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Harming to Signal: Child Marriage vs. Public Donations in Malawi

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Harming to Signal: Child Marriage vs. Public Donations in Malawi*

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June 20, 2020

Abstract

Do people conform to social norms at least partly to signal their social preferences? Using a vignette experiment, we find that parents who *do not* marry off their under-age daughters in Malawian villages where child marriage is prevalent are perceived as *less* altruistic, reciprocal, and trustworthy. If parents indeed “harm to signal” in this setting, could alternative signals encourage them to abandon the practice, by offering them other means of showcasing pro-sociality? Randomly assigning public donation drives across 412 villages, we find that those who do not support child marriage are no longer perceived as less pro-social than others in treated high-prevalence villages. Consistent with a new signaling equilibrium, child marriage and teenage pregnancies decrease by nearly 30% in those villages, one year after the intervention.

Keywords: Child Marriage; Social Norms; Social Preferences; Signaling

JEL Classifications: D91, J12, Z10

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1 Introduction

Over 650 million women alive today married before they turned 18 years old.¹ As many traditional practices, child marriage can be characterized as a *social norm*: in communities where it is prevalent, it is a normative prescription of behavior – individuals expect others to conform to it, and those who do not conform are expected to experience social sanctions (Fehr and Schurtenberger, 2018). Among those sanctions, this paper documents first-hand that one expects to be perceived as *less pro-social* if they were *not to marry off* their under-age daughter where child marriage is prevalent.² Based on this insight, we then investigate whether a public donation drive can decrease child marriage, by offering villagers an alternative to show they are pro-social *even in the counterfactual scenario* in which they no longer conform to the social norm.

Given the systematic evidence about a strong link between conformity to norms and social image (Bursztyn, Fujiwara and Pallais, 2017; Bursztyn, Egorov and Jensen, 2019), and between social image and social preferences (Perez-Truglia and Troiano, 2018), conforming to norms could indeed be a useful *signal to infer* the extent to which someone is altruistic, reciprocal and trustworthy.³ In trust-intensive societies such as Malawi, where there are few formal institutions to enforce transactions, such signals can be quite valuable.⁴ While child marriage could have emerged historically as a social norm for a variety of reasons (Crone, 2015; Brown, 2015), once it is in place what matters are *beliefs about what would happen* if one were to *no longer conform* to the norm. Such beliefs can lead some individuals (those who value cooperation highly) to keep paying even high costs to ensure they are not categorized among those who are unreliable from the perspective of others.

If supporting child marriage is indeed a signal for social preferences, then alternative signals of pro-sociality might crowd out its signaling value. For instance, a public donation drive might encourage some of those who previously supported child marriage out of social image concerns to abandon the practice, as long as they believe that visible donations could help them maintain their reputation at lower costs. Since contributing to the public drive helps enforce cooperation but has private costs, those who value cooperation more highly would be more willing than others to bear those costs – providing the basis for a new signaling equilibrium. In turn, the larger the number of villagers who abandon child marriage, the *less accurate* it becomes to keep associating support for the former with social preferences, making it easier for others to abandon the practice as its signaling value dies out.

We start by documenting an association between one’s conformity to social norms and others’ beliefs about their social preferences. We ask village chiefs across 412 villages in Malawi to rate 5 random villagers with respect to their altruism, reciprocity, and trustworthiness, drawing on

¹See <https://data.unicef.org/resources/child-marriage-latest-trends-and-future-prospects/>.

²40% of girls in Malawi are married before 18; such prevalence remains basically unchanged over the last 30 years. There are striking differences across villages: while the national prevalence of child marriage below 15 is 10%, within our sample, in 38% of the villages no female respondent claims to have gotten married below 15; for the remainder, prevalence ranges from 4 to 41%.

³While a link between conformity to norms and social preferences has been entertained theoretically (Iannaccone, 1992; Bénabou and Tirole, 2006, 2011), empirical evidence remains scarce.

⁴Villages in our sample average 116 households each, suggesting not all households might interact frequently enough to infer others’ reciprocity and trustworthiness without relying on signals. In fact, Ozella et al. (2020) shows that inter-household interactions in Malawian villages are indistinguishable from random networks.

Falk et al. (2016)’s validated measures.⁵ We also ask the village chief to assess whether each of the villagers would be in favor that their daughters get married before they turn 18. Consistent with the existence of a separating equilibrium at baseline, village chiefs perceive those who do *not* support child marriage as significantly *less pro-social* than others in villages where the practice is high-prevalence.

Is this really evidence that conforming to social norms is perceived as a signal of social preferences, or does it merely capture a correlation between support for child marriage and other characteristics correlated with pro-sociality? To investigate whether that link is causal, we resort to a vignette experiment. In the experiment, subjects are randomly assigned to drawings (accompanied by verbal descriptions) either of a Malawian father who supports child marriage, or of an otherwise identical father who does not; the descriptions showcase the exact same family, holding constant observable characteristics such as housing conditions and family structure. Having subjects rate that Malawian father along the three above-mentioned dimensions of pro-sociality, we find that being assigned to the vignette in which the father marries off his under-age daughter leads him to be perceived as less pro-social than his ‘harmless’ version only in villages where prevalence of child marriage is low. In turn, as the local prevalence of child marriage increases, the perceived altruism, reciprocity and trustworthiness of that father increase as well, eventually becoming higher than those of his ‘harmless’ version at the high-end of the distribution of child marriage.

Having shown that conformity to child marriage is a signal of pro-sociality in high-prevalence villages, we turn to the question of whether alternative signals, such as public donations, could substitute for those norms. To do that, we randomly assign some villages to a public donation drive. In each treated village, a *box holder* is selected to coordinate donations, with the incumbency of (i) letting other villagers know that the drive would collect maize (Malawi’s staple crop) to be redistributed to the poorest local households, (ii) of collecting two kilograms of maize per donor, and (iii) of making sure the donation box was publicly displayed. While donations had presumably always been available as a signaling strategy, the intervention is meant to *increase their signaling value* by making them more visible. In treated villages, boxes were introduced during household listing for the baseline survey (which took place 5 weeks later); in control villages, only listing took place at the same time. The drive was meant to be self-organized: other than appointing a box holder, distributing donation boxes and delivering instructions, box holders were in charge of implementing it; in fact, they were informed that the research team would not come back to redistribute the maize collected through the drive nor to verify whether donations in fact reached the poorest in the village.⁶

We first investigate how the public drive affects charitable behavior in the short and the long runs. Within 5 weeks of the intervention, donations to other households significantly increase in treated villages relative to the control group (by about 26%). Such differences are persistent: 16 months later, while 33% of villagers had donated to others over the course of the previous year in the control group, nearly 55% had done so in the treatment group. Strikingly, the

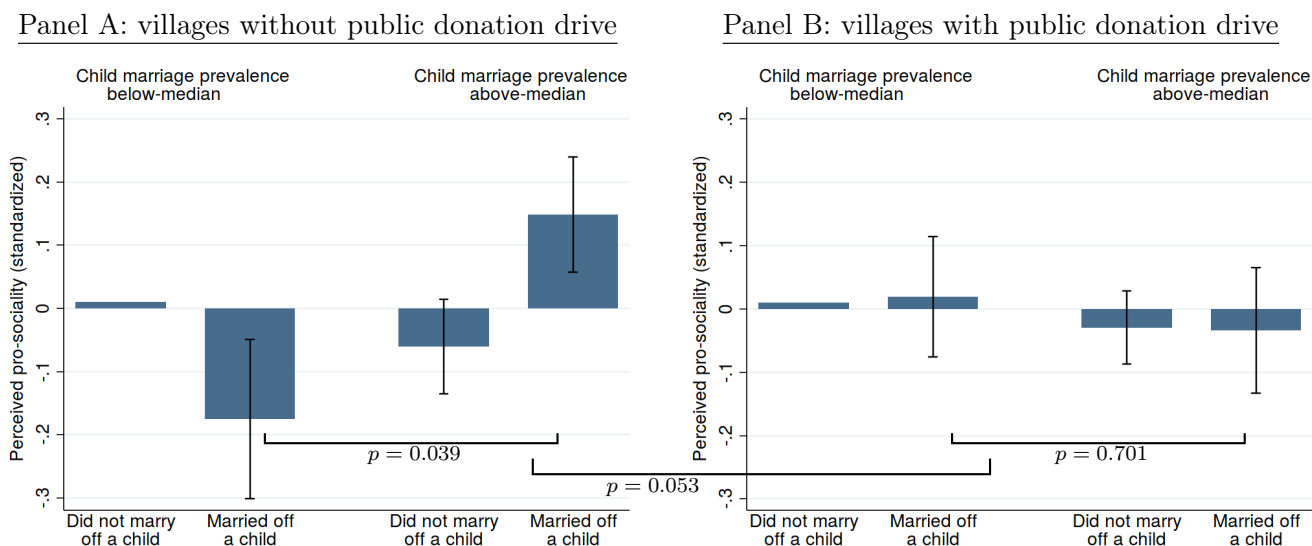
⁵Those pretested survey instruments mimic standard games from experimental economics and have been shown to accurately predict behavior in incentivized choice experiments.

⁶Box holders were randomly assigned: in 50% of treated villages, donations were assigned to be handled by the village chief; in the other 50%, they were handled by another villager (see Haenni and Lichand, 2020).

intervention *changes the profile* of those who donate. In control villages, only a minority of those who support child marriage donate to other households; in contrast, in treated villages, those who support child marriage donate to the same extent as those who oppose it. What is more, the higher the local prevalence of child marriage in the control group, the lower is the share of those married before 18 years old who donate; in the treatment group, this difference disappears, as donations sharply increase in high-prevalence villages. Altogether, the evidence suggests that child marriage and donations might indeed be used as alternative signals.

Next, we turn to the core question of the paper: does the public donation drive affect the nature of the signaling equilibrium? 16 months after the intervention, we re-elicited perceived social preferences about a sample of villagers and link those to their history of child marriage. As Figure 1 shows, in control villages, it is still the case that, where child marriage was high-prevalence at baseline, those who married off a girl before 18 remain perceived as more pro-social. In contrast, in treated villages, child marriage is no longer associated with altruism, reciprocity or trustworthiness. All in all, treatment effects on the profile of those who donate and on the association between child marriage and perceived social preferences provide compelling evidence that the public donation drive disrupted the separating equilibrium based on child marriage.

Figure 1: Perceived pro-sociality conditional on child marriage



Notes: Villagers' pro-sociality as perceived by the survey respondent in villages where the prevalence of child marriage was low vs high at baseline, according to a median split. The pro-sociality measure is an equally weighted, standardized combination of individual measures for altruism, reciprocity, and trust (see Section 3.4.1). Estimates are from an ordinary least squares regression, including village-level controls (village size, population density, and urban) and district and enumerator fixed-effects. Bars stand for standard errors, clustered at the village level. P-values from Wald tests for equality of estimated coefficients.

Since child marriage is no longer a valuable signal of pro-sociality in the treatment group (as documented in Figure 1), are villagers less willing to engage in it as a result? We document that, 16 months after the intervention, child marriage age-by-age is 1.7 percentage points (p.p.) lower in treated villages relative to the control group – a 30% decrease. To get a sense of the magnitudes involved, under the additional assumption that treatment effects persist over time, we estimate that, in control villages, 18% of girls will marry before 15 and 42% before 18,

compared to only 12% and 33%, respectively, in the treatment group. In line with the evidence that child marriage is intimately linked to childbearing and school dropouts (Field and Ambrus, 2008), the expected prevalence of those outcomes is also significantly affected by the intervention: we estimate that, in control villages, 75% of girls will not finish high school and 42% will have children before 18 years old, compared to 70% and 31%, respectively, in the treatment group. Last, consistent with the signaling mechanism, the effects of the intervention are not confined to child marriage, but cut across multiple social norms that harm children: the expected share of girls who undergo sexual initiations rituals by age 13 falls by almost 30% in treated villages relative to the control group.⁷

As our outcomes are measured through surveys (rather than notarized marriage certificates, which do not exist in Malawi), a central concern is whether our estimates conflate experimenter demand effects. Our experiment builds in a series of features to rule out that alternative. First, our intervention was introduced during listing, before we had surveyed any household, and without mentioning child marriage (or even children) at any point, precisely to prevent any association between the intervention and future surveys. Second, at baseline (5 weeks after listing), we elicited whether adult females got married before 18 years old; that variable can be used for a placebo test, since past behavior could not have been affected by the intervention (other than through possible experimenter demand effects). As expected, child marriage rates at baseline do not differ across the treatment and control groups.⁸ Third, using priming techniques from De Quidt, Haushofer and Roth (2018), we find no evidence that reported attitudes are driven by social desirability. Fourth, in our follow-up survey, 16 months later, we refrained from asking households directly about whether their under-age daughters got married in the previous year; instead, we relied on multiple external sources.⁹ We went over the list of all girls who lived in sampled households at baseline, asking respondents to report on several characteristics of each girl – including their marriage status. Our results are robust to focusing exclusively on girls with unanimous information from all respondents.

Are the effects on child marriage really the outcome of a new signal for social preferences? Or, alternatively, do donations disrupt the baseline equilibrium by signaling other components of social image (e.g. wealth)? To study this question, we add additional arms to our experimental design, cross-randomizing villages to host the public donation drive (or not) and to host the distribution of red rubber bracelets (or not). In villages assigned to hosting both the box and bracelets, the latter are distributed in exchange for two kilograms of maize; in those assigned to host the public donation box only, donors get nothing in exchange for their donations; in villages assigned to bracelets only, those are made available for sale, for a price equivalent to two kilograms of maize. In pure control villages, neither public donations were organized nor bracelets distributed or sold. While bracelets could act as an alternative signal of social image (given the tendency for in-group-out-group biases; Everett, Faber and Crockett, 2015), those are

⁷Initiation rituals mark the transition from childhood to adulthood. While they rarely involve female genital cutting, they involve other harmful practices – from labia stretching to rape; see Section 2.

⁸This is not because the time elapsed since the intervention had been insufficient to induce treatment effects: respondents in treated villages were 20% less likely to claim at the time that the right age for a women to marry is less than 18 years old, relative to those in the control group.

⁹Checking whether girls still live with their families (as in Buchmann et al., 2019) would not have been feasible in our setting because Malawi is mainly matrilineal, with husbands commonly moving in with the bride's family.

not perceived as a signal of social preferences *except in villages where they stand for donations*. It turns out bracelets alone do not systematically decrease child marriage and initiation rituals 16 months after the intervention: its effects on such outcomes are sometimes even positive and, when negative, are much smaller than those of public donations and statistically insignificant.

Last, evidence on respondents' willingness to pay (WTP) for the public donation drive at the end of the experiment helps understand why the coordination problem does not get sorted out by villagers themselves. The WTP for the public donation drive increases sharply with prior exposure to treatment in villages where child marriage was high-prevalence at baseline – precisely where treatment effects are the largest. Hence, it does not seem that local leaders purposefully block social norms' change; rather, it seems that they did not anticipate that a public donation drive would unravel long-standing social norms before our study.

Our contribution is two-fold. First, we show that parents might go to incredible heights to *signal their social preferences* – even when that means *destroying* their children's human capital in conforming to social norms. Second, we demonstrate that parents might readily abandon *even long-standing traditions* once social preferences can be signaled by other means. Recent papers document a relationship between signaling and social image.¹⁰ For instance, Karing (2018) and Karing and Karim (2018) show that distributing bracelets for parents who bring their infants in for immunization and de-worming in Sierra Leone and Kenya significantly increases vaccination rates, suggesting that such behavior may also affect how parents are perceived by others in the community. While it is intuitive that conforming to social expectations about having one's child immunized might be driven by a desire to signal pro-sociality, it could also be motivated by other social image concerns (such as in-group bias). Our vignette experiment provides a methodological improvement relative to those papers by explicitly documenting the link between conformity to social norms and how social preferences are perceived by others.

Past research on the mechanisms behind conformity to social norms has mostly focused on preferences (Kearney and Levine, 2015; Vogt et al., 2016), incentives (Buchmann et al., 2019; Corno, Hildebrandt and Voena, 2020; Corno and Voena, 2016; Vogt et al., 2016; Ashraf et al., 2020) and beliefs about prevalence or higher-order beliefs (Campante and Yanagizawa-Drott, 2015; Bursztyn, González and Yanagizawa-Drott, 2018; Bursztyn, Fujiwara and Pallais, 2017; Bursztyn, Egorov and Jensen, 2019; Perez-Truglia and Troiano, 2018; Butera et al., 2019). We document a *new driver of conformity*: signaling social preferences. Such a mechanism might help explain why certain norms might *inefficiently persist* even when individuals dislike it, face private costs from conforming to it, and do not have biased beliefs about what others do or expect them to do.¹¹

¹⁰Outside of the domain of investments in children, Bursztyn et al. (2018) shows that platinum credit cards are used for social signaling, and that social image and self-image are substitutable.

¹¹Following traditions can be beneficial for societies, especially if they live in an environment with high cross-generational stability (Giuliano and Nunn, 2017). However, dynamically inefficient norms may emerge out of static trade-offs (Acemoglu and Robinson, 2006) or coordination problems (Basu, 2018) and can persist even long after conditions that originally gave rise to them are no longer in place.

This new mechanism also sheds light on why long-standing norms may change rapidly – crowded out by the emergence of alternative signals of pro-sociality.^{12,13} The finding that alternative signals could, under certain conditions, *substitute for* social norms in the context of child marriage may well extend to other inefficient outcomes, from corruption (Tirole, 1996) to inequality (e.g. driven by discrimination, Alesina, Giuliano and Nunn, 2013; Teso, 2019; Fernández, Parsa and Viarengo, 2019) to other norms that destroy children’s human capital (e.g. child labor, Basu, 2018, and female genital mutilation, Vogt et al., 2016).

Our intervention is primarily a mechanism experiment (Ludwig, Kling and Mullainathan, 2011), meant to shed light on the drivers behind conformity to norms and the process through which those can be changed. Having said that, public donation drives are cheap and easily scalable, and could be used to effect change across a range of different behaviors. While most interventions are norm-specific, from *edutainment* (La Ferrara, Chong and Duryea, 2012; Vogt et al., 2016) to informational treatments (Bursztyn, González and Yanagizawa-Drott, 2018), our results suggest that alternative signals could affect multiple traditions at once, tantamount to the effects of cash transfer programs (Baird et al., 2010; Baird, McIntosh and Özler, 2011) or educational programs (Duflo, Dupas and Kremer, 2015) but, presumably, at much lower costs.

2 Background

Malawi has a diverse cultural heritage with numerous common traditional practices. In this paper we focus primarily on child marriage, but also provide evidence on other traditional practices that also potentially destroy children’s human capital, such as sexual initiation rituals.

Child marriage in Malawi predominantly affects girls. In Figure A.1, Panel A shows the prevalence of girls’ marriage across the different districts of Malawi. Early marriages are very common all over the country, and in many districts their prevalence is above 50%. That is the case even though marriage before 18 was made illegal in 2017 by the Malawian government. In fact, as has been documented across several contexts (Platteau, Camilotti and Auriol, 2018), the legal change had no immediate effects: the share of women who married under 18 in 2016 and 2018 is the same in our sample.

Interestingly, attitudes towards traditions and their prevalence do not always coincide. Figure 2 shows the distribution of the age at first marriage alongside that of the “ideal age” of marriage for girls, reported by our survey respondents (see Section 3.4). While marriage before age 15 is relatively rare, marriages between 15 and 18 years old are very common. While the distributions of ideal age of marriage and that of actual marriage both have 18 as the modal answer, reported ideal ages below 18 are rare – despite the high prevalence of actual marriages before that age. Such differences could be explained by multiple factors: from changes in the social norm over time (such that several of those who got married as children no longer support the practice)

¹²While the mechanisms behind the persistence of social norms have been extensively studied, we still know little about how and why social norms change. Vogt et al. (2016) and Fernandes (2008) document two instances where attitudes towards long-standing social norms change at fast pace. La Ferrara, Chong and Duryea (2012), Bursztyn, González and Yanagizawa-Drott (2018), and Blattman et al. (2019) document rapid changes in behavior linked to social expectations.

¹³It might also help understand why norms such as child marriage and female genital cutting are declining at fast rates in several parts of the world (e.g. Kandala et al., 2018), especially in rapidly urbanizing regions; see Section 7.

Figure 2: Distribution of actual and ideal marriage age for females



to factors that influence marriage decisions regardless of individual attitudes (e.g. financial incentives, peer pressure, or beliefs about what others do or expect them to do) to reporting biases (such as social desirability or cognitive dissonance).

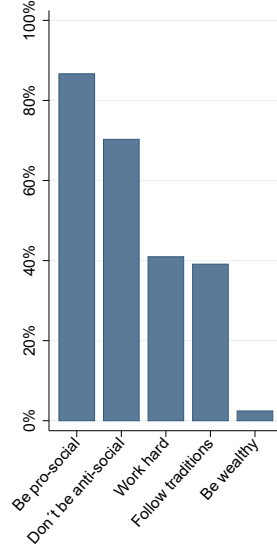
In Malawi, sexual initiation rituals are also particularly relevant for girls. In Figure A.1, Panel B shows its prevalence across the districts of Malawi. Sexual initiation rituals are mostly concentrated on the populous Southern districts. Figure A.2 displays descriptive evidence on the typical activities conducted as part of initiation rituals in Panel A, and on the most important decision-makers behind participation in those practices in Panel B (as stated by survey respondents, see Section 3.4). Activities range from counseling to different forms of female genital mutilation and forced sexual intercourse. Girls seldom decide themselves to participate in these rituals, but are mostly required to attend by their families or other decision-makers in the village.

Why do Malawian families follow such traditions? Economists, historians, and anthropologists have analyzed multiple factors behind why such practices might have emerged (and eventually disappeared, in some parts of the world) throughout history, from economic incentives (Platteau, Camilotti and Auriol, 2018; Voigtländer and Voth, 2013) to preserving kinship and social ties (Crone, 2015) to religious discourse (Brown, 2015).

In our sample, 40% of respondents state that following traditions might contribute to social image (see Section 3.4), in line with the hypotheses that conforming to social norms could signal social preferences (a component of social image), which we test in Section 4. Further common determinants of social image are showcased in Figure 3. Besides conforming to traditional practices, acting pro-socially and working hard are its most often-cited determinants, suggesting there might be scope for substitution between alternative signals, the main hypotheses of this paper, which we test in Section 5. In contrast, being wealthy is not seen as an important determinant of social image.

Figure 3: Main determinants of social image

What would someone in your village most likely do to get a great social image or a great reputation?



Notes: Compiled from an open question in which respondents could list multiple answers. *Be pro-social* includes being helpful, loving and peaceful; *Don't be anti-social* includes not gossiping and not stealing; *Work hard* also includes working as a farmer; *Follow traditions* also includes practicing a specific religion.

