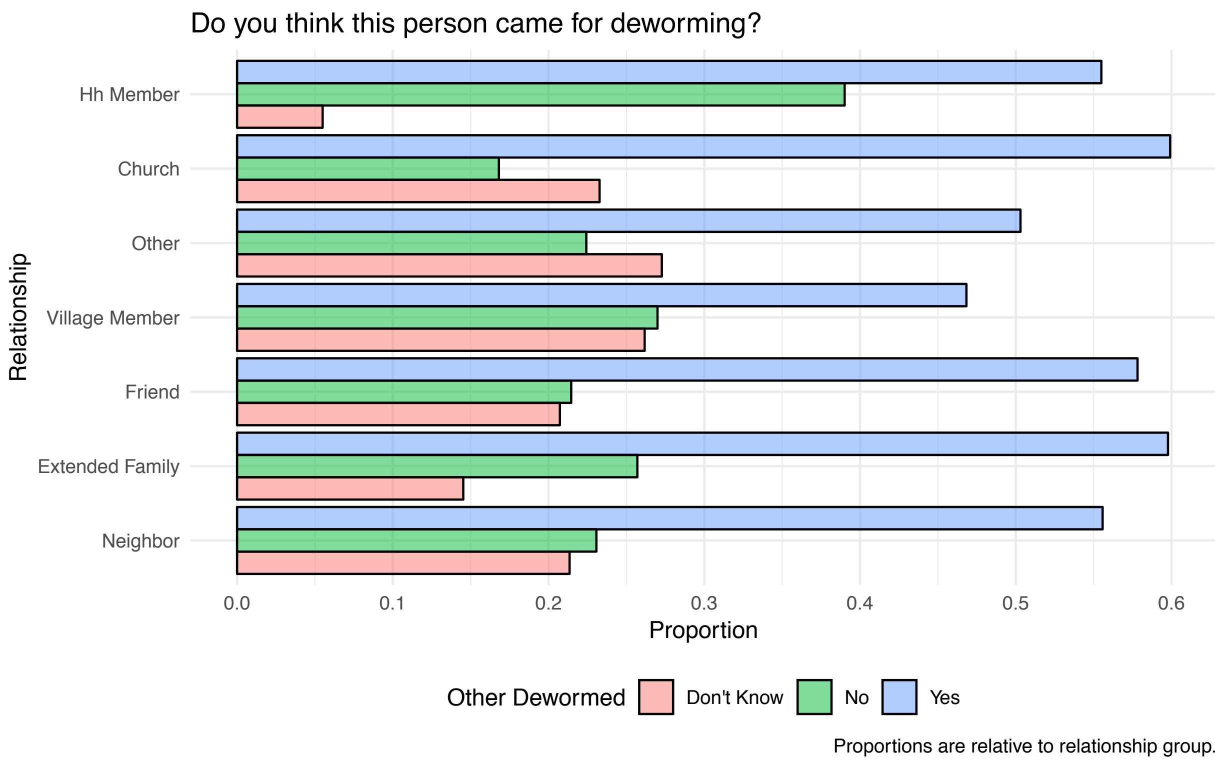


Figure 2.17: Social Connections



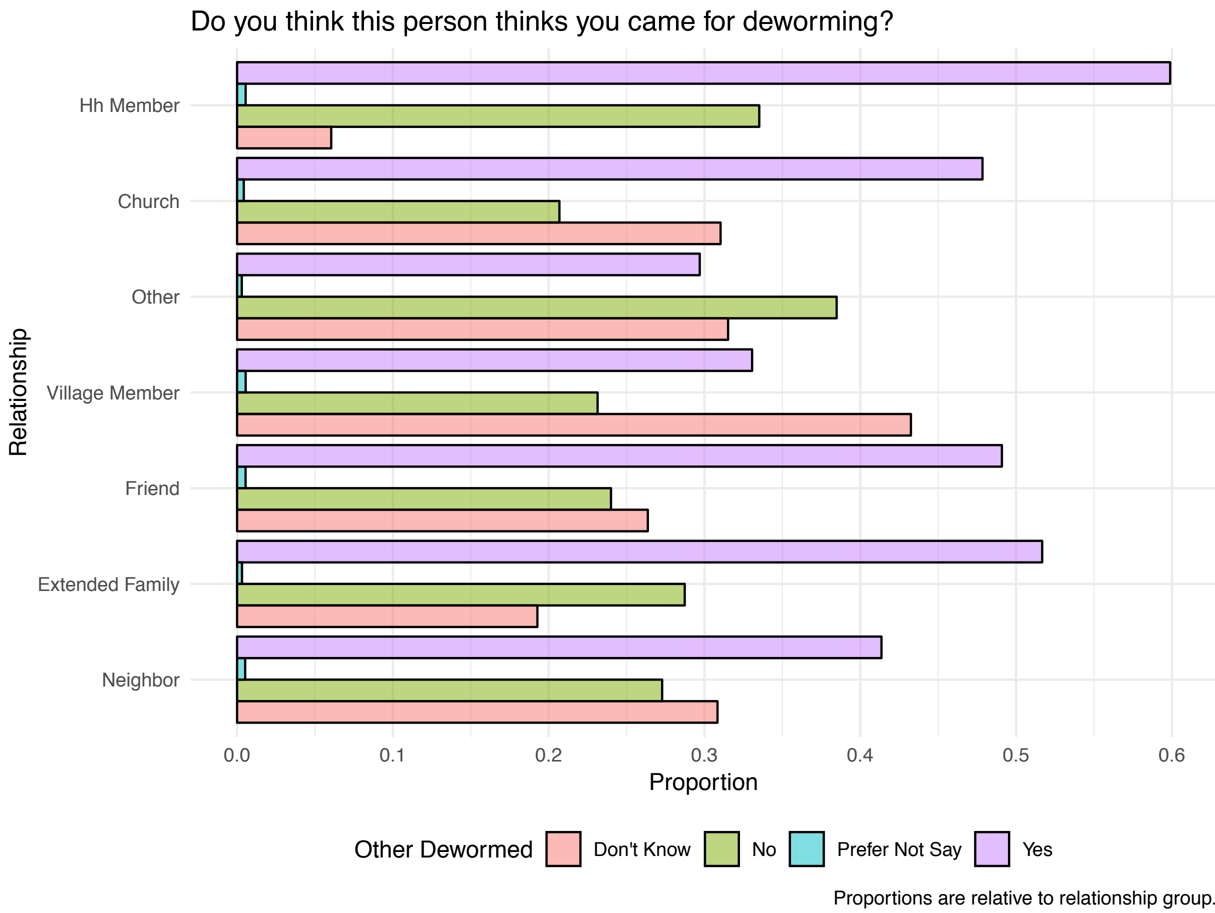
This figure displays the relationship between endline respondents and other adults in their community. Half of the sample of endline respondents (N = 1,626) were asked about a random subset of ten adults in their community. Among the 16,260 other adults named, 7,520 (46.25 percent) were recognized by respondents. Conditional on a name being recognized the respondent was asked about her/his relationship to the person.

Figure 2.18: First-order Beliefs about Deworming Choices, by Relationship



This figure shows endline respondents' first-order beliefs about other adults' deworming choices, by relationship. The proportions reported here are relative to the relationship group. Answers are pooled across control, ink, calendar and bracelet treatments. Half of the sample of endline respondents (N = 1,626) were asked about a random subset of ten adults in their community. Among the 16,260 adults named, 7,520 (46.25 percent) were recognized by respondents. Conditional on a name being recognized the respondent was asked "Do you think this person came for deworming?". The answer options were "Yes", "No" and "Don't know".

Figure 2.19: Second-order Beliefs about Deworming Choices, by Relationship



The figure shows endline respondents' second-order beliefs about other adults' deworming decision, by relationship. The proportions reported here are relative to the relationship group. Answers are pooled across control, ink, calendar and bracelet treatments. Half of the sample of endline respondents (N = 1,626) were asked about a random subset of ten adults in their community. Among the 16,260 adults named, 7,520 (46.25 percent) were recognized by respondents. Conditional on a name being recognized the respondent was asked "Do you think this person thinks you came for deworming, or do you think they think you did not come for deworming?". The answer options were "Yes", "No" and "Don't know".

Figure 2.20: Second-order Beliefs (Levels)

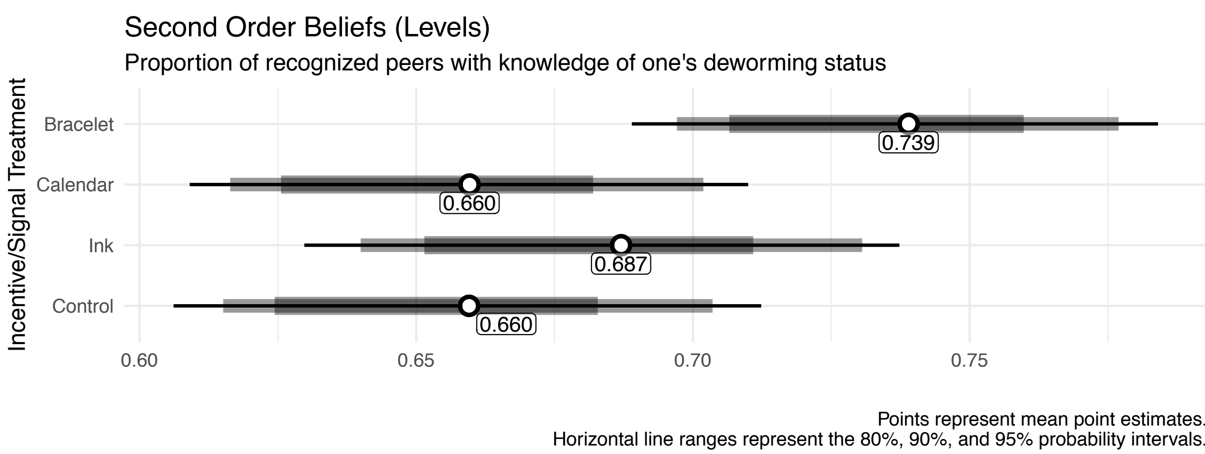


Figure 2.21: First-Order Beliefs (Levels)

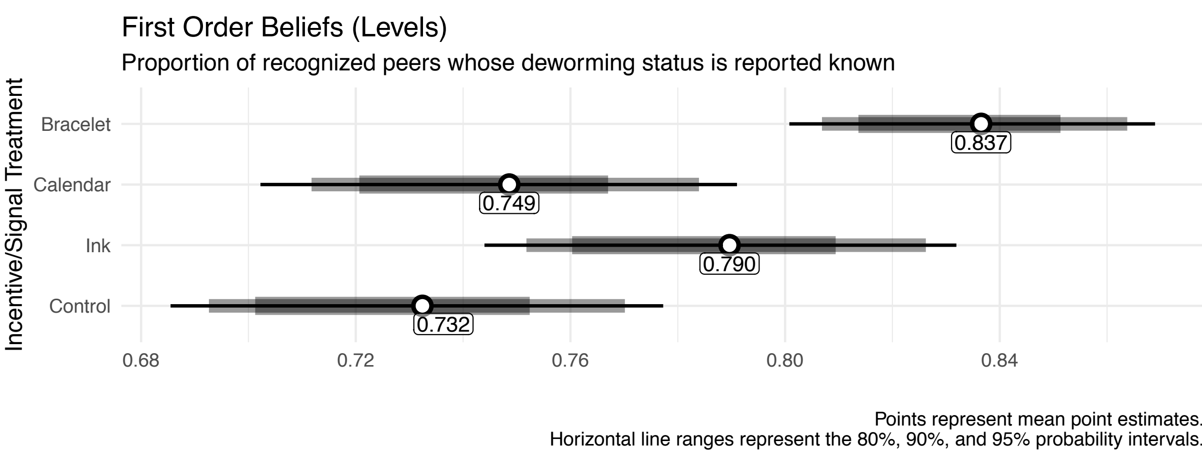


Figure 2.22: Social Information Reported to Adults, split by Distance

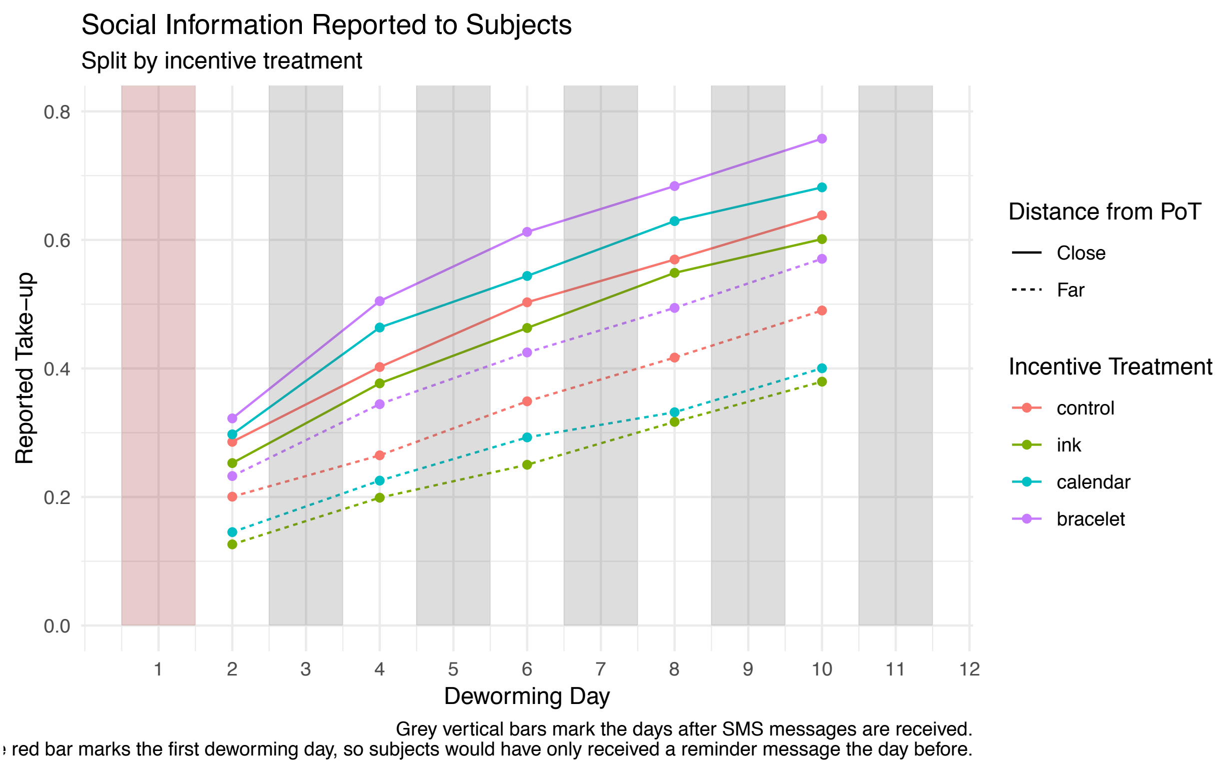


Figure 2.23: Posterior Distribution of Deworming Take-up Levels (with no SMS Treatment)

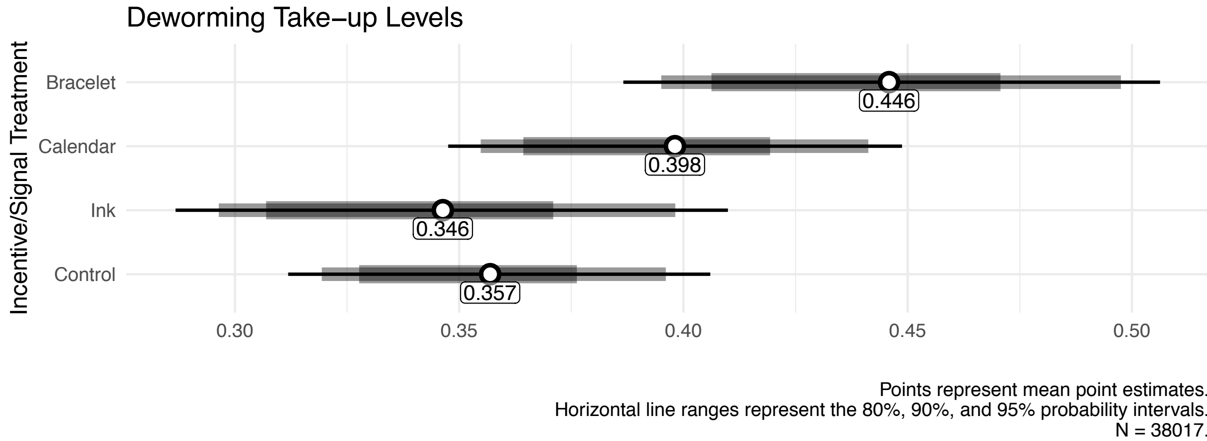
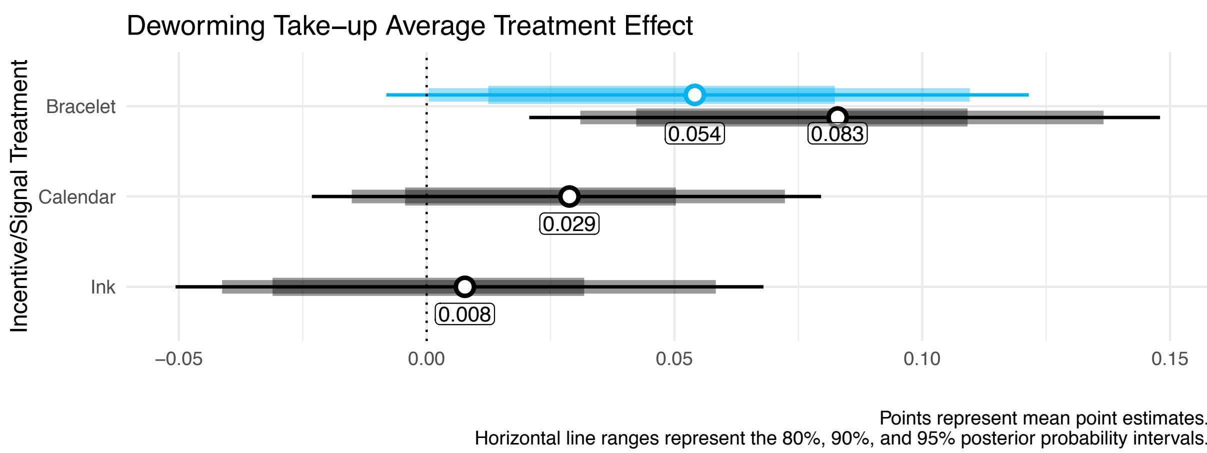


Figure 2.24: Posterior Distribution of Incentive Treatment Effects Compared with the Control Arm (with no SMS Treatment)



Black lines show average treatment effects compared to control group; blue line shows the bracelet treatment effect compared to the calendar treatment.

Figure 2.25: Posterior Distribution of Incentive Treatment Effects Compared with the Control Arm (conditional on SMS Treatment)

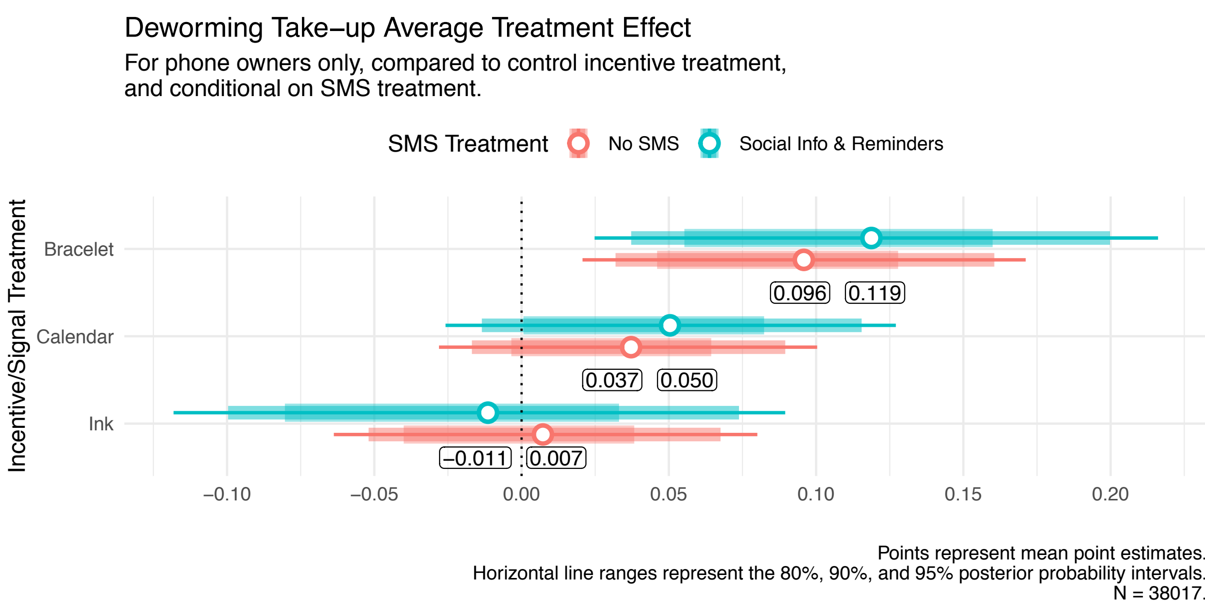


Figure 2.26: Posterior Distribution of SMS Treatment Effect, conditional on Incentive Treatment

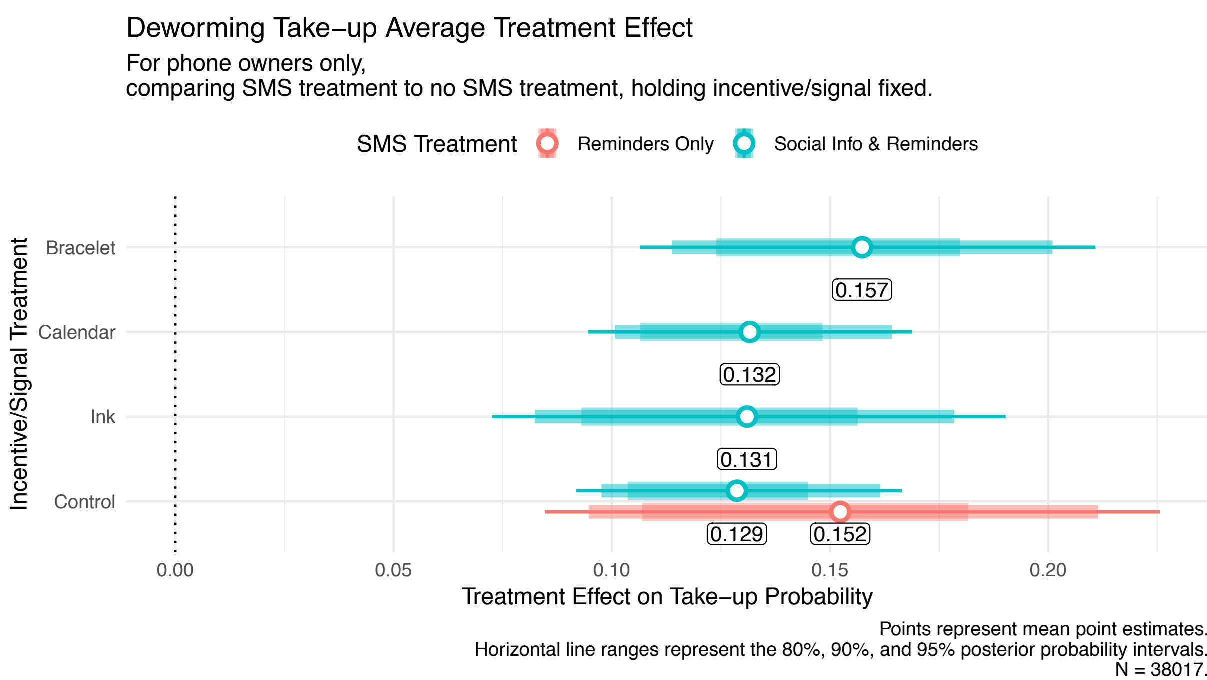


Figure 2.27: Posterior Distribution of Deworming Take-up Levels (with no SMS Treatment), split by Distance

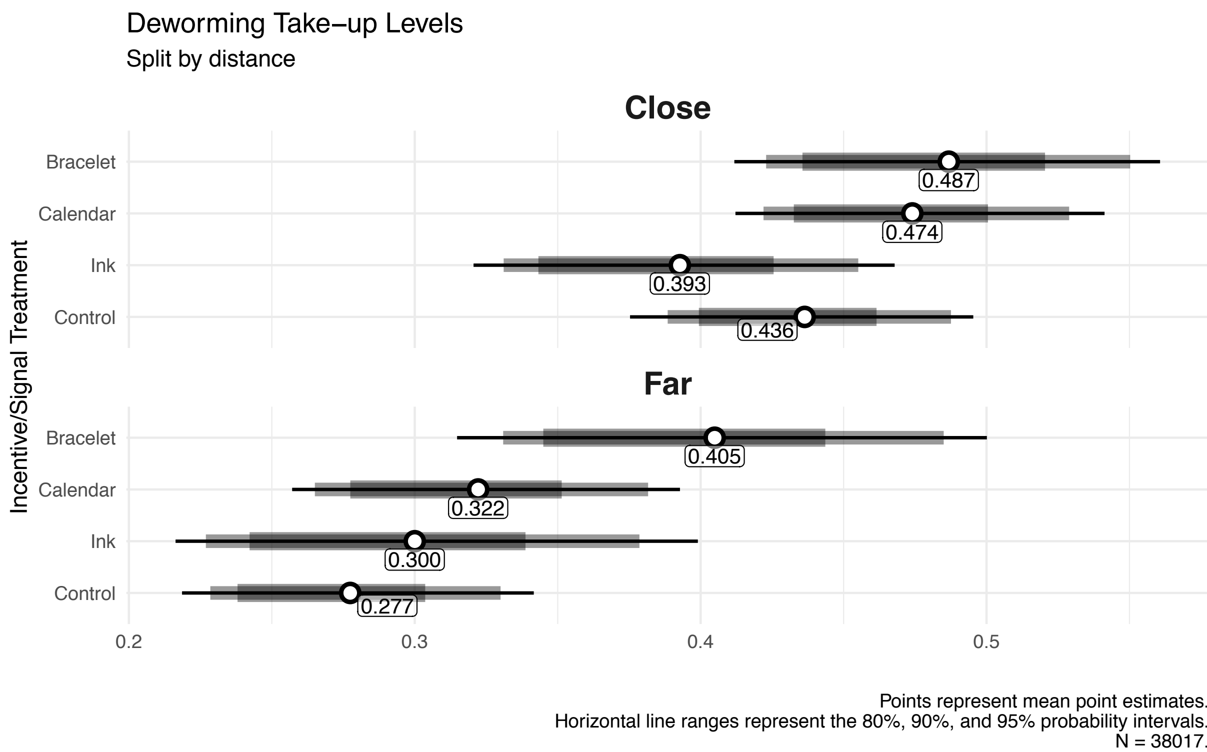


Figure 2.28: Posterior Distribution of Deworming Take-up Treatment Effects (with no SMS Treatment), split by Distance

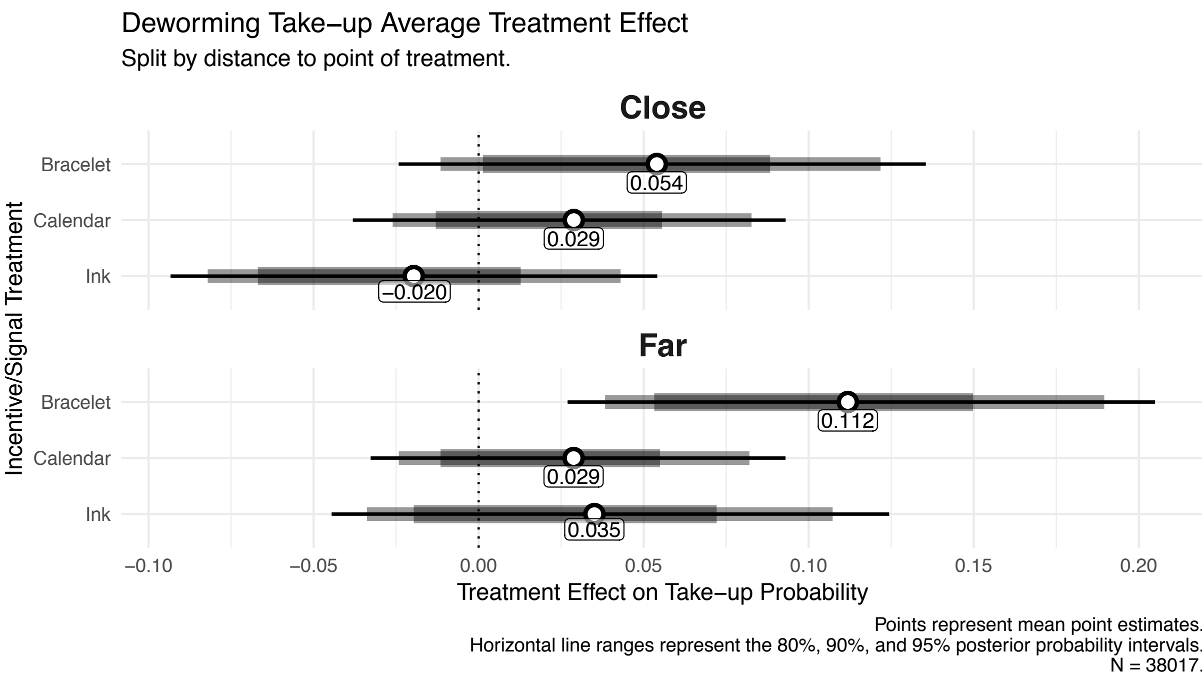


Figure 2.29: Posterior Distribution of Incentive Treatment Effects Compared with the Control Arm (conditional on SMS Treatment), split by Distance