3 Empirical Strategy

This section starts by laying out a simple conceptual framework, in subsection 3.1, to inform the design of our experiments and the interpretation of our results. Next, subsection 3.2 presents the details of our research design for both the vignette experiment and the randomly assigned public donation drive, followed by details on compliance and balance tests in subsection 3.3. Subsection 3.4 describes our data sources, and how we elicit subjects' perceptions, attitudes, and conformity to social norms. Next, subsection 3.5 summarizes the timeline of the experiments and surveys. Last, subsection 3.6 introduces the equations that we estimate and our treatment of standard errors.

3.1 Conceptual framework

This subsection summarizes a simple model – presented in detail in Appendix D –, heavily based on standard signaling models (Spence, 1973; Zahavi, 1975), to help motivate our empirical strategy and guide the interpretation of the experimental results.

We assume individuals interact repeatedly for an indefinite number of periods, paired at random in each period, through a repeated prisoners' dilemma. Interactions entail mutual benefits of cooperation, but players have an incentive to exploit each other by unilaterally defecting – which yields higher payoffs as long as the other player cooperates. We further assume that there is heterogeneity in the extent to which different individuals value future payoffs. To simplify, assume there are only two types of individuals in society: some are more patient – i.e they value long-term cooperation to a greater extent (*pro-social* types) –, and others

are less patient – i.e. they value short-term benefits to a greater extent (*individualistic* types). Individuals’ types and their history of interactions are private information.

Appendix D shows that, under certain conditions for the difference in patience parameters across types, there is an equilibrium in which pro-social individuals play a grim-trigger strategy (start cooperating, and then defect ever after if paired with a player that defects), while individualistic types always defect. As such, pro-social players would like to be matched to others of the same type, to avoid being exploited by individualistic players in the short run, and enjoy cooperation gains with pro-social players in the long run. In turn, individualistic types would like to be matched to others of different type in order to exploit them.

Because in each round pairs are randomly matched, however, it is difficult for individuals to recognize each others’ types. Although extreme, the random matching model seems to be a good description of the Malawian villages in our sample: using data from contact sensors, Ozella et al. (2020) shows that inter-household interactions in those villages are indistinguishable from random networks.

To avoid the risk of being exploited, it may be worthwhile for pro-social individuals to obtain a costly signal that separates them from individualistic ones. To be part of an equilibrium, such a signal needs to be cheap enough to be attractive for pro-social types but costly enough to prevent individualistic types from also obtaining it. It turns out this might be possible precisely because pro-social individuals value long-term payoffs to a greater extent. As such, they are more willing to pay for an effective signal than individualistic types – even though both would like to be matched to pro-social types – as long as repeated cooperation generates a high enough net present value to compensate for signaling costs.

Whether such a separating equilibrium can ultimately be sustained depends on beliefs about what others do off-equilibrium path, since even pro-social individuals could benefit in the short-term from defection in a prisoners’ dilemma. A subgame perfect separating equilibrium can be sustained by beliefs that uncooperative outcomes are followed with eternal defection by peers (similarly when it comes to enforcement of this punishment), as long as pro-social types are sufficiently more patient.

3.1.1 Harming to signal

In the context that we study, we entertain the hypothesis that following local traditions stands for a signal in a baseline separating equilibrium, distinguishing those with an interest in mutual long-term cooperation from other villagers. In particular, marrying off one’s under-age daughter entails multiple costs: pecuniary (like organizing the wedding party) and non-pecuniary (if parents actually dislike marrying their daughter too early, or anticipate that early marriage decreases her future utility flows). If beliefs are such that only those who value long-term cooperation enough are willing to marry off their under-age daughters, then child marriage could be sustained as a signal in a baseline separating equilibrium.

3.1.2 Harmless or no signaling

This simple model suggests two possibilities for how promoting an alternative signal might disrupt a baseline separating equilibrium.¹⁴ On the one hand, if the alternative signal is less costly than the baseline signal but still costly enough to enforce a separating equilibrium, this alternative signal could replace the original signal in a *new separating equilibrium*.

On the other hand, promoting the alternative signal could introduce a *pooling equilibrium*, in which either (i) signaling is no longer optimal for the pro-social type – not even using the ‘old’ signal –, or (ii) using the ‘new’ signal is optimal for both types, leading pro-social individuals to stop using the ‘old’ one. Each case arises under different circumstances. The former emerges if the intervention affects beliefs. If the expected share of pro-social individuals increases (making the risk of being exploited sufficiently low for pro-social types), then the value of an effective signal decreases. In that case, pro-social types would no longer signal, and a pooling equilibrium would emerge in which local traditions formerly used to signal pro-sociality lose support. Second, it might be the case that the alternative signal is so cheap that every type adopts it (a ‘babbling’ equilibrium). Under specific beliefs about what individuals do off-equilibrium path, it can also be the case that local traditions formerly used to signal pro-sociality lose support in that case.

3.1.3 Equilibria and welfare

While both a new separating equilibrium or either pooling equilibria would cause pro-social types to abandon the original signal, which equilibrium ultimately emerges has very different implications for welfare. If it is a new separating equilibrium, based on a lower-cost signal, then welfare must necessarily increase: gains from cooperation within pro-social types are still enforced – only at lower costs.

Conversely, if a babbling equilibrium emerges, then welfare must necessarily decrease: cooperation is no longer enforced, and even though the costs from the higher-cost signal are no longer paid, a condition for the original equilibrium to be separating is that cooperation gains are higher than signaling costs. What is more, individuals still pay the costs of the new signal.

Last, if a pooling equilibrium emerges in which no signals are used, then welfare implications are not obvious. On the one hand, gains from cooperation within pro-social types are still enforced at no costs. On the other hand, they are exploited by individualistic types when they are paired. As a result, in this equilibrium, individualistic types are better off, but pro-social types might be worse off than if they could separate (but beliefs off-equilibrium path prevent this from happening).

Since different equilibria have different welfare implications, it is important to distinguish across them in the data in case promoting a new signal disrupts the baseline separating equilibrium. We discuss how we use the model to guide this exercise in the next subsection.

3.1.4 Model’s predictions

We collect the predictions from the model in this subsection, to inform our empirical analysis moving forward.

¹⁴Those two forces are captured by equations 5-6 in Appendix D.

[Prediction 1] Harming to signal: If there is a baseline separating equilibrium based on child marriage in certain villages, then those who marry off their under-age daughters should be perceived as more pro-social in those villages. We test this prediction in Section 4.

[Prediction 2] Signal substitution: If a separating equilibrium based on child marriage ceases to exist in certain villages, then those who marry off their under-age daughters should no longer be perceived as more pro-social in those villages. We test this prediction in Section 5. Moreover, in that case, child marriage should decrease in those villages; we test this prediction in Section 5.2.

[Prediction 3] Effect on donations: If the end-line equilibrium is pooling (not based on any signal) in certain villages, then the use of alternative signals of pro-sociality (like donations) should not increase in those villages; otherwise, the end-line equilibrium is either separating or babbling. We evaluate this prediction in Section 5.3.

[Prediction 4] Effect on perceived social preferences: If the end-line equilibrium is separating (based on an alternative signal) in certain villages, then, in those villages, those who signal this way should be perceived as more pro-social; conversely, if it is a babbling equilibrium, then those who donate should not be perceived as more pro-social. We evaluate this prediction in Section 5.2.

[Prediction 5] Effect on cooperation: If the end-line equilibrium is separating (based on an alternative signal) in certain villages, then, in those villages, cooperation should be enforced to the same extent as in the baseline; conversely, if it is a babbling equilibrium, then cooperation should no longer be enforced. We evaluate this prediction in Section 5.5.

In the context of our study, we distinguish villages with low and high prevalence of child marriage at baseline, since those villages are presumably characterized by different norms.¹⁵ As predictions 1 and 2 relate to local norms, conformity corresponds to supporting child marriage only in high-prevalence villages (while the opposite is true in low-prevalence villages).

If prediction 2 is confirmed in high-prevalence villages (contrasting those assigned to the public donation drive to those in the control group), predictions 3 to 5 allow us to distinguish across different equilibria at end line. We can start by looking at whether donations increase in treated villages relative to the control group. If they do not, the new equilibrium is pooling, not based on any signal. If they do, then we have to look at how those who donate are perceived and at what happens to cooperation in those villages. If those who donate are perceived as more pro-social and cooperation is still enforced, then the new equilibrium is separating; otherwise, the new equilibrium is babbling.

3.2 Experimental design

Taking those predictions to the data is challenging, for two reasons. First, the *signaling value* of child marriage and that of alternative signals is not randomly assigned. Some villages display much higher conformity to traditions such as child marriage than others, or display much higher donations to poorer villagers than others, but those features are often correlated with a

¹⁵This is how we have pre-registered the analysis plan; see Appendix C.

set of other village characteristics. Hence, merely comparing villages with different prevalence to assess whether donations crowd out the signaling value of child marriage – a key element of predictions 2, 3, 4 and 5 – would be misleading. Second, the *adoption* of child marriage and that of alternative signals is also not random. Individuals who decide to signal by conforming to traditions such as child marriage instead of through alternative signals like donations typically differ in many other characteristics. As such, merely comparing individuals with different histories of conformity to local norms (or with different charitable behaviors) to assess the causal effect of either signal on how one’s social preferences are perceived by others – a key element of predictions 1, 2 and 4 – would once again be misleading.

We tackle those challenges by combining a randomized control trial (RCT) that exogenously varies the salience of an alternative strategy to signal pro-sociality – increasing its signaling value by making it more visible – with a vignette experiment that randomly varies the adoption of different signals.

Our experimental design was pre-registered at the AEA RCT Registry as trial AEARCTR-0002856. The pre-analysis plan is presented in full in Appendix C. The details of each experiment are presented in the two following subsections.

3.2.1 Randomized control trial: salience of alternative signals

We randomize the promotion of alternative signals across 412 villages in Malawi by organizing a public donation drive in villages assigned to the *treatment* group. While donations had, presumably, always been available as a signaling strategy – in fact, about 60% of subjects in our baseline survey point out that “helping others” contributes to social image –, the intervention is meant to *increase their signaling value* by making them more visible.

In each treated village, a *box holder* is selected to coordinate donations, with the incumbency of (i) letting other villagers know that the drive would collect maize (Malawi’s staple crop) to be redistributed to the poorest local households, (ii) of collecting two kilograms of maize per donor, and (iii) of making sure the donation box was publicly displayed. In treated villages, boxes were introduced as part of household listings by enumerators from the National Statistical Office of Malawi in preparation for the upcoming national survey (which took place 5 weeks later; see subsection 3.5); in *control* villages, regular listing took place at the same time.

To study whether the effects of the public donation drive (if any) are really the outcome of a new signal for social preferences or, alternatively, whether donations act as a signal for other components of social image (e.g. wealth), we add additional arms to our experimental design, cross-randomizing public donation boxes and red rubber bracelets. In villages assigned to hosting both the box and bracelets, the latter are distributed in exchange for two kilograms of maize; in those assigned to host the public donation box only, donors get nothing in exchange for their donations; in villages assigned to bracelets only, those are made available for sale, for a price equivalent to two kilograms of maize. In pure control villages, neither public donations were organized nor bracelets distributed or sold. Panel A in Figure 4 summarizes the RCT design.

Cross-randomization serves multiple purposes. Besides studying the effects of bracelets alone, in order to investigate if other signals (unrelated to pro-sociality) could also disrupt the baseline

Figure 4: Experimental Design

Panel A: Public donation drive			Panel B: Vignette experiment		
		Box			
		Yes	No	Yes	No
Bracelets	Yes	117 villages	88 villages	3,510 HH	3,468 HH
	No	118 villages	89 villages		

separating equilibrium based on child marriage, the design also allows investigating whether bracelets magnify the effects of the public donation drive in villages where they are distributed in exchange for donations – by making that alternative signaling strategy even more salient. Most importantly, taking advantage of the fact that bracelets are assigned different meanings across treatment cells (implying pro-sociality only when in exchange for donations), it allows testing the model’s prediction about the causal effect of donations on perceived social preferences, with the help of the vignette experiment (see Section 3.2.2).

In all villages (across all cells), the local chief was asked during listing to enumerate the ten villagers most likely to support needy households in their village, “for example by giving food or other important things”. We use this set to determine the identity of the *box holder*. Box holders are randomly assigned: in 50% of treated villages, donations are assigned to be handled by the village chief; in the other 50%, they are handled by the last person cited by the local chief as the most likely to support the needy households in the village.¹⁶ In this paper, we abstract from the identity of the box holder except in robustness tests; the randomization is explored in a companion paper (Haenni and Lichand, 2020).

We also use the set of most pro-social villagers pointed out by local chiefs to kick-off the intervention in villages assigned to host public donation boxes. In those with both boxes and bracelets, village chiefs were informed that the households listed as most likely to help others would be granted a red rubber bracelet to “show everyone that they can be counted on”; those households were handed out the bracelets and informed along the same lines. The chief was then told that other households may also want to obtain such bracelets to signal that they can be counted on. For this purpose, a box holder was endowed with a big collection box, a measuring cup, and 80 additional bracelets. The box holder was instructed to hand out two bracelets to every household who would donate two kilograms of maize to be later distributed to the neediest households in the village.

In villages assigned to host public donation boxes only, the procedure was identical, expect that no rubber bracelets were distributed. The box holder was instructed to collect donations of two kilograms of maize by households that would like to “show everyone that they can be counted on”, to be redistributed to the neediest households in the village. For this purpose, the box holder was also given a big collection box and a measuring cup.

¹⁶Pilots suggested that households listed at the bottom were less likely to be the immediate family members or directly connected to the village chief.

In villages assigned to host bracelets only, the village chief was informed that ten households were *randomly assigned* to receive bracelets; those households were handed out the bracelets and informed along the same lines. Those households were really drawn from a lottery rather than from the set of most pro-social villagers according to the village chief. The chief was then told that other households may also want to obtain such bracelets. For this purpose, a *bracelet holder* (as in the case of box holders, either the last subject listed by the village chief or the chief him/herself) was given 80 additional bracelets and instructed to sell them for 200 MWK (the monetary equivalent of two kilograms of maize in local markets). Payments were to be kept by the bracelet holder.

Last, in control villages (not assigned to public donation boxes or bracelets), village chiefs were simply asked to list the ten households most likely to support the needy households in their village. Even though the last individual listed by the chief plays no role in those village, we still keep track of that *counterfactual box holder* when we collect long-run data on child marriage in these villages (see Section 3.4.3).

The public donation drive (or the commercialization of bracelets) was meant to be self-organized: other than appointing a box (bracelet) holder, distributing boxes (and/or bracelets) and delivering instructions, box holders were in charge of implementing it. Box holders were informed that the research team would not come back to redistribute the maize collected through the drive nor to verify whether donations in fact reached the poorest in the village. As mentioned, payments were meant to be kept by bracelet holders.

3.2.2 Vignette experiment: adoption of different signals

About five weeks after the introduction of the public donation drive, we conducted a vignette experiment that randomly assigns the adoption of different signals. Randomization takes place at the household level, within each village. In the experiment, households have to rate how pro-social a hypothetical Malawian father is. Different households are randomly assigned to different versions of that father’s background story. In the *treatment* version, the vignette depicts a Malawian father who supports child marriage: the top left picture in Figure A.3 is displayed, accompanied by the following description:

“I would now like to introduce John to you. John is a farmer. He has been married for a long time to his wife Melina. Together, they have 4 children - 3 boys and 1 girl. The family lives in a small house that they built themselves. The girl is now 14 years old. Last year, after she had her first period, the family decided that she would attend the initiation ceremonies in her village. John now considers her a grown up woman and encourages her to get married soon. On this picture you can see John, next to his daughter, when she gets married.”

In the control version, the vignette depicts a “harmless” version of the same father, who does not think his daughter is ready to get married. The visual and verbal descriptions showcase the exact same family, holding constant observable characteristics such as housing conditions and family structure. Subjects assigned to the control version of the vignette were shown the top right picture in Figure A.3, accompanied by the following description:

"I would now like to introduce John to you. John is a farmer. He has been married for a long time to his wife Melina. Together, they have 4 children - 3 boys and 1 girl. The family lives in a small house that they built themselves. The girl is now 14 years old. Last year, after she had her first period, the family decided that she would not attend the initiation ceremonies in her village. John does not think his daughter is ready to get married yet but would prefer if she waited for some more years. On this picture you can see John, next to his daughter, eating together."

While marriage under 15 years old is relatively rare in Malawi (its national prevalence is around 10%), focusing on this extreme version of the practice has the advantage of allowing for clean spatial heterogeneity analysis: according to our baseline survey, the median prevalence of under-15 marriage is only 5% (and it is completely absent from 38% of the villages in our sample), while it reaches 41% in villages at the upper-half of the distribution. As such, it is much sharper to claim that child marriage is *not a norm* in low-prevalence villages when such a small share of households conforms to it than if a larger share did (as in the case of under-18 marriage), even if to a lesser extent than in villages where prevalence is above the median.

The design of the vignette experiment is summarized in Panel B of Figure 4. Whenever multiple subjects were interviewed within a household (see Section 3.4), all of them were presented with the same version, to avoid contamination. After being presented with John's background story, respondents were asked to evaluate his pro-social preferences according to the following dimensions: *altruism*, *reciprocity*, and *trustworthiness*, adapted from Falk et al. (2016)'s survey module on social preferences (see Section 3.4.1).

At the end of the survey (about 15 minutes later), all respondents are again confronted with the same background story (holding constant the assignment they were presented with the first time around); only, this time, John and his wife are depicted wearing red rubber bracelets (in both versions, depicted in the bottom row of Figure A.3).¹⁷ Bracelets are designed to look exactly like the ones we use in villages assigned to host red rubber bracelets in the RCT. The difference in the picture is pointed out to respondents by enumerators, without providing any further context and without mentioning what bracelets are supposed to stand for. Once again, respondents are then asked to rate John's pro-social preferences.

3.2.3 Testing the model's predictions

Combining the vignette experiment and the RCT allows us to test all five predictions of the model. Contrasting how subjects rate the two versions of the Malawian father in the vignette experiment allows us to investigate whether pro-sociality varies with conformity to local norms in a baseline separating equilibrium based on child marriage (prediction 1). If following social norms signals social preferences, then we expect that the "harmless" version is perceived as more pro-social in villages where child marriage is low-prevalence, while the opposite should be true in high-prevalence villages.¹⁸

¹⁷For only 3 respondents the survey ended before they could rate John the second time around; we drop these observations from the analyses.

¹⁸See Appendix C for how we pre-registered that we would estimate heterogeneous treatment effects of each experiment according to the baseline prevalence of child marriage in each village.

Next, within villages *where bracelets stand for donations* (those assigned to host boxes and bracelets), exploring *between-subject* variation in how they rate the two versions of the Malawian father in the vignette experiment *when he is wearing a bracelet* allows us to investigate whether the intervention disrupts the separating equilibrium based on child marriage (prediction 2). If the public donation drive destroys the separating equilibrium based on child marriage, then we expect that the “harmful” version is not perceived as more pro-social in high-prevalence villages where bracelets stand for donations – but remains perceived as more pro-social in all other high-prevalence villages. Incidentally, the same comparison within villages assigned to sell bracelets allows testing whether signals of other dimensions of social image could have disrupted the baseline signaling equilibrium based on child marriage.

An alternative, direct test of the model’s prediction that the intervention disrupts the baseline separating equilibrium can be undertaken outside of the vignette experiment. Comparing villages assigned to the public donation drive to those in the control group, we can test (i) whether the association between one’s support for child marriage and how his/her social preferences are perceived by others is *weaker* in the treatment group, and (ii) whether the share of girls married before 18 years old (measured in a follow-up survey, 16 months after the intervention) is *lower* in the treatment group.

If prediction 2 is confirmed, then contrasting charitable behavior in high-prevalence villages assigned to the public donation drive to charitable behavior in control villages allows us to test whether the new equilibrium is pooling (prediction 3). If it is not, then, within villages *where bracelets stand for donations* (those assigned to host boxes and bracelets), exploring *within-subject* variation in how they rate the same Malawian father in the vignette experiment (when he is wearing a bracelet compared to when he is not) allows us to investigate whether donations signal social preferences (prediction 4). Also here, the same comparison within villages assigned to sell bracelets provides a placebo test of whether signals of other dimensions of social image conflate perceived social preferences.

Last, contrasting villages assigned to the public donation drive to those in the control group when it comes to the extent to which villagers cooperate (also elicited in the follow-up survey) further allows us to distinguish across a new separating equilibrium, based on donations, and a babbling equilibrium (prediction 5).

3.3 Compliance and balance tests

The RCT design entails four different conditions. As discussed, during listing, field team supervisors were responsible for implementing the pre-assigned treatment, delivering instructions and materials to the box/bracelet holder. We assessed compliance with treatment assignment at the time of the first survey. Figure A.4 showcases that compliance with the assignment across villages was very high, although not perfect.¹⁹ We discuss how we deal with imperfect compliance in the estimation of treatment effects in Section 3.6.

¹⁹In the few villages that did not comply with treatment assignment, it was either because team supervisors did not properly follow the assignment protocol, or because box/bracelet holders disposed of the distributed materials over the course of the 5 weeks before the survey.

When it comes to the vignette experiment, perfect compliance with the assignment was ensured thanks to tablet-based implementation and pre-assignment of the vignettes to each household identified during listing.

Last, balance checks for the RCT and for the vignette experiments are displayed in Tables B.2-B.4. All covariates are balanced across treatment conditions, and we cannot reject the hypothesis that subjects' characteristics across different cells within each experiment are statistically identical.

3.4 Outcomes

We collected baseline data on traditional practices in Malawi through a nationally representative survey across 412 randomly selected villages, between July and August 2018, in collaboration with the National Statistical Office of Malawi, the University of Malawi and UNICEF Malawi. Table B.5 shows summary statistics. We surveyed 7,388 households, randomly drawn among all those with children from 8-17 (our eligibility criterium). Tables B.2 and B.3 document that villages in our sample are mostly rural and reasonably large. 82% of them are located in rural areas, with an average of 116 households, each with approximately 5 household members.

Surveys contained three modules: (i) a household module, that elicited household characteristics (such as composition, income, and spending) and charitable behavior; (ii) an individual module, that elicited several characteristics, including social preferences, previous participation in initiation rituals, age of marriage, and attitudes towards traditional practices; and (iii) an experiment module, that showcased the pre-assigned vignettes and elicited perceptions about the depicted fathers' pro-sociality. For village chiefs, an extra module elicited their perceptions about other villagers' behaviors and social preferences.