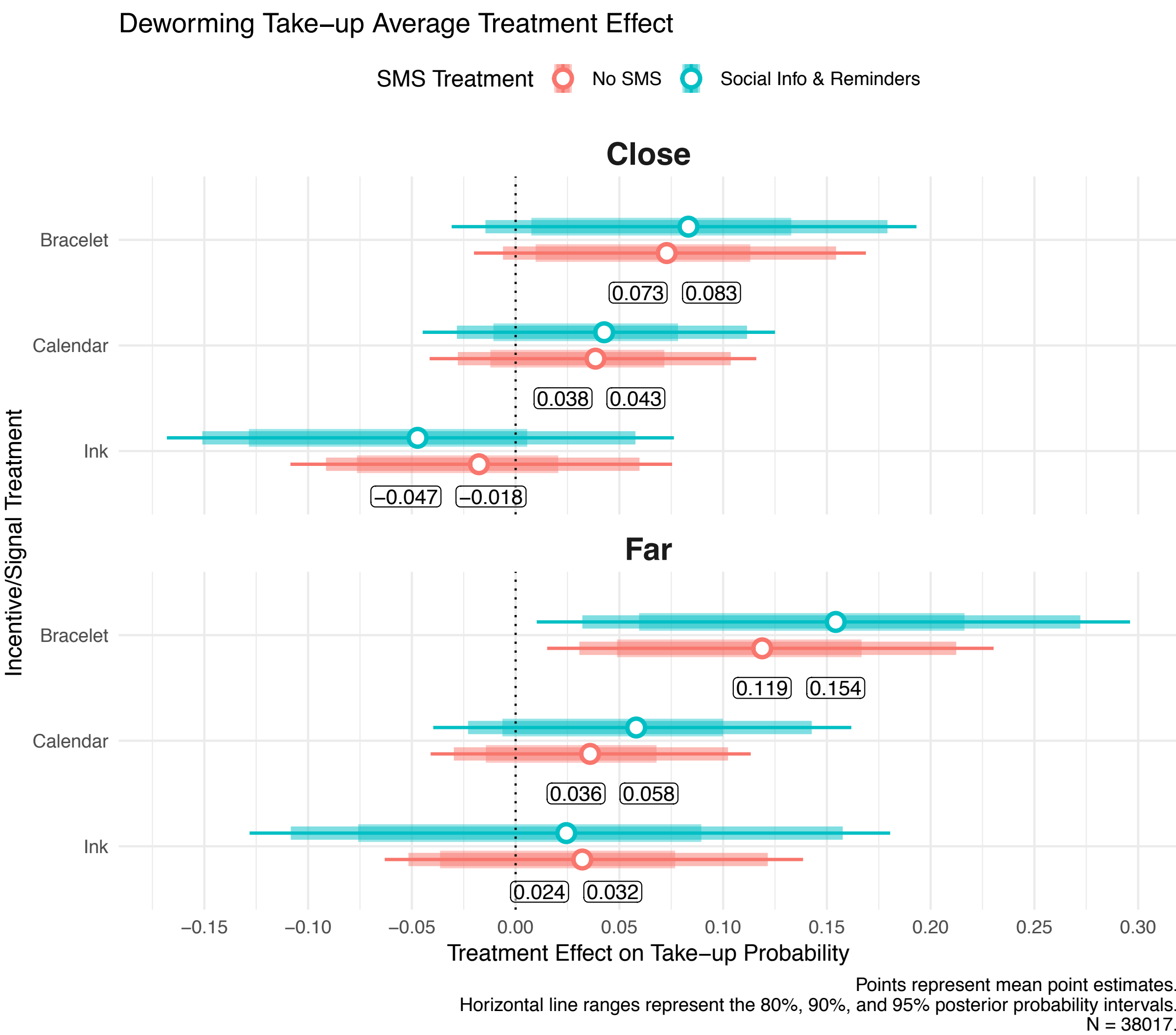


Figure 2.30: Posterior Distribution of Incentive + SMS Treatment Effects Compared with the Control Arm (with no SMS Treatment), split by Distance

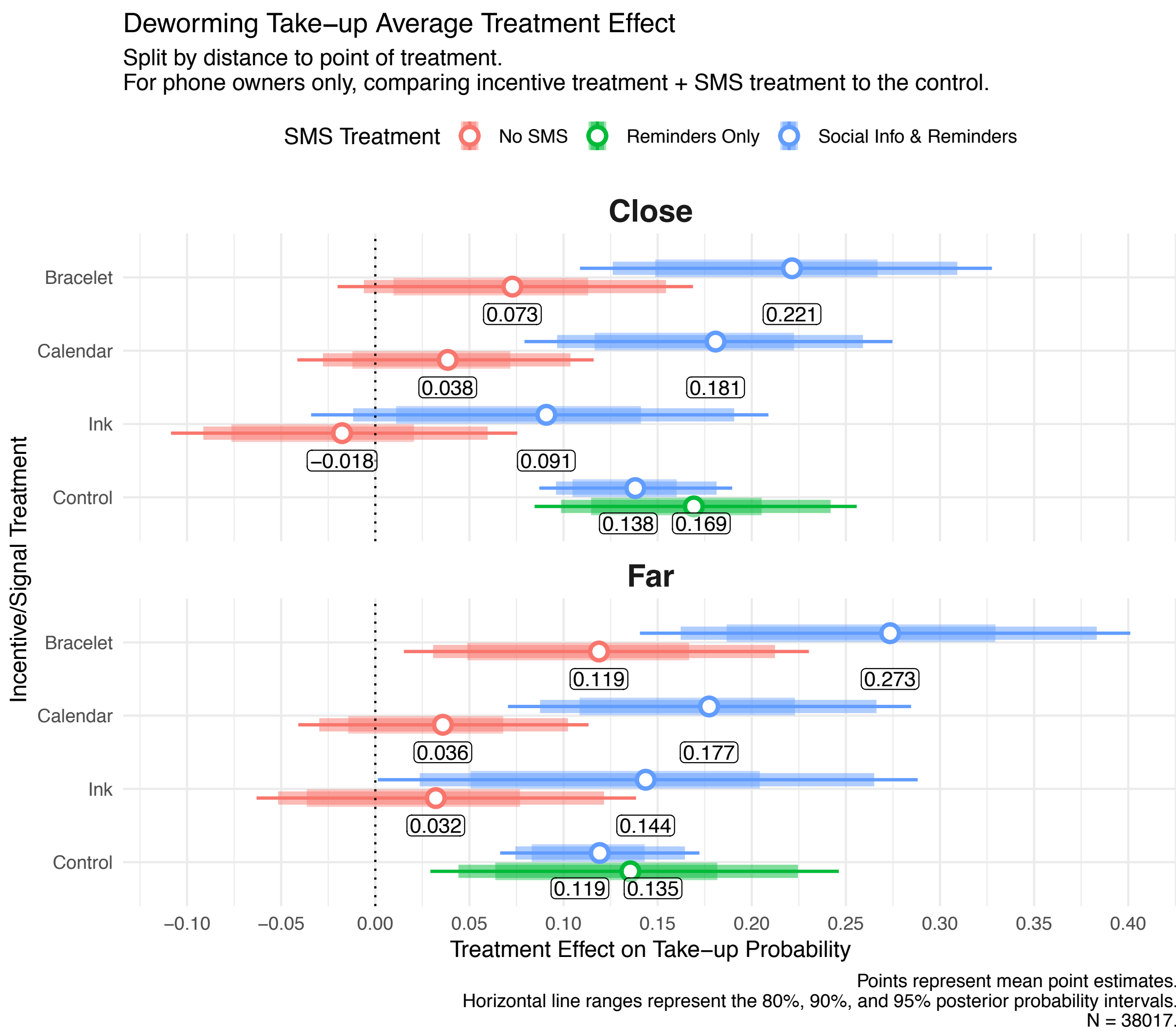


Figure 2.31: Dynamic Model Incentive Treatment Effect

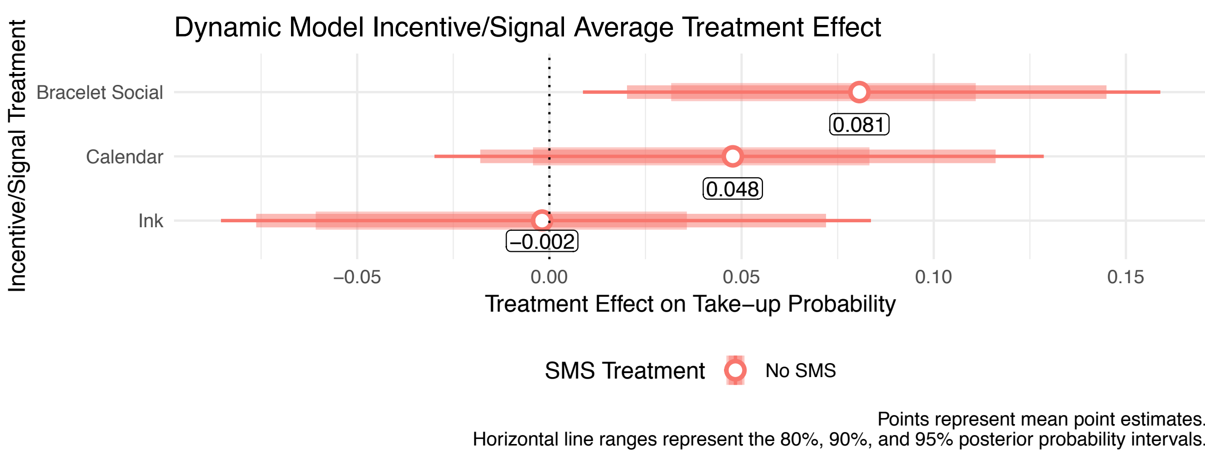


Figure 2.32: Signaling Treatment Effects, net any Dynamic Effects

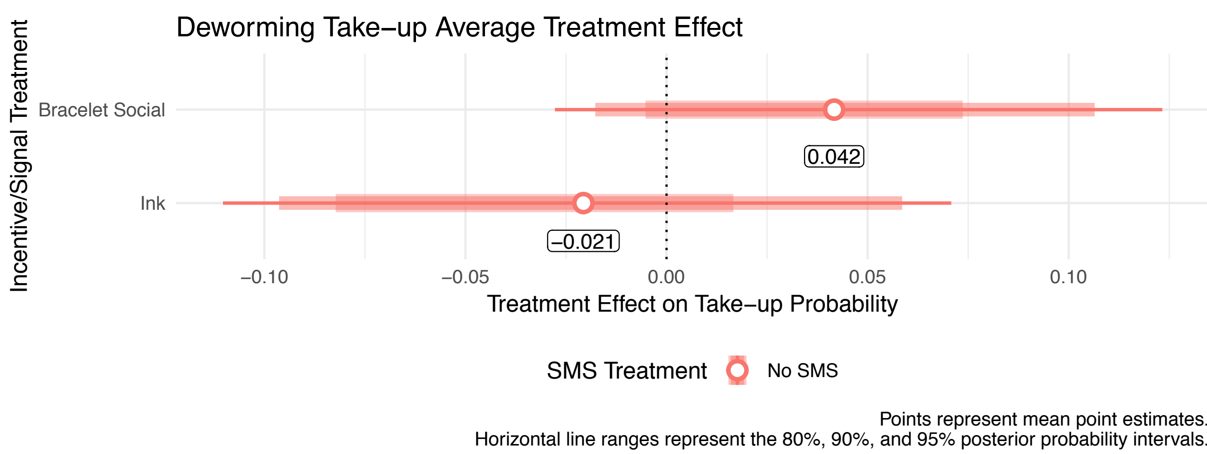


Figure 2.33: Daily Take-up Non-Cumulative Proportion for all Incentive Treatment Arms (no SMS Treatment)

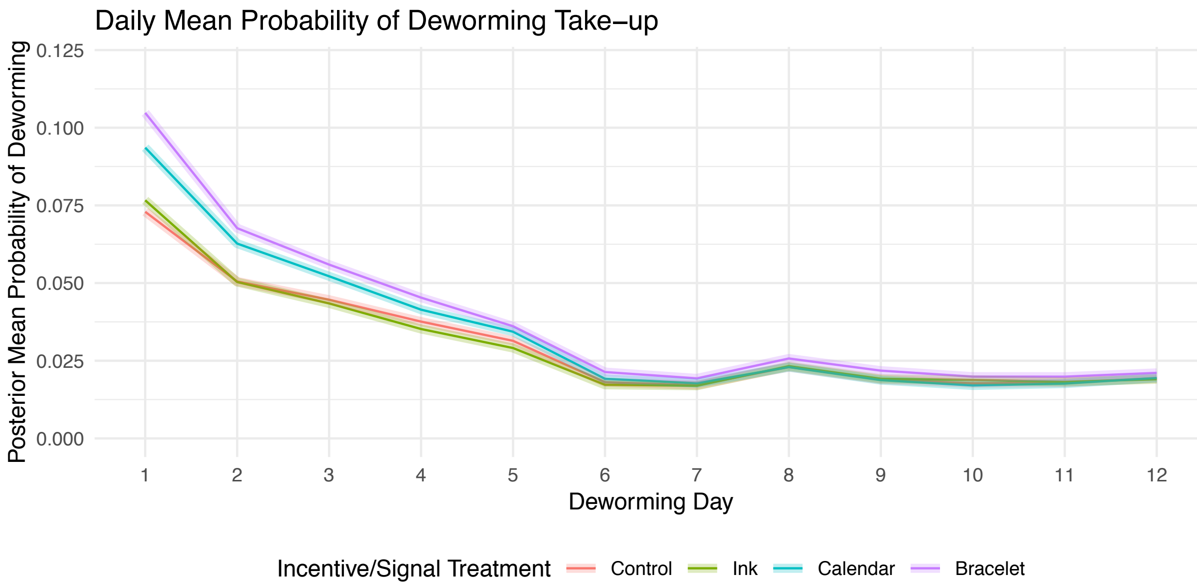


Table 2.1: Number of Clusters and Individuals in Targeted Communities, by County and Treatment Arm

	Clusters					Individuals				
County	Control	Ink	Calendar	Bracelet	All	Control	Ink	Calendar	Bracelet	All
Busia	8	9	7	10	34	2,603	2,437	1,785	2,615	9,440
Kakamega	18	19	19	20	76	5,588	5,680	5,542	5,917	22,727
Siaya	8	8	9	9	34	2,038	1,834	1,648	1,614	7,134

Table 2.2: Mean Distance to Treatment Location (Meters)

Arm	Close	Far	Mean Difference
(All)	840.0	1,860	1,020.2
Bracelet	936.8	1,701	764.3
Calendar	867.2	1,846	978.9
Control	777.3	1,736	958.4
Ink	784.5	2,152	1,367.9

Table 2.3: Posterior Estimates for Willing-to-Pay Parameters

	Mean	SE_{Mean}	SD	2.5%	5%	50%	95%	97.5%	N_{eff}
μ	-0.62	2.13	120.73	-237.06	-193.50	-0.46	189.57	242.44	3200.00
σ	61.38	0.06	3.56	55.01	55.73	61.25	67.58	68.96	3200.00
μ_1	41.46	0.11	6.23	29.54	31.33	41.25	51.96	53.97	3200.00
μ_2	41.91	0.08	3.96	34.40	35.63	41.81	48.83	50.22	2584.61
μ_3	52.92	0.12	6.53	40.51	42.56	52.83	63.69	65.83	3200.00
$\Pr[V_i^c > V_i^b]$	0.50	0.01	0.34	0.02	0.03	0.50	0.97	0.98	3200.00
$\Pr[V_i^c > V_i^b \mid k = 1]$	0.73	0.00	0.03	0.67	0.68	0.73	0.77	0.78	3200.00
$\Pr[V_i^c > V_i^b \mid k = 2]$	0.73	0.00	0.02	0.69	0.70	0.73	0.76	0.76	3200.00
$\Pr[V_i^c > V_i^b \mid k = 3]$	0.77	0.00	0.02	0.72	0.73	0.77	0.81	0.82	3200.00

Appendix A: Reduced Form Regression Results

Figures 2.34-2.37 present the OLS stratified regression analysis results, using robust standard errors clustered at the village level (Imbens and Rubin 2015). The overall pattern of effects (direction and magnitudes) is similar to the main analysis results in section 2.6. As expected, estimates are not exactly the same. Regression analyses report sample and null hypothesis test statistics, while multilevel Bayesian analyses rely on generative models to impute counterfactuals and calculate causal estimands. Using a null hypothesis testing framework with multiple comparisons is vulnerable to picking up spurious statistically significant effects (Gelman, Hill, and Yajima 2012; Young 2018), and thus we use a multilevel model to provide more conservative estimands and to avoid over-fitting.

Figure 2.34: OLS Regression Incentive Treatment Effects Analysis (No SMS Treatment)

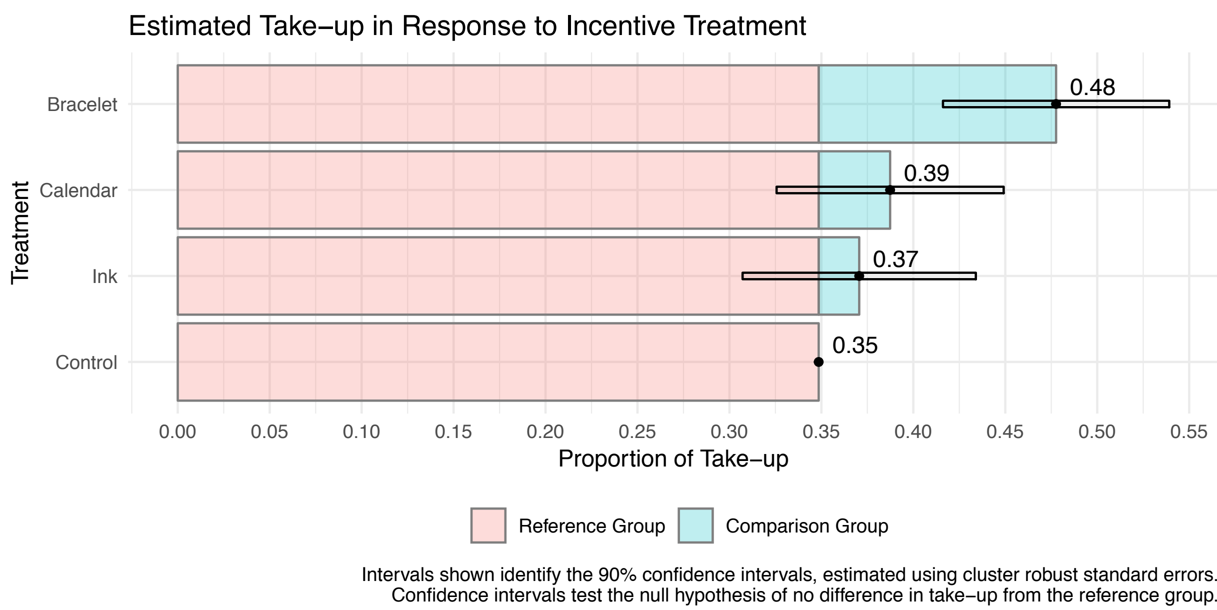


Figure 2.35: OLS Regression Incentive and SMS Treatment Effects Analysis (Phone Owners only)

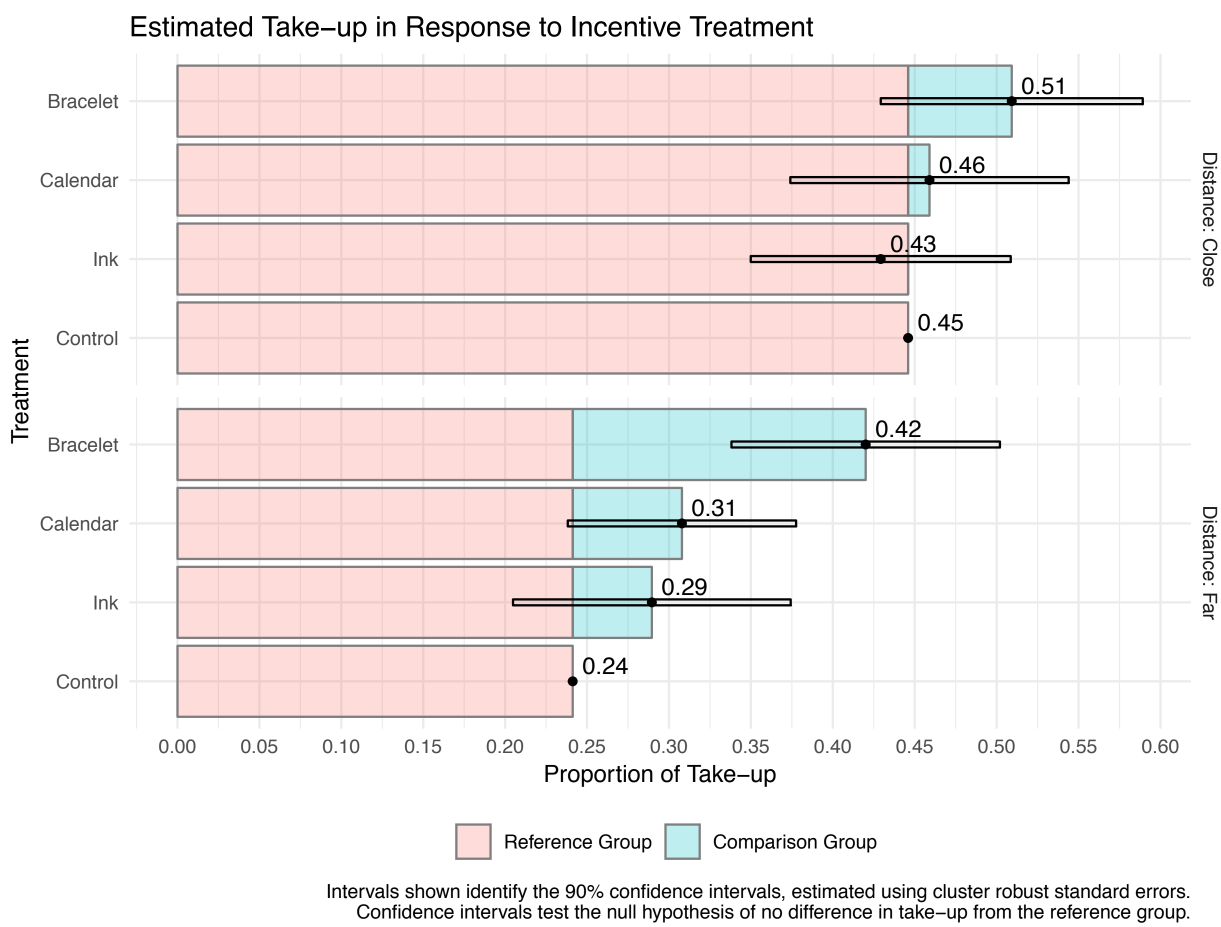


Figure 2.36: OLS Regression Incentive Treatment Effects Analysis (no SMS Treatment), split By Distance

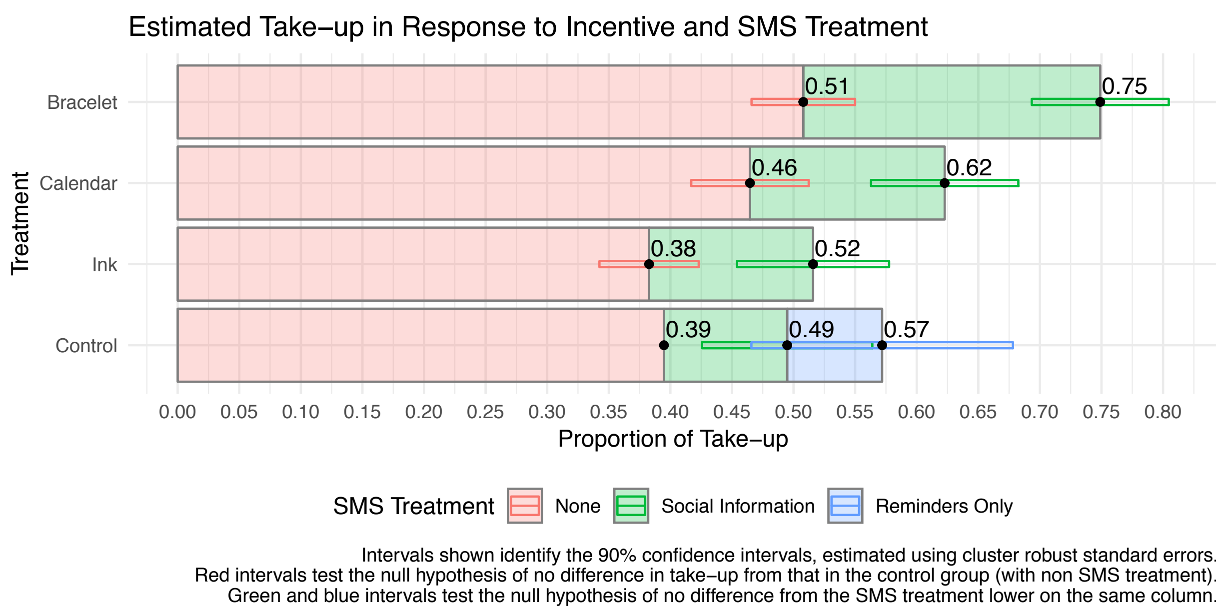
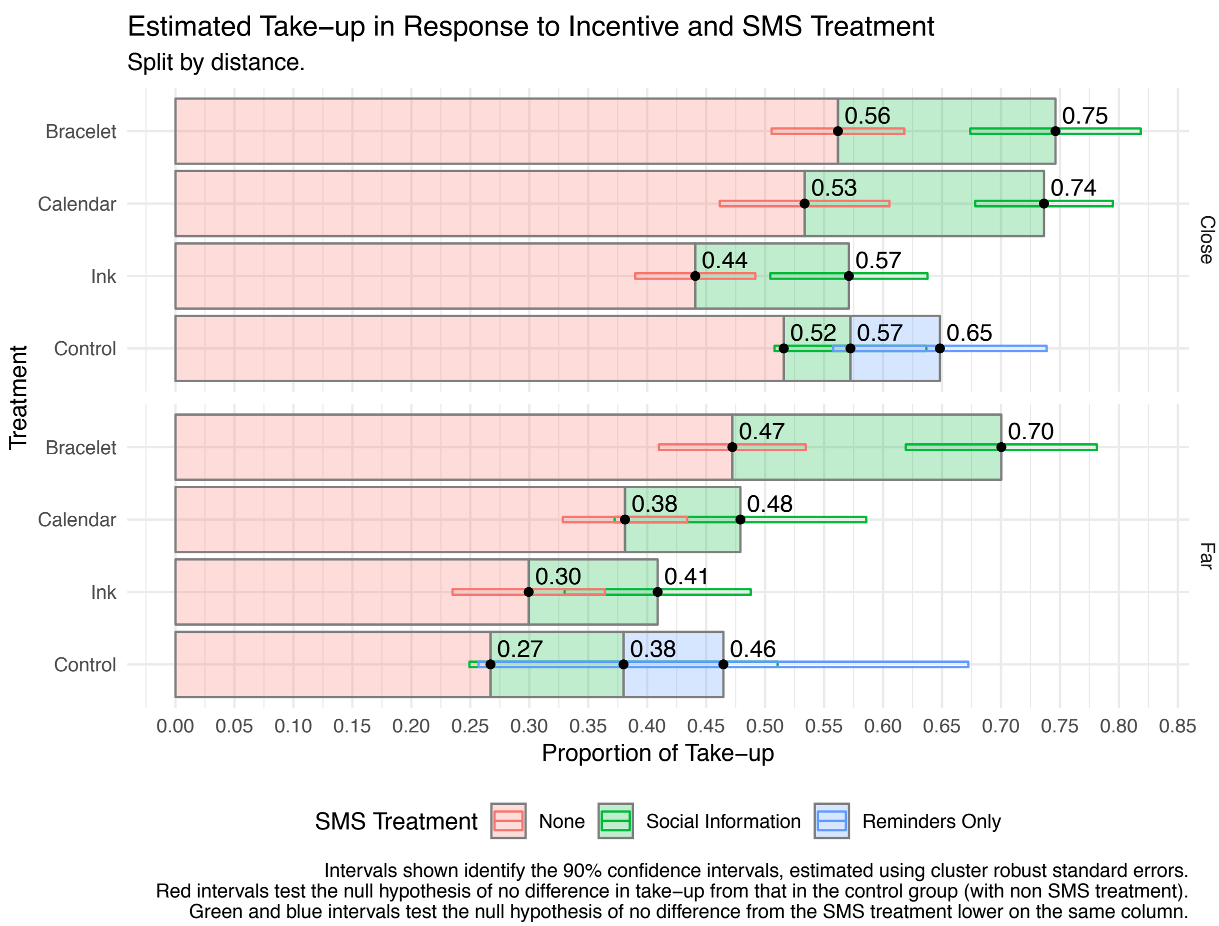

Estimated Take−up in Response to Incentive and SMS Treatment
Treatment
Bracelet
Calendar
Ink
Control
0.51
0.75
0.46
0.62
0.38
0.52
0.39
0.49
0.57
0.00 0.05 0.10 0.15 0.20 0.25 0.30 0.35 0.40 0.45 0.50 0.55 0.60 0.65 0.70 0.75 0.80
Proportion of Take−up
SMS Treatment
None
Social Information
Reminders Only
Intervals shown identify the 90% confidence intervals, estimated using cluster robust standard errors.
Red intervals test the null hypothesis of no difference in take−up from that in the control group (with non SMS treatment).
Green and blue intervals test the null hypothesis of no difference from the SMS treatment lower on the same column.

Figure 2.37: OLS Regression Incentive and SMS Treatment Effects Analysis (Phone Owners only), split by Distance



Appendix B: Flyers

Figure 2.38: Control Group Flyer

Flyers were distributed by Community Health Volunteers to all households prior to the start of the deworming program. The message on the flyer says: "Treat worms: improve the health of your community. Help to eradicate/fight worms in your community. Where: [Cluster's Treatment Location]. When: [Dates Wave 1/2]. Medicines will be provided free of charge. Kenya's government against worms treatment for people over age 18".