Not every household member had to answer every survey module. If present, household heads always completed the household module as well as the individual module. Their spouses also completed the individual module. Last, if the household also had a minor between 15 and 17 years old, s/he also completed the individual module. In case there was more than one eligible minor, we randomly drew one of them to be part of the survey. The experiment module was conducted with at most two individuals per household. We randomly drew one adult household member to take part in the experiments. If the household had any 15-17 year-old minors, the one who answered the individual module also participated in the experiment module. We conducted a total of 14,821 interviews. Tables B.2 and B.3 show that survey respondents are on average 36 years old; 59% of them are female.

Follow-up data was collected between September and November 2019, in collaboration with Innovations for Poverty Action (IPA). At this time, we surveyed village chiefs and (counterfactual) box holders about several characteristics (including marriage status, childbearing and school enrollment) of all girls in the village who were 10-17 years old and unmarried at the baseline. We were able to return to 98% of villages, and successfully elicited information for 76% of the girls sampled at baseline.²⁰ Completion rates are balanced across treatment arms (see Table B.4).

²⁰Road blockages caused by heavy rainfalls prevented enumerators from reaching the remainder 2% of the villages.

3.4.1 Pro-social preferences

Capturing the effects of the availability and adoption of different signals on perceived pro-social preferences requires a comprehensive measure of the latter. For this purpose, we adapt Falk et al. (2016)’s pre-tested Preference Survey Module on social preferences to the Malawian context.²¹ These survey modules mimic standard games from experimental economics and have been shown to predict behavior in incentivized choice experiments very accurately.

To control for family-wise error rates and prevent inflated test sizes from multiple hypotheses testing, we build a summary measure, following Kling, Liebman and Katz (2007), by standardizing and averaging over the three components of pro-social preferences.²²

In our context, we can correlate this measure with charitable behavior (captured by donations to other villagers) in the control group. Donating in the last 5 weeks is associated with a 0.26 standard deviation increase in the pro-sociality summary measure ($p=0.025$; see Table B.1).

We elicit these measures at four different instances: (1) when asking subjects to assess pro-sociality of a hypothetical Malawian father in the vignette experiment; (2) when asking subjects to assess their own pro-sociality; (3) when asking village chiefs to assess the pro-sociality of selected subjects in their village at baseline; and (4) when asking respondents to assess the pro-sociality of selected subjects in their village at end line.

3.4.2 Conformity to traditional practices and attitudes at baseline

The second set of outcomes involves conformity to child marriage and initiation rituals, and subjects’ attitudes towards these traditional practices. Prevalence of child marriage is based on measures for marriage below age 15/18, based on the question “*How old were you when you started living with your (first) husband/wife?*”, while prevalence of sexual initiation rituals is based on the question “*In some regions of Malawi initiation rites for girls involve sexual components. We refer to these as sexual initiation rituals. How common are these sexual initiation rituals for girls in your village?*”.²³

We also elicit support towards child marriage, inferred from whether subjects state that the ideal age of marriage is under 18 years old, based on the question “*In your opinion, what is the right age for a woman to get married?*”, while support towards initiation rituals is based on the question “*Please tell me whether you agree or disagree with the following statements. Sexual initiation rites for girls should be continued.*”.

²¹Our survey covers three dimensions of pro-social preferences: altruism, reciprocity, and trust. Following Falk et al. (2016), subjects rate each of these dimensions on scales from 0 (not at all) to 10 (completely) over multiple questions. Additionally, subjects pick a monetary amount that the one being rated would likely contribute to charity (part of the altruism component) and a monetary amount that the one being rated would likely donate to someone who has helped them (part of the reciprocity component).

²²This was pre-registered as part of trial AEARCTR-0002856 in the AEA RCT Registry on June 11, 2018.

²³Focus group discussions showed that self reports about involvement in sexual initiation rituals are extremely sensitive to social desirability bias. For this reason, we elicit individuals’ beliefs about the share of households who conform to those practices in their village, rather than ask about their own involvement, to obtain accurate data on village-level prevalence.

3.4.3 Conformity to traditional practices and other outcomes for girls at end line

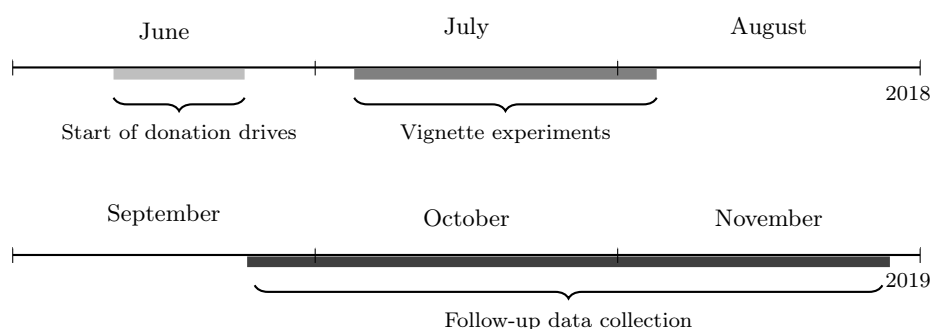
The third set of outcomes involves conformity to child marriage and initiation rituals 16 months after the intervention took place, as well as childbearing and school enrollment for girls at that time – as child marriage has been linked to early pregnancies and school dropouts (Field and Ambrus, 2008). We capture those outcomes by surveying village chiefs and (counterfactual) box/bracelet holders in each village about all girls in the village who were 10-17 years old and unmarried at the baseline. We did not survey households directly to minimize concerns with experimenter demand bias. The age of a girl was not mentioned at any point of the survey to avoid inducing social desirability bias.²⁴ Importantly, checking whether girls under 18 years old still live with their families to infer child marriage through direct observation (as in Buchmann et al., 2019) as an alternative to surveys would not have been feasible in our context; since Malawi is mostly matrilineal, it is husbands who typically move in with the bride’s family.

Survey responses for each girl are weighted by the stated degree of familiarity of the respondent with each girl.²⁵ We show that results are robust to omitting girls with conflicting information. We were able to collect at least one response for 76% of the girls, and two responses for nearly 60% of them; in those cases, for only 2% of girls respondents provide conflicting information.

3.5 Timeline

To fix ideas, Figure 5 summarizes the timeline of the experiments and surveys. The village-level RCT was carried out in June 2018. About five weeks later (the average time span between the field visits for listing and surveying, across the 412 villages), we conducted the household-level vignette experiment as part of a nationally representative survey on traditional practices in Malawi. Finally, about 16 months after the experiments, we collected follow-up data.

Figure 5: Timeline



²⁴Pilots have shown that respondents are generally bad at guessing the age of a girl but accurate in determining their marital status.

²⁵Pre-registered in the update to Trial AEARCTR-0002856 on September 22, 2019, before the start of the follow-up data collection.

3.6 Estimation

Throughout the paper, we estimate treatment effects based on intention to treat, using OLS regressions. When it comes to the effects of the RCT, we estimate the following equation:

$$Y_{vhi} = \beta_0 + \beta_1 \text{Treatment}_v + \beta_2 X_{vhi} + \epsilon_{vhi}, \quad (1)$$

where Y_{vhi} is an outcome for individual i at household h in village v ; $\text{Treatment}_v = 1$ if village v is assigned to the treatment arm relevant for that analysis (public donation boxes, boxes and bracelets, or bracelets only), and 0 otherwise; X_{vhi} is a vector of individual and village-level characteristics; and ϵ_{vhi} is the error term. In some specifications, we include indicators for multiple treatment arms. For the effects of the RCT on village-level outcomes (such as charitable behavior), we compute \bar{Y}_v by averaging over Y_{vhi} within village v , and use the former as dependent variable.

Next, when it comes to the vignette experiment, we estimate the following equation:

$$Y_{vhi} = \beta_0 + \beta_1 \text{Treatment}_h + \beta_2 X_{vhi} + \theta_v + \epsilon_{vhi}, \quad (2)$$

where $\text{Treatment}_h = 1$ if household h is assigned to the version of the vignette in which John marries off his under-age daughter, and 0 otherwise; and θ_v stands for village fixed-effects. In some specifications, we also include interactions of the treatment indicator in the RCT with the treatment indicator in the vignette experiment.

Following pre-registration, for several analyses we interact treatment indicators with the village-level baseline prevalence of under-15 marriage, as follows:

$$Y_{vhi} = \beta_0 + \beta_1 \text{Treatment}_h + \beta_2 \text{Share_childmarriage}_v + \theta_v + \beta_3 \text{Treatment}_h \times \text{Share_childmarriage}_v + \beta_4 X_{vhi} + \epsilon_{vhi}. \quad (3)$$

Last given imperfect compliance with the RCT assignment (see Figure A.4), in robustness checks we also estimate instrumental variable regressions to obtain local average treatment effects, as follows:

$$\begin{aligned} 1^{st} \text{ stage: } & \text{ActualTreatment}_v = \beta_0 + \beta_1 \text{Treatment}_v + \beta_2 X_{vhi} + \epsilon_{vhi} \\ 2^{nd} \text{ stage: } & Y_{vhi} = \delta_0 + \delta_1 \overbrace{\text{ActualTreatment}_{vhi}} + \delta_2 X_{vhi} + \xi_{vhi}, \end{aligned} \quad (4)$$

where $\overbrace{\text{ActualTreatment}_{vhi}}$ stands for the predicted probability of being assigned to that treatment arm of the RCT.²⁶ Results for the IV estimates are presented in Appendix G.

We cluster standard errors at the village level across all specifications, allowing errors to be arbitrarily correlated across households within each village.

²⁶Even though treatment assignment is at the village-level, its predicted counterpart potentially varies at the individual level because the first stage includes individual- and household-level controls.

4 Does Supporting Child Marriage Signal Social Preferences?

This section investigates whether conformity to local norms signals social preferences, in line with prediction 1. Subsection 4.1 starts with descriptive evidence of the correlation between one’s support for child marriage and how their pro-social preferences are perceived by others. Subsection 4.2 then takes advantage of the vignette experiment to document causal evidence that, in villages where child marriage is high-prevalence at baseline, parents marry off their under-age daughters to signal their pro-sociality.

4.1 Chiefs’ perceptions

We start with evidence that conforming to local norms on child marriage correlates with how villagers’ social preferences are perceived. We randomly draw five household heads in each village (based on listing) and ask local chiefs to point out if each of them would allow their daughters to marry before 18 years old, and to rate each of them when it comes to how pro-social they are (according to the three dimensions we measure; see Section 3.4.1). Figure 6 shows the correlation between support for child marriage and the pro-sociality summary measure, separately for villages where under-15 marriage is low-prevalence (according to our baseline survey, on the left-hand side), and for villages where under-15 marriage is high-prevalence (on the right-hand side).²⁷ While household heads who support child marriage are perceived as less pro-social than others in villages where child marriage is low-prevalence, the opposite is true within villages where it is more prevalent: in those villages, chiefs rate those who support child marriage as 0.4 s.d. *more pro-social* than those who do not conform to the local norm. The correlation is large, roughly identical to that between the summary measure of pro-sociality and charitable giving (Table B.9). The average difference in pro-sociality summary measures of those who support child marriage in low- vs. high-prevalence villages is statistically significant at the 5% level.

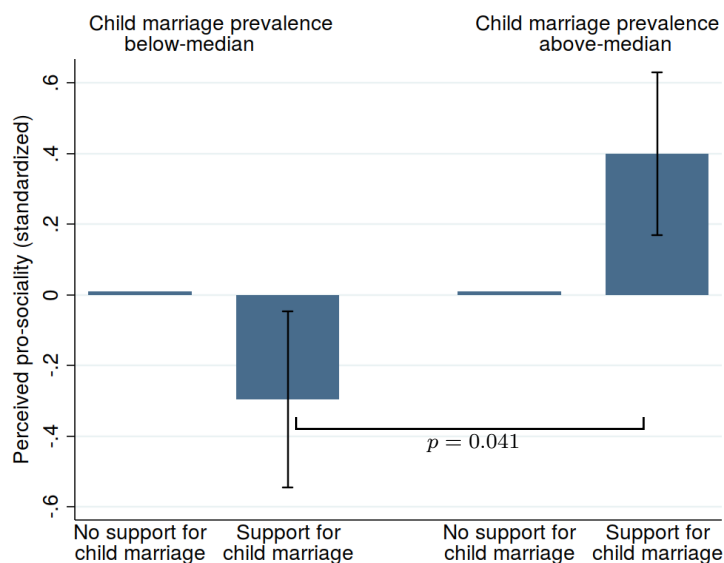
Naturally, conformity to child marriage is not randomly assigned; chiefs’ inference about social preferences could be based on other individual characteristics that correlate with support for local traditions, such as wealth or education. For this reason, the next subsection turns to the results of the vignette experiment, which randomly assigns the adoption of different signals.

4.2 Vignette experiment

In the vignette experiment, households are randomly assigned to a vignette featuring John, a Malawian father who either marries off his 14-year-old daughter or not. We turn to equation 3 to estimate the causal effects of conforming to local norms on perceived social preferences. For this analysis, Y_{vhi} stands for the summary measure of John’s perceived pro-social preferences rated by subject i at household h in village v ; $Treatment_h$ equals 1 if John marries off his under-age daughter, and 0 otherwise; and $Share_childmarriage_v$ is the share of the sample in village v who married before the age of 15 (from the baseline survey).

²⁷We split villages by median under-15 marriage prevalence to be consistent with the specification in the pre-analysis plan for estimating treatment effects within our experiments.

Figure 6: Villagers' perceived pro-social preferences as a function of their support for child marriage and prevalence of child marriage in the village



Notes: Villagers' pro-sociality as perceived by the survey respondent in villages where the prevalence of child marriage was low vs high at baseline, according to a median split. The pro-sociality measure is an equally weighted, standardized combination of individual measures for altruism, reciprocity, and trust (see Section 3.4.1). Estimates are from an ordinary least squares regression, including village-level controls (village size, population density, and urban) and district and enumerator fixed-effects. Bars stand for standard errors, clustered at the village level. P-values from Wald tests for equality of estimated coefficients.

In Table 1, Column (1) displays the results for the summary measure, while Columns (2) to (4) report treatment effects on each of its components (*altruism*, *reciprocity*, and *trustworthiness*) to assess whether results are driven by any specific dimension of pro-sociality.

Results are as follows. Individuals who live in villages without child marriage attribute significantly lower pro-social preferences to John if he supports child marriage, an effect size of about 0.7 standard deviation (Column 1; significant at the 1% level). The interaction term with the share of the village married before age 15 is positive (also significant at the 1% level), indicating that pro-social preferences associated with support for child marriage *increase* with the local prevalence of child marriage. In column 1, John who supports child marriage would be attributed more pro-social preferences than the one who does not in villages with prevalence 38.9% or higher, which is close to the maximum prevalence for child marriage before age 15 we observe in our sample (41%).²⁸ That is the case even though our vignette experiment presumably under-estimates the signaling value of child marriage, since marriage before age 15 is a rarer and more extreme manifestation of the practice – and it is likely that subjects perceive John in the control condition to still be likely to marry off his daughter before 18 in high-prevalence villages. The effects are similar across all components of pro-social preferences we measure (Columns 2-4).

The findings are consistent with the correlation between support for child marriage and chief's ratings of villagers' pro-social preferences outside of the experiment (Figure 6). To-

²⁸The lower bound of the 95% confidence interval for the tipping point is 18.6%.

Table 1: Effect of engagement in traditional practices on perceived pro-social preferences

Dependent variable: Pro-social preferences attributed to John	Summary Measure (1)	Individual components		
		Altruism (2)	Reciprocity (3)	Trustworthiness (4)
John supports child marriage	-0.728*** (0.0354)	-0.500*** (0.0350)	-0.429*** (0.0382)	-0.909*** (0.0387)
John supports child marriage × Share married < 15	1.873*** (0.556)	1.641*** (0.602)	1.080* (0.618)	2.006*** (0.589)
Individual controls	✓	✓	✓	✓
Village fixed effects	✓	✓	✓	✓
Observations	6,978	6,978	6,978	6,978

Notes: The summary measure (1) is an equally weighted, standardized average of standardized individual measures for (2) altruism, (3) reciprocity, and (4) trustworthiness (see Section 3.4.1). Regressions additionally include individual controls (female, age, age², age³, and measures for own pro-sociality) plus a constant. Standard errors, clustered at the village level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

gether, the evidence supports the model’s prediction 1: it is consistent with the existence of a baseline separating equilibrium based on child marriage, whereby conforming to the local norm is perceived as a signal for social preferences.

5 Do Public Donations Substitute for Child Marriage as Signal?

This section assesses whether promoting alternative signals disrupts the baseline separating equilibrium based on child marriage, taking advantage of the randomized control trial in combination with the vignette experiment. In line with prediction 2, subsection 5.1 studies how the introduction of the public donation drive affects the relationship between child marriage and perceived pro-sociality, followed by its effects on child marriage and related outcomes for girls in subsection 5.2. Next, we investigate which equilibrium arises at end line, by evaluating the effects of the intervention on donations (prediction 3) in subsection 5.3, on perceived social preferences of those who donate (prediction 4) in subsection 5.4, and on cooperation (prediction 5) in subsection 5.5. Last, subsection 5.6 presents the results of extensive robustness tests of our main findings, from techniques to rule out data quality issues to heterogeneity results.

5.1 Effect on perceived social preferences of who supports child marriage

We start by investigating if the introduction of the public donation drive affects the relationship between one’s history of child marriage and how their pro-social preferences are perceived by others. As pre-registered, we explore heterogeneity in the baseline prevalence of under-15 marriage across villages in all following analyses. Table B.8 documents that the baseline prevalence of child marriage (captured by the survey 5 weeks after listing – when the drive was introduced in treated villages) is not affected by the intervention, and hence can be considered as pre-determined.

In our follow-up survey, we ask village chiefs and (counterfactual) box holders to rate the pro-sociality of all sampled villagers with 10-17 year-old girls in the household at baseline and

link this to whether those girls got married in the previous 16 months. Panel A of Figure 1 shows that, just as in Figure 6 (which documents the correlation between perceptions at baseline about villagers’ support for child marriage and those about their social preferences), it is still the case 16 months later that conformity to local norms signals social preferences in the control group. Within that group, those with a recent history of child marriage are perceived as 0.3 standard deviation more pro-social in high-prevalence villages than in low-prevalence villages ($p=0.039$). In contrast, in the treatment group, conforming to local norms does not correlate with social preferences in either low- or high-prevalence villages at end line. What is more, Panel B of Figure 1 shows that those with a recent history of child marriage are no longer differentially perceived across low- and high-prevalence villages ($p=0.701$). That happens because the public donation drive causally disrupts the signaling value of child marriage: restricting attention to high-prevalence villages, the pro-sociality summary measure of those who recently married off an under-age daughter decreases by nearly 0.2 standard deviation when we move from control to treated villages.

While the treatment effect on the correlations above is causal, could it be that it conflates other effects of the public donation drive? As an example, if the intervention affects households’ wealth (through general equilibrium effects), then treated villages could experience a simultaneous shift in child marriage (as poverty affects “selling brides”; Corno, Hildebrandt and Voena, 2020) and in perceptions (as wealth affects social image) unrelated to the baseline signaling equilibrium. To rule out such alternative mechanisms, we return to the vignette experiment – since it holds characteristics such as wealth and family structure constant across its different versions.

We draw on the vignette experiment in two steps. First, in Table 2, we re-estimate equation 3 restricting attention to the second instance when households rate John (when he and his wife are wearing red rubber bracelets). Because of the RCT, those wearing bracelets should be understood as having contributed to the public donation drive in villages assigned to host boxes and bracelets, but not anywhere else.

Table 2: Signal substitution when John wears a bracelet

Dependent variable: Pro-social preferences attributed to John	Box & Bracelets (1)	Control (2)
John supports child marriage	-0.477*** (0.0761)	-0.607*** (0.0760)
John supports child marriage × Share married < 15	-0.331 (1.504)	2.228** (0.857)
Individual controls	✓	✓
Village fixed effects	✓	✓
Observations	2,020	1,460

Notes: The summary measure (1) is an equally weighted, standardized average of standardized individual measures for (2) altruism, (3) reciprocity, and (4) trustworthiness (see Section 3.4.1). Regressions additionally include individual controls (female, age, age², age³, and measures for own pro-sociality) plus a constant. Standard errors, clustered at the village level, in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2 documents that, in villages where under-15 marriage does not exist (the first-row coefficients), supporting child marriage still adversely affects John’s perceived pro-sociality, just as in Table 1. While the point estimate is less negative in villages where bracelets are associated with charitable behavior (those where those were distributed in exchange for donations, in Column 1) than in control villages (Column 2), it is still large and significantly different from zero (at the 1% level). Most importantly, the interaction term with the baseline prevalence of under-15 marriage is *no longer positive* in villages where bracelets imply participation in the public donation drive, while it remains large and statistically significant in control villages. Hence, at end line, child marriage no longer signals social preferences in high-prevalence villages for individuals who contributed to the public donation drive.

Second, we take advantage of the fact that each subject rated John with and without a bracelet to investigate if the treatment effect on the signaling value of child marriage within high-prevalence villages is really linked to John’s contribution to the drive. To do that, we stack the two individual responses (for each instance when they rated John’s pro-social preferences, wearing bracelets and not) and re-estimate equation 3 including respondent fixed-effects and adding extra interaction terms to allow treatment effects to differ additionally by whether John is portrayed wearing a bracelet (see Appendix E). Comparing ratings *within subject*, Column 1 in Table E.1 shows that, in the “harmless” version of the vignette, donating significantly increases John’s perceived pro-sociality as the share of under-15 marriage increases. In contrast, when John marries off his under-age daughter, donating significantly decreases perceived pro-sociality as the share of under-15 marriage increases. Signal substitution in high-prevalence villages happens exclusively where bracelets stand for charitable behavior: the triple interaction coefficient is not statistically significant in any other cell (Columns 2-4). Together, results confirm the model’s prediction that the public donation drive disrupted the baseline signaling equilibrium based on child marriage, crowding out its signaling value in high-prevalence villages.

5.2 Effect on child marriage and related outcomes for girls

If promoting charitable behavior crowds out the signaling value of child marriage, do individuals engage less in it as a result? This section analyzes treatment effects of the intervention on child marriage, as well as on teenage pregnancies and school dropouts. We also document treatment effects on the likelihood of participating in sexual initiation rituals, another harmful traditional practice that could be affected as the signaling value of conforming to local norms dies out.

We estimate treatment effects of the public donation drive on child marriage, childbearing, school dropout, and participation in sexual initiation rituals for all girls aged 10-17 at baseline. Following our pre-analysis plan, for child marriage and childbearing we consider all girls up to 18 years old. For school dropouts, we consider girls up to 17 years old (the right age-for-grade at the end of high school), since we only know if a girl is still in school – not if she graduated. Last, for sexual initiation rituals, we consider all girls up to 15 years old (since those typically happen at age 10-15; see Feng, Haenni and Lichand, 2020).

Table 3 shows that the intervention decreases child marriage age-by-age on average by 1.7 p.p., a 30% reduction relative to the control group.²⁹ This effect size is comparable to that

²⁹For local average treatment effects using IV, see Table G.1.

Table 3: Average effects, age-by-age

	(1) Married <18	(2) Children	(3) School dropout	(4) Initiated
Public donation drive	-0.0178** (0.00830)	-0.0155** (0.00765)	-0.0245* (0.0146)	-0.0171 (0.0149)
Control mean	0.058	0.053	0.166	0.069
Observations	3,436	3,436	3,153	2,429
District fixed effects	✓	✓	✓	✓
Enumerator fixed effects	✓	✓	✓	✓
Controls	✓	✓	✓	✓

Notes: Regressions additionally include individual controls (age, age², and age³) and village-level controls (village size, population density, urban, and baseline prevalence of child marriage or initiation rituals) and enumerator indicators. Standard errors, clustered at the village level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

of conditional incentives (Buchmann et al., 2019), shown to decrease child marriage by 25% in Bangladesh. Similarly, early pregnancies age-by-age decrease on average by 1.5 p.p. in the treatment group (29% less than in the control group), and school dropouts age-by-age decrease on average by 2.5 p.p. in the treatment group (14% less than in the control group). Consistent with the disruption of a baseline signaling equilibrium based on conformity to local norms, the intervention also decreases sexual initiation rituals (the probability of participation age-by-age decreases on average by 1.9 p.p., a 28% reduction relative to the control group), although that effect is only imprecisely estimated. Last, Figure 7 displays hazard rates over the relevant ages for each outcome on the left-hand side of each panel, documenting that, for all outcomes, the intervention shifts the probability distribution downward over almost the entire age range.

To get a better sense of the magnitudes, it is useful to translate the treatment effects on hazard rates into expected prevalence rates at the point when all girls in our sample will have turned 18 years old. Under the additional assumption that the treatment effects that we estimate are persistent, we can compute cumulative effects: the expected prevalence of outcome o is simply $Pr[o] = 1 - \prod_{t=11}^T (1 - \hat{\beta}_t^o)$, where $\hat{\beta}_t^o$ is the difference in prevalence of outcome o across treatment and control villages among girls of age t . For all outcomes, hazard rates and cumulative prevalence at each age are displayed in Figure 7. Table 4 summarizes treatment effects on the expected prevalence of different outcomes at critical ages, with p-values computed from bootstrapped standard errors, clustered at the village level.³⁰

Results are as follows. Table 4 documents an 8 p.p. lower expected prevalence of child marriage in treated villages (a 19% reduction relative to the control group). Similarly, the expected probability of having given birth to a child by age 18 decreases by 11 p.p. (27% less than in the control group), and high-school dropouts decrease by 6 p.p. (8% less than in the control group). While the expected participation in sexual initiation rituals by age 15 decreases by 6 p.p. (a 20% reduction relative to the control group), this effect is not precisely estimated. Figure 7 showcases that differences in the hazard rates of participation in sexual

³⁰We bootstrap standard errors to account for the fact that the expected prevalence of each outcome is a non-linear function of estimated hazard rates.

Table 4: Cumulative treatment differences over age groups

	(1) Married <18	(2) Mother at 18	(3) School dropout	(4) Initiated
Public donation drive	-0.0805* (0.0450)	-0.113** (0.0441)	-0.0583* (0.0311)	-0.0603 (0.0458)
Control mean	0.416	0.419	0.755	0.308
Observations	3,436	3,436	3,153	2,397
District fixed effects	✓	✓	✓	✓
Enumerator fixed effects	✓	✓	✓	✓
Controls	✓	✓	✓	✓

Notes: Regressions additionally include individual controls (age, age², and age³) and village-level controls (village size, population density, urban, and baseline prevalence of child marriage or initiation rituals) and enumerator indicators. Bootstrapped standard errors, based on 1,000 replications and clustered at the village level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

initiation rituals across the treatment and control groups are more pronounced earlier on, with significant differences in expected prevalence (nearly 30%) by age 13.

5.3 Effect on donations

Having documented that the public donation drive disrupted the baseline separating equilibrium based on child marriage, it remains to assess which new signaling equilibrium arises at end line. As discussed in Section 3.1.3, the type of signaling equilibrium matters to understand the welfare implications of the intervention. The first step towards that is assessing whether donations systematically increase as part of the new equilibrium. If that is the case, we can rule out a pooling equilibrium in which no signals are used.

We start by documenting that the RCT assignment affected villagers' behavior in the short-term as expected. Figure A.5 shows that, within 5 weeks, the intervention had the intended effects on the take-up of donations and bracelets, leading to differences in charitable behavior across different treatment arms. Villages assigned to the public donation drive collected $\approx 50kg$ of maize; those assigned to distribute bracelets (for sale or in exchange for donations) handed out ≈ 25 pairs of bracelets.

Most importantly, Figure A.6 documents that the intervention had persistent effects on donations. 16 months after the introduction of the public drive, the share of villagers who had donated over the previous year in treated villages is over 20 p.p. higher than in the control group (in Panel B).³¹ This pattern is inconsistent with the emergence of a pooling equilibrium.

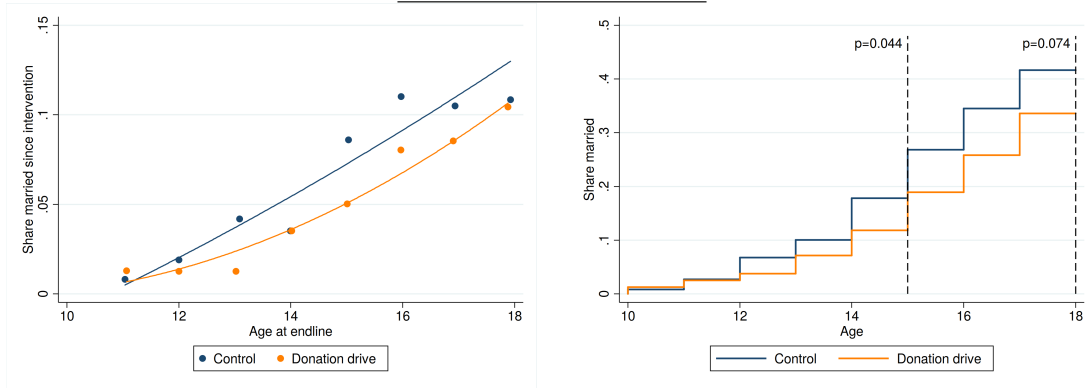
5.3.1 Who donates?

Above and beyond documenting that donations increase in the new signaling equilibrium, we can explore donation patterns to study two important questions linked to our main result. Since the public donation drive makes child marriage no longer a signal of social preferences in high-prevalence villages, one would expect donations to increase particularly in high-prevalence

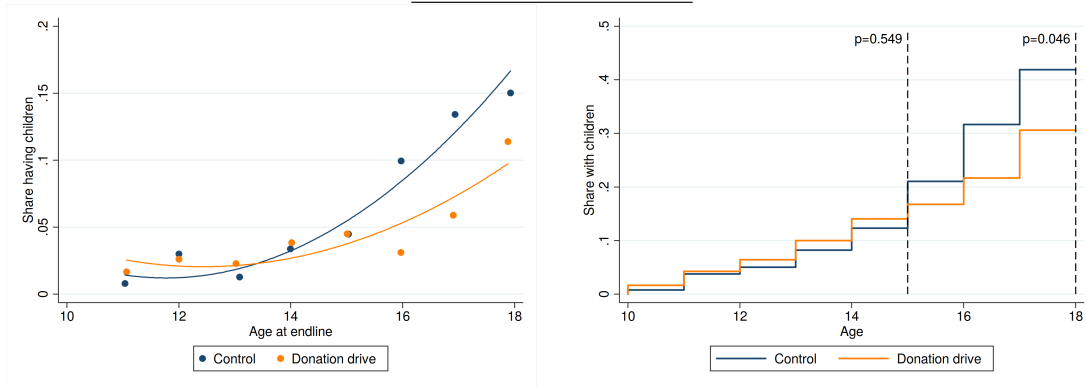
³¹Similar differences apply both to self-reported donations (Panel A) and to the reports about the share of other villagers who donate (Panel B).

Figure 7: Effects on behavior after 16 month

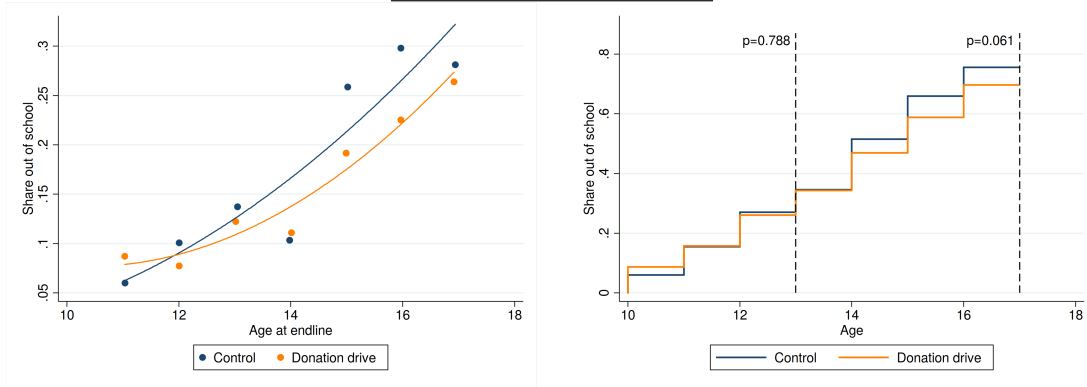
Panel A: Child marriage



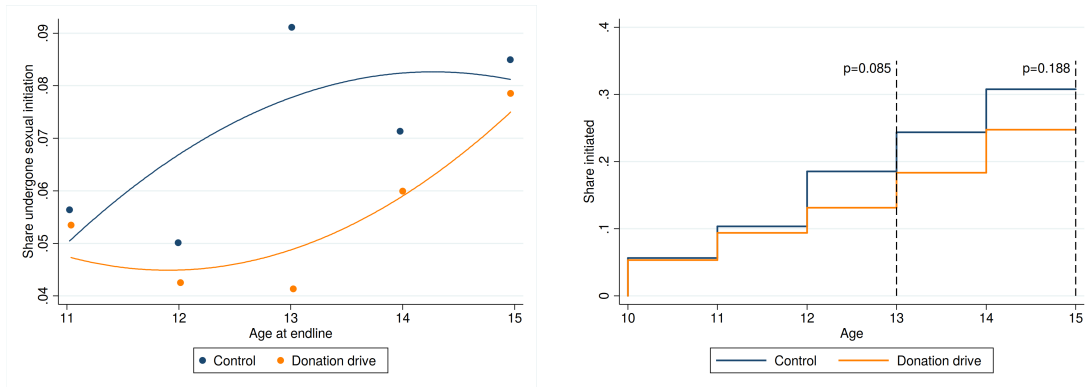
Panel B: Childbearing



Panel C: School dropouts



Panel D: Sexual initiation rituals

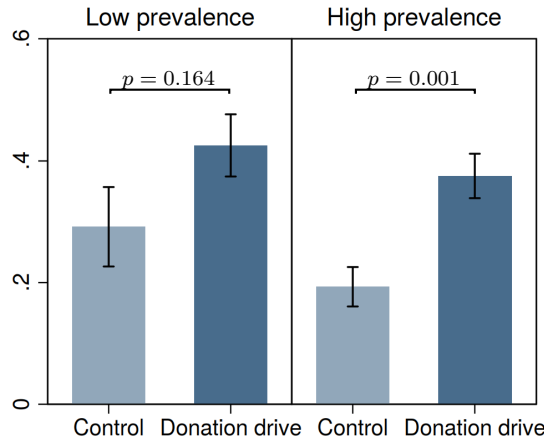


Notes: Binned scatter plots on the left and cumulative hazard rates based on those estimates on the right. Figures are residualized for village-level controls (village size, population density, urban, and baseline prevalence of child marriage or initiation rituals) as well as enumerator and district indicators. T-tests of differences based on bootstrapped standard errors, with 1000 replications and clustered at the village level.

villages, and particularly among those who supported child marriage at baseline. This subsection tests this hypothesis.

We start by showing in Figure 8 that, in the control group, the share of respondents who married before 18 and donate at baseline is almost 50% higher in villages where under-18 marriage is low-prevalence than in high-prevalence villages. This is consistent with the idea that charitable behavior was used to a lesser extent to signal social preferences in high-prevalence villages, where households harmed to signal in the baseline separating equilibrium based on child marriage. In contrast, in the treatment group, the share of those married before 18 who donate at baseline is much more similar across villages with high and low baseline prevalence of child marriage. This arises because, consistent with our hypothesis, the effect of the public donation drive on donations is significantly larger within high-prevalence villages; to that effect, the treatment effect on donations by sub-sample (based on the median split) is only significant within high-prevalence villages.³²

Figure 8: Share of respondents with a child marriage history that donate in villages with low vs high prevalence of child marriage



Notes: Analyses within the sub-sample whose age at first marriage was less than 18 years old (reported at baseline). Donations were recorded 5 weeks after setting up the donation drives by asking "Did you or someone else in your household give food during a recent food collection?". High- and low-prevalence villages are defined according to a median split in terms of the share of under-18 marriage in the village at baseline. Standard error clustered at the village level. P-values from Wald tests for equality of averages.

We dig deeper into the profiles of those who engage in charitable behavior by documenting who had donated to other households (either contributing to the drive or otherwise) 5 weeks after the intervention, and then contrasting these profiles across different treatment conditions.³³ Table B.6 reports descriptive statistics of those who donate, across control (Columns 1-3) and treated villages (Columns 4-6).³⁴ In control villages, there are considerable differences in the composition of those who state to have recently donated and those who do not. In particular, individuals who donated report to be more altruistic and reciprocal (although not statistically

³²We compare treatment effects across villages with different baseline prevalence of child marriage using the whole sample in Section 5.6.3.

³³We cannot do that at end line since we did not re-survey subjects directly; see Section 3.4.

³⁴As Section 3.4 explains, only 1/3 of the sample was surveyed about donations as part of the experiment module.

significant), have significantly larger consumption expenditures, and are less likely to engage in and support child marriage. In contrast, in villages assigned to the public donation drive, the composition of those who donate and those who do not is much more similar with respect to *all* characteristics: those who typically do not donate in the control group *sort into* donations.³⁵ Last, Table B.7 documents the joint distribution of support towards child marriage and donations in control and treated villages. In the former, individuals who support child marriage seldom donate. In contrast, in the treatment group, individuals who support child marriage donate to a much greater extent – *even more so* than parents who do not support child marriage. All in all, results confirm the scope for substitution between child marriage and charitable behavior as signals of social preferences, which is consistent with our findings that confirm the model’s prediction 2.

5.4 Effect on perceived pro-social preferences of who donates

Having discarded that the end-line signaling equilibrium is pooling, we now use the two final model’s predictions to disentangle a new signaling equilibrium based on donations from a babbling equilibrium. In this subsection, we assess if donating affects how one’s pro-social preferences are perceived by others.

We start with manipulation checks to confirm that respondents perceive donations and bracelets as intended. For this purpose, we ask subjects in an open question what *food collections* and *red rubber bracelets* mostly stand for, collecting answers into two categories: *sharing* and *wealth*. Figure A.5 displays the results for maize donations in Panels A2 and A3, and for bracelets in Panels B2 and B3. Food donations represent sharing for roughly 40-50% respondents across all treatment conditions, while very few households perceive food donations as a signal of wealth.³⁶ In turn, there are significant differences regarding perceptions of what *red rubber bracelets* stand for across treatment conditions. In villages assigned to the box and bracelets (where bracelets were handed out in exchange for donations), households associate bracelets with sharing to a much greater extent. Interestingly, bracelets are nowhere perceived as a signal for wealth – not even where those were available for sale –, presumably because their price (equivalent to 2kg of maize) was very cheap.

To get at the causal effect of charitable behavior on how one’s social preferences are perceived by others, we return to the vignette experiment, analyzing how wearing a bracelet affects perceptions about John’s social preferences across villages assigned to different treatment conditions. Concretely, we explore within-subject variation, comparing John’s pro-sociality summary measure when he wears a bracelet to when he does not, across villages where bracelets were exchanged for donations and all other villages (where bracelets are not associated with sharing, as discussed above).

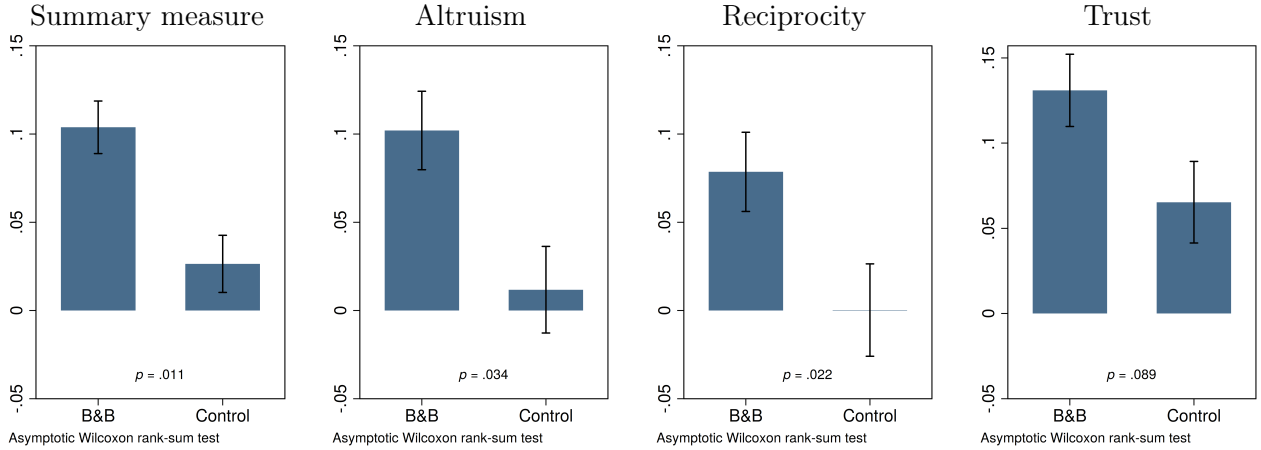
Figure 9 shows that John is perceived as systematically more pro-social when he wears a bracelet only within villages where bracelets stand for donations – but not in other villages.³⁷ Table B.9 reports additional correlational evidence that, when respondents assess other villagers’

³⁵In Section 5.4 we show that introducing the donation drive does not affect self-reported pro-social preferences.

³⁶This is in contrast to Glazer and Konrad (1996), which argues that charitable giving is driven by the desire to signal wealth.

³⁷See Appendix F for all details.

Figure 9: Change in John’s perceived social preferences when he wears a bracelet vs. not



Notes: Difference between John’s pro-sociality summary measure when he is depicted wearing a bracelet and that when he is not. The summary measure is an equally weighted, standardized average of standardized individual measures for altruism, reciprocity, and trustworthiness (see Section 3.4.1). Standard errors clustered at the village level. P-values from asymptotic Wilcoxon rank-sum tests.

social preferences at end line, donating over the last year and over the previous month strongly increases perceived pro-sociality. The fact that donations still signal pro-social preferences at end line is consistent with a new separating equilibrium based on charitable behavior.

5.5 Effect on cooperation

If a new separating equilibrium emerges at end line, we expect that cooperation should still be sustained as part of this equilibrium, in line with the final prediction of the model. This subsection tests this hypothesis.

Table 5 estimates treatment effects of the intervention on two measures of cooperation at end line: (1) perceived pro-sociality of household heads (captured by the summary measure of altruism, reciprocity and trustworthiness), and (2) the likelihood that households receives help from other villagers. Both measures are elicited from the village chief and the (counterfactual) box holder in our follow-up survey, covering all sampled households in the village with 10-17 year-old girls unmarried at baseline.

Results show that the intervention does not decrease the perceived pro-sociality of other villagers nor the perceived likelihood of receiving help from others. Consistent with the emergence of a new separating equilibrium – rather than a babbling equilibrium –, the evidence points out that the new signal can still sustain cooperation on the equilibrium path. In the context of the model that we use to guide our empirical strategy and the interpretation of our results, this means that the intervention was welfare-improving, as discussed in Section 3.1.3.

5.6 Robustness checks

This subsection condenses several robustness checks of our main results. We start by assessing the reliability of the end-line data, in subsection 5.6.1, followed by extensive analyses of experimenter demand effects in subsection 5.6.2. Next, subsection 5.6.3 presents heterogeneous

Table 5: Effects of donation drive on cooperation

	(1)	(2)
	Perceived pro-sociality (std. summary measure)	Likelihood of receiving help by other villagers (std.)
Public donation drive	0.0434 (0.0432)	-0.0259 (0.0447)
District fixed effects	✓	✓
Enumerator fixed effects	✓	✓
Controls	✓	✓
Observations	4,146	4,146

Notes: The summary measure is an equally weighted, standardized combination of individual measures for altruism, reciprocity, and trust. The likelihood of receiving help was elicited on an integer scale from 0 (completely unlikely) to 10 (completely likely). Regressions additionally include village-level controls (village size, population density and a urban indicator) and enumerator indicators. Standard errors, clustered at the village level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

treatment effects of the public donation drive. Subsection 5.6.4 then discusses additional results on alternative signals and on signal intensity.

5.6.1 Reliability of the end-line survey data

Our results on child marriage and other outcomes for girls could not be based on notarized marriage certificates, which do not exist in Malawi, neither on home visits to check whether under-age girls still live with their parents, as Malawi is mostly matrilineal (husbands typically move in with the bride’s family in many parts of the country). Since we rely on survey measures, a central concern is whether our estimates conflate experimenter demand effects.

In our follow-up survey, 16 months later, we refrained from asking households directly about whether their under-age daughters got married in the previous year; instead, we relied on external sources. We surveyed local chief and (counterfactual) box holders about all 10-17 year-old girls in the village who were unmarried at baseline, asking them to report on several characteristics of each girl – including their marriage status. Respondents could answer “I don’t know”; when they had an answer, they were asked to express their degree of familiarity with each girl to provide a measure of uncertainty. We pre-registered that, whenever we had access to two responses for a girl, we would weight them by respondents’ stated degree of familiarity with that girl (see Appendix C).

While this strategy presumably limits reporting biases, it could still be the case that the identity of respondents makes them particularly prone to experimenter demand effects. Chiefs arguably have stronger concerns about their village reputation when documenting traditional practices, and this could be exacerbated by having collective action in the village triggered by the intervention. In this subsection, we assess the robustness of our findings by (1) omitting village chiefs’ responses, and (2) restricting attention to girls for whom both respondents agree when it comes to their marriage status.