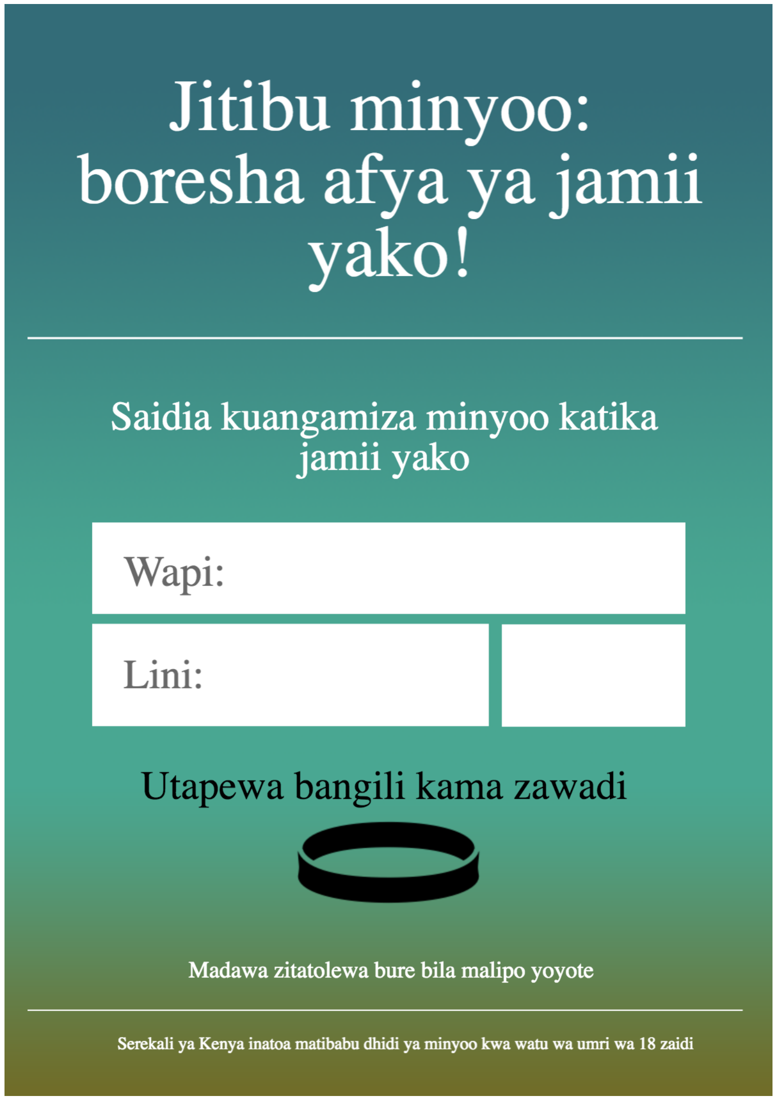
Figure 2.39: Ink Group Flyer

Flyers were distributed by Community Health Volunteers to all households prior to the start of the deworming program. The message is identical to Control group and includes added message: "Show your support by putting ink thumb".

Figure 2.40: Calendar Group Flyer

Flyers were distributed by Community Health Volunteers to all households prior to the start of the deworming program. The message is identical to Control group and includes added message: “You will be given a calendar as a gift”.

Figure 2.41: Bracelet Group flyer



Flyers were distributed by Community Health Volunteers to all households prior to the start of the deworming program. The message is identical to Control group and includes added message: "You will be given a bracelet as a gift".

Chapter 3

Social Signaling and Childhood Immunization: A Field Experiment in Sierra Leone

3.1 Introduction

Childhood immunization is one of the most cost-effective ways of reducing child mortality (UNICEF 2018).[1] Over the past decade, remarkable progress has been made in increasing the availability and reliability of immunization services (UNICEF and WHO 2016). In 2008, almost 20 percent of children in Sierra Leone had not received their first vaccine by the age of one (DHS 2008). This number had dropped to five percent by 2013 (DHS 2013). Despite this improvement in initial vaccination rates, only 58 percent of children complete the first-year series of vaccinations, a pattern that is common across many low-income countries.[2] In this essay, I ask two questions: Can we increase timely and complete vaccination, by allowing parents to signal to others that they vaccinated their child? Beyond visibility, what are the mechanisms through which social signals affect decision-making in a dynamic, real-life setting?

To answer these questions, I design a field experiment based on Bénabou and Tirole's (2006; 2011) theory of social signaling, which states that individuals' utility depends on the expectations that others form about their type, based on the actions they take. In the

[1] The benefits of vaccines go beyond the direct health impacts: vaccines contribute to higher educational outcomes, reduced poverty, and lower household spending (Verguet et al. 2013; van der Putten et al. 2015). It is estimated that every 1 USD invested in immunization programs, results in at least 16 USD in net health and economic benefits (Ozawa et al. 2016).

[2] Global immunization coverage continues to stagnate. For example, in India (DHS 2017), Peru (DHS 2014), and Kenya (DHS 2015), while 91 percent, 91 percent, and 96 percent of children, respectively, begin vaccinations, only 54 percent, 62 percent, and 71 percent complete the full first-year series. Demand-side factors play an increasingly important role in accelerating progress (Strategic Advisory Group of Experts on Immunization 2017).

context of my study, there are strong social norms surrounding the importance of vaccination – 83 percent of communities believe that parents who fail to vaccinate their children are negligent. As vaccines are currently imperfectly observable, I create an opportunity for parents to publicly show that they correctly vaccinated their child by introducing a durable signal - in the form of differently colored bracelets - that children receive upon vaccination. I experimentally vary the information that the bracelets provide about the take-up of different vaccines, by randomizing 120 clinics into three treatment arms and one control group.[3] For each clinic, I randomly select two adjacent communities (0 to 2 miles away) and three far communities (2 to 5 miles away), to create a final sample of 578 communities. I exploit two important features of childhood immunization in my experimental design: (1) individuals have to take multiple actions, as children require five vaccinations before the age of one; (2) individuals make decisions over a long time horizon, from the first vaccination at birth to the last vaccination at 9 months of age (WHO 2018).

Using (1), I randomly vary access to three different bracelets with varying signaling content. In each of the first two treatment groups (hereafter Signal 4 treatment and Signal 5 treatment), children receive a yellow bracelet upon their first vaccine. In the Signal 4 treatment, the yellow bracelet is exchanged for a green bracelet once a child completes the fourth vaccine. In the Signal 5 treatment, the yellow bracelet is exchanged for a green bracelet once a child completes the fifth vaccine. The last treatment, the Uninformative Bracelet, conveys no information about a child's later vaccinations. Parents choose a yellow or green bracelet at the first vaccine and the child keeps the same color bracelet for all subsequent vaccinations. This design allows me to both test the extent to which signaling preferences vary with the perceived benefits of vaccines, and isolate the effect of these preferences from alternative mechanisms such as increased salience, consumption utility, or social learning about vaccines. Finally, the time variation between the various vaccinations allows me to examine the extent to which future signaling payoffs affect parents' decisions to vaccinate their child today.

I combine survey and administrative data for over 6,000 children to estimate the partial effect of social signaling preferences on vaccination decisions. In addition, I collected detailed survey data on individuals' preferences and first- and second-order beliefs about children's vaccine status to test the underlying mechanisms of the theory for a random subsample of 1,314 parents. The beliefs data reveal large information asymmetries: parents in the control group have accurate information about other children's vaccinations for only 47 percent of children in their community. Similarly, parents believe that only 46 percent of other parents in their community have knowledge about their own child's vaccinations. Both the Signal 4 and Signal 5 treatments led to a decrease in information asymmetries (15 and 18 percent, respectively). Parents use signals to learn about the number of vaccines that other children received, consistent with Bayesian learning, updating their beliefs conditional on the bracelet color observed. I find no evidence of learning effects for the Uninformative Bracelet

[3] There is a total of 1,221 public clinics in Sierra Leone. The experiment was implemented at a large scale, covering ten percent of clinics.

treatment, in spite of similar rates of bracelet retention and visibility. This indicates that parents were able to correctly understand the different bracelet treatments.

The signaling treatments led to a significant increase in the share of children that received the fourth and fifth vaccine, increasing timely shares from 73 to 80 percent, and from 54 to 62 percent, respectively, compared to the control group. The effect is masked by substantial heterogeneity: Signal 4 led to small and insignificant increases of 2.8 percentage points for vaccine four, and 3.8 percentage points for vaccine five, in the share of children vaccinated. Signal 5 led to significant and large increases of 10.6 percentage points for vaccine four, and 13.7 percentage points for vaccine five. Effects remain large and significant (8.1 and 8.2 percentage points) when comparing Signal 5 to the Uninformative Bracelet, providing further evidence for social signaling preferences. Moreover, treatment effects persist for children born 12 months after the launch of the experiment. This finding raises the question of why Signal 5 worked, while Signal 4 did not, if both signals were equally potent in terms of increasing the visibility of vaccinations. Survey data shows that individuals assigned a higher importance to vaccine five than vaccine four, considering the fourth vaccine as the least important among the five. This result suggests that for signals to be effective, they need to be both informative about others' actions and linked to actions that are sufficiently valued. Reassuringly, I find no significant differences in individuals' preferences for different vaccines across treatment and control groups, ruling out that the observed treatment effects are purely due to normative influence of signals or social learning.

In addition to the treatment effects at vaccines five and four, Signal 5 also led to significant increases in the share of children that were vaccinated for vaccines three (7.1 percentage points) and two (4.3 percentage points). Combining the reduced-form estimates for all five vaccinations, Signal 5 significantly increased the average total number of vaccines completed from 4.0 to 4.4 compared to the pure control group. Importantly, parents were more likely to vaccinate their children for earlier vaccines, responding to a signaling benefit half a year in advance, without necessarily making it all the way to vaccine five. This pattern of treatment effects is consistent with theoretical predictions from a signaling model where individuals make decisions dynamically under uncertainty. More generally, these findings imply that individuals aim to complete later vaccines, but may drop out early due to unforeseen cost or preference shocks.

I structurally estimate a dynamic discrete-choice model that takes into account these features and uses distance to the clinic as a numeraire to quantify the value of social signaling. On average, parents' valuation of social signaling is equivalent to 7 to 10 miles walking distance to clinics. Taken together, these findings are of substantive policy importance: a signal that allows parents to broadcast an action they value for their child's health increased timely and complete vaccination to levels necessary for herd immunity, at a cost of 1 USD per child, far less than estimates from existing interventions.[4]

[4]Vaccine four includes, among other diseases, diphtheria, for which reaching herd immunity requires 83-85 percent of children to be vaccinated, and pertussis, for which reaching herd immunity requires 92-94 percent of children to be vaccinated (Anderson and May 2013). Signal 5 reaches the former when assessing

This study makes four contributions. First, to my knowledge, this is the first field experiment designed to test for social signaling in a dynamic setting. Existing studies have shown that individuals are willing to incur considerable costs when faced with a decision to take an immediate action that allows them to signal their type to others (Bursztyn and Jensen 2017). My findings show that signals can motivate individuals to take an action more than six months in advance, even when there is substantial uncertainty about whether signaling benefits can be realized. Importantly, observed behavior changes are very likely due to social signaling preferences, because I experimentally only vary the margin at which individuals can signal, which allows me to control for leading alternative mechanisms. This is also one of the first experimental studies to examine the effect of a durable signal that allows individuals to continuously signal their type to others (with the exception of Bursztyn et al. (2018)).

Second, this study contributes to a nascent literature of field experiments examining the mechanisms underlying social image concerns (Bursztyn et al. 2018, 2017; Bursztyn and Jensen 2017; Chandrasekhar et al. 2018). In contrast to many existing studies (Ashraf et al. 2014; DellaVigna et al. 2016; Perez-Truglia and Cruces 2017), my experimental design moves beyond manipulating the visibility of actions, by introducing multiple signals that are linked to different actions. By drawing an important distinction between the role of signals in providing information about others' actions and the opportunity they provide to signal one's type, this essay shows that the impact of signals varies significantly with the social desirability of actions. This result illustrates the limitations of social signaling as a mechanism to increase public goods, when individuals assign low private valuation to an action that has large externality benefits.

Third, this essay provides the first evidence on social signaling in health, and therefore contributes to a large literature on incentives to increase the use of health services and public goods in low-income settings (Thornton 2008; Banerjee et al. 2010; Ashraf et al. 2014; Sato and Takasaki 2017; Karing and Naguib 2018). Recent studies have found large effects of cash and consumption incentives. For example, Banerjee et. al (2010) find that offering 1 kg of raw lentils for each vaccination visit and a metal plate upon completion of the full series increases vaccination rates in India from 18 to 39 percent. In contrast, my paper looks at immunization in a context where initial take-up is close to universal and completion rates are much higher than in India, identifying social signals as a potential low-cost way to address the "last-mile problem" of reaching immunization thresholds.

Fourth, the results of this paper have the potential to inform policy strategies for increasing the demand for timely vaccination. Current immunization programs rely heavily on health campaigns and outreach activities to achieve target immunization levels. This

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the share of children vaccinated timely at six months for vaccine four (85 percent), and the latter when assessing the share of children having completed vaccine four by one year of age (93 percent). Gibson et al. (2017) increase full immunization by 12 months age from 82 percent to 90 percent by sending SMS reminders for vaccine 2, 3, 4 and 5 and providing a USD 2 incentive for each timely vaccination (total incentive cost of 8 USD). Banerjee et al. (2010) find that offering 1 kg of raw lentils for each vaccine and a metal plate upon completion of the full series increases vaccination rates in India from 18 to 39 percent (total incentive cost of 6.64 USD).

paper shows that social signals can increase parents' willingness to travel further to receive vaccinations. This provides relevant information to governments who face trade offs between keeping health workers at central clinics and mobilizing them to more remote areas. Further, this paper provides one of the first estimates of the value of social signaling in a low-income country. While most social signaling studies have been implemented in high-income countries, this study demonstrates the feasibility of implementing a more subtle behavioral intervention through government institutions in a low-resource setting.

The remainder of this chapter is organized as follows. Section 2 provides an overview of the empirical setting, including the application of social signaling to childhood immunization in general and the context of Sierra Leone in particular. Section 3 describes the experimental design, discusses the implementation and randomization checks. Section 4 presents the experimental results, providing a separate discussing of mechanisms and main outcomes. In Section 5, I provide a structural estimate of the value of social signaling. Section 6 concludes.

3.2 Childhood Immunization and Sierra Leone

This section provides a brief description of the routine immunization schedule, the health benefits of immunization, and the setting of childhood immunization in Sierra Leone. The information is important for the experimental design and an understanding of individuals' binding constraints to timely and complete vaccination.

Childhood Immunization

A child under the age of one needs to receive five routine vaccinations: the first vaccine, BCG, at birth or shortly thereafter, the second, third, and fourth vaccines, diphtheria, tetanus, and pertussis (DTP) 1, DTP 2, and DTP 3, at 1.5, 2.5, and 3.5 months of age, respectively, and the fifth vaccine, Measles, at 9 months of age (WHO 2018).[5] At the same time that DTP 1, 2, and 3 are administered, a child also receives one dose of the Polio, Rotavirus, and PCV vaccine.[6] While the first and last vaccine can be administered together with other vaccines, DTP 1, 2, and 3 need to be given one month apart.[7] According to WHO guidelines, the DTP series should be completed by six months of age (WHO 2018). Complete and timely vaccination provides private benefits by protecting infants from potentially life-threatening diseases, as the immunity from their mother wanes off, and social benefits by increasing overall immunization rates to herd immunity levels.[8] Private and social benefits may not

[5]BCG protects against tuberculosis. DTP is a 3-dose series offering protection against diphtheria, tetanus and whooping cough.

[6]Pneumococcal conjugate vaccine protects against diseases caused by the bacterium Streptococcus pneumoniae.

[7]For example, a child can receive BCG and DTP1, or DTP3 and Measles together in one vaccination visit.

[8]Infants and young children are at the highest risk to fall ill and die from these diseases: one out of five children who get diphtheria at age younger than 5 years old dies (WHO 2017).

perfectly align: DTP doses 1 and 2 are, for most children, sufficient to obtain protection against the disease; the third dose is necessary in order for 94 to 100 percent of children to have protective antibody levels and hence to reach herd immunity.[9] The latter is particularly important as pertussis predominantly affects children younger than six months, who therefore may be too young to be protected by immunizations.

Low-Income Country Context of Sierra Leone

Sierra Leone has one of the highest infant and under-five mortality rates, with 92 and 156 deaths per 1,000 live births, respectively. One in every 11 Sierra Leonean child dies before reaching age one and one in every 7 does not survive to her fifth birthday (DHS 2013). Rotavirus is the most common cause of severe and fatal diarrhea in young children worldwide; in Sierra Leone, it is estimated that one third of all under-five diarrheal disease hospitalizations are caused by rotavirus (PATH 2017).

The country is one of the poorest in the world, ranking 181 out of 188 in the Human Development Index (UNDP 2016). Women are the primary caregivers of children, taking them for vaccinations over 99.99 percent of the time. 47 percent of mothers in my endline sample have no education, 30 percent have any primary education, and only 22 percent have any secondary education. 74 percent of mothers are engaged in farm work, and fewer than 12 percent possess a mobile phone. Birth rates are high, with mothers having, on average, three children by the age of 26 years.

In Sierra Leone, vaccines are free of charge and readily available.[10] A possible concern is that, even if vaccines are free of charge, clinics may run out of them. Table 3.2 provides relevant information: at baseline, fewer than 14 percent of clinics in my study sample reported having a stock-out of one or more vaccines, and during the study period, only 8 percent of clinics experienced any stock-outs of on immunization days.[11] Immunization services are offered on a fixed schedule, either on a weekly (65 percent of sample) or monthly (35 percent of sample) basis, and clinics have, on average, two staff trained in child health. At the same time that vaccinations are given, children's weight and height are recorded, and their overall health checked. Vaccinations, both in Sierra Leone and many other low-income countries, are therefore the main point of contact for monitoring newborns' health and detecting problems such as malnutrition. The functionality of the supply side is reflected in communities' perceptions, see Table 3.1: 83 percent of communities name, as the most common reason, negligence of parents, for delayed or missed vaccination. Distance to clinics

[9]The antibody level increases after the second dose of diphtheria toxoid and it is much higher after the third dose; while most children have a base level of protection from the first two doses of DTP, the third doses is necessary for 94-100 percent of children to have protective antibody levels > 0.01 IU/mL and reach herd immunity thresholds (WHO 2017).

[10]Healthcare for children under the age of five, pregnant women, and lactating mothers is free in Sierra Leone since the introduction of the Free Healthcare Initiative in 2010.

[11]The stock-outs were mainly for BCG and Measles vaccines. Less than 3 percent of clinics reported stock-outs for the DTP vaccine.

and user fees are ranked as secondary factors, mentioned by 34 percent and 15 percent of communities respectively. Importantly, child vaccination is a well-known technology: 94 percent of communities at baseline know that children need five vaccinations, and are aware of the health benefits of vaccinations.[12]

3.3 Experimental Design

The first part of this section introduces the signaling mechanism used in this study and the different experimental treatments used to test the theoretical predictions. Next, I describe the selection and randomization of clinics and communities, followed by a discussion of the identification of signaling preferences. I then provide information about the timeline and the data sources collected at different points of the experiment. Finally, I discuss balance checks and compliance with the implementation.

Experimental Treatments: Bracelets as Signals

To create visibility in actions, I experimentally introduce a signal - in the form of colored bracelets that children receive upon vaccination at public clinics. The bracelets create an opportunity for parents to publicly signal that they correctly vaccinated their child. Specifically, I introduce experimental variation in two ways to test the theoretical predictions of the model: (1) I increase the visibility of vaccination decisions; (2) I exploit the fact that children need to receive multiple vaccinations and place signals at different vaccination. Figure 3.1 displays the four experimental groups and the specific bracelet treatments that health workers implement at each of the five vaccinations:

Control Group: No bracelets are given to children at vaccinations.

Signal at 4: Children receive a yellow "1st visit" bracelet when coming for the first vaccine. Children keep the same bracelet for vaccines two and three. When a child comes in a timely way (before reaching six months age) for vaccine four, health workers exchange the yellow bracelet for a green "4th visit" bracelet. If a child comes late for vaccine four, the bracelet is exchanged for an identical yellow "1st visit" bracelet. At vaccine five, the bracelet is exchanged for a new but identical green "4th visit" bracelet (or yellow "1st visit" bracelet if the child was late for vaccine four).

Signal at 5: Children receive a yellow "1st visit" bracelet when coming for the first vaccine. Children keep the same bracelet for vaccines two and three, and the bracelet

[12]Individual surveys corroborate this finding: 96 percent of mothers attending vaccinations, who were randomly sampled for short surveys during their clinic visit, were aware that children under the age of one require five vaccinations.

is exchanged for an identical yellow "1st visit" at vaccine four. If a child comes in a timely way (by 11 months age) for vaccines five, health workers exchange the yellow bracelet for a green "5th visit" bracelet. If a child comes late for vaccine five, the bracelet is exchanged for an identical yellow "1st visit" bracelet.

Uninformative Bracelet: Parents can choose a green or yellow "1st visit" bracelet at vaccine one. Children keep the same bracelet for vaccines two and three. At vaccines four and five the bracelet is exchanged for a new identical "1st visit" bracelet of the originally chosen color.

In all three signaling treatments actions are grouped into two signals. In Signal at 4, others can only tell whether a child was vaccinated for four or more vaccines, or whether a child received fewer than four vaccines. In Signal at 5, the yellow and green bracelets allows others to observe if a child received five vaccines, or fewer. The Uninformative Bracelet allows parents to signal that their child started vaccination but provides no information about the completion of later vaccinations.

Figure 3.2 shows the actual bracelets that were given out at clinics. All bracelets were made out of silicone and were size-adjustable so that they could comfortably fit the wrist of a child between the ages of zero and twelve months. The latter was key for the experimental design i) as it made the bracelet a *durable signal* that could be observed by others and allow for comparisons beyond the time of the vaccination, and ii) so that the size of the bracelet would not be informative about the number of vaccinations a child has completed.[13] Over the course of the experiment, a total of 36,000 bracelets were handed out by health workers.

Identifying Effects

The combined effect of increased salience (e.g. reminder, nudge effects), consumption utility, and social signaling preferences is captured by the comparison of the share of children vaccinated at vaccines four and five in the Control Group to Signals at 4 and 5.[14]

The comparison of Signal at 4 and Signal at 5 to the Uninformative Bracelet at vaccines four and five allows me to isolate the effect of social signaling preferences on vaccination decisions. I implement bracelet hand outs and exchanges in all three signaling treatments at the same vaccines in order to hold constant any additional consumption utility of bracelets. By distributing bracelets and using the colors green and yellow in all three signaling treatments, I further hold constant salience and reminder effects that are due to (1) the general visibility of vaccinations through bracelets, and (2) the introduction of new colors over time.

[13]As a child's wrist grows, even in the absence of a change in bracelet color, a too small bracelet that no longer fits, could be informative about whether a child is up-to-date with its vaccinations.

[14]I omit the comparison of the Uninformative Bracelet and the Control at vaccine one, since take-up is almost universal for the first vaccine.

In other words, the only difference remaining is what actions can be signaled, that is, the completion of a specific vaccine.

A larger increase in the share of children who are timely vaccinated for vaccine four in Signal at 5 compared to Signal at 4 implies a higher social signaling value in treatment Signal at 5 compared to treatment Signal at 4: $\lambda\omega_4\triangle(\hat{v}_4) < \lambda\omega_5\triangle(\hat{v}_5)$. This could be due to two reasons: (i) differences in the social desirability parameter for the timely completion of vaccine four and five, that is, $\omega_4 < \omega_5$, how much does society value the completion of these vaccines, or (ii) differences in the type expectations that others form upon observing the timely completion of vaccine four versus vaccine five, such that Signal at 4 is less informative about different types, that is, $E_{-i}(v|a_i \geq 4) - E_{-i}(v|a_i < 4) < E_{-i}(v|a_i = 5) - E_{-i}(v|a_i < 5)$.

An increase in the share of children who complete earlier vaccines (vaccines 1, 2, or 3 for Signal at 4; vaccines 1, 2, 3, or 4 for Signal at 5), without transition probabilities from vaccines three to four and four to five respectively equaling one, demonstrates that individuals make decisions dynamically and under uncertainty. Parents who vaccinate their children for earlier vaccines due to an increase in the future value of vaccination but do not make it to vaccine four (for Signal at 4) or vaccine five (for Signal at 5), must have *targeted* to complete four or five vaccines but stopped earlier due to unforeseen cost or preference shocks.

Finally, a comparison of the share of children vaccinated at vaccine five in Signal at 4 to the Uninformative Bracelet quantifies the extent to which observed treatment effects are due to some form of social learning or normative influence. If individuals have incorrect priors over the share of parents that vaccinate their children, and are uncertain about the benefits of vaccination, observing signals about timely take-up of vaccine four or five, could lead them to update their beliefs about take-up levels and the usefulness of vaccinations. Similarly, health workers giving a "reward" to parents for vaccination, could act as a signal about the importance of vaccinations for children's health. By design, parents in the Signal at 4 treatment have no signaling incentive to complete vaccine five, as green bracelets do not allow for a distinction between parents who took their children for four vaccines, versus those who went for five. An increase in the share of children vaccinated at vaccine five could therefore be due to two reasons: (i) if uncertainty plays an important role, some parents who now complete vaccine four in Signal at 4 treatment receive a positive cost or preference shock and also take vaccine five; (ii) parents learn from signals about the benefits of vaccinations, leading them to also increase their valuation of vaccine five. To distinguish (i), which still falls within the predictions of the signaling model, from (ii) which is an alternative behavioral mechanism, I can compute the transition probability between vaccine four and five. If I observe an increase in the transition probability in Signal at 4 treatment relative to the Uninformative Bracelet, it strongly suggests that learning is a relevant alternative mechanism.

Lastly, to address concerns regarding learning about the importance of vaccine five in Signal at 5 compared to the Uninformative Bracelet or Control Group, I elicit individuals' preferences for the different vaccinations and test for differences across arms.

Clinic Randomization and Community Selection

Treatment was randomized at the clinic level so that every child living in the catchment area[15] of a clinic was eligible for the same bracelet treatment.[16] In total, I selected 120 clinics across four of Sierra Leone's 14 districts to be part of the study. To randomly draw 230 clinics from the pool of 335 public clinics across the four districts, I used an acceptance-rejection method whereby I randomly picked clinics, checked their acceptability based on their overlap with already selected clinics, and if accepted, added them to the selected sample. This process was repeated until it had selected the requisite number of clinics. If no acceptable clinic remained before completion, the whole process was restarted. Each clinic had a 5 mile radius as catchment circle. A clinic was considered acceptable if its catchment circle did not leave any of the already selected clinics' non-overlapping catchment circle smaller than 35 percent of its area. Clinics were then randomly assigned, stratified over the four districts and two implementation waves[17], to the three different treatments and the Control Group. Figure 3.3 shows the geographic span of the experiment across the four districts in Sierra Leone and the final selection of clinics. During the launch of the study in each clinic, surveyors selected - using in-field randomization - two communities at close distance (0 to 2 miles) and three communities at far distance (2 to 5 miles) from the clinic, from the pre-specified non-overlapping catchment are of each clinic. Figure 3.17, the upper map, shows the non-overlapping catchment areas and the lower map provides an example map for one of these clinic areas, that surveyors were given for the in-field selection. In total, the experiment included 578 communities. Table 3.17 provides a break down of the number of communities by district, as well as the mean travel distances between clinics and communities. On average individuals walk 2 miles to clinics.[18]

Information Treatment

While such a high level of randomization significantly increased the logistical demands of the experiment, it was key to reducing the risk of incorrect implementation by health workers, and to creating a common understanding of the bracelets among individuals.

At the launch of the experiment, surveyors visited each of the selected 578 communities to hold an information meeting with the community (see Table 3.1 Panel B for balance tests). The objective was to highlight the health and economic benefits of timely and complete

[15] A catchment area of a clinic is defined by the communities surrounding it that the clinic serves.

[16] Children that were born before the launch of the experiment and had already started vaccinations, would receive their first bracelet when coming for their next vaccination (e.g. "4th visit" green if came for vaccine five timely in Signal at 5 treatment).

[17] The experiment was phased in in two waves: wave one from mid-June to mid-July where 44 clinics were launched (11 in each of the intervention arms), and wave two from end of September to end of November where the remaining 76 clinics were launched.

[18] 86 percent of parents surveyed during clinic visits report to travel to clinics by foot. 13 percent travel by motorbike and 1 percent by car. The average one-way travel time to a clinic is 49 minutes, the median time 35 minutes.

vaccinations, to discuss existing barriers, and in signaling treatments, to inform a wide range of community members about the bracelets and create common knowledge about their meaning. The average meeting attendance was 43 people, with almost all meetings attended by a health representative (95 percent, e.g. a community health worker or traditional birth assistant) and a community leader (98 percent e.g. chief). A second information meeting was held with each community two to four months later, to again go over the importance of timely and complete vaccinations and discuss the meaning of the bracelets, now that clinics were handing them out.

Experiment Timeline and Data

Below, I detail the timeline of the experiment implementation and the main data collection activities.

Jun '16 - Nov '16	Experiment launch: baseline clinic and community survey; training of 348 government health workers across 120 clinics in messaging to parents and implementation of bracelets; information meetings about the benefits of vaccination and meaning of bracelets in 578 communities including close to 25,000 adults.
Jul '16 - Apr '18	Monitoring of implementation: health workers hand out bracelets as part of regular monthly or weekly routine vaccination services at clinics; surveyors regularly visit clinics (every 1-2 months) to verify the correct hand out and exchanges of bracelets, messages given to parents, and recording of vaccine visits; training of new clinic staff in implementation; digitization of administrative records for ~ 37,892 children; follow-up information visits in communities.
Sep '17 - Jan '18	Listing survey: comprehensive listing of 14,061 children in selected communities.
Feb '18 - Apr '18	Endline data collection: survey of 1,314 parents and 120 nurses in charge of vaccination services; choice experiment with 123 parents in control group.

I will use several data sources that I collected at different points of the experiment for the analysis:[19]

(1) *Baseline data:*

(i) Clinic survey: survey with nurses in charge of clinic, recording staff numbers, regularity of vaccination services (monthly versus weekly), supply side conditions (stock outs, cold

[19]The analysis includes 119 clinics, excluding one clinic in the urban Western Area where the implementation and data collection were seriously impeded by turn-over of clinic staff, relocation of selected communities and compliance in monitoring and data collection by the surveyor.

chain), and list of catchment communities and their characteristics (distance to clinic, size, proximity to other clinics) to determine eligibility for selection.

(ii) Community survey: survey conducted with participants of information meetings, knowledge about vaccinations, and perceived barriers to complete and timely vaccination; further captured data on attendance and implementation of meetings.

(2) *Administrative data:* Throughout the experiment, surveyors digitized vaccination records of children that visited our study clinics including names of children and parents, date of birth, vaccine received, date of vaccination and whether the child received a bracelet, the color of the bracelet, and whether the child had lost the bracelet.

(3) *Listing survey data:* surveyors conducted a census of all children (age 0 to 18 months) residing in the 578 selected communities, recording the status of children (residing in community, traveling, permanently moved, deceased), names of children and parents, date of birth, list of vaccines received (from vaccine card and memory), date of vaccination, bracelet ownership and observability.

(4) *Endline data:* survey of 1,314 mothers[20] across 381 communities that were randomly sampled, stratified by distance (2 far and 1 close community for each clinic) from the list of 578 communities, eliciting first- and second-order beliefs about other children's vaccinations, bracelet and color, preferences and knowledge about vaccinations.[21]

Balance Checks and Compliance with Implementation Protocol

Tables 3.1 and 3.2 report the experimental balance checks. I report results separately for clinic, community and individual level characteristics, as well as implementation of the experiment launch and main listing survey. 8 of 138 coefficients are statistically significant at the 10 percent level across all comparisons. Attrition is low and not affected by treatment: 11 percent of children had moved or were permanently traveling, and 2.6 percent of children were deceased at the time of the listing survey. There are no statistically significant differences in the timing of the clinic launches or the survey implementation across treatments. I further find no statistically significant difference in pre-trends.

To verify whether health workers correctly handed out and exchanged bracelets, surveyors asked each parent to report the bracelet color that was given to the child during vaccination, and the number of vaccines the child had received by that time. Figure 3.4 shows the fraction of children in each group that received a yellow, green, or no bracelet, conditional on the number of vaccines received. Almost every child had a bracelet (94%), with no

[20] I only surveyed mothers as they are the ones who take children for vaccines. I did not have sufficient funding to also survey other household members e.g. fathers. If mothers did not understand the signals, and there was no impact on beliefs, I would not expect to find effects for other household members.

[21] Choice experiment analysis to be added in future: including 123 parents from a random sample of 12 control clinics and 42 communities, eliciting preferences for bracelet color and love of variety through a two-stage choice experiment where mothers were first given a bracelet (random color assignment) as a gift for the participation in the endline survey and two weeks later the opportunity to exchange the bracelet for a new bracelet of the same or a different color for a cost, mimicking the cost of travel to the clinic.

significant differences across arms. In the Uninformative Bracelet treatment, there is no overall significant relationship between the number of vaccines a child has received and the reported bracelet color.[22] We can see that the majority of individuals prefer the color yellow (63%) over the color green (37%).

For Signal 4 and Signal 5, there is a clear relationship between child's bracelet color and the number of vaccination received: there is a large increase (up to 62% for Signal 4 and 70% for Signal 5) in the share of children with a green bracelet at vaccine four and five, respectively. Children who received vaccine four and/or five but had a yellow bracelet either came late for the vaccine or received the incorrect bracelet from health workers (see Figure 3.18 in Appendix). Therefore, a yellow bracelet on an older child[23] provides a noisy signal about the number of vaccines received. Conversely, almost no child (2.23%) is reported to have received a green bracelet before the signaling threshold. A green bracelet is therefore a highly informative signal about a child having received vaccine four or five in the Signal at 4 and Signal at 5 treatments.

3.4 Do Signals Affect Vaccination Decisions?

I now present the main results of this paper, separately discussing the mechanisms underlying the theory. I will first test the extent to which individuals use signals to learn about others' actions and make subsequent type inferences, and second test the extent to which individuals' value the opportunity to signal that they correctly vaccinated their child for vaccine four and five.

Informativeness of Signals

In this experiment, the bracelet signals are aimed to create an opportunity for parents to show that they correctly vaccinated their child. For this to work, individuals must (1) learn about others' actions from signals, and (2) form expectations about others' types conditional on the signals observed. In this subsection, I will empirically verify these mechanisms and the assumptions associated with them.

$$\textit{Signals} \quad \underbrace{\rightarrow}_{(1)} \quad \textit{Beliefs about actions} \quad \underbrace{\rightarrow}_{(2)} \quad \textit{Inferences about types}$$

[22]There is only a small significant increase in the share of children with a green bracelet for vaccine five.
[23]Child that is 6+ months in the Signal at 4 treatment or 11+ months in the Signal at 5 treatment.

Method

I first elicit individuals' first- and second-order beliefs about vaccine decisions and the perceptions of others.[24] To measure **beliefs**, I gave each mother at endline a random sample of five other children in her community and asked separately for each child the following questions:[25]

1. "What is your relation to the child's mother?"

First-order beliefs

2. "How many vaccinations do you think this child has received?"[26]
3. "Does the child have a bracelet?; If so, what color bracelet does the child have?"

Second-order beliefs

4. "Do you think the mother knows that you have taken your child for [x] vaccines?"
5. "Do you think that the woman knows you have a [color] bracelet?"

To measure **perceptions**, each mother was asked about her perceptions of others' concerns about her own child's vaccinations:

1. "Is there anyone in your community or your house who is concerned about your child's vaccination?; If so, who?"
2. "How would community members view you?" and "What actions would they take if you?"
    a. "...took your child for all vaccinations?"
    b. "...missed taking your child for vaccinations?"

The sample used for the beliefs analysis is mothers of all children who were eligible to have received a specific vaccine. The sample size therefore differs across different outcomes.[27] For the analysis of perceptions, the answers of all mothers are included, as these questions are not specific to a particular vaccine, and therefore age category. All regressions include strata fixed effects, and standard errors are cluster bootstrapped at the clinic level. Beliefs regressions include controls for own or other child age and the relationship between endline respondents and other mothers. To assess the accuracy of first-order beliefs about other

[24] These questions were extensively piloted over the course of the experiment, to be easily understandable for respondents - regardless of their level of education - and to mitigate social desirability biases.

[25] If a mother did not recognize the name of another child/mother, she was given the name of a different child/mother until she identified a total of 5 children. On average, respondents were asked about 6.5 other children in their community and recognized 4.6 children. 74.39 percent of respondents were able to recognize 5 children. For those who recognized fewer, there were either fewer than 5 children in the community, or respondents were unable to recognize 5 other children. There are no significant differences in the average number of children recognized or number of children asked about across treatment arms.

[26] This question was incentivized: mothers received a small reward in form of a maggi seasoning cube (value of US 3 Cents) for each child they correctly guessed the number of vaccines.

[27] For example, the sample of children used in the analysis of beliefs about completion of vaccine four will be larger than that used for beliefs about vaccine five, since a greater number of children will have reached 3.5 months age (the time when vaccine four can be administered) by the time the endline survey was conducted, and fewer that were born since the start of the experiment would be 9 months age (the time when vaccine five is due) or older at endline.

children's vaccinations, I linked respondents' answers with administrative clinic records of children.[28,29]

Do Individuals Learn from Signals about Actions?

Assumptions

For individuals to draw new information from signals, two assumptions have to be met: (i) individuals have imperfect information about other parents' vaccination decisions, (ii) signals are publicly visible. Table 3.4 quantifies the information asymmetries, revealing they are large. Columns 1 and 2 indicate that mothers in the Control Group have accurate knowledge about the number of vaccinations a child received for only 45.1 to 46.5 percent of children in their community age 3.5 months and older.[30] Similarly, Columns 3 and 4 show that mothers believe that only 47.2 to 45.6 percent of other mothers in their community have knowledge about their own child's vaccination, if their own child is 3.5 months and older. There is no statistically significant difference in these information asymmetries across mother-pairs with distant and close relationships.[31] These findings suggest there is scope for signals to provide information about others' vaccination decisions.

Second, Table 3.13 shows that bracelets were highly visible in all three signaling treatments. Column 1 presents respondents' knowledge about whether other children in their community have a bracelet, while Column 2 presents respondents' beliefs about other mothers' knowledge of their own child's bracelet color. For 90 percent of children, mothers report knowing whether they have a bracelet.[32] For 95 percent of these children, respondents also report knowing the child's bracelet color.[33] Importantly, for the majority of children (87 percent), respondents state that they know the baby has a particular color of bracelet because they saw the child with that bracelet color. Only for 10 percent of children do respondents state that they know from the number of vaccines the child has or because every child receives a bracelet (reverse inference).

[28] The challenge with vaccinations is: as children are all of different ages, they all have different due dates for the specific vaccines. In order to accurately measure the correctness of beliefs, vaccination data has to be collected at the (almost) same time as beliefs are elicited. Using earlier collected vaccine data, such as the listing data, would mismeasure information asymmetries. Digitizing administrative clinic records, also allowed me to verify beliefs for a larger sample of other children - instead of just for respondent-other mother pairs who were surveyed at endline.

[29] Only 2 percent of respondent-other mother belief answers could not be verified, since surveyors were unable to find administrative records for 49 children (out of the total 2353 other children). There is no significant difference in the share of children not found across intervention arms.

[30] The age range at which they are eligible for Vaccine four and five.

[31] 39 percent of other mothers were identified as regular community members, while 35 percent as relatives (see Table 3.15).

[32] Only four percent of children are believed to have no bracelet (with equal probability across arms).

[33] There is a significant difference in respondents' reported knowledge about other children's bracelet color between treatment groups. 98 percent of Signal at 4 and Signal at 5 treatment groups report knowing other children's colors. This number drops to 90 percent in the Uninformative Bracelet group - a significant difference of eight percentage points.

Similarly, respondent mothers believe that 76.8 percent of other mothers know about their own child's bracelet color, with no significant differences across signaling treatments. The perceived knowledge of others about the color is key for any potential differential impact of Signal at 4 and 5, compared to the Uninformative Bracelet. The visibility of bracelets for all signaling treatments is further verified by the fact that retention of bracelets was similar across groups (see Appendix, Table 3.14).

Beliefs Updating

Figure 3.5 shows mothers' beliefs about the number of vaccinations other children in their community received, conditional on bracelet color, testing the underlying mechanism that signals convey information about others' actions:[34]

$$Pr_{-i}(a_i \geq r|\text{Green}_i) - Pr_{-i}(a_i \geq r|\text{Yellow}_i) > 0.$$

Using respondents' joint beliefs about the color of bracelet a child has and the number of vaccines the child has completed, I compute the conditional probabilities of a child having completed at least three, four, or five vaccines, conditional on having a yellow or green bracelet. The almost perfectly overlapping green and yellow bars for the Uninformative Treatment group [35] in Figure 3.5 demonstrate that there is no significant difference in the probabilities that mothers assign to children having completed vaccines three, four, and five when comparing children with yellow bracelets to those with green bracelets. In contrast, for Signal at 4 and Signal at 5, I observe large and significant differences in the probabilities assigned: mothers in both treatments believe that 57 and 68 percent of children (respectively) with a yellow bracelet completed vaccine four, compared to 90 and 98 percent of children with a green bracelet - an increase by 34 and 30 percentage points respectively. The same applies to vaccine five: mothers in both treatments believe that 36 and 37 percent of children (respectively) with a yellow bracelet completed vaccine five, compared to 62 and 78 percent of children with a green bracelet - an increase by 28 and 41 percentage points respectively. While different in magnitude, there is no statistically significant difference between individuals' inferences in the Signal at 4 and Signal at 5 treatments. Both signals were equally potent in providing information about other parents' vaccinations decisions.

Figure 3.19 reveals that individuals' beliefs are consistent with Bayesian learning. Mothers in Signal at 4 and Signal at 5 correctly recognize that some children with a yellow bracelet came for vaccines four and five (either because of untimeliness or implementation errors). The comparison further reveals that mothers do not fully update their beliefs in response to

[34]The probability that others assign to a mother's own child having completed vaccine a conditional on her child's bracelet color, is equivalent to the probability that the mother assigns to other children having completed vaccine a conditional on their bracelet color.

[35]The difference between the conditional probabilities for vaccine five for children with green versus yellow bracelets, in the Uninformative Bracelet treatment, is not statistically significant.

bracelet signals: the probabilities assigned to a child having attended vaccine four in Signal at 4, and vaccine four and five in Signal at 5 should have been one.

To what extent did signals reduce information asymmetries about actions? Columns 1 and 2 in Table 3.4 show that mothers have more accurate knowledge about other parents' vaccination decisions in their community: mothers are between 17 and 23 percent more likely to correctly infer the number of vaccines that children have received in Signal at 4 and 5 treatments compared to the Control Group - for both younger (eligible for vaccine four) and older children (eligible for vaccine five). This result is corroborated by the treatment effects on second-order beliefs displayed in Columns 3 and 4: mothers are significantly more likely to believe that other mothers have greater knowledge about their own child's vaccinations, with significant increases between 10 to 17 percentage points over the control means of 47.2 and 45.6 percent, for children eligible for vaccine four and five respectively. Treatment responses are larger, up to twice in magnitude, for Signal at 4 and 5 compared to the Uninformative Bracelet though I cannot reject that the coefficients for the three treatment groups are equal. I find no significant difference in changes in information asymmetries across mothers with both distant and close social connections.

Do Individuals Learn from Signals about Types?

Figure 3.20 shows that mothers believe that community members[36] form different opinions about them - in terms of their intrinsic motivation - depending on the vaccinations that their child completed. 92 percent of mothers state that others would view them as "caring" if they took their child for all vaccinations, and "careless" if they missed any, verifying the underlying mechanism that higher actions are linked to higher types, that is:

$$E_{-i}(v|a_i \geq r) - E_{-i}(v|a_i < r) > 0.$$

On the contrary, few believed that others link their vaccine decision to their knowledge about benefits $B(a_i)$ (e.g. "know of importance", or "are ignorant") or cost-factors $C(a_i)$ (e.g. "are too busy with work", or "too poor to travel to the clinic"). These answers also shed light on the question of what individuals are trying to signal to others when making actions visible (Bursztyn and Jensen 2015). There are two immediate explanations in my context: (i) mothers want to signal that their child is healthy and does not pose a threat to other children in terms of spreading diseases ($\sim$ inference about child's health status); (ii) mothers want to show that they look after their child's health ($\sim$ inference about responsible parent). The first explanation does not seem to be a motive for signaling: the majority of mothers view vaccines as beneficial only to their own child's health and lack an understanding of the externalities of vaccination. Specifically, fewer than 20 percent believe that other, unvaccinated children

[36] Community members are one of four main reference groups mothers believe are concerned about their child's vaccinations. 61 and 62 percent of mothers respectively named their husband/father of the child and family members as individuals who are concerned, and named second, with 30 and 36 percent respectively, regular community members and community health workers/nurses.

can be harmful to their own child's health, or that their child could be harmful to others if not vaccinated.[37]

Taken together, the mechanism results show that mothers in the Signal at 4 and 5 treatments, as intended, used the color of bracelets to learn about other children's vaccinations, and make different inferences about parents' motivation to look after their child's health conditional on their vaccine decisions.[38]

Effect of Signals on Vaccine Decisions

The main outcome of the experiment is the share of children vaccinated in a timely manner for a given vaccine. The experimental design allows for a direct test of the effect of social signaling preferences on the outcome. Having established that bracelets as signals were informative about parents' actions and their types, this subsection tests to what extent parents value signaling that they look after their child's health. Specifically, the reduced form tests if the parameters λ and ω jointly are greater than zero.

Empirical Strategy

My preferred specification for the main outcome is:

$$Vaccine_i = \alpha + \beta T_{j(i)} + \delta X_i + \rho_{s(i)} + \varepsilon_i \tag{3.1}$$

in which Vaccine$_i$ denotes the binary outcome variable for a child i being vaccinated for a given vaccine $a \in \{1, 2, 3, 4, 5\}$ by the age of 3 months for vaccine one, 4 months for vaccine two, 5 months for vaccine three, 6 months for vaccine four, and 11.5 months for vaccine five; $T_{j(i)}$ are treatment indicators for Signal at 4, Signal at 5, and the Uninformative Bracelet assigned at the clinic level (j); X_i denotes the control variables of child age and an indicator for the administrative data; and $\rho_{s(i)}$ denotes the strata fixed effects. Standard errors are cluster bootstrapped at the clinic level.

The timeliness cut-offs were determined following WHO guidelines that state that the DTP series should be completed by six months of age (WHO 2018). I allow for an equal

[37]At endline 91 percent of mothers believe that vaccinations are helpful for their own child's health, stating that "[they] help my child to grow well and healthy" and "prevent my baby from paralysis [and] blindness". Only 15 and 19 percent of mothers respectively agree that other children can pose a risk to their child when not being vaccinated, or that their child could be harmful to others if she is not vaccinated, stating reasons such as: "Because if she is not immunized, she can transfer diseases like measles if she happens to contact it". When mothers are asked why they think their vaccination decisions cannot help others, common answers were: "'Because they do not have the same body, or same blood" or "because the vaccines in my child won't jump and help other children".

[38]Beyond the *opinions* that mothers believe others will form about them as parent, they also name specific *actions* that they believe others will take. 74 percent of mothers (see Figure in the Appendix) believe that others would scold them if they missed vaccinations, while 22 percent said they would be praised in the community and people would speak well about them.

2.5 months buffer window for each vaccine such that for vaccine one, which is due at birth or shortly thereafter, the timeliness cut-off is set at 3 months, for vaccine two which is due at 1.5 months, the timeliness cut-off is set at 4 months, etc. In the main specification, I code children that received a given vaccine before the timeliness cut-off as one and zero otherwise. In the later part of the analysis, I will also consider the effect of signals on complete vaccination by the age of one year, independent of the time a child received the vaccine.

I combine data collected during the listing survey with data from administrative clinic records to measure outcomes. The listing survey data provides the sample of all children that reside in the selected communities and were born since the launch of the experiment. I use the administrative data to extend the vaccine history for children that had not yet reached one year of age at the time of the listing survey.[39] Given the sequential timing of vaccines and the corresponding timeliness cut-offs of 3, 4, 5, 6 and 11.5 months, I observe more children for vaccine one and two than for vaccines four or five. I include all available data and the sample size therefore differs across the five different vaccine outcomes. In total, I observe 7,482 children for vaccine one, 7,052 for vaccine two, 6,095 for vaccine three, 5,909 for vaccine four and 2,350 children for vaccine five across 119 clinics and 578 communities.[40] For children age one year and above, I observe a total of 1,972.

Effect of Signals on Timely Completion of Vaccines 4 and 5

The discussion of the empirical results follows the theoretical predictions outlined in Chapter 1 Section 2.2.1 and the experimental identification outlined in Section 3.2.

I first examine the effect of signals on timely completion of vaccines:

$$\frac{\partial Pr(a_i(v) \geq r))}{\partial x} > 0$$

Figure 3.6 shows the combined effect of Signals at 4 and 5 on the share of children timely vaccinated for all five vaccines over the Control Group. Vaccination levels in the Control Group reveal a sharp drop-off between vaccines three and four (11.7 percentage points), and vaccines four and five (19.3 percentage points), illustrating the scope for parents to signal the timely completion of these vaccines. The signaling treatments led to a significant increase in the share of children that received vaccine four and five, increasing timely shares from 73 to 80 percent and from 54 to 62 percent, respectively. The effects indicate that the signaling treatment reduced drop-off by 56 and 44 percent, respectively.[41]

[39] As indicated in the timeline in subsection 3.5, the listing survey was implemented between September 2017 and January 2018, while the administrative data was collected between February and April 2018 and therefore provides further information about children's vaccinations.

[40] One clinic of the 120 selected, located in Western Area (WA) Rural district is excluded from the analysis due to serious complications in the implementation and data collection.

[41] Regression results for all comparisons can be found in Table 3.5.

The effect is masked by substantial heterogeneity.[42] Figures 3.7 and 3.8 show treatment responses for each signal separately: Signal at 4 led to a small and insignificant increase of 2.8 percentage points for vaccine four, and 3.8 percentage points for vaccine five. Signal at 5, on the other hand, led to a significant and large increase of 10.6 percentage points for vaccine four, and 13.7 percentage points for vaccine five. A comparison between the Uninformative Bracelet and the Control Group, in Figure 3.9, reveals that the effect of bracelets as a consumption incentive and reminder nudge was limited: I find small to moderate treatment effects of the Uninformative Bracelet of 2.5 and 5.5 percentage points for vaccine four and five respectively.[43] As a result, the effects of Signal at 5 for vaccines four and five remain large and significant (8.1 and 8.2 percentage points) when compared to the Uninformative Bracelet, providing compelling evidence for social signaling preferences. Bracelets as signals for completion of vaccine five increased timely completion of the DTP series to levels necessary to reach herd immunity for diphtheria.[44]

Social Desirability of Different Signals

I now examine the social desirability of different signals:

$$\frac{\partial^2 Pr(a_i(v) \geq r))}{\partial x \partial \omega} > 0$$

Health workers in both Signal at 4 and Signal at 5 implemented the same bracelet hand outs and exchanges,[45] with the only difference being the vaccine at which children receive a green bracelet.[46] Moreover, as shown in the previous subsection, bracelets were equally visible and informative about actions across both signaling treatments. Observed differences in treatment responses therefore must be linked to differences in the signaling *value* of each bracelet, either caused by (i) differences in the social desirability of actions, i.e. ω or (ii) differences in type expectations, i.e. $\triangle(\hat{v})$. The similarly large drop-off between vaccines three and four and vaccines four and five, and mothers' awareness of both (see Figure 3.5), suggests that there should be a similar wedge in type expectations for Signal at 4 and Signal at 5, rendering (ii) an unlikely reason to explain such a large difference in treatment effects.[47]

[42] Regression results for all comparisons can be found in Table 3.6.

[43] The effects on vaccine five are mainly driven by a large positive effect early in the experiment. See treatment effects for first two birth cohorts after the launch in Figures 3.13 and 3.14.

[44] Herd immunity for diphtheria requires 83-85 percent (Anderson and May 2013) of the population to be vaccinated with all three doses.

[45] Table 3.13 Column 3 shows that there are no significant differences in bracelet exchanges at vaccines four and five across Signal at 4, Signal at 5, and the Uninformative Bracelet.

[46] While there are fewer children that have a green bracelet in Signal at 5 compared to Signal at 4 treatment, I find no evidence for that scarcity or abundance of green (compared to yellow) bracelets could drive the observed differences in treatment effects.

[47] Comparing mothers' beliefs about take-up, the probabilities assigned to a child (unconditional on bracelet color) completing vaccines four and five are approx. 90 and 70 percent respectively.

To capture differences in social desirability, mothers were asked at endline what they considered to be the most (and second most) important vaccine.[48] Figure 3.12 shows that mothers assign a higher importance to vaccine five than vaccine four, considering the fourth vaccine overall to be the least important among the five and ranking vaccine five as the second most important vaccine after vaccine one. These preferences (taken at face value) imply a low valuation of a signal for vaccine four, and a higher valuation of a signal at vaccine five.

This raises the question: how informative is Signal at 4 about a child having received vaccine five? Put differently, if Signal at 4 is as informative about the completion of vaccine five, as is Signal at 5 then we would expect to see similar treatment effects for both, despite the differences in preferences. Figure 3.12 (Vaccine 5) shows that both Signal at 4 and 5 were significantly more informative about the completion of vaccine five than was the Uninformative Bracelet. In terms of magnitude, Signal at 4 was approximately two-thirds as informative about the completion of vaccine five as Signal at 5.[49] Scaling the observed treatment effect on vaccine four for Signal at 5 accordingly, we would expect to see a treatment effect of around 7.2 percentage points on vaccine four for Signal at 4.[50] The actual point estimate is 2.8 and therefore 2.5 times smaller. Given the noisiness of the coefficient one should consider the confidence interval of the estimate, which does include the value.[51] I interpret these results as evidence for the importance of linking signals to actions that are commonly perceived as valuable, and that the information they provide about other closely-related actions might be down-weighted by individuals.

Reassuringly, Table 3.10, shows that there are no significant differences in individuals' preferences for different vaccines across treatment and control groups, ruling out that the observed treatment effects for Signal at 5 are due to normative influence of signals or social learning.

Effect of Signal at 5 on Timely Completion of Earlier Vaccines

I next examine the effect of Signal at 5 on vaccinations before the signaling threshold at vaccine five:

$$\frac{\partial Pr(a_i(v) \geq r - \tau))}{\partial x} \geq 0$$

Figures 3.8 and 3.10 depict that in addition the treatment effects at vaccines five and four, Signal at 5 also led to significant increases in the share of children that were vaccinated

[48]Ideally, I would also have elicited second-order beliefs about preferences, asking mothers what they thought others thought were the most important vaccines. Piloting showed that these question are difficult to implement.

[49]Simple calculation: $\frac{Pr_{-i}^{S5}(a_i \geq 4|Green) - Pr_{-i}^{S5}(a_i \geq 4|Yellow)}{Pr_{-i}^{S4}(a_i \geq 4|Green) - Pr_{-i}^{S4}(a_i \geq 4|Yellow)} = \frac{0.28}{0.41} = 68.29$.

[50]10.6 percentage points $\cdot$ 0.68 = 7.2 percentage points.

[51]Note that the confidence interval is: [-5.43,11.03].

for vaccines three (7.1 and 4.2 percentage points) and two (4.3 and 1.8 percentage points compared to the Control Group and Uninformative Bracelet).[52] The pattern of treatment responses reveals that parents were more likely to vaccinate their children for earlier vaccines, without necessarily making it to vaccine five. That is, parents responded to a signaling benefit at vaccine five (∼ option value of signaling) six to nine months in advance, without being able to necessarily realize the benefit. These effects are consistent with the theoretical predictions from the signaling model discussed in Chapter 1 Section 3 where individuals make decisions dynamically under uncertainty. More generally, this responses to treatment imply that individuals aim to complete later vaccines, but drop out early due to unforeseen preference or cost shocks.

Table 3.7 Column 1 combines the reduced form treatment estimates for all five vaccinations. Signal at 5 significantly increased the average total number of vaccines completed from 4 to 4.4, over the Control Group and from 4.2 to 4.4 over the Uninformative Bracelet. I find no significant difference between the Uninformative Bracelet and Signal at 4.

Treatment Effects over Time

Figures 3.13 and 3.14 plot the time trends of average treatment effects of Signal at 4, Signal at 5, and the Uninformative Bracelet, compared to the Control Group for vaccines four and five, by birth cohorts. Children are binned into birth cohorts of two months. The vertical grey line represents the time of the launch of the experiment. Looking at effects over time for Signal at 4, there is some indication of a positive trend in treatment effects for children born six to 12 months after the roll out. Such patterns are consistent with a signal with an initially low value, due to it being linked to an action that is not considered relevant for social image concerns, but that becomes more valuable as the visibility and salience of the action increases the relevance that people assign to it. For the Uninformative Bracelet, I observe the opposite trend: the bracelet led to large and significant increases in timely take-up of vaccine four for children born zero to four months after the roll out, but had zero effect for cohorts born six to 12 months after the launch. Importantly, for Signal at 5, the patterns across time show consistently high treatment effects of 10 percentage points for vaccine four, which persist for children born 12 months after the launch of the experiment (see Figure 3.13). For vaccine five, where I observe fewer cohorts (see Figure 3.14), treatment effects seem to increase over time, from 7 percentage points for children born immediately after the roll out to 16 percentage points for children born six months into the implementation.

Intensive versus Extensive Margin Effect of Bracelets

Signals were tied to the timely completion of vaccinations. An alternative measure used in public health is the share of children that received a given vaccination by the age of one year. Table 3.8 Columns 1 to 3 show that almost all children had received vaccine

[52]The effect for vaccine two of Signal at 5 compared to the Uninformative Bracelet is only marginally significant with a p-value of 0.11.

one, two and three by twelve months age, with levels of completion at 98.9, 97.8 and 95.3 percent. However, there is still a substantial drop off for vaccines four and five, with 88.1 and 67.6 percent of children completing those. Columns 4 and 5 shows the effects of all three bracelet treatments on the share of children vaccinated for vaccines four and five, compared to the Control Group.[53] Signal at 5 treatment not only led to intertemporal shifts, encouraging parents to vaccinate their children more timely, but also led to shifts on the extensive margin, with more children getting vaccinated by the age of one: shares increased by 5.2 (to 93.3 percent) and 13.5 percentage points (to 81.1 percent) for vaccines four and five respectively. Treatment effects are similarly large for Signal at 4 and the Uninformative Bracelets for vaccines four (5.4 and 5.8 percentage points) and five (10.1 and 8 percentage points respectively). Bracelets, as small rewards, incentivizing parents through their consumption value, or by increasing the salience of vaccines, acting as a reminder, had a significant and large effect on the completion of routine vaccinations by the age of one year. Particularly relevant for protection levels against these diseases, bracelets raised completion rates for the DTP series to over 93 percent, reaching immunization rates necessary for herd immunity against whooping cough, and increasing Measles vaccination rates up to 81 percent.

Discussion

The preceding analysis yields three main takeaways. First, the results provide the first field experimental evidence of the impact of social signaling in a low-income setting, showing that individuals are willing to take meaningful actions to signal their type as good parents. Parents vaccinated their children more timely, and completed on average an additional 0.5 vaccinations at a cost of 1 USD per child. This finding provides compelling evidence for the potential of social signaling, as an informal enforcement mechanism[54], to increase public goods. Second, the findings show that for signals to be effective, they need to both be informative about individuals' actions and to be *clearly* linked to actions that are sufficiently valued and therefore considered as socially desirable. By placing a signal on an action that is commonly valued, individuals can be motivated to take actions they value less, such as taking their child more timely for vaccine four. Alternatively, signals may need to be combined with a normative messaging intervention, that highlights the externality effects of an action and increases social image concerns through that. Third, these results show that parents make dynamic decisions when deciding about the optimal number of vaccinations. Parents respond to the option value of signaling, by taking their children timely for earlier vaccines, without necessarily making it to vaccine five and realizing the benefit. This is relevant information

[53]Note: by changing the definition to children vaccinated by the age of one, I restrict the sample to children who were at least one year old by the end of the experiment, which results in a sample that is composed of birth cohorts who were early on exposed to the intervention. Given the dynamics observed in Figures 3.13 and 3.14 for the Uninformative Bracelet, it is plausible that extensive margin effects would look different for this treatment for children that were born later.

[54]Compared to formal laws that require parents to vaccinate their child for them to be allowed to attend daycare, like in the U.S..

when considering the optimal structure of signaling or other types of incentives. For example, there is a multitude of (preventative or curative) health behaviors where individuals are required to follow through with multiple visits but after initial take-up of treatment people drop out (Bai et al. 2017). My results highlight that a non-linear incentive scheme, with a social signaling benefit in the far future, can be effective at mitigating drop out. However, given the continued "gap" between individuals' target number of vaccinations and the actual number of vaccinations they complete, a linear incentive scheme, with a benefit at each vaccine could potentially lead to further increases in completion rates.