Tables B.10 and B.11 present the results.^{38,39} Estimates are remarkably similar to our main findings in Table 3.⁴⁰

5.6.2 Experimenter demand effects

This subsection considers experimenter demand effects in more detail. Even though boxes and bracelets were distributed about 5 weeks before any survey was undertaken, and even though nothing either at that point or later, during the surveys, indicated their connection to traditional practices, one could still worry that the intervention makes subjects more willing to conform to expectations of what surveyors might want them to say.

We start by investigating whether respondents' self-reported age of marriage and their stated previous participation in sexual initiation rituals, at our baseline survey, are affected by the intervention. Since neither past engagement in initiation rituals or marriage age could have been affected by the intervention – even though self-reports about those are, presumably, equally sensitive to experimenter demand effects – , this corresponds to a placebo test. Table B.14 shows that neither measures of baseline prevalence are affected by the introduction of the public donation drive.

Is this really evidence against experimenter demand effects, or could it be that the time elapsed since listing had been insufficient to induce treatment effects, only 5 weeks after the intervention? To test that hypothesis, we turn to data on *attitudes* towards child marriage and sexual initiation rituals at baseline. Support towards child marriage is captured by the binary indicator whether respondents state that the ideal age of marriage is below 18, while support towards sexual initiation rituals is captured by the statement that such practices should continue. Table B.13 shows that support towards child marriage and forced sexual initiation rituals 5 weeks after the intervention decreases by 30% and 20%, respectively.^{41, 42}

Nevertheless, one might still worry that current attitudes and behavior are more sensitive to experimenter demand effects than previous behavior that might have taken place many years before the survey. To provide a more thorough test of this hypothesis, we follow De Quidt, Haushofer and Roth (2018) in estimating bounds for experimenter demand effects. Specifically, we ask a sub-sample of respondents (1/3, randomly drawn) whether they agree with the following statement about traditional practices: "*There are common cultural practices in this village that may harm children*". The last part of the statement makes it a value judgment – not merely a factual claim – and, hence, should reasonably approximate attitudes towards those practices.

³⁸It can be seen from the control mean across different outcomes that respondents often answer *no* whenever they are unsure.

³⁹The smaller sample size in this table is not driven by conflicting information about girls (which only occurs in 2% of the cases), but rather by missing information: in many cases, one of the two respondents answers "I don't know".

⁴⁰A different concern is whether results are driven by instances when the village chief was in charge of the intervention; see Section 5.6.3.

⁴¹For local average treatment effects using IV, see Table G.2.

⁴²Consistent with the results in Section 5.3.1, Table B.15 documents that the effects of the intervention on support for child marriage are driven by households with girls at the relevant age as well as by female respondents, and are larger in high-prevalence villages. Notably, treatment effects are concentrated on the sub-sample that states to have married for traditional reasons, while it is zero among those who married out of an emergency situation (although the difference is not statistically significant; in this analysis, we have a smaller sample size as we restrict attention to subjects who (1) were ever married and (2) stated one of those factors as the reason).

Before expressing whether they agree with that statement, respondents are primed about surveyors' expectation. We randomly assign subjects to either a "Demand to agree" condition, in which they are told before reading the statement that *"We expect that participants to whom we ask the following question this way agree to it more often than they normally would"*; to a "Demand to disagree" condition, in which we told respondents before reading the statement that *"We expect that participants to whom we ask the following question this way agree to it less often than they normally would"*; or to a control condition, where no priming precedes the statement.⁴³ The idea is that priming subjects about expected responses should create experimenter demand effects that are arguably much stronger than any implicit effects that could have been triggered over the course of the survey.

In Table 6, Column (1) presents general bounds for experimenter demand effects, and Column (2) estimates bounds conditional on treatment assignment. We find that priming generates experimenter demand effects that go in the expected direction in each case, but turn out to be rather small (Column 1). The "Demand to agree" condition increases agreement by 1.65 p.p. (a 2% increase compared to the control condition, and statistically insignificant), along the same lines as the "Demand to disagree" condition. This is not due to floor or ceiling effects: about 77% of respondents agree with the statements in the control condition. Most importantly, experimenter demand effects do not systematically vary with treatment assignment: interaction terms are small and insignificant (Column 2). This confirms that our results are not driven by the desire to conform to surveyors' expectations.

Table 6: Bounding experimenter demand effects

Dependent variable: <i>"Harmful practices are common"</i>	(1)	(2)
Demand to agree	0.0165 (0.0152)	0.0233 (0.0246)
Demand to disagree	-0.0166 (0.0147)	-0.00798 (0.0231)
Public donation drive		0.0127 (0.0248)
Agree \times Public donation drive		-0.0119 (0.0312)
Disagree \times Public donation drive		-0.0151 (0.0299)
Constant	0.773*** (0.0122)	0.765*** (0.0190)
Observations	4,865	4,865

Standard errors clustered at the village level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

5.6.3 Heterogeneous treatment effects

Next, we estimate heterogeneous treatment effects of the public donation drive to address important concerns about the robustness of our main findings. First, we analyze heterogeneity

⁴³This question is the very last in the survey, to avoid that the priming contaminates other survey responses.

with respect to who was in charge of the drive – the village chief or the (counterfactual) box holder. This helps assess whether effects are driven by the desire to signal specifically *to the chief* or to other villagers more generally. Second, we analyze heterogeneity with respect to the baseline prevalence of the social norm, extending the analysis of Figure 8 to the whole sample. Since the public donation drive makes child marriage no longer a signal of social preferences in high-prevalence villages, one would expect the intervention to be particularly effective in those villages. Third, we estimate treatment effects by household spending at baseline, contrasting ultra-poor households to all others within each village. This helps rule out that the effects of the public donation drive work through income effects (as redistribution was targeted at the poorest households in the village) rather than through the signaling mechanism, since poverty correlates with the decision to “sell daughters” in the presence of bride prices (Corno and Voena, 2016).

Tables B.12 displays heterogeneous treatment effects for under-18 marriage. Column (1) presents the average effect for the whole sample; Columns (2) and (3) present treatment effects for the sub-sample of villages where the (counterfactual) box holder was the village chief or another villager, respectively; Columns (4) and (5), for the sub-sample of villages with high and low prevalence of under-15 marriage at baseline, respectively; and Columns (6) and (7), for the 10% poorest households and the remaining 90%, respectively. When it comes to who was in charge of the public donation drive, there are no significant differences in treatment effects comparing Columns (2) and (3). If anything, the effect size of the public donation drive on child marriage is larger when another village was in charge of the intervention. As such, there is no evidence that treatment effects of the public donation drive are driven by a desire to signal to the chief. Comparing treatment effects across low- and high- prevalence villages, in Columns (4) and (5), we find that the effect size of the intervention within high-prevalence villages is two-fold its effect within villages where under-15 marriage is low-prevalence – even though we do not have statistical power to precisely detect that difference. Last, results in Columns (6) and (7) readily discard the alternative explanation that treatment effects are driven by those targeted by redistribution. The effect size of the intervention is virtually identical across the 10% poorest and the remaining 90%.

5.6.4 Alternative signals and intensity of pro-social signaling

Last, in this subsection we study the effects of bracelets on child marriage; first, in isolation (possibly as a signal of other dimensions of social image) and, next, in combination with the public donation drive, whereby it arguably increases the intensity of the intervention by making charitable behavior even more salient as an alternative signal.

We start by analyzing the effects of bracelets in isolation to document the extent to which other signals, unrelated to social preferences, could also have disrupted the baseline separating equilibrium based on child marriage. While bracelets could act as a signal of other components of social image (e.g. wealth), we have shown in Section 5.4 that they are not perceived as a signal of social preferences *except in villages where they stand for donations*. Table B.17 compares child marriage and other outcomes related to girls across the different treatment arms, 16 months after the intervention. We find that having bracelets available for sale does not decrease child marriage; in the contrary, early marriages systematically *increase* in those villages relative to

the control group. The effect size is very large (and statistically significant at the 1% level), and the large increase in early pregnancies in those villages is consistent with that finding.⁴⁴ While we did not anticipate such effect, we speculate that it could be due to in-group-out-group tendencies being strengthened by bracelets, reinforcing the value of existing signals and making the baseline separating equilibrium even more entrenched in those villages.

Next, we investigate if distributing bracelets in exchange for donations magnifies the treatment effects of the public donation drive on child marriage, 16 months after the intervention. Table B.18 shows that, consistent with the salience mechanism, there is a clear ranking of effect sizes: across all outcomes, treatment effects are larger in villages assigned to both box and bracelets. For child marriage, the effect size of the public donation drive in combination with bracelets is almost three-fold that of the drive by itself. Having said that, the magnitude of the effect for villages assigned to host a public donation box without bracelets is still very large (0.9 p.p., a 16% reduction relative to the control group) and, because child marriage is a relatively infrequent outcome at younger ages, the difference between the two versions of the intervention is not precisely estimated ($p = 0.128$).

6 What Prevents Harmful Social Norms From Changing?

Given how simple the intervention we evaluate is, and how large the effect sizes are, why is it that the coordination problem – organizing public donation drives or other alternative signals of social preferences – does not get sorted by the community itself?

Coordination problems often generate dynamic inefficiencies, even long after the original conditions that gave rise to the norm disappear (Acemoglu and Robinson, 2006; Basu, 2018). Local leaders might be key to break away from such equilibria (e.g. by creating focal points that eliminate lower-ranked equilibria; Acemoglu and Jackson, 2014). If, however, local leaders extract power or rents from the baseline equilibrium (Henn, 2018), they might have incentives to block social norms from changing. Alternatively, norms could inefficiently persist even when villagers and local leaders alike would support change. In our setting, that might arise if, for instance, leaders under-estimate the effects of alternative signals on the baseline separating equilibrium based on child marriage.

In order to test which is the case, we elicit respondents' willingness to pay (WTP) for the intervention. If village chiefs and (counterfactual) box holders have a low WTP for the intervention regardless of its treatment effects, that would be consistent with the hypothesis that local leaders block social norms' change. In turn, if WTP increases with the baseline prevalence of under-15 marriage in villages assigned to the public donation drive, that would corroborate the hypothesis that villagers did not anticipate that the intervention would unravel long-standing social norms – as its benefits (when it comes to its effects on child marriage) are proportional to the baseline prevalence of child marriage.

In the follow-up survey, we asked respondents to state their willingness to pay for the public donation drive by using a Becker-DeGroot-Marshak (BDM) elicitation method (Becker, DeGroot and Marschak, 1964; Cornsweat, 1962). To ensure incentive compatibility, subjects were

⁴⁴ Bracelets on their own do not systematically affect school dropouts or participation in sexual initiation rituals in Table B.17.

informed that decisions would be eventually implemented in 10 villages, selected through a lottery after the interviews were concluded.

Table B.16 estimates how respondents' WTP varies across the treatment and control groups, allowing it to vary also with the baseline prevalence of child marriage within each group. Column (1) shows that, in villages where under-15 marriage was low-prevalence at baseline and without prior exposure to the public donation drive, respondents are willing to pay 5,218 MWK (≈ 7 USD) on average for the intervention. This is a non-negligible amount, equivalent to 8% of the average monthly spending by households in our sample. Interestingly, in villages where under-15 marriage was low-prevalence at baseline, prior exposure to the donation drive is associated with a slight reduction in the WTP (-702 MWK), whereas in villages where child marriage was prevalent, prior exposure sharply increases WTP: for every 10 p.p. higher baseline prevalence of under-15 marriage, prior exposure to the public donation drive increases WTP by roughly 900 MWK.

Such responses are consistent with villagers privately disliking child marriage. Since treatment effects are driven by high-prevalence villages, there is less reason to pay for the intervention where child marriage is rare – where, incidentally, charitable behavior was already more prevalent at baseline. In turn, experience with treatment effects over the previous year generates demand for the intervention, the more so the higher the baseline prevalence of the social norm.⁴⁵

As such, the evidence is inconsistent with the hypothesis that local leaders purposefully block social norms' change.⁴⁶ Rather, it suggests that villagers privately dislike the traditional practice, but do not anticipate the effects of alternative signals. Prior exposure makes respondents in high-prevalence villages happy to pay for an intervention that successfully disrupts the baseline separating equilibrium based on child marriage while still sustaining long-term cooperation.

7 Concluding Remarks

This paper provides first-hand evidence that individuals engage in traditional practices that effectively destroy children's human capital, like child marriage and sexual initiation rituals, at least partly to signal their social preferences. We further document that making an alternative signal of pro-sociality more salient gives rise to a new equilibrium in which child marriage is no longer informative about social preferences; instead, donations to other households increase and those who engage in charitable behavior are perceived as more pro-social. As a result, individuals are able to abandon long-standing traditions without impeding cooperation with other villagers. Since cooperation is sustained at lower signaling costs, the transition to the new separating equilibrium improves social welfare.

Our findings might help explain why support for traditional practices (from child marriage to female genital cutting) is decreasing at a fast pace in some parts of the world (see UNICEF's Demographic Health Surveys). Rapid urbanization across Sub-Saharan Africa and Southeast Asia may be giving rise to less trust-intensive societies, in line with the historical trajectories of

⁴⁵In Table B.16, Column (2) undertakes a placebo test, replicating the analysis for respondents' WTP for peanuts (instead of the intervention) as dependent variable. In that case, neither prior exposure to the intervention nor its interaction with baseline prevalence of child marriage affect respondents' WTP, as expected.

⁴⁶See Haenni and Lichand (2020) for a more involved discussion of when local elites matter for social norms' change.

developed and developing countries (Greif, 1993; Munshi and Rosenzweig, 2006). When there is less need to identify who is pro-social – because there are other mechanisms to enforce cooperation, like legal courts –, the signaling value of conforming to traditional practices decreases, even in the absence of alternative signals. That rationale is also consistent with the claim that urban anonymity causes some institutions to break down (Glaeser, 2014).

When it comes to policy implications, we showcase that social signaling mechanisms have the potential to discourage undesirable behaviors at the same time as boosting positive behaviors. Such interventions can be carried out by villages themselves, without external enforcement or supervision. In the context of persistent and locally entrenched traditional practices, such community-driven participatory programs might be particularly promising, as they signal rootedness in the community – above and beyond altruism in general –, which is precisely what conformity to long-standing traditions represents.

Having said that, social engineering norms at scale is a challenging endeavour. First and foremost, because the higher the share of society who adopts the signal, the higher the chance that a pooling equilibrium emerges (as the signaling value of the intervention gets crowded out; see Butera et al., 2019). In our model, it is not guaranteed that the intervention improves social welfare under a pooling equilibrium, as cooperation is no longer sustained. Second, because changes in signaling equilibria might cut across multiple dimensions – as we show for the case of child marriage and sexual initiation rituals in Malawi – possibly crowding out unanticipated (and, perhaps, even desirable) behaviors. The net effect of social engineering might be hard to predict, as it depends on whether different norms are complements or substitutes (Feng, Haenni and Lichand, 2020; Platteau, Camilotti and Auriol, 2018).

Is the new signaling equilibrium triggered by the public donation drive bound to persist in the long-term? The effect of the intervention on charitable behavior seems to fade-out over time, as differences across treatment and control villages in donations over the previous month are much less pronounced than in donations over the previous year. If differences eventually disappear, it could as well be that old traditions re-emerge in villages that used to be high-prevalence. Having said that, there is reason to believe that this is unlikely to happen. Treatment effects on willingness to pay for the intervention corroborate that individuals privately dislike child marriage and come to appreciate the effect of the intervention after they have been exposed to it. Willingness to pay for the donation box and red rubber bracelets is high and suggests that local leaders could keep collective action (through new drives or otherwise) moving forward – offering villagers opportunities to signal social preferences other than by conforming to traditions that harm girls.

In this paper, we have ignored the issue of whether local elites should be involved in social norms' change. While village chiefs' willingness to pay for the intervention suggests that local leaders do not block the process in our context, in a companion paper (Haenni and Lichand, 2020) we discuss how chief's attitudes towards social norms' change might depend on their ability to extract rents from the standing norm, and whether they can control the process through which the norm changes.

Our experiments shed light on multiple knowledge gaps, but also leave several open questions. In particular, we document that introducing alternative signals of social image unrelated to social

preferences (when bracelets were available for sale) led to a sharp increase in child marriage and teenage pregnancies relative to the control group. Such puzzling finding suggests that different dimensions of social image might differentially crowd in or out conformity to social norms. This is a promising avenue for future research.

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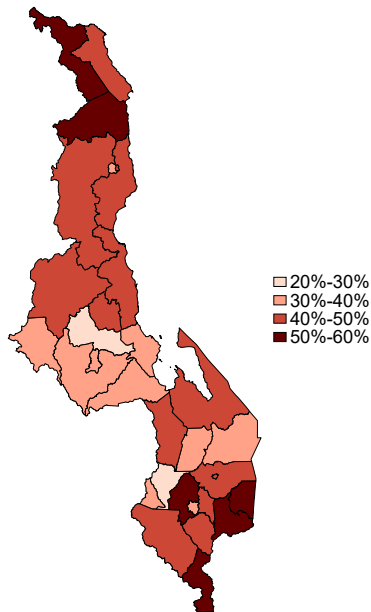
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Appendix A Supplementary figures

Figure A.1: Prevalence of Child Marriage and Sexual Initiation Rituals Across Districts of Malawi

Panel A: Prevalence of Girls' Marriage

Share of ever married women who married before age 18



Panel B: Prevalence of Sexual Initiation Rituals

Share of respondents stating sexual initiation rituals of girls are practiced in their village

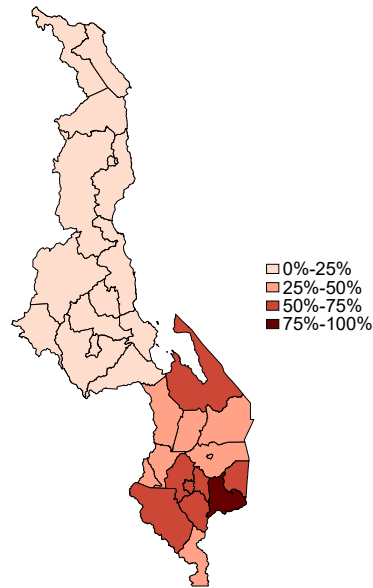
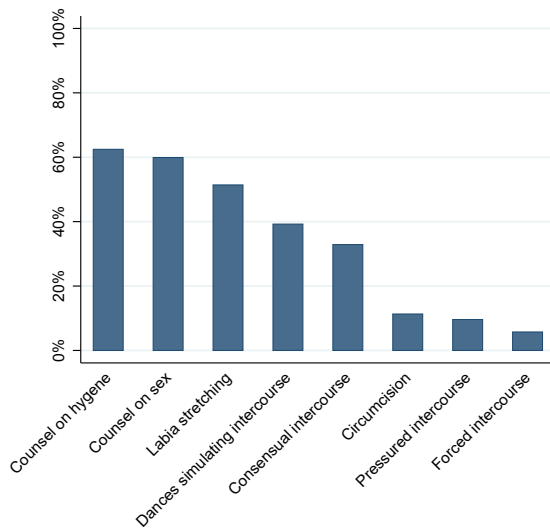


Figure A.2: Activities and decision makers of sexual initiation rituals of girls in Malawi. Share of respondents mentioning each category in multiple-choice questions.

Panel A: Activities at Sexual Initiation Rituals



Panel B: Decision Makers of Participation at Rituals

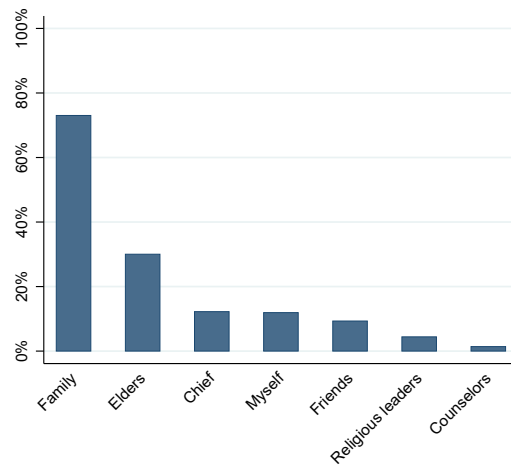
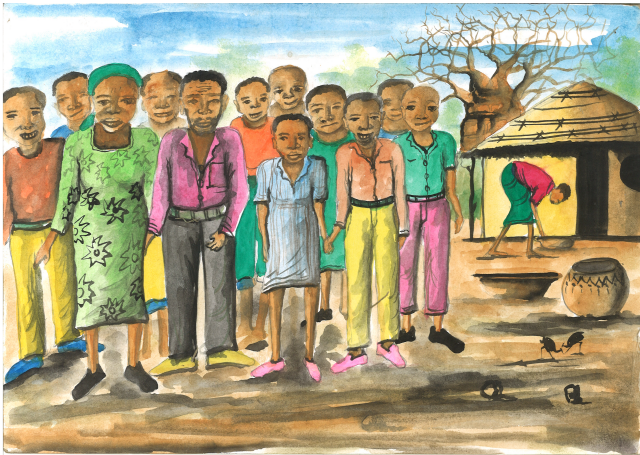


Figure A.3: Framing John as (not) supporting child marriage

Treatment A: John plans to marry off his 14 year old daughter soon



Treatment B: John does not plan to marry off his 14 year old daughter soon



Treatment A': John plans to marry off his 14 year old daughter soon.
John and his wife wear red rubber bracelets.



Treatment B': John does not plan to marry off his 14 year old daughter soon.
John and his wife wear red rubber bracelets.



Figure A.4: Compliance with Experiments

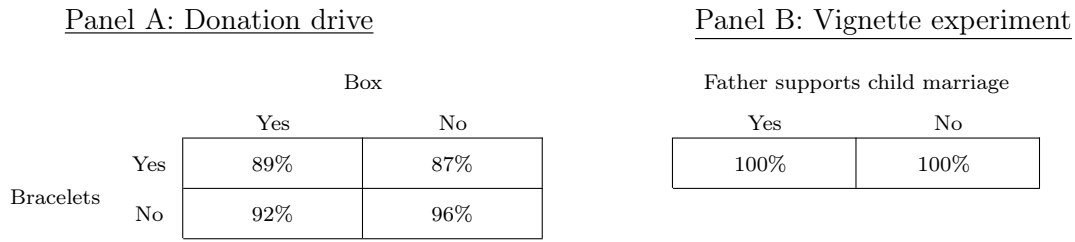
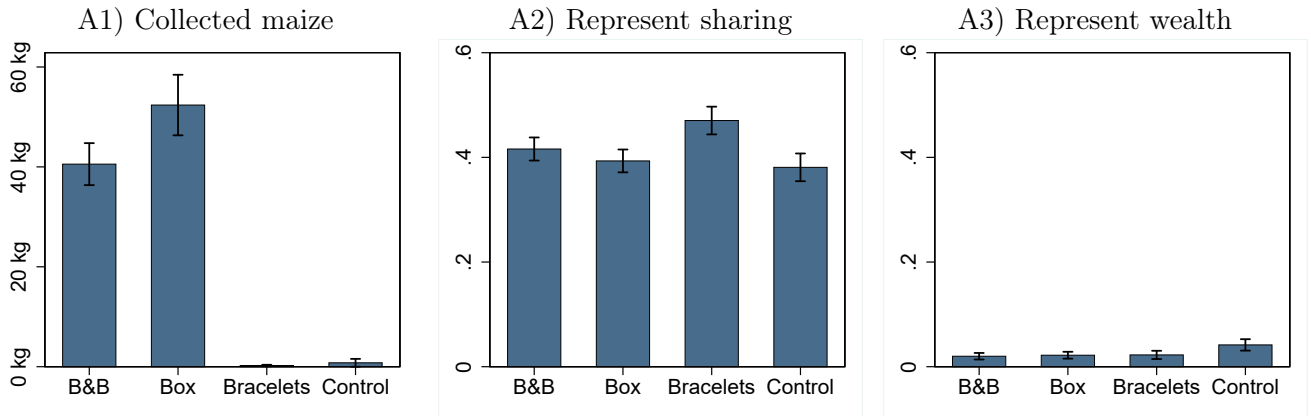
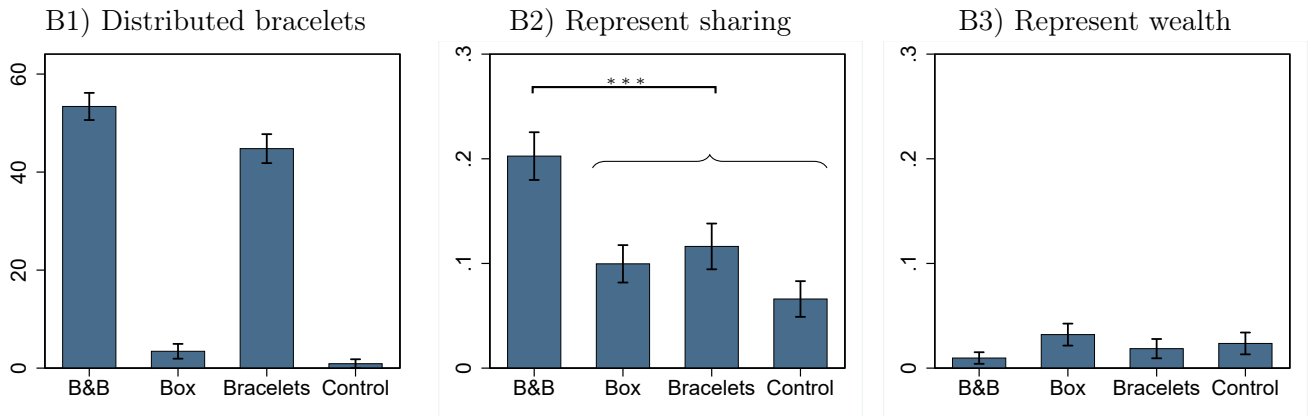


Figure A.5: Manipulation checks after 5 weeks

Panel A: Take-up and Public Perception of Food Collections

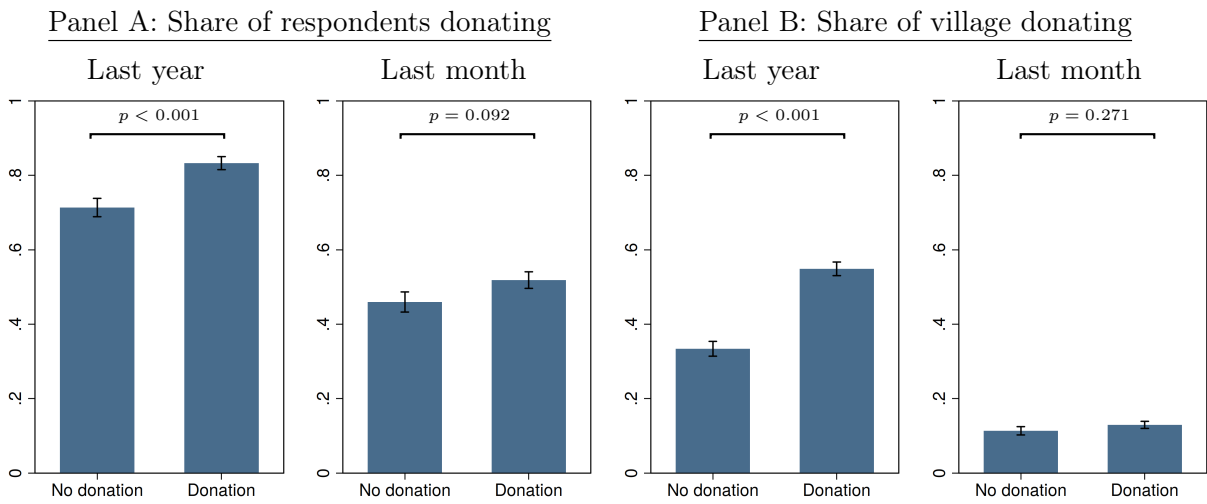


Panel B: Take-up and Public Perception of Red Rubber Bracelets



Notes: Take-up reported by village chiefs and public perception measured by share of respondents stating in an open survey question that food collections/red rubber bracelets represent sharing or wealth. Other common perceptions of bracelets were *something to wear*, *nothing*, and *friendship*, while other common perceptions of food collections were *helpfulness*, *nothing*, and *charity*.

Figure A.6: Treatment differences in food donations 16 months after the intervention



Note: Residualized donation shares, absorbing village-level controls (village size, population density, urban) as well as district and enumerator indicators, along with standard error bars. T-tests clustered at the village level.

Appendix B Supplementary tables

Table B.1: Correlation between self-reported pro-social preferences and revealed donations in the control group

Dependent variable:	(1) Summary Measure	Individual components		
		(2) Altruism	(3) Reciprocity	(4) Trust
Donated in the last 5 weeks	0.262** (0.117)	0.258** (0.116)	0.202* (0.108)	0.0358 (0.108)
Female	-0.0313 (0.105)	0.0761 (0.103)	-0.181* (0.0995)	0.0460 (0.0998)
Age	-0.00117 (0.00314)	-0.00403 (0.00294)	-0.00165 (0.00307)	0.00347 (0.00289)
Weekly spending (in USD)	0.00329 (0.00345)	0.00757** (0.00382)	0.000115 (0.00339)	-0.00147 (0.00389)
Constant	-0.0234 (0.164)	-0.106 (0.165)	0.154 (0.149)	-0.0689 (0.155)
Observations	445	445	445	445

Notes: The summary measure (1) is an equally weighted average of standardized individual measures for (2) altruism, (3) reciprocity, and (4) trustworthiness (see Section 3.4.1). Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.2: Balance of covariates (vignette experiment)

Variable	(1) Control	(2) Child marriage	(3) Diff.
Urban	0.177 (0.382)	0.181 (0.385)	0.005 (0.009)
Village size (#HH)	116.360 (31.097)	116.532 (30.805)	0.172 (0.742)
Number of surveyed HH	18.488 (1.670)	18.501 (1.666)	0.014 (0.040)
Distance to neighbor (km)	0.024 (0.019)	0.023 (0.018)	-0.001 (0.000)
Weekly food consumption (\$)	11.422 (8.641)	11.222 (8.431)	-0.201 (0.204)
Weekly non-food consumption (\$)	7.249 (7.296)	7.169 (7.112)	-0.080 (0.172)
Household size	4.943 (2.048)	4.954 (1.988)	0.011 (0.048)
Female	0.587 (0.492)	0.589 (0.492)	0.001 (0.012)
Age	35.846 (16.553)	35.353 (16.514)	-0.492 (0.396)
F-test of joint significance (p-val.)			0.698
Observations	3,468	3,510	6,978

Means with standard deviations in parentheses. Joint test based on standard errors clustered at the village level.

Table B.3: Balance of baseline covariates (donation drive)

Variable	(1) Box&Bracelets	(2) Box	(3) Bracelets	(4) Control	(5) F-test (p-val.) (1)=(2)=(3)=(4)
Urban	0.185 (0.388)	0.195 (0.397)	0.187 (0.390)	0.185 (0.388)	0.996
Village size (#HH)	115.401 (31.995)	116.723 (30.304)	115.619 (32.133)	116.593 (30.468)	0.986
Number of surveyed HH	18.414 (1.552)	18.550 (1.497)	18.438 (1.854)	18.160 (2.133)	0.456
Distance to neighbor (km)	0.024 (0.016)	0.024 (0.017)	0.025 (0.022)	0.021 (0.016)	0.480
Weekly food consumption (\$)	11.055 (8.488)	11.379 (8.702)	11.230 (8.421)	11.748 (8.554)	0.629
Weekly non-food consumption (\$)	7.361 (7.616)	7.232 (7.166)	7.420 (7.376)	7.060 (7.046)	0.895
Household size	5.077 (2.139)	5.143 (2.045)	4.969 (1.967)	5.098 (2.121)	0.450
Female	0.589 (0.492)	0.587 (0.492)	0.568 (0.495)	0.589 (0.492)	0.165
Age	37.716 (16.072)	37.621 (16.298)	37.888 (16.698)	37.721 (16.010)	0.965
χ^2 -test of joint significance (p-val.)					0.312
Observations	5,051	5,197	3,802	3,722	17,772

Means with standard deviations in parentheses. Joint tests based on standard errors clustered at the village level.

Table B.4: Balance of covariates (16 month follow-up sample)

Variable	(1) Control	(2) Donation drive	(3) Diff.
<u>Panel A: Girl-level covariates</u>			
Age at follow-up	14.155 (2.072)	14.210 (2.139)	0.055 (0.073)
Household spending at baseline (in USD)	19.761 (14.851)	19.254 (14.502)	-0.507 (0.509)
F-test of joint significance (p-val.)			0.307
Individual observations	1,397	2,039	3,436
<u>Panel B: Village-level covariates</u>			
Share of girls recognized from baseline	0.752 (0.244)	0.762 (0.236)	0.010 (0.024)
Urban	0.184 (0.389)	0.179 (0.384)	-0.005 (0.039)
Village size (#HH)	115.437 (30.592)	115.908 (30.568)	0.471 (3.078)
Distance to neighbor (km)	0.023 (0.019)	0.024 (0.017)	0.001 (0.002)
F-test of joint significance (p-val.)			0.954
Village observations	174	229	403

Means with standard deviations in parentheses. Joint tests based on standard errors clustered at the village level.

Table B.5: Summary statistics of the baseline sample

Number of households	7,388
Household size	4.64
Share of rural households	0.82
Number of individual surveys	14,821
Average age of respondents	37.92
Share of female respondents	0.56
Villages	412

Table B.6: Characteristics of Individuals by Donation Status

Variable	Control			Donation drive		
	(1) No donation	(2) Donation	(3) Diff	(4) No donation	(5) Donation	(6) Diff
Female	0.616 (0.487)	0.476 (0.500)	-0.140*** (0.041)	0.630 (0.483)	0.563 (0.496)	-0.066** (0.031)
Age	35.788 (16.934)	33.796 (16.185)	-1.992* (1.094)	34.195 (16.552)	35.300 (16.633)	1.106 (0.879)
Altruism	-0.063 (1.003)	0.069 (1.019)	0.132 (0.084)	0.041 (0.973)	0.121 (1.000)	0.079 (0.057)
Positive Reciprocity	-0.010 (1.023)	0.073 (1.004)	0.082 (0.078)	-0.023 (1.010)	0.014 (0.995)	0.037 (0.060)
Trust	0.017 (1.006)	-0.150 (0.977)	-0.167** (0.080)	0.028 (1.043)	-0.026 (1.040)	-0.054 (0.062)
Support for child marriage	0.065 (0.247)	0.023 (0.151)	-0.042*** (0.012)	0.034 (0.181)	0.043 (0.203)	0.009 (0.011)
Married before age 18	0.349 (0.477)	0.206 (0.405)	-0.143*** (0.040)	0.337 (0.473)	0.280 (0.450)	-0.057* (0.033)
Support for sex. initiation	0.077 (0.267)	0.066 (0.249)	-0.011 (0.017)	0.066 (0.249)	0.067 (0.250)	0.001 (0.014)
Sexually initiated	0.448 (0.498)	0.520 (0.501)	0.073 (0.046)	0.425 (0.495)	0.489 (0.500)	0.063* (0.034)
Weekly \$ food spending	11.046 (8.243)	12.477 (8.866)	1.431** (0.609)	11.204 (8.710)	11.313 (8.718)	0.109 (0.518)
Weekly \$ non- food spending	6.382 (6.349)	7.637 (7.392)	1.255** (0.522)	7.303 (7.366)	7.240 (7.085)	-0.062 (0.439)
Observations	677	334	1,011	821	584	1,405

Notes: Individual statistics conditional on self-reported donation to recent food collection. Columns (i)-(iii) show statistics for villages where no food collection box was distributed (*Bracelets* and *Control*) while columns (iv)-(vi) show statistics for villages where food collection boxes were distributed (*Box&Bracelets* and *Box*). Number of observations is reduced as question about donation status was only asked to a subset of households.

Table B.7: Joint Distribution of Signaling Strategies

	Control			Donation drive		
	No support for child marriage (94.88%)	Support for child marriage (5.12%)	Total	No support for child marriage (96.23%)	Support for child marriage (3.77%)	Total
No donation	65.49	84.78	66.48	58.09	52.08	57.86
Donation	34.51	15.22	33.52	41.91	47.92	42.14
Total	100.00	100.00	100.00	100.00	100.00	100.00

Table B.8: Predetermination of Child Marriage Norm in Village

Dependent variable:	Share married < age 15
Box&Bracelets	-0.00482 (0.00803)
Box	-0.00953 (0.00773)
Bracelets	-0.00544 (0.00867)
Constant	0.0568*** (0.00637)
F-test: Box&Bracelets=Box (p-value)	0.513 (0.474)
Observations	8,009

Standard errors clustered at the village level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.9: Perceived pro-social preferences of households as a function of their past donations and the donation drive

Dependent variable:	(1) Summary measure (std.)	(2) Summary measure (std.)
Donated in past year	0.378*** (0.0678)	
Donated in past year \times Donation drive	0.103 (0.0963)	
Donated in past month		0.541*** (0.0793)
Donated in past month \times Donation drive		-0.135 (0.101)
Weekly spending (in USD)	0.00216* (0.00117)	0.00249** (0.00117)
Constant	-0.264*** (0.0354)	-0.145*** (0.0251)
Observations	2,761	2,761
Village fixed effects	✓	✓

Notes: The summary measure is an equally weighted standardized average of standardized individual measures for altruism, reciprocity, and trust (see Section 3.4.1). Standard errors, clustered at the village level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.10: Average effects over all age groups (omit chiefs' answers)

	(1) Married <18	(2) Children	(3) School dropout	(4) Initiated
Donation drive	-0.0188* (0.0102)	-0.0147 (0.00929)	-0.0182 (0.0174)	-0.0417* (0.0225)
Control mean	0.0591	0.0494	0.138	0.0927
Observations	2,403	2,411	2,191	1,633
District fixed effects	✓	✓	✓	✓
Enumerator fixed effects	✓	✓	✓	✓
Controls	✓	✓	✓	✓

Notes: Regressions additionally include individual controls (age, age², and age³) and village-level controls (village size, population density, urban, and baseline prevalence of child marriage or initiation rituals) and enumerator indicators. Standard errors, clustered at the village level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.11: Average effects over all age groups (twice validated responses only)

	(1) Married <18	(2) Children	(3) School dropout	(4) Initiated
Donation drive	-0.0151** (0.00731)	-0.0251*** (0.00687)	-0.0122 (0.0160)	-0.0131 (0.0123)
Control mean	0.027	0.031	0.077	0.028
Observations	1,816	1,823	1,447	1,236
District fixed effects	✓	✓	✓	✓
Enumerator fixed effects	✓	✓	✓	✓
Controls	✓	✓	✓	✓

Notes: Regressions additionally include individual controls (age, age², and age³) and village-level controls (village size, population density, urban, and baseline prevalence of child marriage or initiation rituals) and enumerator indicators. Standard errors, clustered at the village level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.12: Child marriage: cumulative treatment differences over age groups

	Overall	Box holder		Baseline prevalence of child marriage		Baseline spending (village percentile)	
	(1)	(2) Chief	(3) Other	(4) High	(5) Low	(6) Poorest 10%	(7) Richest 90%
Married < 18	-0.0805* (0.0450)	-0.0520 (0.0644)	-0.107 (0.0668)	-0.110* (0.0667)	-0.0572 (0.0690)	-0.0766 (0.125)	-0.0730 (0.0491)
Test of equality (p-val.)		0.562		0.590		0.979	
Control mean	0.416	0.368	0.460	0.441	0.396	0.391	0.413
Observations	3,436	3,436		3,436		3,436	
District fixed effects	✓	✓		✓		✓	
Controls	✓	✓		✓		✓	

Notes: Cumulative effects over yearly age brackets. Regressions additionally include village-level controls (village size, population density, urban, and baseline prevalence of child marriage) and enumerator indicators. Indicator for baseline prevalence of child marriage is based on a median split and the median age in that sample is 35 years or ≈ 1 generation before the present data collection. Bootstrapped standard errors, based on 1000 replications and clustered at the village level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.13: Effect of public donation drive on attitudes towards traditional practices

	(1) Child Marriage	(2) Initiation
Donation drive	-0.0140*** (0.00504)	-0.0136* (0.00820)
Control mean	0.054	0.077
Chi ² -test Donation jointly=0, (p-val.)	0.0135	
Observations	11,123	7,243
Individual controls	✓	✓
Village-level controls	✓	✓

Notes: Regressions additionally include individual controls (female, age, age², and age³) and village-level controls (village size, population density, urban, and local prevalence of sexual initiation in columns (iii)-(iv)), plus a constant. Standard errors, clustered at the village level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.14: Effect of the donation drive on short term behavior (Placebo)

	(1) Child Marriage	(2) Initiation
Donation drive	-0.00940 (0.0110)	-0.00105 (0.00159)
Control mean	0.30	0.27
Chi ² -test Donation jointly=0, (p-value)	1.229 (0.541)	
Observations	8,534	5,238
Individual controls	✓	✓
Village-level controls	✓	✓

Regressions additionally include individual controls (female, age, age², and age³) and village-level controls (village size, population density, urban, and local prevalence of sexual initiation in columns (iii)-(iv)), plus a constant. Standard errors, clustered at the village level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.15: Effect of public donation drive on attitudes towards traditional practices, heterogeneities (2nd stages from IV)

	Child Marriage				Sexual Initiation				
	(1) HH with Girls 12-17	(2) Gender of respondent	(3) Prevalence of child marriage	(4) Reason for own marriage	(5) Poorest households	(6) HH with Girls 12-14	(7) Gender of respondent	(8) Prevalence of initiations	(9) Poorest households
HH w/ girls × Donation	-0.0184** (0.00740)					-0.0203 (0.0136)			
HH w/o girls × Donation	-0.0146* (0.00767)					-0.0139 (0.0109)			
HH w/ girls	-0.0133* (0.00761)					-0.00695 (0.0116)			
Female × Donation		-0.0214*** (0.00782)					-0.0239* (0.0123)		
Male × Donation		-0.00869 (0.00738)					-0.00411 (0.0133)		
Above median prevalence × Donation			-0.0276*** (0.00802)					-0.0208 (0.0195)	
Below median prevalence × Donation			0.000344 (0.00750)					-0.0129 (0.0105)	
Above median prevalence			0.0280*** (0.00854)					0.0238 (0.0256)	
Traditional reason × Donation				-0.0404 (0.0410)					
Emergency reason × Donation				-0.00404 (0.0104)					
Traditional reason				0.0139 (0.0354)					
10% poorest × Donation					-0.0229 (0.0154)				0.00429 (0.0228)
All other households × Donation					-0.0151** (0.00602)				-0.0172* (0.00988)
10% poorest					0.0127 (0.0121)				-0.00275 (0.0177)
Control mean			0.054			0.077			
1 st -stage F-stat of instrument	491.4	468	223.4	427.4	356.7	472.4	437.4	343.2	329.9
Chi ² -tests (p-val.):									
HH w/ = w/o girls	0.697					0.675			
Female = Male		0.202					0.251		
Above = Below median prev.			0.011					0.722	
Tradition = Emergency				0.384					
Poor=Non-poor					0.623				0.368
Observations	11,123	11,123	11,123	2,577	11,038	7,243	7,243	7,243	7,183
Additional controls	✓	✓	✓	✓	✓	✓	✓	✓	✓

Regressions additionally include individual controls (female, age, and age²) and village-level controls (village size, population density, urban, and local prevalence of sexual initiation in columns (iii)-(iv)), plus a constant. Standard errors, clustered at the village level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.16: Willingness to pay for the intervention

Dependent variable:	(1) WTP for donation drive (MWK)	(2) WTP for peanuts (MWK)
Public donation drive	-701.8* (375.5)	-1.508 (5.176)
Public donation drive \times Share married < age 15	9,016** (4,504)	11.33 (63.18)
Share married < age 15	-1,094 (3,105)	-18.96 (46.17)
Constant	5,218*** (291.2)	69.68*** (3.843)
Observations	733	733

736 of the sampled respondents were interviewed and out of those 733 were able to pass the test questions about the WTP elicitation, explaining the smaller sample size. Standard errors clustered at the village level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.17: Average effects of selling bracelets on behavior

	(1) Married <18	(2) Mother at 18	(3) School dropout	(4) Initiated
Bracelets only	0.0349*** (0.0130)	0.0288** (0.0129)	-0.0251 (0.0219)	-0.00559 (0.0178)
Control mean	0.058	0.052	0.178	0.069
Observations	1,397	1,397	1,291	994
District fixed effects	✓	✓	✓	✓
Enumerator fixed effects	✓	✓	✓	✓
Controls	✓	✓	✓	✓

Notes: Regressions additionally include individual controls (age, age², and age³) and village-level controls (village size, population density, urban, and baseline prevalence of child marriage or initiation rituals). Standard errors, clustered at the village level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.18: Average effects of public visibility of donations on behavior

	(1) Married <18	(2) Mother at 18	(3) School dropout	(4) Initiated
Box	-0.00918 (0.0104)	-0.0112 (0.00857)	-0.0128 (0.0169)	-0.0159 (0.0181)
Box & Bracelets	-0.0261*** (0.00948)	-0.0193** (0.00937)	-0.0373** (0.0179)	-0.0224 (0.0156)
F-test: Box=Box & Bracelets	0.128	0.390	0.197	0.683
Control mean	0.058	0.052	0.178	0.069
Observations	3,436	3,436	3,153	2,397
District fixed effects	✓	✓	✓	✓
Enumerator fixed effects	✓	✓	✓	✓
Controls	✓	✓	✓	✓

Notes: Regressions additionally include individual controls (age, age², and age³) and village-level controls (village size, population density, urban, and baseline prevalence of child marriage or initiation rituals). Standard errors, clustered at the village level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix C Pre-analysis plan

The purpose of this appendix is to guide the reader through the pre-analysis plans. This paper is part of a larger research project that was jointly pre-registered as trial AEARCTR-0002856 in the AEA RCT Registry. We want to increase transparency by helping the reader navigate through the extensive pre-analysis plans.