3.5 The Value of Social Signaling under Dynamic Decision-Making

In order to quantify the value of social signaling taking into account i) the dynamic nature of decision-making, where parents respond to the option value of social signaling and uncertainty over future cost or preference shocks, and ii) type selection effects at later vaccines, I structurally estimate a dynamic discrete-choice model. I use distance to the clinic as a numeraire to price out the signaling value. To do so, in this section, I first demonstrate the reduced form relationship between distance and its impact as a cost on vaccination outcomes. Secondly, I set up the dynamic model estimating the relevant parameters.

Distance as Cost in Reduced Form

Figure 3.23 plots a bin scatter of the average number of timely vaccines completed against the travel distance from communities to clinics, separately for the Control Group and Signal at 5. Distance has a linear effect on the number of vaccinations completed: in the Control Group, the total number of vaccines completed declines from 4.25 at zero miles to 3.5 vaccines at five miles. Figure 3.15 shows the effect of distance on the share of timely vaccinated children by vaccine. Each vaccine graph plots a bin scatter of the share of children vaccinated (for vaccine 2, 3, 4 and 5) against the distance from communities to clinics, separately for the Control Group and Signal at 5. It is evident again that distance has a linear effect on the share of children vaccinated for each vaccine. Importantly, both figures make clear that Signal at 5 mitigated the negative effect of distance, increasing the share of children vaccinated at four miles to that of children vaccinated at zero miles. Differently put, the reduced form results show that Signal at 5 increased parents' willingness to walk for a given vaccine by four miles distance to the clinics.

It is important to note that distance was not exogenously varied in this experiment. We should therefore be worried about the effect of distance on vaccination behavior being confounded by other observable or unobservable characteristics. While I cannot account for the latter, Tables 3.11 and 3.12 show that the inclusion of relevant observable characteristics, such as mothers' education, economic status, or the birth order of children, has no significant effect on the impact of distance on vaccinations in the endline sample.[55]

Quantifying Social Signaling Utility

Following the discussion of the model of signaling under uncertainty in Chapter 1 Section 3, I empirically specify the flow utility at time $t \in \{1, 2, 3, 4, 5\}$ as follows:

$$U_{it} = v_i - \kappa D_i + S_4 T_{4i} \mathbb{1}\{t = 4\} + S_5 T_{5i} \mathbb{1}\{t = 5\} + \epsilon_{it}. \tag{3.2}$$

The model includes two dimensions of unobservable heterogeneity: (i) ϵ_{it} cost or taste shocks which are independent and identically logistically distributed, and (ii) individuals differ in their type v, which is assumed to be randomly drawn from a normal distribution in period zero and is persistent across time t. The mean μ_v and variance σ_v of the type distribution will be identified in the structural estimation as I observe individuals making decisions across multiple periods. Further, the model includes two dimension of observable heterogeneity: (i) individuals' travel distance D_i which discretely varies from zero to five miles and (ii) the signaling treatments T_{4i} and T_{5i} which are exogenously assigned. The parameter κ captures the marginal disutility of one additional mile distance to the clinic. The parameters S_4 and S_5 capture the social signaling utility $\lambda \omega_r \triangle(\hat{v}_r)$.

The reduced form effects of the Signal at 5 treatment at earlier vaccines operate solely through option value. The implied valuation must be filtered through individuals' expectations about the probability that they make it to the end and receive the signaling payoff. At $t = 5$ there is no option value component left and the problem becomes a static one, but the valuation is that of a non-random subset of individuals (in terms of their type v), and not the type population as a whole. Computing the valuation from the reduced form requires linking of all the choice probabilities and treatment effects at each t together. The structural model allows me to do that. I estimate the model using maximum likelihood.

Table 3.9 presents the results from the structural estimation, with Column 1 showing the parameters from an estimation where I compare the shares of children vaccinated timely in Signal at 5 and Signal at 4 to those in the Control Group, and Column 2 showing the parameter estimates comparing both signaling treatments to the Uninformative Bracelet. Taking the ratio of the parameters S_5 and κ gives an estimate of the social signaling utility in miles. On average, parents' valuation of social signaling is equivalent to 7 to 10 miles walking distance to the clinic.

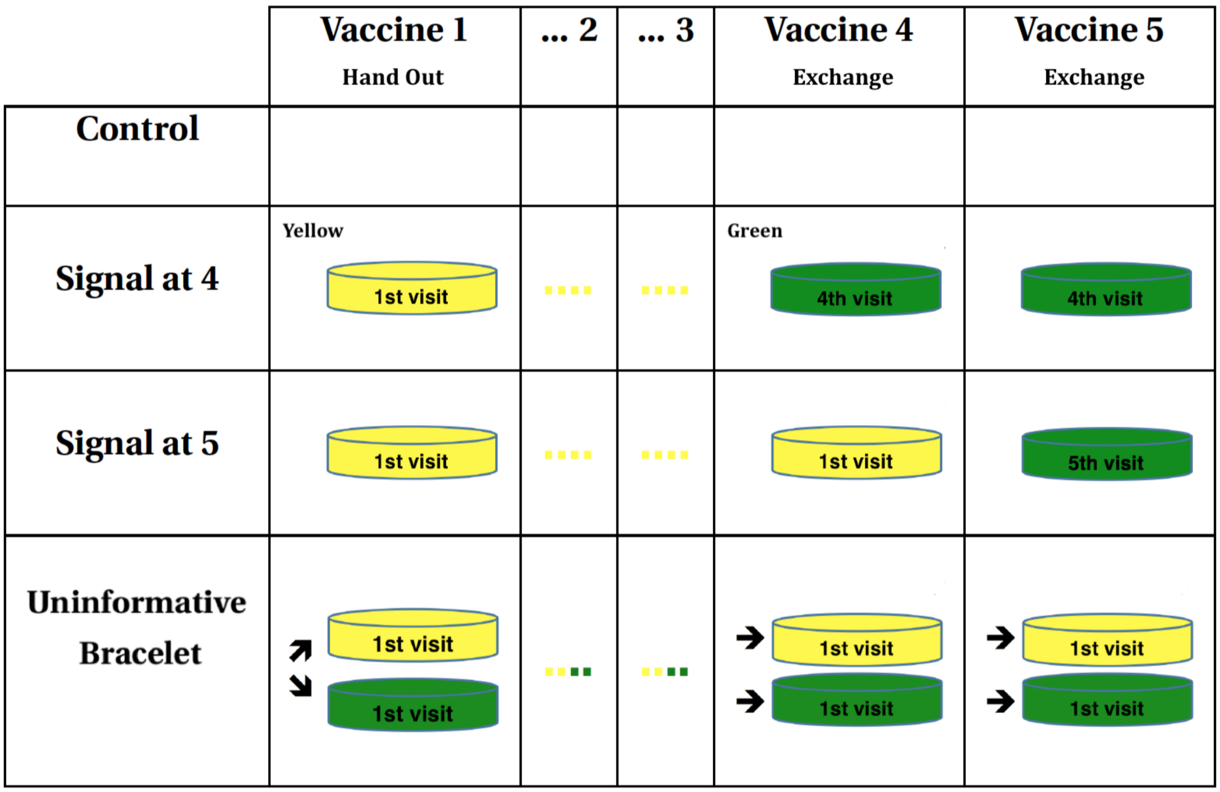
[55]The sample includes all children (age 4 months and above, to be counted for vaccine 3 etc.) whose parents were surveyed at endline and for whom I therefore observe socio-economic characteristics. The variables Floor cement, Roof corrugated iron, Has any education, Works on farm and Trader are all indicator variables that take the value one if the respondent's floor is made of cement etc. and zero otherwise. The distance variable takes the values zero to five miles, specifying how far the respondent's community is from the clinic. Zero is the excluded category in all regressions.

3.6 Conclusion

This study analyzes the effect of social signaling in the dynamic setting of childhood immunization, examining how individuals respond to the opportunity of signaling to others that they are responsible parents. Different to most studies, the experiment implements a durable signal that allows parents to continuously signal their type over the first year of a child's life. My results suggest that the effects of social signals are large, when the action signaled is sufficiently valued. This provides impetus for future research on how the effects of social signals could be enhanced if they are combined with normative messages that emphasize the otherwise undervalued social benefits of actions (like the completion of vaccination series). Moreover, this study shows that individuals' response to signals is consistent with decision-making under uncertainty, shedding light on the constraints that parents face to timely vaccinating their children in contexts like Sierra Leone. It is a question for further research whether a non-linear incentive scheme, where a signaling benefit is only provided at completion of all vaccines is optimal, or if a more linear scheme with signals at multiple points could lead to further reductions in drop-off. On the one hand, signaling benefits might be smaller if there is less scope for parents to separate themselves from others in their intrinsic motivation; on the other hand, if the variance of cost shocks is large, even a smaller signaling benefit at each vaccine could compensate parents for unanticipated cost shocks.

Overall, the findings of this study are of substantive policy importance: signals increased immunization rates to levels necessary for herd immunity at a cost of 1 USD per child. Moreover, they address a problem pertinent to many low-income countries: scarcity of trained health workers and relatively low rural population density. As social signals increase parents' willingness to travel further to receive vaccinations, health workers can remain at clinics and make themselves available to as many patients as possible. Importantly the effects of this intervention persist for children 12 months after the launch of the experiment, demonstrating that a subtle behavioral intervention like this can feasibly be implemented at a large-scale through existing government institutions.

Figure 3.1: Experimental Treatment Groups

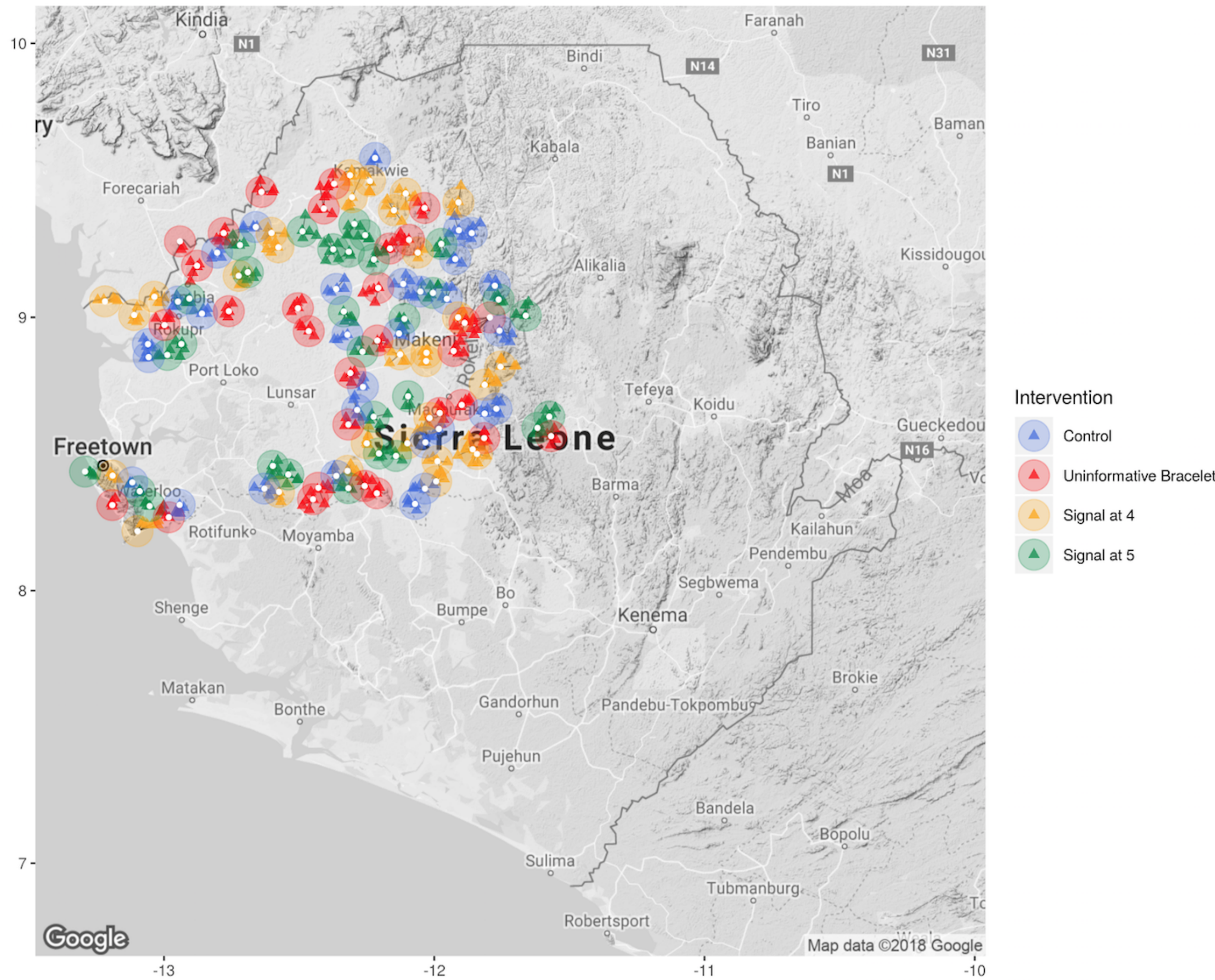


This figure displays the four different treatment groups and the bracelet hand out and exchanges that take place at each of the five vaccinations. At vaccine one children receive a bracelet that has written on it "1st visit" and has the color yellow in Signal at 4 and Signal at 5 treatments. In the Uninformative Bracelet, parents can choose for their child a yellow or green bracelet. A child keeps the same bracelet for vaccines two and three. At vaccine four, in the Signal at 4 treatment, the yellow bracelet is exchanged for a green bracelet that says "4th visit" if the child comes timely (i.e. before 6 months age), otherwise the bracelet is exchanged for another identical yellow bracelet. In the Signal at 5 the bracelet is exchanged for another identical yellow bracelet. In the Uninformative Bracelet treatment, the bracelet is exchanged for an identical bracelet, of the same color as the parent chose at the first visit. At vaccine five, in the Signal at 4 treatment, the green (or yellow, depending on whether the child was timely at vaccine four) is exchanged for an identical bracelet. In the Signal at 5 treatment, the bracelet is exchanged for green bracelet that says "5th visit" if the child comes timely (i.e. by 11 months age). In the Uninformative Bracelet, the bracelet is again exchanged for an identical "1st visit" bracelet of the color that the parent originally chose.

Figure 3.2: Different Bracelets handed out across Three Signaling Treatments

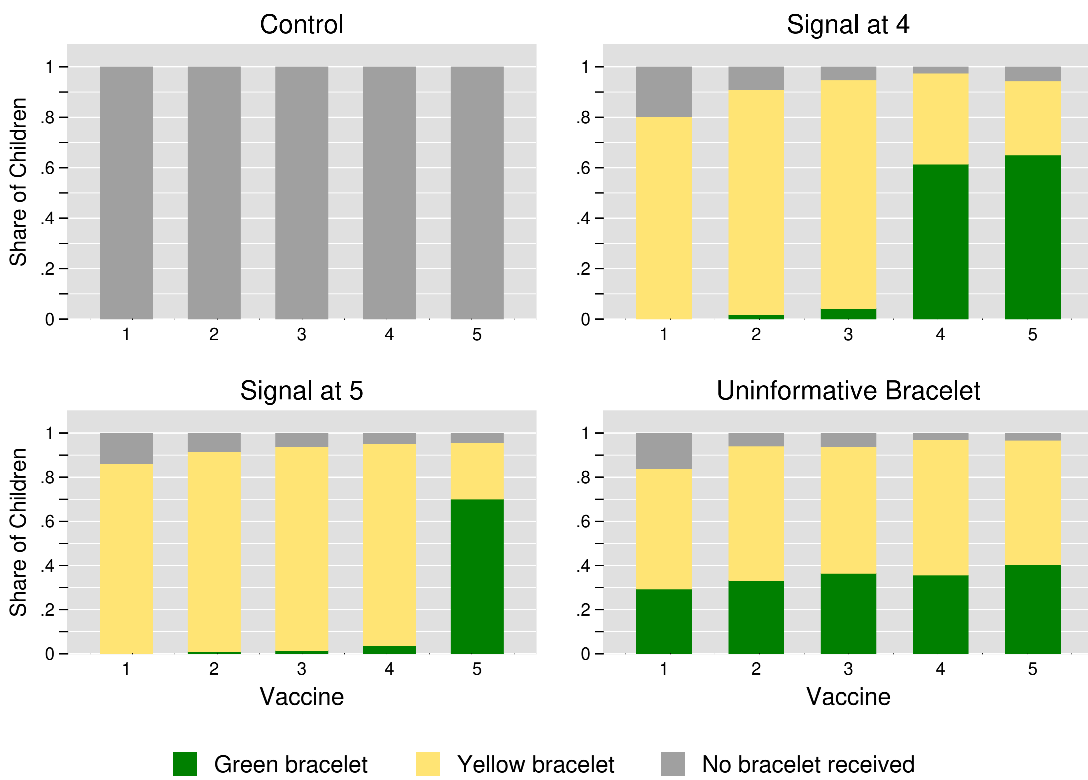
The image displays the actual bracelets that health workers give out at clinics: the top yellow “1st visit” bracelet is used in Signal at 4, Signal at 5 and the Uninformative Bracelet treatment; the second green “1st visit” bracelet is only given to children in the Uninformative Bracelet treatment; the green “4th visit” bracelet is given to children in the Signal at 4 treatment and the bottom green “5th visit” bracelet to children in the Signal at 5 treatment. All bracelets are made out of silicone and are *size-adjustable* so that they can comfortably fit the wrist of a child between the ages of zero and twelve months. The latter was important for the experimental design i) as it made the bracelet a durable signal that could be observed by others and allow for comparisons beyond the time of the vaccination, and ii) so that the size of the bracelet would not be informative about the number of vaccinations a child has completed. As a child’s wrist grows, even in the absence of a change in bracelet color, a too small bracelet that no longer fits, could be informative about whether a child is up-to-date with its vaccinations. Over the course of the experiment, a total of 36,000 bracelets were handed out by health workers.

Figure 3.3: Clinic Randomization



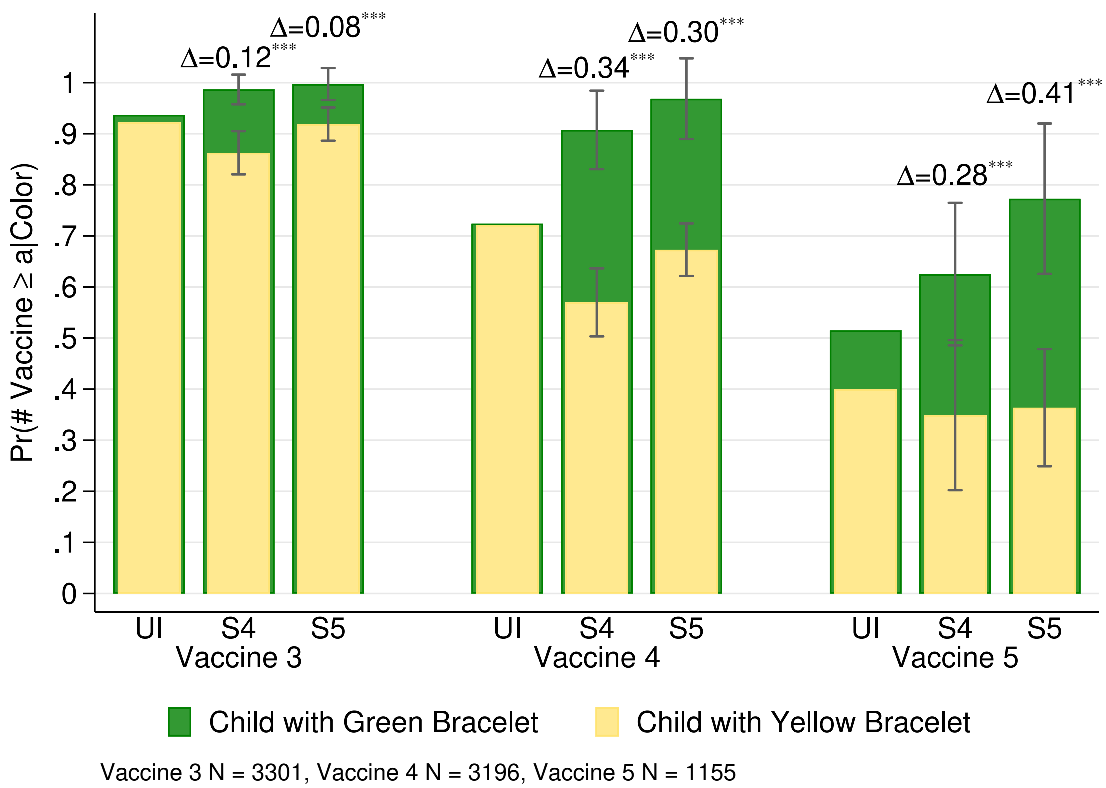
Map of Sierra Leone that shows the geographic span of the experiment, with 120 clinics, that is ten percent of Sierra Leone's public clinics, being randomized into the four different treatment groups. The clinic randomization was stratified by district. Four out of Sierra Leone's 14 districts were selected for the experiment in collaboration with the Government and partners, based on the criteria: i) baseline vaccination rates, ii) Ebola affectedness, iii) reliability of supply side, and iv) other ongoing interventions. To avoid spillovers, the set of 120 clinics was chosen from a sample of 243 clinics, using an algorithm that ensured that each selected clinic had a catchment radius of 5 miles, of which at least 35 percent of the area was non-overlapping with any adjacent clinic's catchment area.

Figure 3.4: Correct Hand Out of Bracelets by Treatment Groups



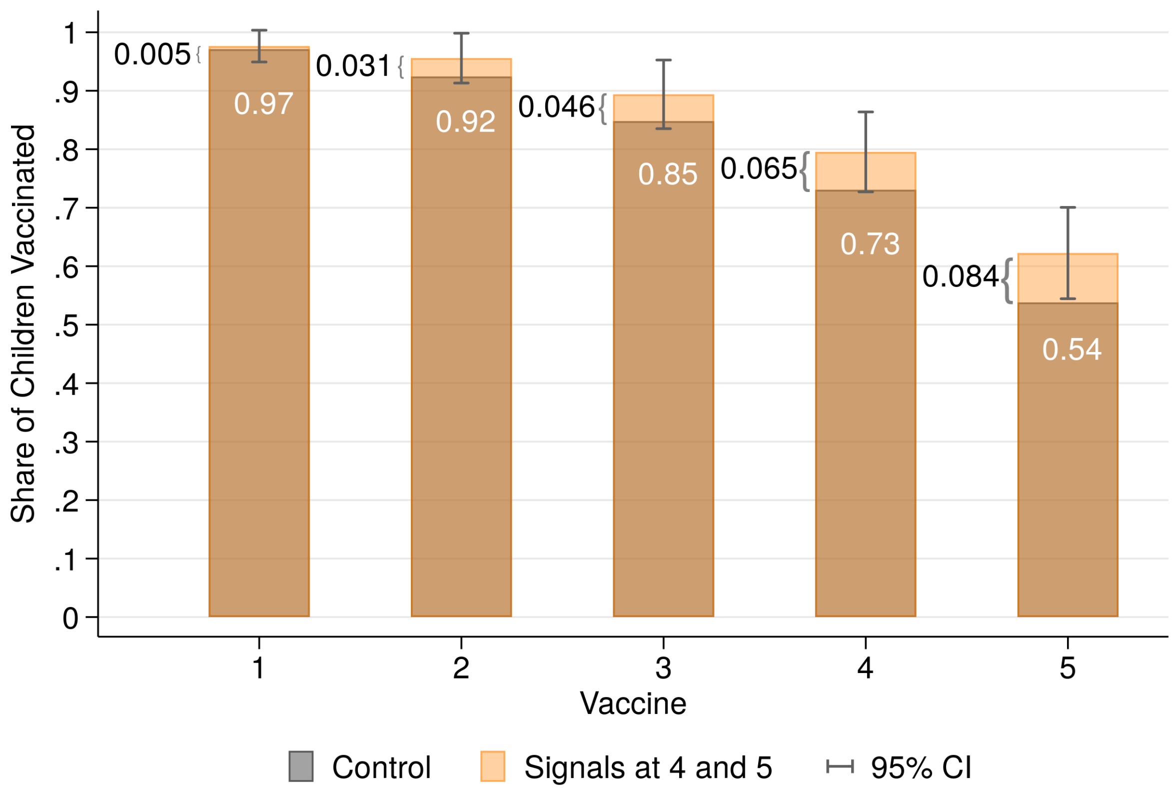
This figure displays the share of children with a green, yellow, or no bracelet conditional on the number of vaccines a child has received, separately for each treatment arm. The sample includes 6,922 children that were born after the experiment was launched and that were surveyed during the listing survey, which took place 12 - 15 months after the intervention was launched in a particular clinic. Surveyors asked each parent "What color bracelet was your child given when you went for vaccination?" and recorded all vaccines the child had received up to that point. The share of children with "No bracelet received" shows that almost every child received a bracelet (94%) across all three bracelet treatments. In the Uninformative Bracelet treatment, there is overall no significant relationship between the number of vaccines a child received and the color of bracelet (there is only small significant increase in the share of children with a green bracelet for vaccine five). For Signal at 4 and Signal at 5, there is a clear relationship between color of bracelet and the number of vaccines a child received: there is a large spike - from close to zero to 62 percent for Signal at 4 and 70 percent for Signal at 5 - in the share of children with a green bracelet at vaccine four and five respectively. Children who had taken vaccine four and/or five but had a yellow bracelet had either come late for the vaccine (~ one-third) or health workers had missed to give the correct bracelet (~ two-thirds).

Figure 3.5: The Effect of Signals on Beliefs about Other Children's Vaccinations



This figure shows endline respondents' beliefs about the number of vaccinations a child received conditional on the color of bracelet. Beliefs are shown by vaccine, and by treatment, where UI = Uninformative Bracelet, S4 = Signal at 4, S5 = Signal at 5. The yellow and green bars show the conditional probability Pr(# Vaccine ≥ a | Color) of a child having received (at least) vaccine 3, 4, or 5 (i.e. a = $\{3, 4, 5\}$) conditional on the respondent observing the child having a yellow or green bracelet. Vaccines one and two are excluded from the figure since individuals believe that (close to) 100 percent of children complete these vaccines. The confidence intervals (at 95 percent) for Signal at 4 and Signal at 5, on the green and yellow bars respectively, compare the beliefs in the signaling treatments to those in the Uninformative Bracelet. $\triangle$ denotes the difference between the two conditional probabilities: Pr(# Vaccine ≥ a | Green) - Pr(# Vaccine ≥ a | Yellow). The samples used for each vaccine include all children below the age of one who were eligible for the specific vaccine: age 2.5, 3.5 and 9 months and older for vaccines three, four and five respectively. Using the estimated joint probabilities from regressions of a binary variable for a child having a green (yellow) bracelet and at least a vaccines (fewer than a vaccines), on treatment indicators for Signal at 4 and Signal at 5, with the Uninformative Bracelet as excluded category (e.g. Pr(Green and Vaccine # ≥ 4) and Pr(Green and Vaccine # < 4) I compute the marginal probabilities for bracelet color (e.g. Pr(Child has Green Bracelet)) and finally the conditional probabilities e.g. Pr(# Vaccine ≥ 4 | Green) = $\frac{\text{Pr(Green and Vaccine\#}\geq 4)}{\text{Pr(Child has Green Bracelet)}}$. Estimating the probabilities in a regression framework, I control for the mean take-up level of vaccine a at the clinic and child age. Both controls are demeaned. All regressions include strata fixed effects. Standard errors are clustered at the clinic level.

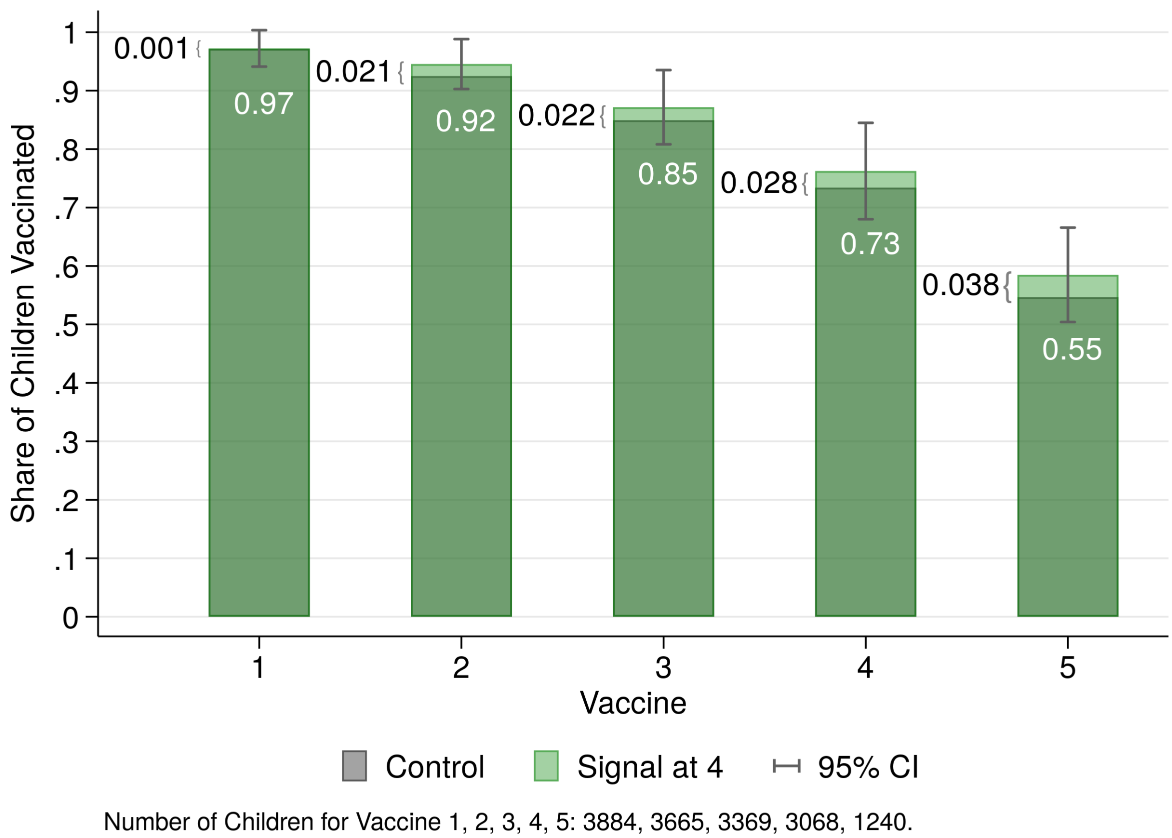
Figure 3.6: The Combined Effect of Signals at 4 and 5 on Timely Vaccinations



Number of Children for Vaccine 1, 2, 3, 4, 5: 5753, 5429, 5006, 4536, 1819.

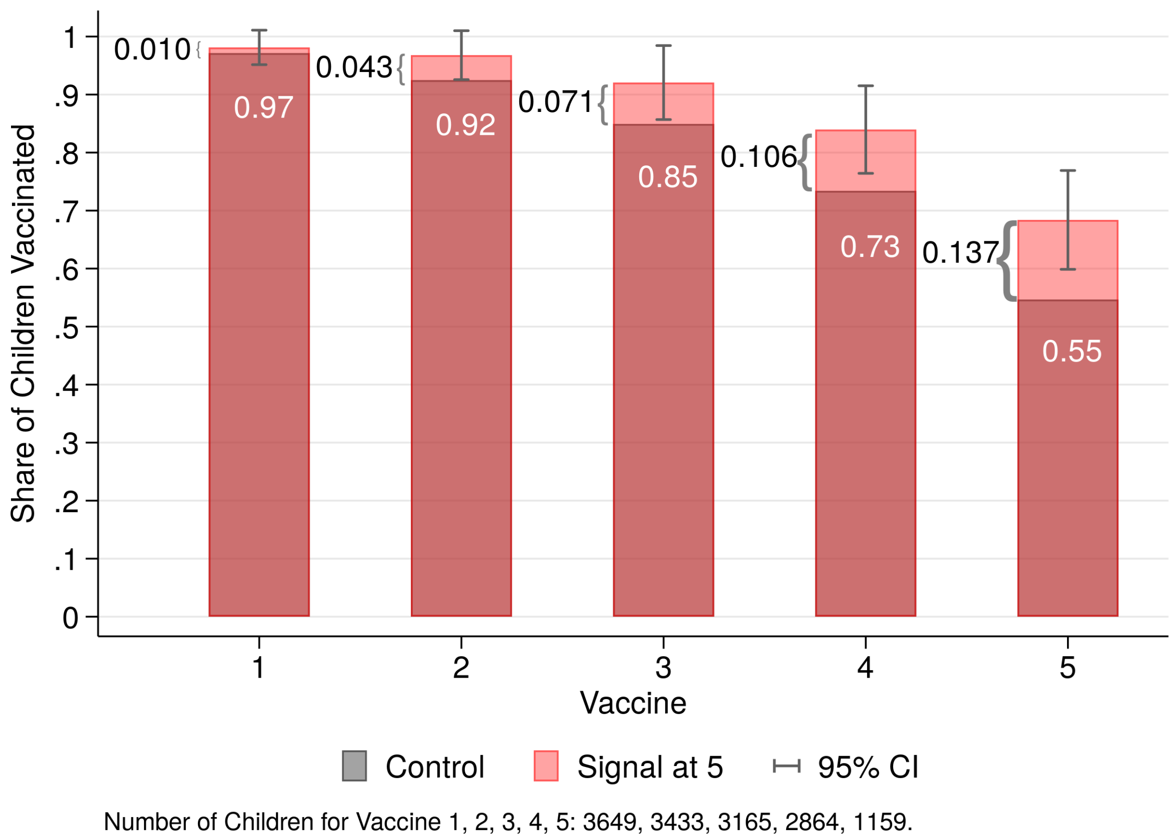
This figure shows the results from a linear probability model of the binary outcome variable for a child being vaccinated for 1, 2, 3, 4, or 5 vaccinations by the age of 3, 4, 5, 6 and 11.5 months, respectively, on a treatment indicator for Signal at 4 and 5, with the omitted category being the Control Group. The sample includes all children born since the launch of the experiment. All regressions include strata-fixed effects, the demeaned control for child age, and an indicator that is coded one if the vaccine entry comes from the administrative data. The 95 percent confidence intervals were computed using standard errors that are cluster bootstrapped (1000 repetitions) at the clinic level.

Figure 3.7: The Effect of Signal at 4 on Timely Vaccinations



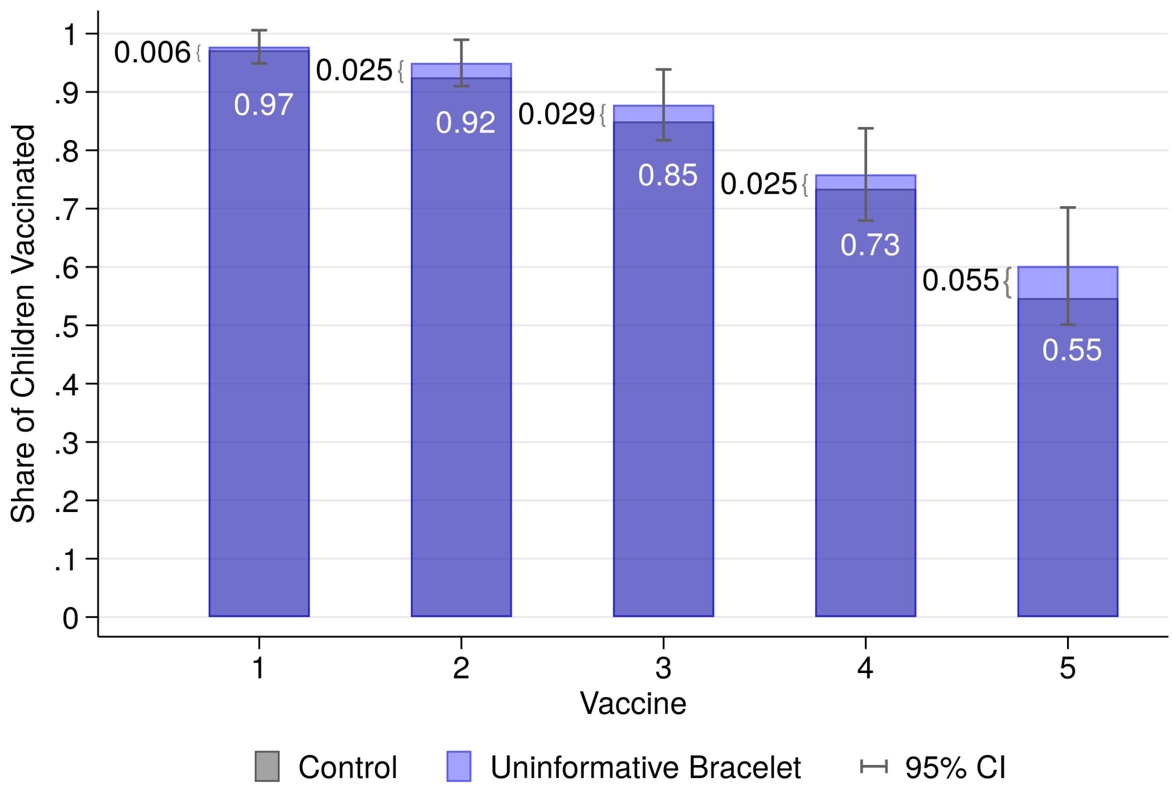
This figure shows the results from a linear probability model of the binary outcome variable for a child being vaccinated for 1, 2, 3, 4, or 5 vaccinations by the age of 3, 4, 5, 6 and 11.5 months, respectively, on a treatment indicator for Signal at 4, with the omitted category being the Control Group. The sample includes all children born since the launch of the experiment. All regressions include strata-fixed effects, the demeaned control for child age, and an indicator that is coded one if the vaccine entry comes from the administrative data. The 95 percent confidence intervals were computed using standard errors that are cluster bootstrapped (1000 repetitions) at the clinic level.

Figure 3.8: The Effect of Signal at 5 on Timely Vaccinations



This figure shows the results from a linear probability model of the binary outcome variable for a child being vaccinated for 1, 2, 3, 4, or 5 vaccinations by the age of 3, 4, 5, 6 and 11.5 months, respectively, on a treatment indicator for Signal at 5, with the omitted category being the Control Group. The sample includes all children born since the launch of the experiment. All regressions include strata-fixed effects, the demeaned control for child age, and an indicator that is coded one if the vaccine entry comes from the administrative data. The 95 percent confidence intervals were computed using standard errors that are cluster bootstrapped (1000 repetitions) at the clinic level.

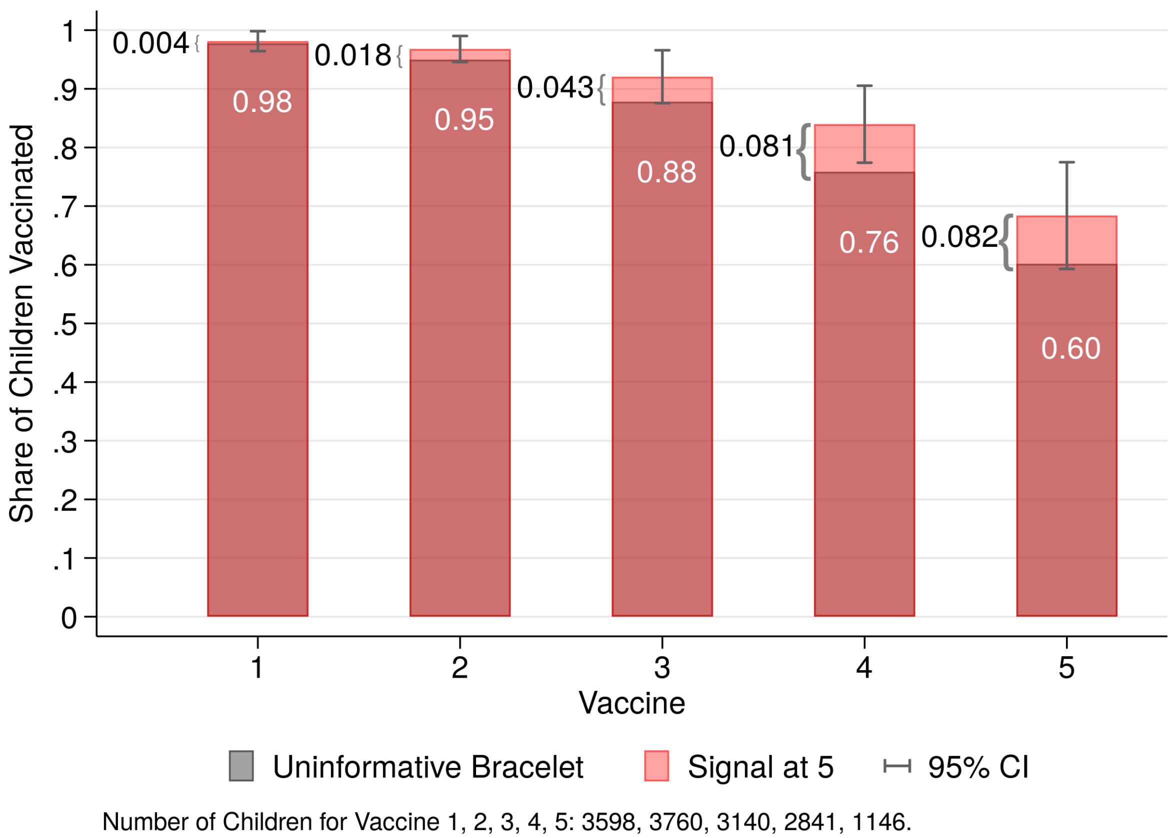
Figure 3.9: The Effect of the Uninformative Bracelet on Timely Vaccinations



Number of Children for Vaccine 1, 2, 3, 4, 5: 3509, 3031, 3031, 2769, 1075.

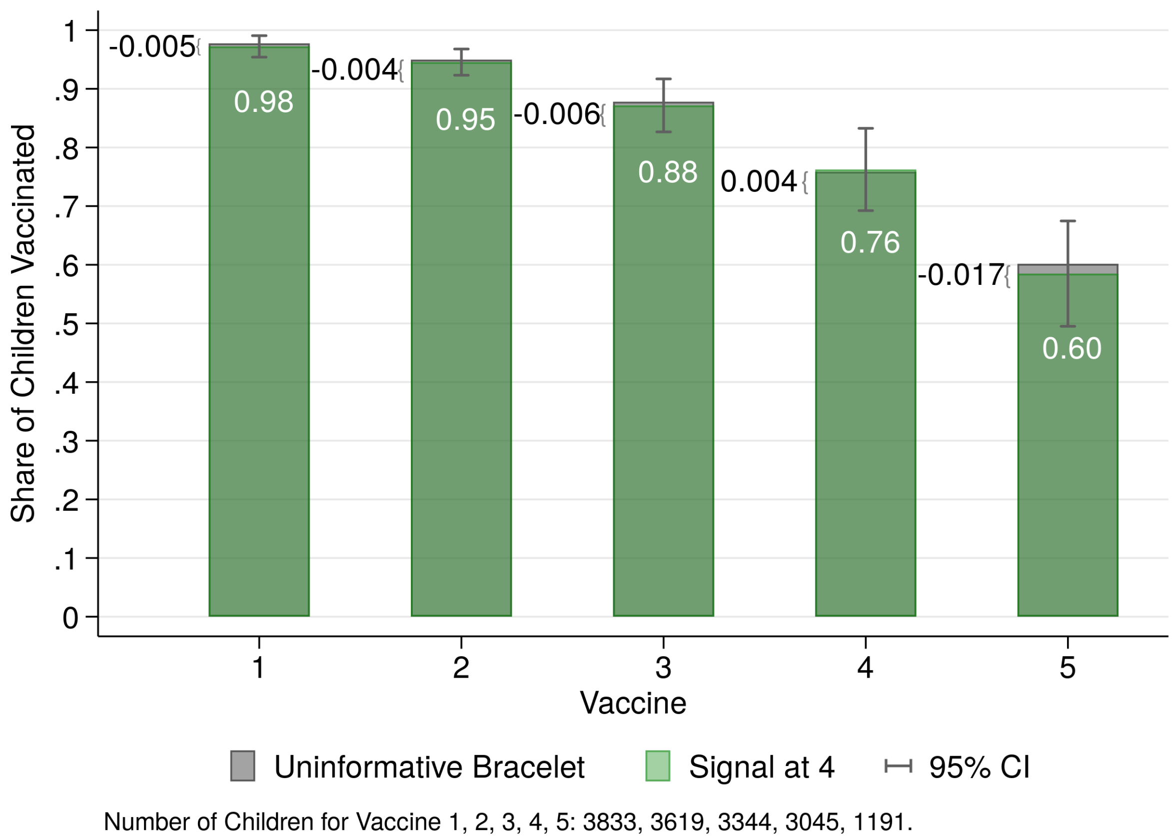
This figure shows the results from a linear probability model of the binary outcome variable for a child being vaccinated for 1, 2, 3, 4, or 5 vaccinations by the age of 3, 4, 5, 6 and 11.5 months, respectively, on a treatment indicator for the Uninformative Bracelet, with the omitted category being the Control Group. The comparison captures the effect of bracelets through increases in consumption utility and salience (e.g. reminder effects). The sample includes all children born since the launch of the experiment. All regressions include strata-fixed effects, the demeaned control for child age, and an indicator that is coded one if the vaccine entry comes from the administrative data. The 95 percent confidence intervals were computed using standard errors that are cluster bootstrapped (1000 repetitions) at the clinic level.

Figure 3.10: The Effect of Signal at 5 versus the Uninformative Bracelet on Timely Vaccinations



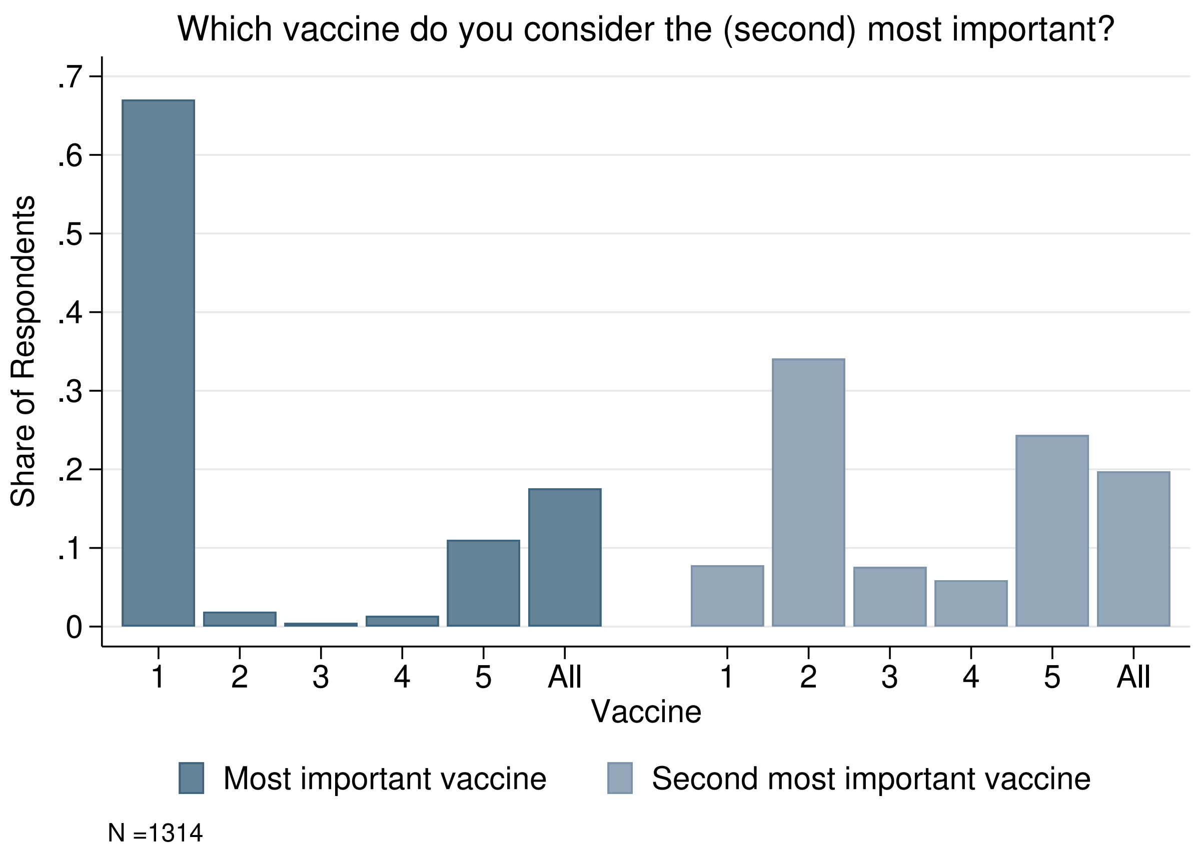
This figure shows the results from a linear probability model of the binary outcome variable for a child being vaccinated for 1, 2, 3, 4, or 5 vaccinations by the age of 3, 4, 5, 6 and 11.5 months, respectively, on a treatment indicator for Signal at 5, with the omitted category being the Uninformative Bracelet. The comparison holds constant the effect of bracelets through increased consumption utility and salience (e.g. reminder effects). The sample includes all children born since the launch of the experiment. All regressions include strata-fixed effects, the demeaned control for child age, and an indicator that is coded one if the vaccine entry comes from the administrative data. The 95 percent confidence intervals were computed using standard errors that are cluster bootstrapped (1000 repetitions) at the clinic level.

Figure 3.11: The Effect of Signal at 4 versus the Uninformative Bracelet on Timely Vaccinations



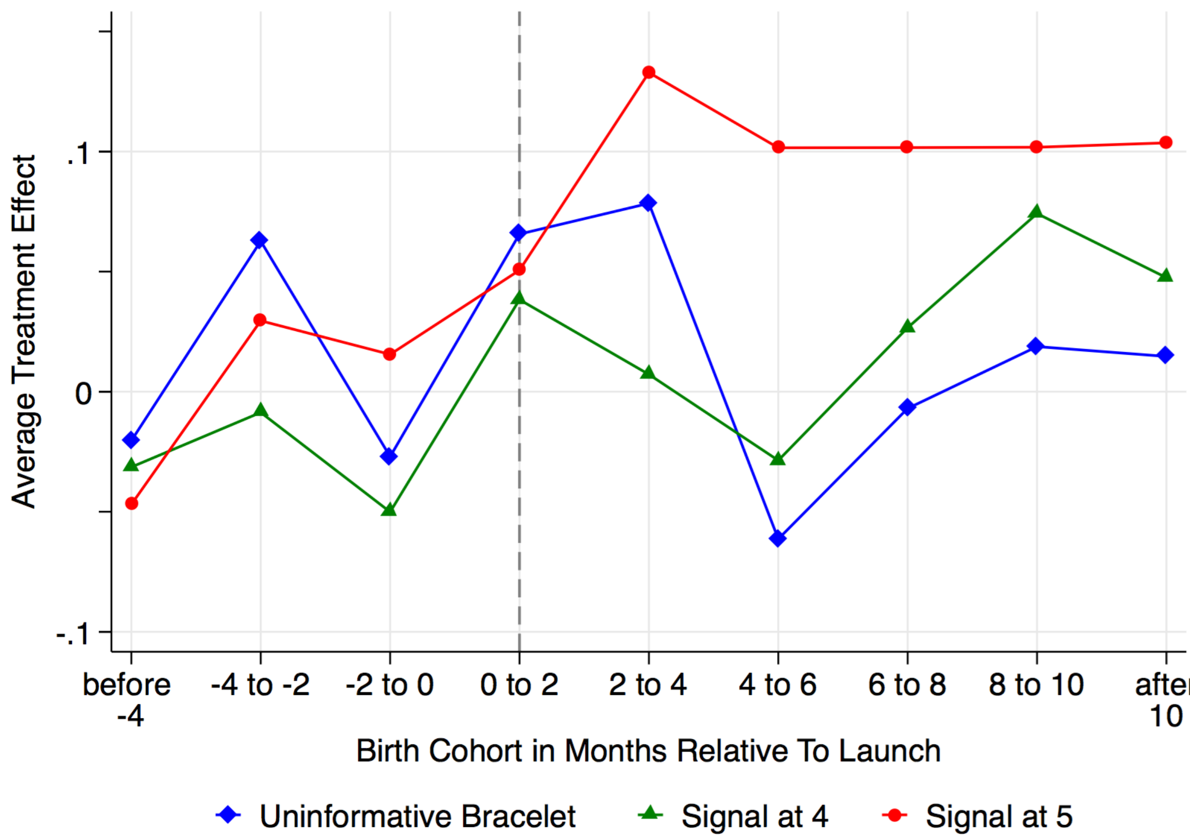
This figure shows the results from a linear probability model of the binary outcome variable for a child being vaccinated for 1, 2, 3, 4, or 5 vaccinations by the age of 3, 4, 5, 6 and 11.5 months, respectively, on a treatment indicator for Signal at 4, with the omitted category being the Uninformative Bracelet. The comparison holds constant the effect of bracelets through increased consumption utility and salience (e.g. reminder effects). The sample includes all children born since the launch of the experiment. All regressions include strata-fixed effects, the demeaned control for child age, and an indicator that is coded one if the vaccine entry comes from the administrative data. The 95 percent confidence intervals were computed using standard errors that are cluster bootstrapped (1000 repetitions) at the clinic level.

Figure 3.12: Preferences for Different Vaccinations



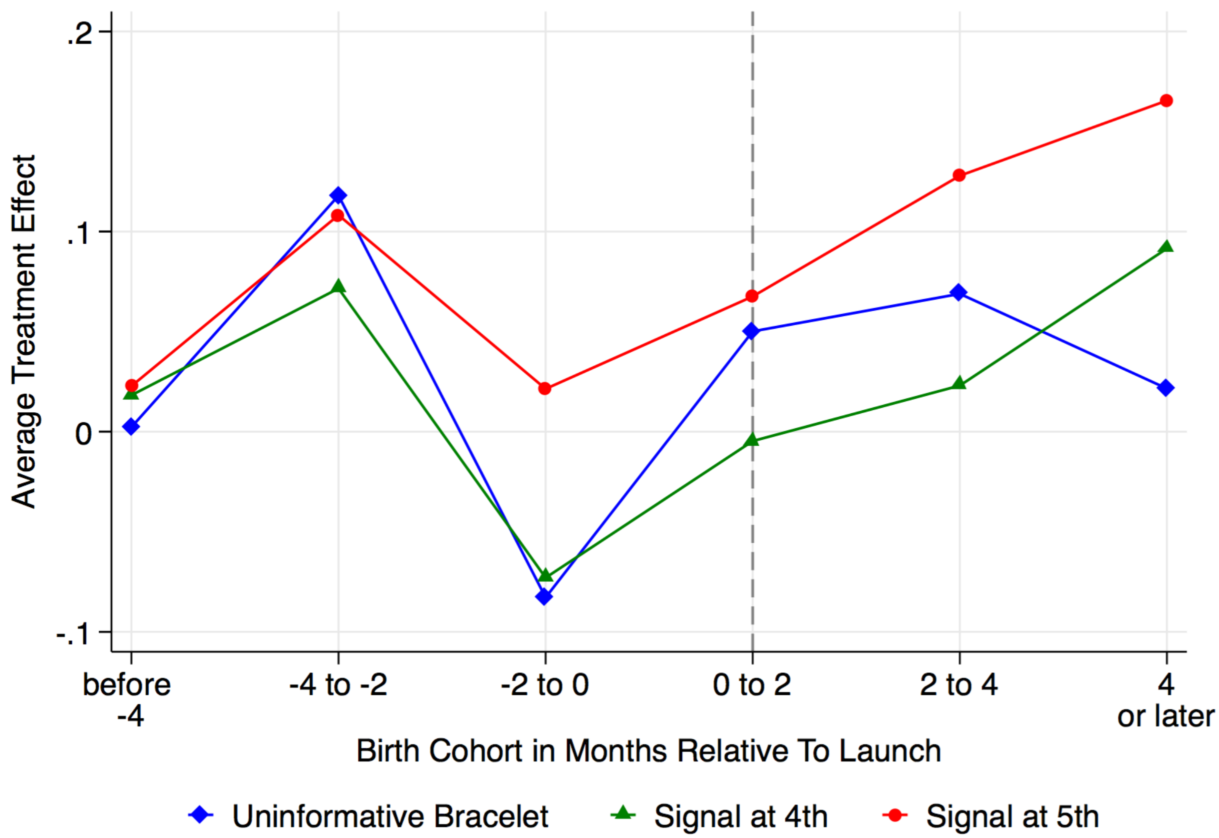
This figure shows mothers' perceptions about the relative importance of the five vaccinations. Mothers were first asked about which vaccination they thought was the most important, and then which one they thought was the second most important (conditional on not having answered "All" to the first question). The figure plots the share of respondents that answered vaccine one, two, three, four, five or all vaccines are the most important (on the left), and the second most important (on the right). The sample includes all mothers that were surveyed at endline. Answers are pooled across treatments. As Table 3.10 shows there is no significant difference in preferences across intervention arms.

Figure 3.13: Treatment Effects over Time for Vaccine 4



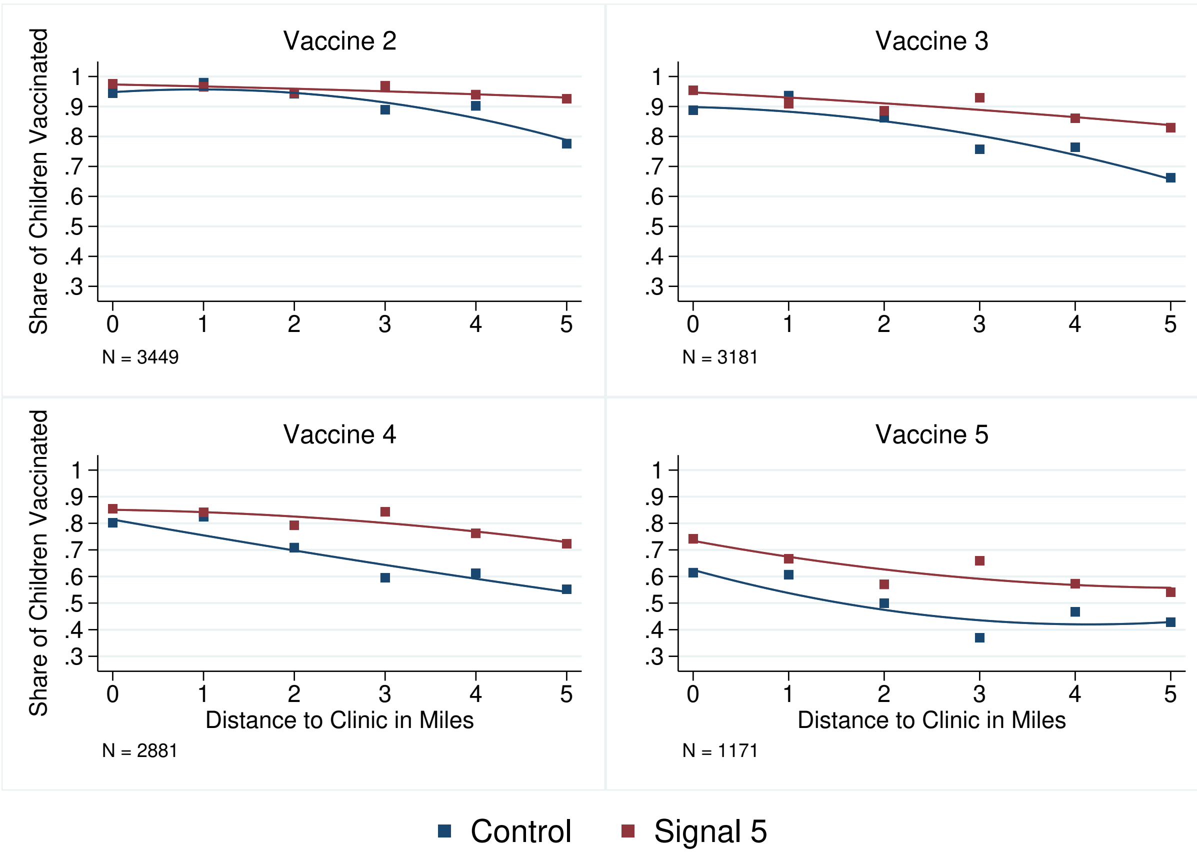
This figure plots the average treatment effect of Signal at 4, Signal at 5 and the Uninformative Bracelet treatment compared to the Control Group for vaccine four, by birth cohorts. Children are grouped into birth cohorts of two months. The dotted line indicates the launch of the experiment. The sample size (number of children) in each bin, starting from the left, is 1455, 501, 899, 918, 939, 948, 1126, 967 and 1024.