There are two relevant documents available under the section *Analysis Plan* online and as appendices C.2-C.3 below. We henceforth refer to pre-analysis-plan_v2.pdf that was pre-registered on 2nd July 2018 (before the original intervention) as *PAP1* and to PAP_follow-up.pdf that was pre-registered on 22nd September 2019 (before the endline data collection) as *PAP2*.⁴⁷

We discuss for each analytical section of this paper which outcomes and analyses were pre-specified and justify deviations from the pre-analysis plan.

C.1 Outline and deviations

3 Sampling

3.2 Experimental design

- Deviations: Out of the pre-registered 413 villages that had been randomly selected from an existing sampling frame, 412 were listed at baseline. In 1 village there were ongoing disputes about chieftaincy that made it impossible to conduct listings. At end-line, only 411 villages were sampled as 2 villages had merged and were consecutively counted as one village.

4 Does Supporting Child Marriage Signal Social Preferences?

4.1 Chiefs' perceptions

- Outcome variables: *PAP1 Section 2, bullet (ii)* and *PAP2 Section 3, bullet (iii)*
- Analyses: *PAP1 Section 3.1.3* and *PAP2 Section 4.1*.
- Deviations: *PAP1 Section 3.1.3* only registers the vignette experiment version of the analysis (see Section 4.2). *PAP2 Section 4.1* refers to Figure 1, the analogue to Figure 6 at endline.

4.2 Vignette experiment

- Design of the experiment: *PAP1 Section 1.3*
- Outcome variables: *PAP1 Section 2, bullet (ii)*
- Analyses: *PAP1 Section 3.1.3*

⁴⁷The document pre-analysis-plan.pdf is outdated and was replaced by pre-analysis-plan_v2.pdf before the start of the study.

- Deviations: We additionally include control variables as well as village fixed effects and rely on a continuous rather than a binary interaction term as we find this is more informative. Results are virtually identical without controls and fixed effects. Thus we report the more conservative version in the paper.

5 Do Public Donations Substitute for Child Marriage as Signal?

5.1 Effect on perceived pro-social preferences of who supports child marriage

- Design of the experiments: *PAP1 Section 1.3* and *PAP1 Section 1.1*
- Outcome variables: *PAP1 Section 2, bullet (ii)* and *PAP2 Section 3, bullet (iii)*
- Analyses: *PAP1 Section 3.1.4* and *PAP2 Section 4.1*
- Deviations: Table 2 additionally includes village fixed effects as well as controls. Results are virtually identical without controls and fixed effects. Thus we report the more conservative version in the paper. We show columns separately to avoid tiple interaction terms.

5.2 Effect on child marriage and related outcomes for girls

- Design of the experiments: *PAP1 Section 1.1*
- Outcome variables: *PAP2 Section 3, bullet (ii)*
- Analyses: *PAP2 Section 4.2.1*
- Deviations:
 - In Table 3 we additionally include fixed effects for precision.
 - The cumulative analyses in Table 4 and Figure 7 were not pre-specified. However, we find the additional representation very informative and thus important to include in the paper.

5.3 Effect on donations

- Design of the RCT: *PAP1 Section 1.1*
- Outcome variables:
 - Short-run: Manipulation checks not pre-specified
 - Long-run: *PAP2 Section 3, bullet (i)*
- Analyses:
 - Short-run: Manipulation checks not pre-specified
 - Long-run: *PAP2 Section 4.2.1* Manipulation check: long-term compliance with the treatment
- Deviations: Additional controls for precision

5.4 Effect on perceived pro-social preferences of who donates

see 5.1

5.5 Effect cooperation

- Design of the RCT: *PAP1 Section 1.1*
- Outcome variable: *PAP2 Section 3, bullet (iii)*
- Analyses: Not pre-specified.

5.6 Robustness checks

5.6.1 Reliability of the end-line survey data

Not pre-specified

5.6.2 Experimenter demand effects

- Design of the experiments: *PAP1 Section 1.1* and *PAP1 Section 1.4.3*
- Outcome variables: *PAP1 Section 2, bullet (i), (iii)*
- Analyses: *PAP1 Section 3.1.5*
- Deviations:
 - Placebo exercise not pre-specified as such, but identical to the effects on attitudes registered in *PAP1 Section 3.1.5*.
 - Priming specification in Table 6 was not pre-specified as it is a straightforward calculation of a treatment effect.

5.6.3 Heterogeneous treatment effects

- Design of the experiments: *PAP1 Section 1.1*
- Outcome variables: *PAP2 Section 3, bullet (ii)*
- Analyses: *PAP2 Section 4.3.3*
- Deviations: The heterogeneity analysis with baseline spending levels was not pre-specified but provides an informative additional robustness check.

5.6.4 Alternative signals and intensity of pro-social signaling

- Design of the experiments: *PAP1 Section 1.1*
- Outcome variables: *PAP2 Section 3, bullet (ii)* and *PAP1 Section 2, bullet (i)*
- Analyses: and *PAP1 Section 3.1.5*
- Deviations: The analysis was only pre-registered with short-term attitudes but not with long-term behavior as outcome variable. For completeness, we report both outcomes.

6 What Prevents Harmful Social Norms From Changing?

- Design of the experiment: *PAP2 Section 2.2*
- Outcome variables: *PAP2 Section 3, bullet (i)*
- Analyses: *PAP2 Section 4.3.1*
- Deviations: Columns (2) of Table B.16 were not pre-specified but provides additional supportive evidence.

Pre-analysis Plan for Harming to Signal (2 July 2018)

1 Interventions and Experiments

1.1 Signaling Intervention

During household listings we implement a signaling intervention in survey villages.

1.1.1 Hypotheses

With this intervention we test the following main hypotheses:

- Does introducing a new signaling language, i.e. a less harmful signaling opportunity, decrease support for other, more harmful signals?
- Can an additional social signal overwrite the signaling effect of engagement in harmful traditional practices?
- Do chiefs enforce harmful social norms as a signaling mechanism because they exercise control over it?
- Are there trade-offs between authority and the erosion of established social norms

1.1.2 Experimental Design

Testing these four main hypothesis leads to the following experimental design where we introduce a new pro-social signaling opportunity at the village level. The signaling opportunity consists of colorful rubber bracelets that can be obtained in return for being pro-social. 4 treatment conditions with/without rubber bracelets and with/without food donations are necessary to clearly isolate the proposed mechanism. More details on the analyses follows in Section 3.1.

- A. Under the *donation boxes/bracelet treatment* bracelets are distributed to the 10 most pro-social households in the village (according to the village chiefs prior assessment). The rest of the village can acquire the bracelets against food donations. Village chief advertises the scheme. Food donations will be - and are announced to be – collected and distributed to the most needy in the village by the manager.
- B. Under the *no donation boxes/bracelet treatment* bracelets are distributed to 10 random households as gifts and bracelets can be bought for the same price as a food donation is valued. Village chief advertises the scheme. Money will be and is announced to be kept by the manager of the scheme.
- C. Under the *donation boxes/no bracelet treatment*, no bracelets are available in return for food donations, but food donations equally advertised and managed by chief as in A.
- D. The *no donation boxes/no bracelet condition* is a pure control.

In order to analyze the extent to which chiefs enforce harmful social norms as a signaling mechanism because they exercise control over it, and to investigate potential trade-offs between authority and

the erosion of established social norms we cross-randomize whether the village chief or a person on the list of pro-social households manages the scheme - on top of conditions A-D

- E. Under the *managed by village chief condition* the village chief is responsible for managing the bracelets and the food donations and for the distribution in the end ($C_c = 1$).
- F. Under the *managed by pro-social person* condition number 10 on the list of pro-social people is responsible for managing the bracelets and the food donations and for the redistribution in the end. The chief is informed about that choice and the reason for that choice ($C_c = 0$).

2x2 Design with cross-randomized variation in manager of the intervention:

	Bracelets ($B_v = 1$)	No Bracelets ($B_v = 0$)
Donation boxes ($D_v = 1$)	A: $\frac{4}{14}N$ (118 villages)	B: $\frac{4}{14}N$ (117 villages)
No Donation boxes ($D_v = 0$)	C: $\frac{3}{14}N$ (89 villages)	D: $\frac{3}{14}N$ (89 villages)

Managed by village chief ($C_c = 1$)	Managed by most pro-social person ($C_c = 0$).
E: $\frac{1}{2}N$ (207 villages)	F: $\frac{1}{2}N$ (206 villages)

Assignment to treatment and control group is done at the village level. Imperfect compliance is taken as intention-to-treat because those are the effects to be expected from any actual program (which depends on villages' compliance). Thus, villages where chiefs do not allow for the chosen treatment are still included in the survey – if permission is given.

1.2 Self-esteem Intervention (E_h)

We manipulate self-esteem in the short-term with a self affirmation task.

1.2.1 Hypotheses

With this intervention we test the following main hypotheses:

- Does higher self-esteem decrease reputational concerns and thereby reduce the willingness to contribute to goods with status signaling component, like local traditions?

- Does self-esteem affect social desirability bias?

1.2.2 Experimental Design

We use a self-affirmation task (Steele 1988, Cohen et al. 2009, Hall et al. 2013, Bursztyn et al. 2017), where we ask treated individuals to reflect on a recent experience or achievement that made them feel proud. Control individuals are asked to talk about their favorite dish. We use the 10-question Rosenberg self-esteem scale as manipulation check of the self-esteem priming.

Treatment ($E_h = 1$)	Control ($E_h = 0$)
$1/2N$ (4130 households)	$1/2N$ (4130 households)

Assignment to treatment and control group is done at the household level. Half the households within each village are randomly assigned to the treatment condition while the other half is assigned to the control condition.

1.3 Experiment on Perceived Public Image (J_h^1, J_h^2)

We test how perceived pro-sociality depends on the engagement in harmful traditional practices.

1.3.1 Hypotheses

- Do individuals use harmful traditional practices for signaling their pro-sociality?
- Can an additional social signal overwrite the signaling effect of engagement in harmful traditional practices?

1.3.2 Experimental Design

In order to learn whether people use harmful traditional practices for signaling their pro-sociality we want to find out how individuals perceive a hypothetical person who does (not) engage in harmful traditional practices. We therefore randomly assign people to one of two conditions.

In condition 1, subjects are asked to evaluate a hypothetical person who wants to marry off his 14 year old daughter and encouraged her to participate in local initiation rituals, on dimensions altruism, reciprocity, and trustworthiness ($J_h^1 = 1$).

In condition 2, subjects are asked to evaluate a hypothetical person who does not want to marry off his 14 year old daughter and did not encourage her to participate in local initiation rituals, on dimensions altruism, reciprocity, and trustworthiness ($J_h^1 = 0$).

Condition 1 ($J_h^1 = 1$)	Condition 2 ($J_h^1 = 0$)
$1/2N$ (4130 households)	$1/2N$ (4130 households)

Assignment to the two conditions is done at the household level. Half the households within each village are randomly assigned to treatment condition 1 while the other half is assigned to treatment condition 2.

Additionally, we ask each subject to evaluate the same hypothetical person after learning that this person recently obtained a rubber bracelet (J_h^2).

1.4 List Experiments

List experiments (Raghavaram and Federer, 1979) are a standard method to account for social desirability bias in survey questions. We adapted the method to work under constraints regarding illiteracy.

1.4.1 Hypotheses

- Is there social desirability bias involved in reporting of attitudes and (planned) engagement in harmful traditional practices?
- Can we measure individual-level susceptibility to social pressure in list experiments?
- Does self-esteem affect attitudes towards harmful traditional practices or exclusively reporting thereof?

1.4.2 Experimental Design

Condition 1	Condition 2
$1/2N$ (4130 households)	$1/2N$ (4130 households)

In condition 1, subjects answer 3 sub questions in List experiments 1-3 and 4 sub questions in List experiments 4-6.

In condition 2, subjects answer 4 sub questions in List experiments 1-3 and 3 sub questions in List experiments 4-6.

Assignment to treatment and control group is done at the household level. Half the households within each village are randomly assigned to treatment condition 1 while the other half is assigned to treatment condition 2.

Additionally, individuals answer 3 sub questions in List experiment 7 and 4 sub questions in List experiment 8, or vice versa, with equal proportions in both treatment conditions of the self-esteem intervention.

1.4.3 Validation Measures

One List experiment is designed in a way to show a lower bound on measured social desirability bias (Statement: “We are (now) in Malawi”. Note: adapted version (2. July 2018) after learning during field training of enumerators that many Malawians do not know the meaning of “Africa” in the previously registered validation question.). Another List experiment serves the double purpose of setting an upper bound and revealing the sensitivity to experimenter demand effects by using the method proposed by Quidt, Haushofer, Roth (2017) (Statement: “There are common cultural practices in this village that may harm children”).

1.4.4 Individual measure of susceptibility to social pressure (*Susceptibility_i*)

We can calculate an individual level measure for susceptibility to social pressure as the difference between blocks of direct and list responses for individuals who answer sensitive questions (only available for half the sample):

$$Susceptibility_i = \sum_j (List_{ij} - Direct_{ij})$$

We validate the proposed sensitivity measure with 1/3 of the population by including 13 items from the validated social desirability scale by Ballard (1992).

2 Outcomes

We consider 4 groups of outcomes: (i) attitudes towards and planned future engagement in harmful traditional, (ii) public perception of an individual who engages in harmful traditional practices, (iii) prevalence of harmful traditional practices, and (iv) village chiefs’ characteristics and self-perception. We have several outcome variables for each of the 4 groups. To account for multiple testing, multiple outcomes are grouped into sub-families and families, with inference conducted using seemingly unrelated regressions, following Kling, Liebman and Katz (“Experimental Analysis of Neighborhood Effects”, ECMA, 2007)

(i) Attitudes towards and planned future engagement in harmful traditional practices: *A*

- Direct elicitation of child marriage, initiation rituals, FGM/C
- List elicitation of child marriage, initiation rituals, FGM/C
- Krupka/Weber elicitation of child labor, child marriage, initiation rituals, FGM/C

(ii) Public perception of an individual that engages in harmful traditional practices: *P*

The following measures are adapted versions after piloting Falk et al's (2016) social preference module.

- Altruism (scale 0-10): *"How willing is John to help other people without expecting anything in return? Helping could for example be lending a tool or giving some money to other households that need it desperately"*
- Reciprocity (scale 0-10): *"When someone treats John unfairly, for instance when a person steals and eats some of John's food, how willing is John to punish this person, for example by blaming him in public?"* and *"When someone does John a favor, for instance when a person helps John to fix his roof, how willing is John to return the favor in the future, for example by also helping this other person to fix something?"*
- Trust/Trustworthiness (scale 0-10): *"John is reliable, honest, and truthful"*

Here, we are interested on the joint measure of pro-sociality, containing altruism, reciprocity and trustworthiness.

(iii) Prevalence of harmful traditional practices: Y

- Direct elicitation of child marriage, initiation rituals, FGM/C, and child labor
- List elicitation of child labor

(iv) Village chiefs' characteristics and self-perception: K , Susceptibility

3 Analysis Plan

3.1 Harming to Signal

3.1.1 Do pro-social individuals follow (Y_i)/support (A_i) harmful traditional practices more often?

$$Y_i = \beta_0 + \beta_1 \text{Prosociality}_i + e_i$$

$$A_i = \beta_0 + \beta_1 \text{Prosociality}_i + e_i$$

We are interested on the joint measure of pro-sociality, containing altruism, reciprocity, and trust from the Falk et al. (2016) social preference module.

Further, it may be informative to consider the interaction with prevalent local social norms. Effects may depend on prevalence of harmful practices in village (Harmful_Norm_v).

$$Y_{iv} = \beta_0 + \beta_1 \text{Prosociality}_i + \beta_2 \text{Harmful_Norm}_v + \beta_3 \text{Prosociality}_i \text{Harmful_Norm}_v + e_{iv}$$

$$A_{iv} = \beta_0 + \beta_1 \text{Prosociality}_i + \beta_2 \text{Harmful_Norm}_v + \beta_3 \text{Prosociality}_i \text{Harmful_Norm}_v + e_{iv}$$

3.1.2 Are individuals with increased self-esteem (E_h)/reduced reputational concerns more likely to oppose social norms related to harmful traditional practices?

Distinguishing between effects on attitudes A_{ih} and effects on reporting (social desirability bias) requires running the following regressions:

$$(i) A_{ih} = \beta_0 + \beta_1 E_h + e_{ih}$$

$$(ii) A_{ih} = \beta_0 + \beta_1 Susceptibility_i + e_{ih}$$

(iii) IV:

$$1. \text{ Stage: } Susceptibility_{ih} = \alpha_0 + \alpha_1 E_h + v_{ih}$$

$$2. \text{ Stage: } A_{ih} = \beta_0 + \beta_1 Susceptibility_{ih} + e_{ih}$$

Validity Check Reporting: As opposed to attitudes and planned future engagement, prevalence of harmful traditional practices cannot plausibly be affected by the interventions, as the time between intervention and measurement is too short in our setting (no behavior change during the experiment). Thus, we can use prevalence measures Y_{ih} to check for differences in reporting about the participation in harmful traditional practices as a consequence of the interventions.

3.1.3 How is the public image of a person affected if this person engages in harmful traditional practices?

$$P_{ih} = \beta_0 + \beta_1 J_h^1 + e_{ih}$$

It may be informative to consider the interaction with prevalent local social norms. Effects may differ between villages that engage in child marriage and initiation rituals and villages that do not. I.e. if the village supports the practices that John engages in ($Identical_Norm_v$).

$$P_{ihv} = \beta_0 + \beta_1 J_h^1 + \beta_2 Identical_Norm_v + \beta_3 J_h^1 Identical_Norm_v + e_{ihv}$$

3.1.4 Can an additional social signal overwrite the signaling effect of engagement in harmful traditional practices

$$P_{ihv} = \alpha_0 + \alpha_1 J_h^2 + \alpha_2 D_v + \alpha_3 B_v + \alpha_4 D_v B_v + e_{ihv}$$

$$P_{ihv} = \beta_0 + \beta_1 J_h^2 + \beta_2 D_v + \beta_3 B_v + \beta_4 D_v B_v + \beta_5 J_h^2 D_v + \beta_6 J_h^2 B_v + \beta_7 J_h^2 D_v B_v + e_{ihv}$$

As the bracelets (B) only have a signaling meaning in the treatment with bracelets and donation boxes (D) at the same time we expect $\beta_4 \neq 0$ and $\beta_7 \neq 0$. I.e. in villages where bracelets have a pro-social meaning, John should be perceived as being more pro-social if he obtained a bracelet.

We gain statistical power by looking at the change of P within subject by subtracting reported P_{inv} under treatment J_h^1 .

It may again be informative to consider the interaction with prevalent local social norms (analogous to above)

3.1.5 Can support for harmful traditional practices be substituted by a pro-social signaling opportunity?

- Does facilitating pro-social signaling affect support for harmful traditional practices?

$$A_{iv} = \beta_0 + \beta_1 D_v + e_{iv}$$

- Does increasing the public visibility of pro-social signaling affect the support for harmful traditional practices?

$$A_{iv} = \beta_0 + \beta_1 B_v + e_{iv} \mid D_v = 1$$

Check if effect of bracelets per se, even in absence of signaling value

$$A_{iv} = \beta_0 + \beta_1 B_v + e_{iv} \mid D_v = 0$$

Control for effect of bracelets in absence of signaling value, if necessary:

$$A_{iv} = \beta_0 + \beta_1 D_v + \beta_2 B_v + \beta_3 D_v B_v + e_{iv}$$

- Does increasing the public visibility amplify the effect of facilitating pro-social signaling?

$$A_{iv} = \beta_0 + \beta_1 D_v + \beta_2 B_v + \beta_3 D_v B_v + e_{iv}$$

We will come back one year after the baseline data collection and look at actual change in behavior.

The pre-analysis plan will be updated accordingly at this position at a later stage.

3.2 Authority vs Norms

For these analyses only villages in treatments A,B, and C should be considered, as the full control condition D is identical if $C_c=1$ and if $C_c=0$.

3.2.1 Are there trade-offs between authority and the erosion of established social norms?

I.e. are chiefs more supportive of harmful traditional practices if they are taken away the power to manage a new signal? (Di Casola, Freddi, and Sichlimiris 2017)

We regress attitudes towards harmful traditional practices of the chiefs, A_c , on treatment C_c

$$A_c = \beta_0 + \beta_1 C_c + e_c .$$

3.2.2 Do village chiefs judge other villagers differently in terms of pro-sociality if the competences to create a public signal are taken away from them?

I.e. do the chiefs base their judgement of pro-sociality P_{ci} about other villagers i more on villager i 's involvement in harmful traditional practices H_i if chief get the authority to manage the new pro-social signal taken away from him/her?

$$P_{ci} = \beta_0 + \beta_1 C_c + \beta_2 H_i + \beta_3 C_c H_i + e_{ci} .$$

3.2.3 Perceived powers

Do village chiefs claim to have more competences/powers (K_c : Allocating resources, collecting money, form marriages, mediate/conflict resolution, influence local traditions, wiggle room for government decisions) if they lose power to manage the signal?

$$K_c = \beta_0 + \beta_1 C_c + e_c .$$

3.2.4 Reputational concerns by the chiefs

Are reputational concerns of village chiefs ($Susceptibility_c$) increased if they lose power to manage the signal?

$$Susceptibility_c = \beta_0 + \beta_1 C_c + e_c .$$

3.3 Effects of liquidity constraints and marriageability concerns

3.3.1 Effect of liquidity constraints on harmful traditional practices

We analyze the effect of liquidity constraints on harmful traditional practices by regressions on exogenous weather variations, i.e. rainfall shocks (R_{rt} : Continuous deviations from historical averages or dummies for extreme floods & droughts (10th / 90th percentile of historical monthly data)) that cause random income shocks through floods and droughts.

$$Y_{rt} = \beta_0 + \beta_1 R_{rt} + e_{rt}$$

Rainfall data is generally not available at the village level. By including village-level questions about recent floods (F_{iv}) and droughts (DR_{iv}), we can improve the precision of these shocks by building a gravity-style measure.