Figure 3.14: Treatment Effects over Time for Vaccine 5



This figure plots the average treatment effect of Signal at 4, Signal at 5 and the Uninformative Bracelet treatment compared to the Control Group for vaccine five, by birth cohorts. Children are grouped into birth cohorts of two months. The dotted line indicates the launch of the experiment. The sample size (number of children) in each bin, starting from the left, is 1455, 501, 899, 903, 720 and 738.

Figure 3.15: The Effect of Distance on Take-up in the Control and Signal at 5 Group



The graph shows the effect of distance on the share of timely vaccinated children by vaccine. Each vaccine graph plots a bin scatter of the share of children vaccinated (for vaccine 2, 3, 4 and 5) against the distance from communities to clinics, separately for the Control Group and Signal at 5. The sample includes all children born since the launch that were at least 4, 5, 6 and 11.5 months old by the end of the experiment, to be considered for vaccine 2, 3, 4 or 5 respectively. Similar to Figure 3.23 Signal at 5 mitigated the negative effect of distance across all vaccines, increasing vaccination rates at four miles to those at zero miles in the Control Group.

Table 3.1: Description of Study Sample from Endline Survey

Variable	(1) Control Mean/SE	(2) Signal at 4 Mean/SE	(3) Signal at 5 Mean/SE	(4) Uninformative Mean/SE	T-test P-value (1)-(2)	(1)-(3)	(1)-(4)	(2)-(3)	(2)-(4)	(3)-(4)	F-test for joint orthogonality
Mother age	26.240	26.275	26.176	26.565	0.918	0.984	0.550	0.808	0.555	0.501	0.911
	(0.454)	(0.293)	(0.366)	(0.366)							
Birth order of child	3.311	3.404	3.376	3.500	0.426	0.462	0.151	0.851	0.449	0.374	0.546
	(0.104)	(0.078)	(0.087)	(0.093)							
Is married	0.603	0.468	0.545	0.539	0.037**	0.398	0.260	0.200	0.257	0.814	0.209
	(0.044)	(0.051)	(0.049)	(0.053)							
Lived in community for over 1 year	0.966	0.976	0.969	0.958	0.412	0.678	0.576	0.618	0.171	0.489	0.625
	(0.009)	(0.008)	(0.010)	(0.012)							
Education											
Has no education	0.437	0.480	0.467	0.500	0.328	0.313	0.139	0.883	0.627	0.500	0.501
	(0.031)	(0.035)	(0.033)	(0.039)							
Has some primary education	0.323	0.330	0.307	0.265	0.909	0.682	0.065*	0.531	0.058*	0.157	0.200
	(0.028)	(0.029)	(0.029)	(0.030)							
Has some secondary education	0.240	0.190	0.226	0.235	0.265	0.459	0.908	0.441	0.293	0.656	0.657
	(0.030)	(0.033)	(0.026)	(0.032)							
Occupation											
Works on farm	0.760	0.734	0.693	0.768	0.619	0.300	0.661	0.654	0.405	0.127	0.449
	(0.032)	(0.046)	(0.038)	(0.035)							
Assets											
Has a mobile phone (1=Yes, 0=No)	0.117	0.107	0.154	0.092	0.719	0.506	0.219	0.203	0.343	0.027**	0.147
	(0.023)	(0.020)	(0.028)	(0.017)							
Floor (1=Cement/Tile, 0=Mud)	0.320	0.373	0.376	0.330	0.292	0.478	0.911	0.787	0.417	0.496	0.661
	(0.031)	(0.037)	(0.044)	(0.041)							
Roof (1=Corrugated iron, 0=Thatch)	0.898	0.902	0.912	0.859	0.899	0.780	0.144	0.839	0.121	0.052*	0.291
	(0.025)	(0.021)	(0.018)	(0.021)							
Observations	338	339	319	318							
Clinics	30	30	29	30							

This table summarizes socio-economic characteristics for a random sample of 1,314 endline survey respondents. All respondents were mothers, who had a child that was born since the start of the experiment, and who resided in one of the selected clinic catchment communities. The table reports mean values of each variable for every treatment group. The final column reports the joint significance level of treatment indicators in a regression with strata-level fixed effects. The value displayed for t-tests and F-tests are p-values. Standard errors are clustered at the clinic level. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table 3.2: Description of Clinic and Baseline Community Characteristics

Variable	(1) Control Mean/SE	(2) Signal at 4 Mean/SE	(3) Signal at 5 Mean/SE	(4) Uninformative Mean/SE	T-test P-value (1)-(2)	(1)-(3)	(1)-(4)	(2)-(3)	(2)-(4)	(3)-(4)	F-test for joint orthogonality
Immunization services											
Number of staff	2.034	1.966	2.241	2.000	0.740	0.575	0.891	0.402	0.799	0.468	0.858
	(0.189)	(0.093)	(0.313)	(0.099)							
Immunization day frequency (1=weekly, 0=monthly)	0.690	0.621	0.690	0.586	0.592	0.991	0.429	0.595	0.815	0.434	0.804
	(0.087)	(0.092)	(0.087)	(0.093)							
Stockout of vaccines in the past 2 months (1=Yes, 0=No)	0.172	0.138	0.138	0.103	0.725	0.710	0.463	0.999	0.687	0.708	0.904
	(0.071)	(0.065)	(0.065)	(0.058)							
Experiment implementation											
Number of days at which launched relative to first clinic	83.207	99.310	92.241	82.241	0.263	0.528	0.979	0.602	0.262	0.527	0.615
	(10.266)	(10.263)	(10.160)	(10.848)							
Number of days listing survey implemented after first clinic	67.647	76.871	73.418	64.057	0.364	0.590	0.758	0.731	0.234	0.414	0.629
	(7.274)	(7.324)	(7.875)	(7.669)							
Number of monitoring visits	10.655	11.138	11.690	11.931	0.373	0.061*	0.037**	0.366	0.231	0.757	0.108
	(0.395)	(0.417)	(0.463)	(0.496)							
Observations	30	30	29	30							
Clinics	30	30	29	30							
Panel B: Community characteristics											
Community knowledge											
Know number of vaccines required (1=Yes, 0=No)	0.949	0.937	0.906	0.951	0.662	0.131	0.972	0.309	0.648	0.128	0.417
	(0.022)	(0.025)	(0.026)	(0.019)							
Perceptions of reasons for parents to miss vaccines											
Negligence from parents	0.848	0.789	0.790	0.874	0.318	0.440	0.653	0.875	0.172	0.257	0.433
	(0.045)	(0.053)	(0.058)	(0.047)							
Lack of knowledge of benefits	0.703	0.690	0.717	0.727	0.902	0.809	0.712	0.799	0.544	0.992	0.952
	(0.069)	(0.065)	(0.060)	(0.061)							
Distance to clinic	0.319	0.373	0.312	0.343	0.287	0.973	0.676	0.361	0.669	0.584	0.708
	(0.055)	(0.053)	(0.053)	(0.058)							
User fees	0.181	0.127	0.145	0.147	0.424	0.647	0.687	0.769	0.688	0.943	0.891
	(0.055)	(0.043)	(0.050)	(0.048)							
Staff attitude	0.101	0.176	0.101	0.091	0.169	0.986	0.765	0.218	0.097*	0.766	0.359
	(0.041)	(0.046)	(0.038)	(0.031)							
Observations	142	147	138	148							
Clinics	30	30	29	30							

This table summarizes relevant clinic and community characteristics collected at the start of the experiment. The table reports mean values of each variable for every treatment group. The final column reports the joint significance level of treatment indicators in a regression with strata-level fixed effects. The value displayed for t-tests and F-tests are p-values. Standard errors are clustered at the clinic level for regressions assessing community characteristics. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table 3.3: Visibility of Bracelets by Treatment Group

	(1)	(2)
Dependent variable:	**Know if other child has bracelet**	**Others know own child's bracelet color**
Signal at 4	0.028	0.046
	(0.032)	(0.037)
Signal at 5	0.005	0.042
	(0.018)	(0.038)
Uninformative Bracelet mean	0.896	0.768
Observations	3340	3130
Age of child	Yes	Yes
Relationship to mother	Yes	Yes

This table shows endline respondents' first- and second-order beliefs about the visibility of bracelets. The unit of observation is a respondent-other mother pair. Column (1) reports first-order beliefs, asking respondents if they know if another (randomly selected, but to the respondent known) child in their community has a bracelet. Know if other child has bracelet is a dummy variable that equals one if the respondent answered "Yes" and zero if the respondent answered "No" or "Don't know". The sample includes answers from all endline respondents across the three bracelet treatments. Column (2) reports second-order beliefs, asking respondents if they thought that another (randomly selected, but to the respondent known) mother in their community knew what color bracelet their own child had. Others know own child's bracelet color is a dummy variable that equals one if the respondent answered "Yes" and zero if the respondent answered "No" or "Don't know". All regressions include strata-fixed effects and controls for child age and relationship to other mother. Controls are demeaned. Standard errors are cluster bootstrapped (1000 repetitions) at the clinic level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.4: The Effect of Signals on First- and Second-Order Beliefs about Vaccine Decisions

Dependent variable:	**Know # of vaccines other children**		**Others know # vaccines own child**	
	(1)	(2)	(3)	(4)
	>3.5 months age	>9 months age	>3.5 months age	>9 months age
Signal at 4	0.079**	0.103*	0.130***	0.134**
	(0.038)	(0.058)	(0.041)	(0.061)
Signal at 5	0.097***	0.100*	0.103**	0.170***
	(0.034)	(0.056)	(0.047)	(0.065)
Uninformative Bracelet	0.056	0.062	0.084**	0.085
	(0.037)	(0.052)	(0.043)	(0.075)
Control Group mean	0.465	0.451	0.472	0.456
Observations	4028	1437	4485	1626
Age of child	Yes	Yes	Yes	Yes
Relationship to mother	Yes	Yes	Yes	Yes
p(UI = S4)	0.557	0.439	0.201	0.468
p(UI = S5)	0.270	0.442	0.642	0.229
p(S5 = S4)	0.632	0.955	0.489	0.485
Joint F-test	0.014	0.253	0.018	0.063

This table shows results from endline respondents' first- and second-order beliefs about other children's and own child's vaccinations. I linked respondents' answers with administrative records to assess the correctness of first-order beliefs; that is, if respondents had more accurate beliefs about other parents' vaccine decisions. The unit of observation is a respondent-other mother pair. Columns (1)-(2) show regression results of a binary variable for correct knowledge of the number of vaccinations another child has received (~ first-order beliefs) on treatment indicators for Signal at 4, Signal at 5 and Uninformative Bracelet, with the Control Group as excluded category. The outcome variable is coded one if respondents correctly guessed the number, and zero if the answer was incorrect or the respondent answered "Don't know". Column (1) displays the result for the sample of other children ages 3.5 months and above (i.e. who are eligible for Vaccine 4 and therefore receive a green bracelet in Signal at 4); Column (2) the results for other children ages 9 months and above (i.e. who are eligible for Vaccine 5 and therefore receive a green bracelet in Signal at 5). Columns (3)-(4) show regression results of a binary variable for respondent's belief about another mother's knowledge of her own child's number of vaccinations (~ second-order beliefs). The outcome variable is coded one if a respondent answered "Yes", i.e. the other mother knows, and zero if a respondent answered "Don't know" or "No", i.e. the other mother does not know. Column (3) displays the result for the sample of own children age 3.5 months and above (i.e. who are eligible for Vaccine 4 and a green bracelet in Signal at 4 therefore); Column (4) displays the results for own children age 9 months and above (i.e. who are eligible for Vaccine 5 and a green bracelet in Signal 5 therefore). The bottom rows give the p-values from a test that the effect of the Uninformative Bracelet (UI) is equivalent to the effect of Signal at 4 (S4) or to Signal at 5 (S5), and that the effect of Signal at 4 is equivalent to that of the Signal at 5. Last is a joint hypothesis test of all three bracelet treatments. All regressions include strata-fixed effects and controls for child age and relationship to other mother. Controls are demeaned. Standard errors are cluster bootstrapped (1000 repetitions) at the clinic level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.5: The Combined Effect of Signals at 4 and 5 on Timely and Complete Vaccination

Dependent variable:	**Vaccine 1**	**Vaccine 2**	**Vaccine 3**	**Vaccine 4**	**Vaccine 5**
	(1)	(2)	(3)	(4)	(5)
Panel A:	Compared to Control Group				
Signal at 4 and 5	0.005	0.031	0.046	0.065*	0.084**
	(0.014)	(0.022)	(0.030)	(0.035)	(0.040)
	[0.611]	[0.065]	[0.080]	[0.053]	[0.024]
Control Group mean	0.971	0.924	0.848	0.731	0.538
Observations	5753	5429	5006	4536	1819
Panel B:	Compared to Uninformative Bracelet				
Signal at 4 and 5	-0.002	0.005	0.016	0.040	0.031
	(0.007)	(0.009)	(0.019)	(0.031)	(0.045)
	[0.983]	[0.660]	[0.448]	[0.178]	[0.390]
Uninformative Bracelet mean	0.978	0.949	0.878	0.758	0.602
Observations	5702	5383	4981	4513	1806

This table shows results from a linear probability model of the binary outcome variable for a child being vaccinated for 1, 2, 3, 4, or 5 vaccinations by the age of 3, 4, 5, 6 and 11.5 months, respectively, on a treatment indicator for Signal at 4 and 5, with the omitted category being the Control Group in Panel A and the Uninformative Bracelet in Panel B. The sample includes all children born since the launch of the experiment. All regressions include strata-fixed effects, the demeaned control for child age, and an indicator that is coded one if the vaccine entry comes from the administrative data. Standard errors are cluster bootstrapped (1000 repetitions) at the clinic level. Values in brackets [] show the p-values from randomization inference, that were computed using the *ritest* command in Stata with treatment being randomly reassigned 5000 times.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.6: The Effect of Signals on Timely and Complete Vaccination, Separate by Treatment

Dependent variable:	**Vaccine 1**	**Vaccine 2**	**Vaccine 3**	**Vaccine 4**	**Vaccine 5**
	(1)	(2)	(3)	(4)	(5)
Signal at 4	0.001	0.021	0.022	0.028	0.038
	(0.016)	(0.022)	(0.032)	(0.042)	(0.041)
Signal at 5	0.010	0.043^{**}	0.071^{**}	0.106^{***}	0.137^{***}
	(0.015)	(0.022)	(0.033)	(0.038)	(0.043)
Uninformative Bracelet	0.006	0.025	0.029	0.025	0.055
	(0.014)	(0.020)	(0.031)	(0.040)	(0.051)
Control Group mean	0.971	0.925	0.849	0.734	0.547
Observations	7482	7052	6509	5909	2350
$S_4 > 0$: p(UI = S4)	0.581	0.709	0.784	0.914	0.715
$S_5 > 0$: p(UI = S5)	0.660	0.109	0.064	0.016	0.076
p(S4 = S5)	0.368	0.070	0.044	0.023	0.003
Joint F-test	0.796	0.119	0.082	0.013	0.005

This table shows results from a linear probability model of the binary outcome variable for a child being vaccinated for 1, 2, 3, 4, or 5 vaccinations by the age of 3, 4, 5, 6 and 11.5 months respectively on treatment indicators for Signal at 4, Signal at 5 and Uninformative Bracelet, with the Control Group as the excluded category. The sample includes all children born since the launch of the experiment. The bottom rows give the p-values from a test that the effect of the Uninformative Bracelet (UI) is equivalent to the effect of Signal at 4 (S4) or to Signal at 5 (S5), identifying social signaling preferences ($S_4 > 0$, $S_5 > 0$), and that the effect of Signal at 4 is equivalent to the Signal at 5. Last is a joint hypothesis test of all three bracelet treatments. All regressions include strata-fixed effects, the demeaned control for child age, and an indicator that is coded one if the vaccine entry comes from the administrative data. Standard errors are cluster bootstrapped (1000 repetitions) at the clinic level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.7: The Effect of Signals on the Total Number of Vaccines Completed

Dependent variable:	**Total # of vaccines timely**	**Total # of vaccines by one year age**
	(1)	(2)
Signal at 4	0.083	0.203**
	(0.123)	(0.084)
Signal at 5	0.391***	0.232***
	(0.119)	(0.084)
Uninformative Bracelet	0.187	0.166**
	(0.137)	(0.084)
Control Group mean	3.973	4.482
Observations	2350	1972
$S_4 > 0$: p(UI = S4)	0.380	0.569
$S_5 > 0$: p(UI = S5)	0.090	0.267
p(S4 = S5)	0.002	0.650
Joint F-test	0.002	0.042

This table shows results from regression of the discrete variable "total number of vaccines", coded 1, 2, 3, 4 or 5, on the treatment indicators Signal at 4, Signal at 5 and Uninformative Bracelet, with the Control Group as the omitted category. The sample includes all children born since the launch that were at least 11.5 months old (Column (1)) and 12 months old (Column (2)) by the end of the experiment. Column (1) shows treatment effects on the total number of timely vaccines received, that is by age 3, 4, 5, 6 and 11.5 months for vaccines 1, 2, 3, 4 and 5; Column (2) shows treatment effects on the total number of vaccines received by the age of 12 months, irrespective of the time of vaccination. The bottom rows give the p-values from a test that the effect of the Uninformative Bracelet (UI) is equivalent to the effect of Signal at 4 (S4) or to Signal at 5 (S5), identifying social signaling preferences ($S_4 > 0$, $S_5 > 0$), and that the effect of Signal at 4 is equivalent to the Signal at 5. Last is a joint hypothesis test of all three bracelet treatments. All regressions include strata-fixed effects and the demeaned control for child age and an indicator that is coded one if the vaccine entry comes from the administrative data. Standard errors are cluster bootstrapped (1000 repetitions) at the clinic level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.8: The Extensive Margin Effect of Bracelets: Complete Vaccination by Age one Year

Dependent variable:	**Vaccine 1**	**Vaccine 2**	**Vaccine 3**	**Vaccine 4**	**Vaccine 5**
	(1)	(2)	(3)	(4)	(5)
Signal at 4	0.005	0.011	0.020	0.054^{*}	0.101^{**}
	(0.008)	(0.009)	(0.016)	(0.030)	(0.047)
Signal at 5	0.000	0.007	0.018	0.052^{*}	0.135^{***}
	(0.009)	(0.011)	(0.017)	(0.030)	(0.045)
Uninformative Bracelet	0.003	0.008	0.018	0.058^{**}	0.080^{*}
	(0.008)	(0.010)	(0.015)	(0.029)	(0.046)
Control Group mean	0.989	0.978	0.953	0.881	0.676
Observations	1972	1972	1972	1972	1972
p(UI = S4)	0.737	0.781	0.843	0.859	0.578
p(UI = S5)	0.716	0.904	0.975	0.764	0.104
p(S4 = S5)	0.540	0.743	0.890	0.911	0.309
Joint F-test	0.903	0.729	0.645	0.242	0.026

This table shows results from a linear probability model of the binary outcome variable for a child being vaccinated for 1, 2, 3, 4, or 5 vaccinations by the age of 12 months - ignoring whether a child received a given vaccine on time - on treatment indicators for Signal at 4, Signal at 5 and Uninformative Bracelet, with the Control Group as the excluded category. The sample includes all children born since the launch of the experiment that were 12 months or older when last observed. The bottom rows give the p-values from a test that the effect of the Uninformative Bracelet (UI) is equivalent to the effect of Signal at 4 (S4) or to Signal at 5 (S5), and that the effect of Signal at 4 is equivalent to the Signal at 5. Last is a joint hypothesis test of all three bracelet treatments. All regressions include strata-fixed effects and the demeaned control for child age and an indicator that is coded one if the vaccine entry comes from the administrative data. Standard errors are cluster bootstrapped (1000 repetitions) at the clinic level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.9: Structural Estimation Results Dynamic Discrete-Choice Model

Parameter	**Estimate**	**SE**	**Estimate**	**SE**
	Compared to Control Group		Compared to Uninformative Bracelet	
S_5	0.686	0.109	0.431	0.084
S_4	-0.131	0.098	-0.305	0.096
κ	-0.066	0.008	-0.056	0.009
μ_v	0.824	0.047	1.095	0.062
σ_v	0.284	0.055	0.592	0.058
Signaling utility $\frac{S_5}{\kappa}$	**10.39 miles**		**7.7 miles**	

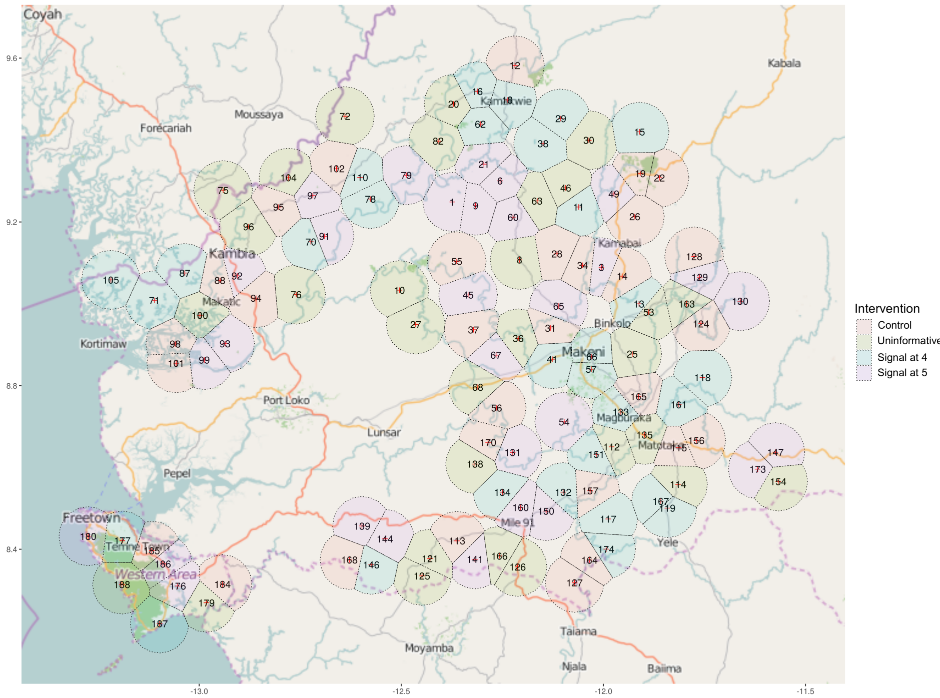
This table shows the parameters estimated from the dynamic-discrete choice model. S_5 and S_4 denote the parameters capturing the signaling utility of treatments Signal at 5 and Signal at 4, κ denotes the parameter measuring the marginal disutility of walking one miles, μ_v and σ_v capture the mean and standard deviation of the normal type distribution. The sample used for the estimation is the same as used in the reduced form estimations, that is, all children that were born since the start of the experiment. Regular standard errors are reported (not clustered). Columns 1 and 2 report the results from the estimation, with the effect of signals being compared to the Control Group. Columns 3 and 4 report the results from the estimation, with the effect of signals being compared to the Uninformative Bracelet.

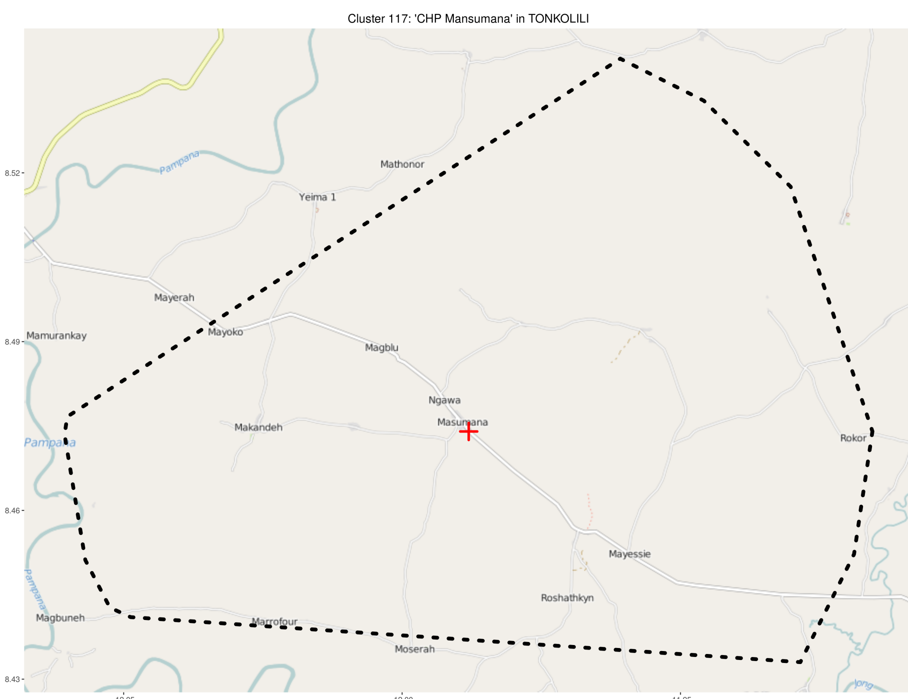
Appendix C: Supplementary Figures and Tables

Figure 3.16: Babies wearing Bracelets

Mothers sitting outside a clinic, waiting for their child to be vaccinated. The children in this photo are wearing yellow "1st visit" bracelets on their wrist.

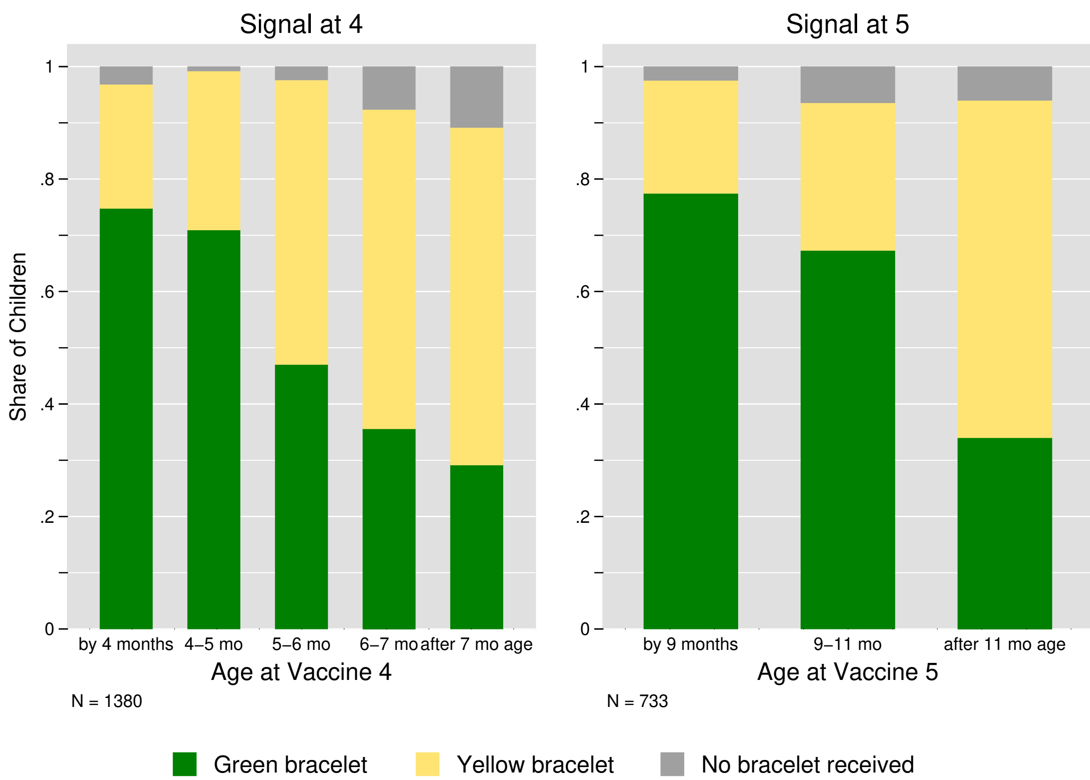
Figure 3.17: Process of Clinic and Community Selection





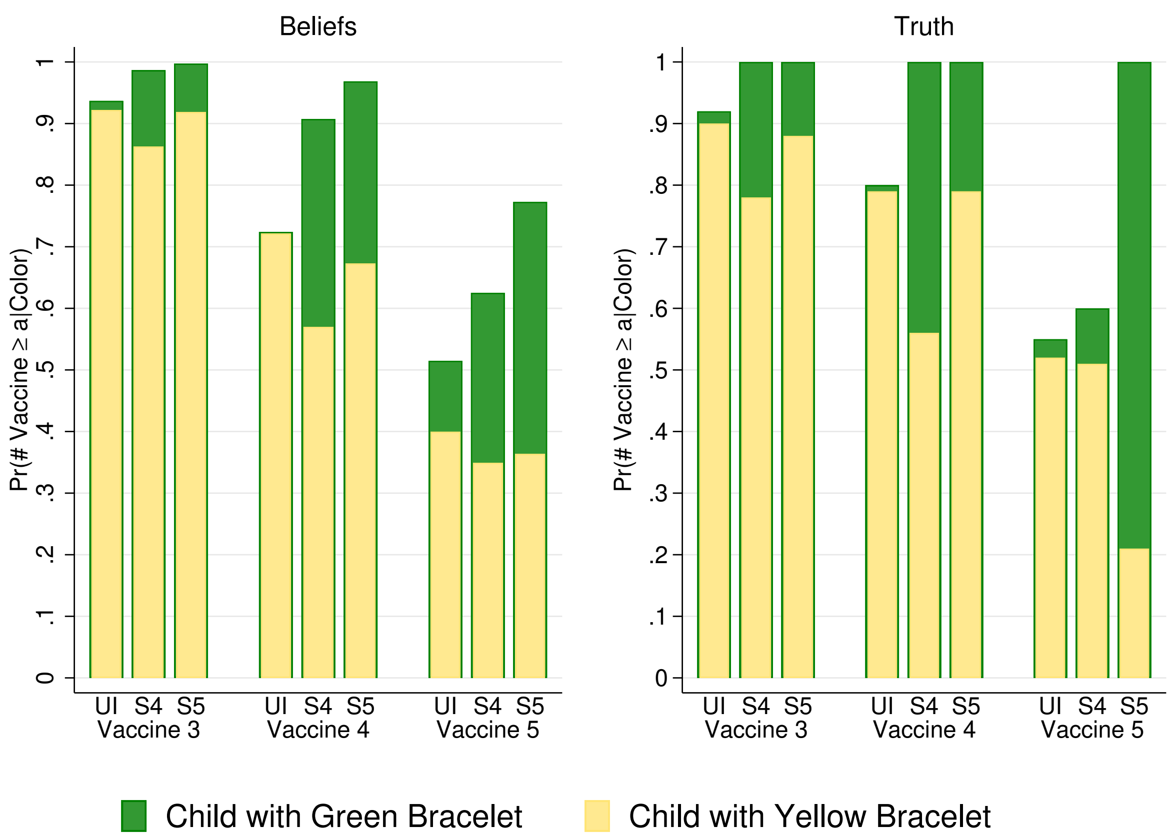
The upper map displays the 120 selected clinics and their non-overlapping catchment areas, with radius of five miles around each clinic. The bottom map displays one out of the 120 maps that surveyors were subsequently given, that showed the area that is non-overlapping and from which they would select five communities (two at close, 0-2 miles distance from the clinic and three communities at far, 2-5 miles distance).

Figure 3.18: Hand Out of Green Bracelets in Signals at 4 and 5 according to Timely Vaccination



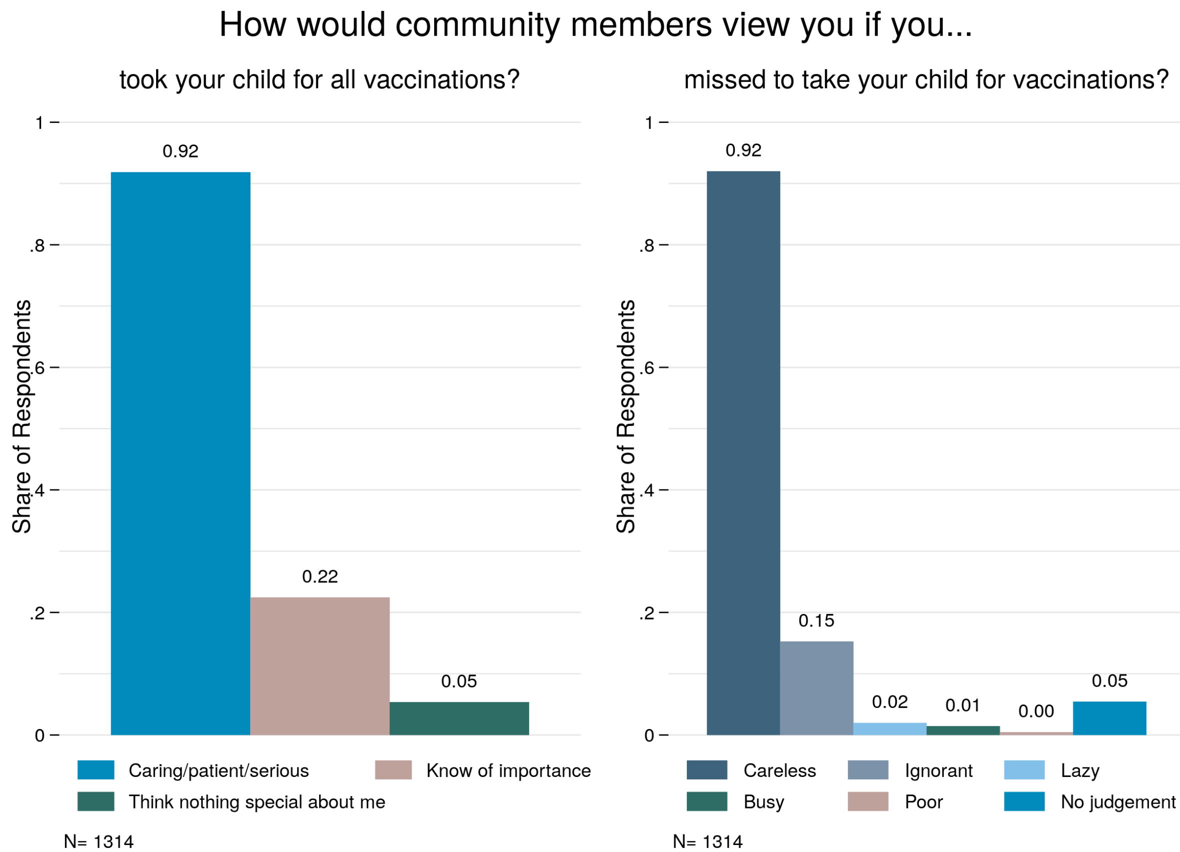
This figure shows the share of children with a green or yellow bracelet according to the time they took vaccine four and five in Signal at 4 and Signal at 5 treatments. Health workers were instructed to give the child a green bracelet if it came for vaccine four before six months of age (Signal at 4) and vaccine 5 by 11 months of age (Signal at 5). If a child came after this time, health workers were instructed to exchange the green bracelet for a new yellow “1st visit” bracelet instead. The sample includes children that were born since the start of the experiment. The column on the left (Signal at 4) shows that the probability of receiving a green bracelet is monotonically decreasing in the age at which the child took vaccine four, from 74.45 percent if the vaccine was taken by four months age, to 70.07, 46.34, 34.78 and 28.81 percent if the child received the vaccine by 5, 6 or 7 months, or after 7 months age. The column on the right (Signal at 5) shows a similar pattern: the probability of receiving a green bracelet is monotonically decreasing in the age at which the child comes for vaccine five, from 77.45 percent if the vaccine was taken by 9 months age, to 67.30 and 34 percent by 11 months and after 11 months age.

Figure 3.19: Stated Beliefs Compared to Beliefs under Bayesian Learning



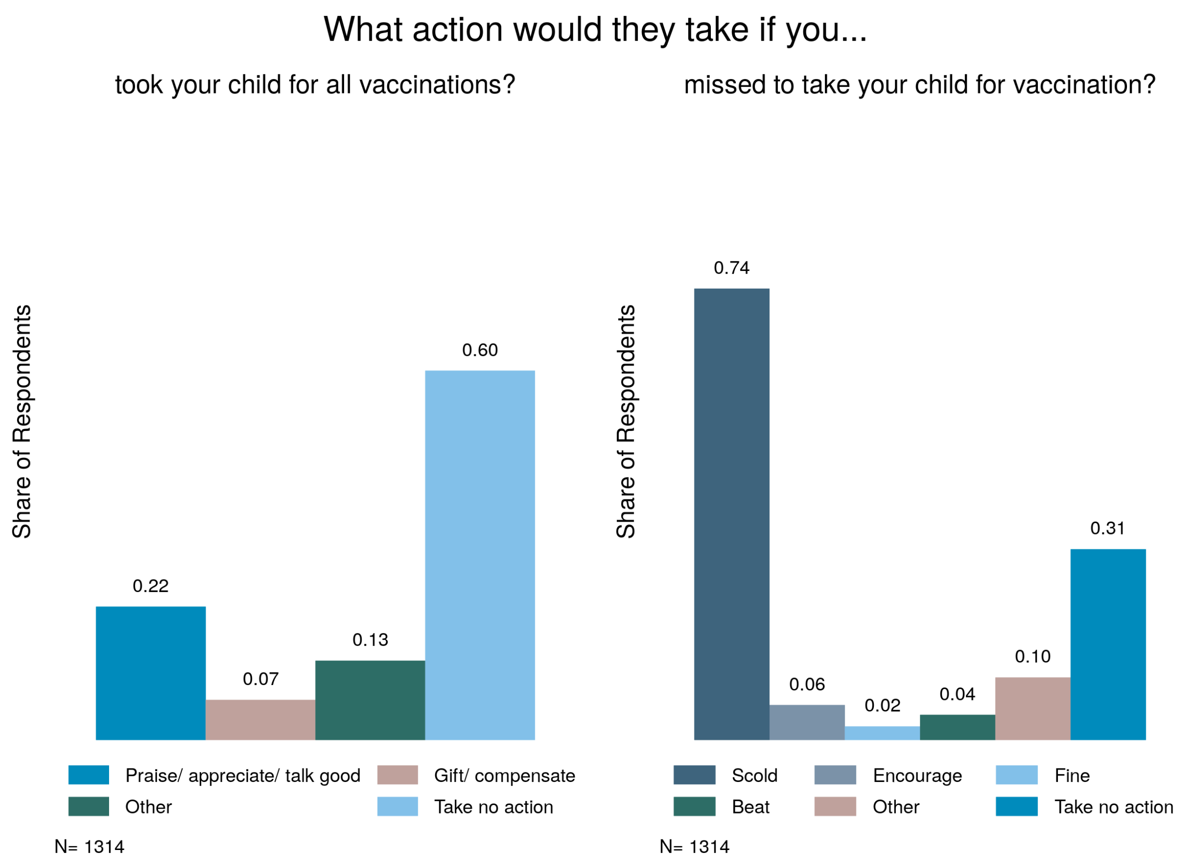
This figure compares individuals' beliefs about the number of vaccines children received conditional on having a yellow or green bracelet ("Beliefs"), to beliefs under Bayesian learning ("Truth"). The latter are simulated using the observed true vaccination outcomes from the survey and administrative data and the probabilities of a child receiving a green or yellow bracelet for a given vaccination and vaccine age from the observed implementation (see Figure 3.4). Same as in Figure 3.5, beliefs are shown by vaccine, and by treatment, where UI = Uninformative Bracelet, S4 = Signal at 4, S5 = Signal at 5.

Figure 3.20: Inferences about Types Conditional on Vaccine Decisions



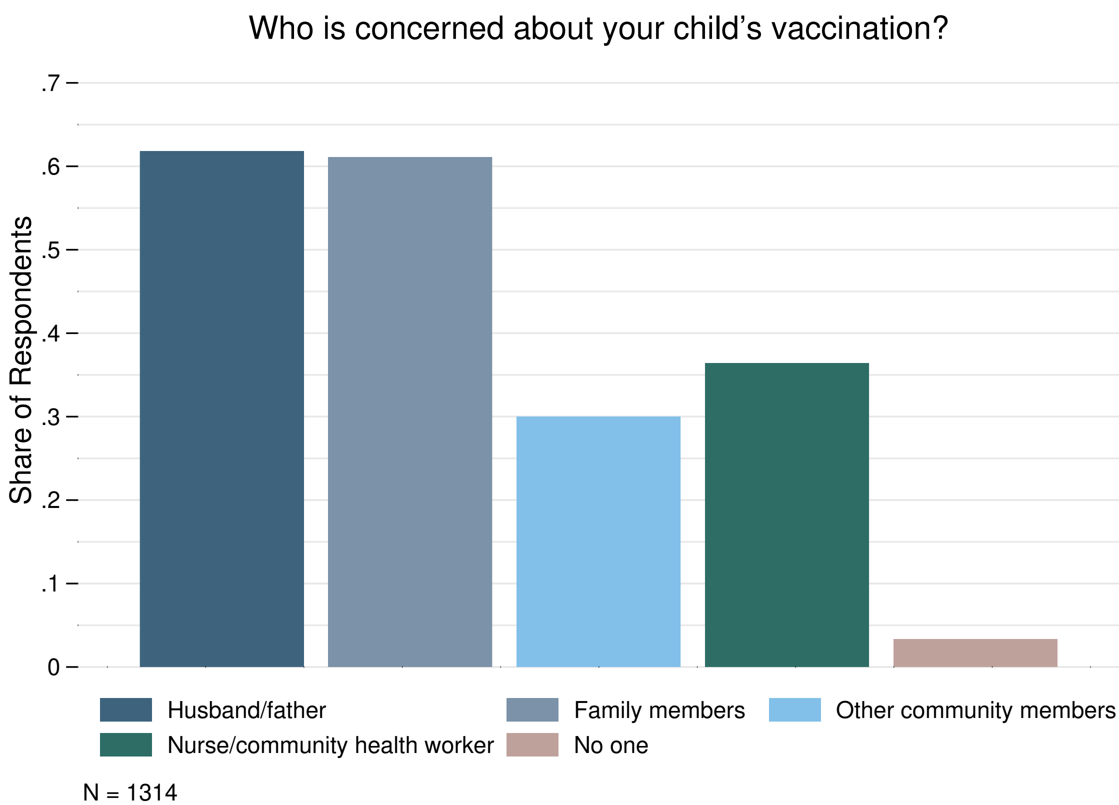
This figure shows mothers' beliefs about the inferences that community members would make, conditional on observing that they took their child for all vaccinations or missed any. The sample includes all endline survey respondents. There are no significant differences for these responses across treatment arms.

Figure 3.21: Motives for Social Signaling



This figure shows mothers' beliefs about the actions that community members would take, conditional on observing that they took their child for all vaccinations or missed any. The sample includes all endline survey respondents.

Figure 3.22: Reference Groups for Social Signaling



This figure displays the different reference groups mothers believe are in general concerned about their own child's vaccinations and might form opinions about their actions. The sample includes all endline survey respondents.

Table 3.10: The Effect of Signals on Preferences for Different Vaccinations

Dependent variable:	**Vaccine 1**	**Vaccine 2**	**Vaccine 3**	**Vaccine 4**	**Vaccine 5**	**All vaccines**
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:			Most Important Vaccine			
Signal at 4	-0.022	-0.012	0.006	-0.015	0.014	0.038
	(0.047)	(0.010)	(0.004)	(0.011)	(0.026)	(0.048)
Signal at 5	-0.021	-0.008	0.003	-0.019*	0.009	0.041
	(0.053)	(0.013)	(0.003)	(0.011)	(0.031)	(0.040)
Uninformative Bracelet	-0.017	0.000	0.010*	-0.008	-0.013	0.037
	(0.045)	(0.012)	(0.005)	(0.012)	(0.026)	(0.045)
Control Group mean	0.685	0.024	-0.000	0.024	0.108	0.147
Observations	1314	1314	1314	1314	1314	1314
p(UI = S4)	0.912	0.212	0.590	0.467	0.209	0.990
p(UI = S5)	0.947	0.509	0.323	0.253	0.427	0.942
p(S4 = S5)	0.974	0.730	0.588	0.606	0.853	0.956
Panel B:			Second Most Important Vaccine			
Signal at 4	-0.025	0.032	-0.002	0.034	-0.027	-0.006
	(0.026)	(0.058)	(0.033)	(0.030)	(0.049)	(0.019)
Signal at 5	0.020	0.090*	-0.030	0.003	-0.064	-0.006
	(0.036)	(0.053)	(0.031)	(0.027)	(0.047)	(0.018)
Uninformative Bracelet	-0.011	0.022	-0.028	0.027	0.014	-0.015
	(0.025)	(0.055)	(0.029)	(0.029)	(0.048)	(0.017)
Control Group mean	0.099	0.379	0.107	0.056	0.314	0.032
Observations	1075	1075	1075	1075	1075	1075
p(UI = S4)	0.463	0.856	0.370	0.809	0.406	0.533
p(UI = S5)	0.321	0.181	0.921	0.327	0.093	0.469
p(S4 = S5)	0.152	0.285	0.364	0.239	0.413	0.986

Notes: This table shows results from a linear probability model of the binary outcome variables for vaccine 1, 2, 3, 4 or 5, or all vaccines being considered as most (second most) important vaccine on treatment indicators for Signal at 4, Signal at 5 and Uninformative Bracelet, with the Control Group as excluded category. The outcome variable is coded one if an endline survey respondent named a vaccine as being the most (second most) important, and zero otherwise. Panel A shows the results for the question "Which vaccine do you consider the most important?" and Panel B results for the question "Which vaccine do you consider the second most important?". The latter question was only asked conditional on a respondent not having answered "All vaccines" to the first question. The respondent sample in Panel B is therefore smaller. The bottom rows give the p-values from binary comparisons between the Uninformative (UI) and Signal at 4 (S4) and Signal at 5 (S5), testing for any significant differences in preferences between bracelet treatments. All regressions include strata-fixed effects. Standard errors are cluster bootstrapped (1000 repetitions) at the clinic level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.11: Correlation of Distance with Socio-Economic Characteristics

Dependent variable:	**Vaccine 3**	**Vaccine 4**	**Vaccine 5**	**Vaccine 3**	**Vaccine 4**	**Vaccine 5**
	(1)	(2)	(3)	(4)	(5)	(6)
Distance 1 mile	0.061**	0.058	-0.129	0.067**	0.057	-0.143
	(0.031)	(0.050)	(0.118)	(0.031)	(0.051)	(0.124)
Distance 2 miles	-0.061	-0.038	-0.191*	-0.050	-0.036	-0.233**
	(0.056)	(0.066)	(0.106)	(0.056)	(0.067)	(0.113)
Distance 3 miles	-0.089**	-0.114**	-0.177*	-0.081**	-0.109**	-0.180*
	(0.042)	(0.051)	(0.096)	(0.041)	(0.051)	(0.098)
Distance 4 miles	-0.066**	-0.101**	-0.250***	-0.054*	-0.094**	-0.266***
	(0.030)	(0.046)	(0.091)	(0.031)	(0.046)	(0.087)
Distance 5 miles	-0.082**	-0.074	-0.313***	-0.072**	-0.075*	-0.342***
	(0.034)	(0.046)	(0.101)	(0.034)	(0.045)	(0.103)
Child age				-0.000**	-0.001***	-0.000
				(0.000)	(0.000)	(0.001)
Birth order				0.003	-0.022	-0.026
				(0.012)	(0.016)	(0.033)
Mother age				-0.001	0.001	-0.006
				(0.003)	(0.004)	(0.008)
Floor cement				0.022	0.036	0.074
				(0.019)	(0.030)	(0.066)
Roof corrugated iron				0.043	0.035	0.032
				(0.034)	(0.049)	(0.115)
Has any education				0.030***	0.024*	-0.016
				(0.011)	(0.014)	(0.032)
Works on farm				0.033	0.061	0.228
				(0.030)	(0.058)	(0.138)
Trader				0.023	0.010	0.118
				(0.033)	(0.062)	(0.158)
Outcome Mean	1.006	0.965	0.760	0.908	0.890	0.887
Observations	1077	958	247	1077	958	247

This table shows the effect of distance on timely completion of vaccine 3, 4 and 5, comparing treatment effects from regressions without and with covariates. Columns (1)-(3) show regression results without covariates, and columns (4)-(6) results for the same specification with covariates. The covariate child age is coded in days, mother age in years; the variable birth order takes values 1 through 6. All regressions include strata-fixed effects. Standard errors are clustered at the clinic level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.12: Test of the Equality of Distance Coefficients from Regressions with and without Covariates

	Distance 1 mile	**2 miles**	**3 miles**	**4 miles**	**5 miles**
	(1)	(2)	(3)	(4)	(5)
Vaccine 5	0.021	0.023	0.001	0.011	0.033
	(0.039)	(0.030)	(0.019)	(0.026)	(0.028)
Observations	247	247	247	247	247
Vaccine 4	0.005	-0.003	-0.006	-0.007	0.002
	(0.011)	(0.008)	(0.008)	(0.011)	(0.009)
Observations	958	958	958	958	958
Vaccine 3	-0.003	-0.011	-0.009	-0.013*	-0.012
	(0.009)	(0.007)	(0.006)	(0.008)	(0.007)
Observations	1077	1077	1077	1077	1077

Notes: This table tests for the equality of the coefficients from the regressions of vaccine 5, 4, and 3 on distance dummy variables with and without covariates (see 3.11), using seemingly-unrelated estimation. The table displays the difference in coefficients and the associated p-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.13: Additional Information on Bracelet Retention and Correct Bracelet Hand Out

Dependent variable:	**Child wears bracelet**	**Child lost bracelet**	**Bracelet was exchanged**
	(1)	(2)	(3)
Signal at 4	-0.058	0.024	0.021
	(0.060)	(0.042)	(0.072)
Signal at 5	-0.019	-0.063*	-0.057
	(0.055)	(0.037)	(0.065)
Uninformative Bracelet mean	0.370	0.224	0.629
Observations	3901	941	742
p(S4 = S5)	0.523	0.008	0.281

This table shows results from a linear probability model of the binary outcome variables (1) for a child wearing a bracelet when observed during the listing survey, (2) whether a child still had or lost her bracelet at endline, and (3) whether a child's bracelet was exchanged when it came for vaccine 4 or 5, on treatment indicators Signal at 4 and Signal at 5, with the Uninformative Bracelet as the omitted category. The sample used for (1) includes all children that were born since the experiment was launched and were physically present during the listing, and surveyors could see the wrist of the child. The sample for (2) includes all children in bracelet treatments that were part of the endline survey; sample (3) does the same but conditions on a child having received vaccine 4 or 5 (as otherwise the child would not have been eligible for an exchange of the bracelet). All regressions include strata-fixed effects. Standard errors are clustered at the clinic level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.14: Supplementary to Table 3.13, Column (1)

Dependent variable:	**Child wears bracelet**
Child age	-0.0008***
	(0.0001)
Outcome Mean	0.5145
Observations	3898

This table shows results from a linear probability model of the binary outcome variable for a child wearing a bracelet when observed during the listing survey on the continuous variable child age, measured in days. Data is pooled across Signal at 4, Signal at 5 and Uninformative Bracelet as no significant differences for "Child wears bracelet" were found in Table 3.13, Column (1). Around 50 percent of children age 3 months or below wear the bracelet when visited during the listing survey. The probability declines to 40 and 33 percent for children of age 3 to 6, and 6 to 10 months respectively. Among children that are 12 months or older, 22 percent wear the bracelet. When asking parents during endline, why the child is not wearing the bracelet, the most common answer was that they are afraid of the child losing the bracelet by biting on it or playing with it. Parents further report that the child wears the bracelet when going to the clinic or on special occasions, when visiting relatives or at community events. The regression includes strata-fixed effects. Standard errors are clustered at the clinic level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.15: Relationship of Respondent to Other Mother.

	Percent
Regular community member	39
Neighbor	14
Relative	35
Friend	7
Other carer	5
Total	100
Observations	5,573

This table displays the social connection between endline respondents and a sample of randomly selected (other) mothers in their community, conditional on the respondent recognizing the other mother. There are 5,573 respondent-other mother pairs in my endline sample, across all four treatment groups, including 1,304 unique respondents and 2,348 unique other mothers from 119 clinics. Ten endline respondents across all treatments (less than 1% of the sample) did not recognize any of the other mothers.

Table 3.16: Number of Clinics and Children across Four Districts

District	**Clinics**					**Children**				
	Control	Signal 4	Signal 5	Uninform	All	Control	Signal 4	Signal 5	Uninform	All
Bombali	11	11	11	11	44	442	629	551	471	2093
Kambia	6	6	6	7	25	425	371	399	394	1589
Tonkolili	11	11	10	10	42	577	766	622	507	2472
WA Rural	2	2	2	2	8	84	75	65	131	355
Total	30	30	29	30	119	1528	1841	1637	1503	6509

The sample includes all children that were born since the start of the experiment, are from one of the selected catchment communities, attend one of the study clinics, and had at least reached the timeliness cut-off for vaccine three. The sample is slightly larger when also including children that were younger (and are included in the estimation of treatment effects for vaccine one and two) and smaller when excluding children that had not yet reached the timeliness cut-off for vaccine four and five (which results in a smaller sample used in the estimation of treatment effects for vaccine four and five). The clinic randomization was stratified by district. One clinic of the 120 selected, located in Western Area (WA) Rural district is excluded from the analysis due to serious complications in the implementation and data collection.

Table 3.17: Number of Communities and Children by Distance to Clinic

Treatment	**All Communities**			**Close (0-2 miles)**			**Far (2-5 miles)**		
	Coms	Distance	Children	Coms	Distance	Children	Coms	Distance	Children
Control	144	1.88 (1.76)	1522	65	0.63 (0.81)	939	78	3.83(0.84)	583
Signal 4	145	1.83 (2.05)	1841	57	0.41 (0.76)	1116	88	4.01 (0.84)	725
Signal 5	141	2.11 (1.90)	1623	69	0.78 (0.87)	963	70	3.99 (0.85)	660
Uninform	148	2.08 (1.99)	1503	61	0.45 (0.78)	833	87	4.11 (0.84)	670
Total	578	2.03 (2.01)	6489	252	0.56 (0.82)	3851	323	3.99 (0.85)	2638

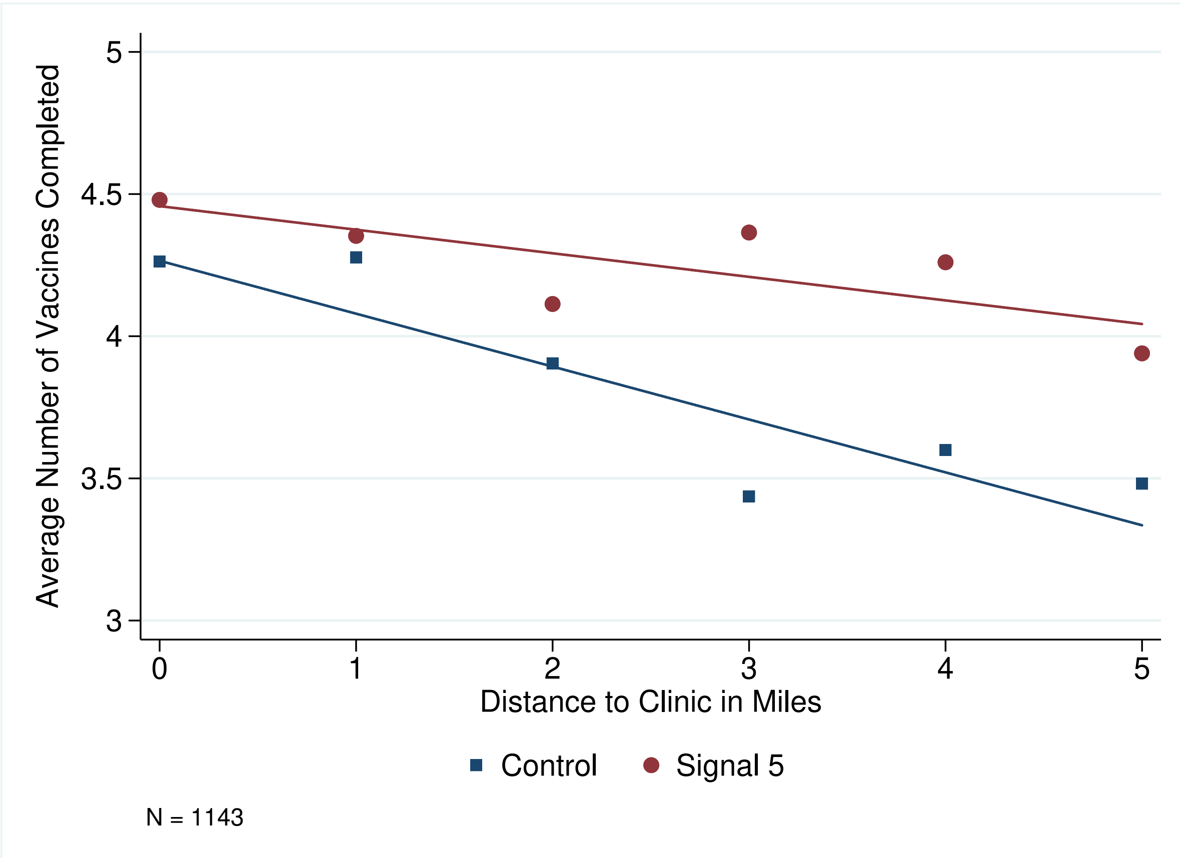
For each clinic, surveyors selected five communities, using in-field randomization. Surveyors obtained a list of all catchment communities from clinic staff. A community was considered as eligible for selection if i) it was primarily served by the clinic (instead of another close-by clinic), ii) if it had at least ten dwelling units (a dwelling unit has on average between three to four households), iii) the community was not an outreach point i.e. community where health workers would regularly travel to vaccinate children. Among the five communities, one was by default the clinic community. In addition, one other close (located up to two miles distance from the clinic) community was randomly selected. Three far communities (located further than two miles up to five miles distance from the clinic) were selected. For clinics that had fewer than three far or two close communities, surveyors were asked to replace the community with another close or far community instead. Means reported. Standard deviation in parentheses.

Table 3.18: Verifying the Correct Implementation of Bracelets, Regression Results for Figure 3.4

Dependent variable:	**Signal at 4**		**Signal at 5**		**Uninformative Bracelet**	
	Green	**Yellow**	**Green**	**Yellow**	**Green**	**Yellow**
	(1)	(2)	(3)	(4)	(5)	(6)
Vaccine 2	0.013	0.095***	0.012***	0.042	0.040	0.069
	(0.009)	(0.033)	(0.004)	(0.040)	(0.060)	(0.054)
Vaccine 3	0.035*	0.107**	0.018**	0.053**	0.070*	0.034
	(0.019)	(0.044)	(0.007)	(0.021)	(0.036)	(0.044)
Vaccine 4	0.613***	-0.437***	0.044***	0.044	0.063	0.064*
	(0.049)	(0.079)	(0.006)	(0.027)	(0.039)	(0.034)
Vaccine 5	0.643***	-0.502***	0.706***	-0.608***	0.106***	0.018
	(0.036)	(0.067)	(0.046)	(0.054)	(0.041)	(0.041)
Vaccine 1 mean	0.003	0.798	-0.005	0.867	0.294	0.546
Observations	2018	2018	1803	1803	1615	1615

This table shows the regression results of a binary variable for green or yellow bracelet on the total number of vaccines a child has received and strata fixed effects, with standard errors cluster bootstrapped (1000 repetitions) at the clinic level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 3.23: The Effect of Distance on the Total Number of Vaccines Completed



The graph plots a bin scatter of the average number of timely vaccines completed against the travel distance from communities to clinics, separately for the Control Group and Signal at 5. The sample includes all children born since the launch that were at least 11.5 months old by the end of the experiment, to be considered for all five vaccinations. The plot shows that distance has a linear effect on the number of vaccinations completed in the Control Group. Signal at 5 mitigated the negative effect of distance: the average total number of vaccines completed at zero miles in the Control Group (4.25) is equivalent to the average number completed at 4 miles in Signal at 5.

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