3.3.2 Are child marriage and initiation rituals complements or substitutes?

We analyze whether child marriage (Y_{irt}) and initiation rituals (I_{it}) are complements or substitutes by instrumenting costly initiation rituals by rainfall shocks in the region ($R_{r,t=l}$) at the usual age of initiation ceremonies.

1. Stage:

$$I_{irt} = \alpha_0 + \alpha_1 R_{r,t=l} + v_{irt}$$

2. Stage:

$$Y_{irt} = \beta_0 + \beta_1 \overset{\blacktriangle}{I_{irt}} + e_{irt}$$

Effects are expected to be different between matrilineal and patrilineal societies and between matrilocal and patrilocal living arrangements. We therefore additionally consider the interaction effects between I_{irt} and binary indicators for Matrilineal and Matrilocal.

Pre-analysis Plan for Harming to Signal Follow-up (19 September 2019)

1 Sampling

Respondents: 411 village chiefs and 411 other villagers.¹

Obtain information about 4,953 girls at risk (age 10-17 at baseline in July 2018) and their 3,674 households from village chiefs and other villagers.

2 Interventions and Experiments

2.1 Signaling Intervention

During initial household listings in 2018 we implemented a signaling intervention in survey villages where households could publicly donate for the needy in their village.

→ See initial pre-analysis plan, registered 2nd July 2018.

2.2 Willingness-to-pay for signaling intervention

We start with a practice round where respondents can decide between a snack and 0-200 Malawian kwachas in cash cards. We measure the respondents' willingness-to-pay through a series of three to four binary choices between receiving money or the snack, following a "staircase" procedure (Cornsweet 1962).

Consecutively, the respondents enter another lottery in which they can earn 0 or 10,000 Malawian kwachas. They are informed that they can choose between receiving that money in cash cards or to instead obtain the signaling intervention with donation box and bracelets, as described section 1.1 in the pre-analysis plan, registered on 2nd July 2018. We again measure the respondents' willingness-to-pay through a series of three to four binary choices between receiving money or the signaling intervention, following a "staircase" procedure (Cornsweet 1962).

1 Two villages that were sampled at baseline turned out to be only one village with one chief, explaining that the number of villages is reduced by 1 compared to baseline.

3 Outcomes

We consider four types of outcomes: (i) Emergence of alternative signaling strategies, (ii) participation of girls in harmful traditional practices, (iii) social image of household heads, (iv) village chiefs'/other household's involvement in shaping traditions and their attitudes.

(i) Emergence of alternative signaling strategies

Two respondents (r) per village are asked about their village (v) and households (h). In the case of *factual* village-level questions, answers of the two respondents are averaged. Opinion questions are treated as separate observations for both respondents. In the case of factual questions about households, answers are weighted and aggregated over both respondents with relative weights corresponding to the degree of familiarity with the particular household (scale 1-5). If a respondent does not know a household at all, the response of the other respondent provides the full answer weight. Opinion questions about households are treated as separate observations, but again weighted by the degree of familiarity of the respondent with the household.

Specific outcomes:

- Frequency of food collections for needy households (answer intervals of 3 month averaged over 2 respondents in village): FC_v
- Share of households in village contributing to food collections in last year/ last month (answer intervals of 10% averaged over 2 respondents in village): $ShareFC_v^M, ShareFC_v^Y$
- Contributions to food collections of households with girls at risk (contributed to food collections in last month/year? → binary): FC_{hv}^M, FC_{hv}^Y
- *Importance of traditions in village (scale 0-10):* $ImportanceTradition_{rv}$
- *Publicity of traditions: scale 0-3 → create dummy for ≥ 2 :* $PubliclyTradition_{rv}$
- Change of importance of local traditions (more important, less important, equally important): $ChangeTradition_{rv}$
- Willingness to pay for public donation intervention (0-10,000 MWK). We remove observations if the enumerator states that respondent did not understand WTP instructions, even after repeated explanations and a practice round: WP_{rv}

(ii) participation in traditional practices

Two respondents (r) per village are asked about all girls (l) at risk in their village. Answers are weighted and aggregated over both respondents with relative weights corresponding to the degree of familiarity with the particular girl (scale 1-5). If a respondent does not know a girl at all, the response of the other respondent provides the full answer weight. Same approach for questions about households (h).

- Elicitation of intended and actual child marriage ($CM_{ihv}^I, CM_{ihv}^{Act}$) and (sexual) initiation rituals ($SI_{ihv}^I, SI_{ihv}^{Act}$), attitudes of households towards child marriage (CM_{hv}^A) and (sexual) initiation rituals (SI_{hv}^A), pregnancies P_{ihv} , and school attendance S_{ihv} for all girls in the village that were 10-17 at baseline in July 2018 and their households.

- Change of child marriage (<15, <18) /sexual initiation frequency, as perceived by respondent (more common, less common, equally common): (CM_{rv}^R, SI_{rv}^R)

(iii) social image of household heads

Two respondents (r) per village are asked about all heads. Answers are weighted over both respondents with relative weights corresponding to the degree of familiarity with the particular head (scale 1-5). If a respondent does not know a household at all, the response of the other respondent provides the full answer weight.

Main measure of social image (adapted versions from Falk et al's (2016) social preference module)

- Altruism (scale 0-10): "How willing is (**head**) to give to good causes without expecting anything in return?"
- Reciprocity (scale 0-10): "When someone does (**head**) a favour, (**head**) is willing to return it."
- Trust(scale 0-10): "(**head**) assumes that people have only the best intentions."

→ Here, we are interested on the joint measure of pro-sociality, containing altruism, reciprocity and trustworthiness: $Image_{hrv}$

Other household characteristics

- Support for child marriage (scale 0-10): $Support_{hrv}^{CM}$
- Religious attendance (Never, weekly, monthly, yearly, less than yearly): $Church_{hrv}$
- Support for sexual initiation rituals (scale 0-10): $Support_{hrv}^{SI}$
- Perceived to follow local traditions even if they harm children (scale 0-10): $SupportTradition_{hrv}$

- Likelihood to receive help by other villagers (scale 0-10): $ReceiveHelp_{hrv}$

(iv) village chiefs'/other household's involvement in shaping traditions and his attitudes

- Frequency of communication about traditions (Daily, weekly, monthly, 3-monthly, bi-yearly, yearly, never): $TalkTradition_{hrv}$

- Frequency of communication about marriage of daughters of other households (Daily, weekly, monthly, 3-monthly, bi-yearly, yearly, never): $TalkChildmarriage_{hrv}$

- Favorable attitudes of chief/other household towards child marriage (binary for ideal age of marriage < 18) and sexual initiation rituals (binary): $(CM_{rv}^{Att}, SI_{rv}^{Att})$

4 Analysis Plan

Clustering: unless noted otherwise, all analyses will be clustered at the village level.

4.1 Social signaling

At baseline, the chief assigned a more positive public image to households that support child marriage, in villages where child marriage is common, and vice-versa in villages without child marriage.

Main specifications:

- Does this relationship still hold in control villages (without the public donation intervention)?

$$Image_{hrv} = \alpha_v + \beta_1 Support_{hrv}^{CM} + \beta_2 Support_{hrv}^{CM} * Childmarriage_v + \epsilon_{hrv} \rightarrow \beta_2 > 0 ? \quad (1)$$

- Does this relationship still hold in treatment villages (with the public donation intervention)?

$$Image_{hrv} = \alpha_v + \gamma_1 Support_{hrv}^{CM} + \gamma_2 Support_{hrv}^{CM} * Childmarriage_v + \epsilon_{hrv} \rightarrow \gamma_2 > 0 ? \quad (2)$$

$\rightarrow \beta_2 > \gamma_2 ?$

Alternative specifications:

- Do (1) and (2) hold for other traditions $(Support_{hrv}^{SI}, Church_{hrv}, SupportTradition_{hrv})$?

$$Image_{hrv} = \alpha_v + \beta_1 SupportOtherTradition_{hrv} + \beta_2 SupportOtherTradition_{hrv} * Tradition_v + \epsilon_{hrv} \rightarrow \beta_2 > 0 ? \quad (3)$$

- Alternative dependent variable: $ReceiveHelp_{hrv}$

4.2 Substituteability of signaling strategies

4.2.1 Can an additional social signal reduce prevalence of child marriage and sexual initiation rituals?

Manipulation check: long-term compliance with the treatment

At baseline, our intervention led to public donations being significantly more common in treatment than in control villages. Do these differences persist?

$$FC_v = \beta_0 + \beta_1 \text{DonationTreatment}_v + \epsilon_v \rightarrow \beta_1 > 0? \quad (4)$$

$$\text{ShareFC}_v = \beta_0 + \beta_1 \text{DonationTreatment}_v + \epsilon_v \rightarrow \beta_1 > 0? \quad (5)$$

ITT analysis

Main:

$$CM_{ihv}^{Act} = \beta_0 + \beta_1 \text{DonationTreatment}_v + \beta_2 X_{ihv} + \beta_3 Z_v + \epsilon_{ihv} \rightarrow \beta_1 < 0? \quad (6)$$

$$SI_{ihv}^{Act} = \beta_0 + \beta_1 \text{DonationTreatment}_v + \beta_2 X_{ihv} + \beta_3 Z_v + \epsilon_{ihv} \rightarrow \beta_1 < 0? \quad (7)$$

Additional:

$$CM_{ihv}^I = \beta_0 + \beta_1 \text{DonationTreatment}_v + \beta_2 X_{ihv} + \beta_3 Z_v + \epsilon_{ihv} \rightarrow \beta_1 < 0? \quad (8)$$

$$SI_{ihv}^I = \beta_0 + \beta_1 \text{DonationTreatment}_v + \beta_2 X_{ihv} + \beta_3 Z_v + \epsilon_{ihv} \rightarrow \beta_1 < 0? \quad (9)$$

$$P_{ihv} = \beta_0 + \beta_1 \text{DonationTreatment}_v + \beta_2 X_{ihv} + \beta_3 Z_v + \epsilon_{ihv} \rightarrow \beta_1 < 0? \quad (10)$$

$$S_{ihv} = \beta_0 + \beta_1 \text{DonationTreatment}_v + \beta_2 X_{ihv} + \beta_3 Z_v + \epsilon_{ihv} \rightarrow \beta_1 \neq 0? \quad (11)$$

IV analysis

$$1^{\text{st}} \text{ stage: } FC_v = \alpha_0 + \alpha_1 \text{DonationTreatment}_v + \alpha_2 X_{ihv} + \alpha_3 Z_v + \epsilon_{ihv} \quad (12)$$

$$2^{\text{nd}} \text{ stage: } Y_{ihv} = \beta_0 + \beta_1 \widehat{FC}_{ihv} + \beta_2 X_{ihv} + \beta_3 Z_v + \epsilon'_{ihv} \text{ for } Y_{ihv} \in [CM_{ihv}^{Act}, SI_{ihv}^{Act}, CM_{ihv}^I, SI_{ihv}^I, P_{ihv}, S_{ihv}]$$

$$\rightarrow \beta_1 < 0? \quad (13)$$

Alternative endogenous variables if first stage is weak with FC_v : $\text{ShareFC}_v^M, \text{ShareFC}_v^Y$

Heterogeneity: Low vs high prevalence villages at baseline (median split)

4.2.2 Are changes in attitudes towards child marriage and sexual initiation rituals as response to the signaling intervention long-lasting

ITT analysis

$$CM_{hrv}^A = \beta_0 + \beta_1 \text{DonationTreatment}_v + \beta_2 X_{hv} + \beta_3 Z_v + \beta_4 W_{rv} + \epsilon_{hrv} \rightarrow \beta_1 < 0? \quad (14)$$

$$SI_{hrv}^A = \beta_0 + \beta_1 \text{DonationTreatment}_v + \beta_2 X_{hv} + \beta_3 Z_v + \beta_4 W_{rv} + \epsilon_{hrv} \rightarrow \beta_1 < 0? \quad (15)$$

IV analysis

$$1^{\text{st}} \text{ stage: } FC_v = \alpha_0 + \alpha_1 \text{DonationTreatment}_v + \alpha_2 X_{hv} + \alpha_3 Z_v + \alpha_4 W_{rv} + \epsilon_{hrv} \quad (16)$$

$$2^{\text{nd}} \text{ stage: } Y_{hrv} = \beta_0 + \beta_1 \widehat{FC}_{hrv} + \beta_2 X_{hv} + \beta_3 Z_v + \alpha_4 W_{rv} + \epsilon'_{hrv} \text{ for } Y_{hrv} \in [CM_{hrv}^A, SI_{hrv}^A] \rightarrow \beta_1 < 0? \quad (17)$$

Alternative endogenous variables if first stage is weak with FC_v : $ShareFC_v^M, ShareFC_v^Y$

Heterogeneity:

Low vs high prevalence villages at baseline (median split)

4.2.3 Has the signaling intervention affected prevalence and perceived importance of traditions in general?

Main:

$$\text{ImportanceTradition}_{rv} = \beta_0 + \beta_1 \text{DonationTreatment}_v + \beta_2 X_{rv} + \beta_3 Z_v + \epsilon_{rv} \rightarrow \beta_1 \neq 0? \quad (18)$$

$$\text{ChangeTradition}_{rv} = \beta_0 + \beta_1 \text{DonationTreatment}_v + \beta_2 X_{rv} + \beta_3 Z_v + \epsilon_{rv} \rightarrow \beta_1 \neq 0? \quad (19)$$

Additional:

$$\text{PubliclyTradition}_{rv} = \beta_0 + \beta_1 \text{DonationTreatment}_v + \beta_2 X_{rv} + \beta_3 Z_v + \epsilon_{rv} \rightarrow \beta_1 \neq 0? \quad (20)$$

Heterogeneity:

Low vs high prevalence villages at baseline (median split)

4.2.4 Type of pooling equilibrium

At end line, we investigate whether those who previously supported child marriage and sexual initiation rituals were faced with lower or higher willingness to cooperate by other villagers in formerly high-prevalence villages, differentially across treatment and control villages.

$$ReceiveHelp_{hrv} = \alpha_v + \beta_1 DonationTreatment_v * BaselineSupport_{hv} + \beta_2 BaselineSupport_{hv} + \beta_3 X_{hv} + \beta_4 W_{rv} + \epsilon_{hrv}$$

$$\rightarrow \beta_1 \neq 0? \quad (21)$$

4.3 Targeting elites for social norms change

4.3.1 Willingness to pay for social norms change

Main analyses:

Does WTP depend on the DonationTreatment implemented at baseline?

$$WP_{rv} = \beta_0 + \beta_1 DonationTreatment_v + \beta_2 X_{rv} + \beta_3 Z_v + \epsilon_{rv} \rightarrow \beta_1 \neq 0? \quad (22)$$

Does WTP depend on who was in charge of implementing the DonationTreatment at baseline, the Chief or another household ($Chief_v$)?

$$WP_{rv} = \beta_0 + \beta_1 Chief_v + \beta_2 X_{rv} + \beta_3 Z_v + \epsilon_{rv} \rightarrow \beta_1 \neq 0? \quad (23)$$

Does WTP depend differentially on DonationTreatment, depending on who was in charge at baseline?

$$WP_{rv} = \beta_0 + \beta_1 DonationTreatment_v + \beta_2 Chief_v + \beta_3 DonationTreatment_v * Chief_v + \beta_4 X_{rv} + \beta_5 Z_v + \epsilon_{rv}$$

$$\rightarrow \beta_3 \neq 0? \quad (24)$$

Additional analyses:

- Heterogeneous treatment effects, depending on rate of child marriage and sexual initiation rituals at baseline and at endline.
- Analyze chief and others separately. Do they differ?

4.3.2 Mechanism: Are chiefs more involved in shaping traditions if they were not in charge of the public donation intervention?

Are chiefs talking more frequently to households about traditions if they are in charge of the public donation intervention?

$$TalkTradition_{hrv} = \beta_0 + \beta_1 Chief_v + \beta_2 X_{rv} + \beta_3 Z_v + \beta_4 W_{hv} + \epsilon_{hrv} \rightarrow \beta_1 \neq 0? \quad | \quad r = chief \quad (25)$$

$$TalkChildmarriage_{hrv} = \beta_0 + \beta_1 Chief_v + \beta_2 X_{rv} + \beta_3 Z_v + \beta_4 W_{hv} + \epsilon_{hrv} \rightarrow \beta_1 \neq 0? \quad | \quad r = chief \quad (26)$$

4.3.3 Is the public donation intervention more/less effective in changing traditional practices if the chief is in charge of the intervention?

Main:

$$CM_{ihv}^{Act} = \alpha_0 + \alpha_1 DonationTreatment_v + \alpha_2 Chief_v + \alpha_3 DonationTreatment_v * Chief_v + \alpha_4 X_{ihv} + \alpha_5 Z_v + \epsilon_{ihv} \\ \rightarrow \alpha_3 \neq 0? \quad (27)$$

$$SI_{ihv}^{Act} = \beta_0 + \beta_1 DonationTreatment_v + \beta_2 Chief_v + \beta_3 DonationTreatment_v * Chief_v + \beta_4 X_{ihv} + \beta_5 Z_v + \epsilon_{ihv} \\ \rightarrow \beta_3 \neq 0? \quad (28)$$

Does the treatment effect only depend on who is in charge for initiation rituals, but not for child marriage?

$$\rightarrow \beta_3 \neq \alpha_3? \quad (29)$$

Additional:

$$CM_{ihv}^I = \beta_0 + \beta_1 DonationTreatment_v + \beta_2 Chief_v + \beta_3 DonationTreatment_v * Chief_v + \beta_4 X_{ihv} + \beta_5 Z_v + \epsilon_{ihv} \\ \rightarrow \beta_3 \neq 0? \quad (30)$$

$$SI_{ihv}^I = \beta_0 + \beta_1 DonationTreatment_v + \beta_2 Chief_v + \beta_3 DonationTreatment_v * Chief_v + \beta_4 X_{ihv} + \beta_5 Z_v + \epsilon_{ihv} \\ \rightarrow \beta_3 \neq 0? \quad (31)$$

$$P_{ihv} = \beta_0 + \beta_1 DonationTreatment_v + \beta_2 Chief_v + \beta_3 DonationTreatment_v * Chief_v + \beta_4 X_{ihv} + \beta_5 Z_v + \epsilon_{ihv} \\ \rightarrow \beta_3 \neq 0? \quad (32)$$

$$S_{ihv} = \beta_0 + \beta_1 DonationTreatment_v + \beta_2 Chief_v + \beta_3 DonationTreatment_v * Chief_v + \beta_4 X_{ihv} + \beta_5 Z_v + \epsilon_{ihv} \\ \rightarrow \beta_3 \neq 0? \quad (33)$$

4.3.4 Is the public donation intervention more/less effective in changing respondents attitudes towards child marriage and initiation rituals if the chief is in charge of the intervention?

$$CM_{rv}^{Att} = \alpha_0 + \alpha_1 DonationTreatment_v + \alpha_2 Chief_v + \alpha_3 DonationTreatment_v * Chief_v + \alpha_4 X_{rv} + \alpha_5 Z_v + \epsilon_{rv}$$

→ $\alpha_1, \alpha_2, \alpha_3 \neq 0?$ (34)

$$SI_{rv}^{Att} = \alpha_0 + \alpha_1 DonationTreatment_v + \alpha_2 Chief_v + \alpha_3 DonationTreatment_v * Chief_v + \alpha_4 X_{rv} + \alpha_5 Z_v + \epsilon_{rv}$$

→ $\alpha_1, \alpha_2, \alpha_3 \neq 0?$ (35)

Appendix D A simple model of harming to signal

Suppose there are two types of individuals in a society, pro-social, cooperative H-types and individualistic L-types with corresponding discount factors $\beta_H > \beta_L$. Due to higher valuation of long-term benefits H-types are more interested in long-term relationships with other individuals while L-types seek short-run benefits. This is equivalent to H-types valuing cooperation with other individuals in society more than low types.

For simplicity assume that all individuals match with another individual to participate in infinitely repeated prisoner's dilemmas of the form

	D	C
D	d,d	c+ ζ ,d- η
C	d- η ,c+ ζ	c,c

where $c > d$ and $\zeta, \eta > 0$, individuals are risk neutral and, their utility is increasing in payoffs. Moreover, individuals cannot determine the type of other players beforehand but form beliefs $p \in (0, 1)$ about the share of H-types in a society.

Proposition 1 *There is an equilibrium in infinitely repeated games taking the following form.*

(i) *L-types always play strategy D.*

(ii) *H-types start by playing strategy C, but once the other individual defects, D is played forever.*

(i) is satisfied for any $\beta_L < \frac{p[X-Y]+Y}{p[X-Y]+Y+p[Z]}$ and (ii) is satisfied for any $\beta_H > \frac{p[X-Y]+Y}{p[X-Y]+Y+p[Z]}$ with $X = u(c + \zeta) - u(c)$, $Y = u(d) - u(d - \eta)$, and $Z = u(c) - u(d)$.

Proof of Proposition 1 For L-types to always defect, the utility from defecting must be larger than the utility from cooperating, taking as given the other players' strategies:

$$p \left[u(c + \zeta) + \sum_{t=1}^{\infty} \beta_L^t u(d) \right] + (1-p) \sum_{t=0}^{\infty} \beta_L^t u(d) > p \sum_{t=0}^{\infty} \beta_L^t u(c) + (1-p) \left[u(d - \eta) + \sum_{t=1}^{\infty} \beta_L^t u(d) \right]$$

$$\Leftrightarrow \beta_L < \frac{p[u(c + \zeta) - u(c) + u(d - \eta) - u(d)] + u(d) - u(d - \eta)}{p[u(c + \zeta) - 2u(d) + u(d - \eta)] + u(d) - u(d - \eta)}$$

The condition is analogous for H-types, reversing the inequality and replacing β_L with β_H . Additionally, for H-types it has to be optimal to keep cooperating even if they know they are matched with another H-type in the second round and could thus obtain the exploitation benefit ζ with certainty:

$$\sum_{t=0}^{\infty} \beta_H^t u(c) > u(c + \zeta) + \sum_{t=1}^{\infty} \beta_H^t u(d) \Leftrightarrow \beta_H > \frac{u(c + \zeta) - u(c)}{u(c + \zeta) - u(d)}$$

Whether L-types and H-types choose to cooperate or defect in the first round depends on the probability p of being matched with another H-type, on their patience (β_L and β_H), and on relative payoffs in the stage game.

Proposition 2 Denote S as a costly signal. There is a value of S such that there is a separating equilibrium in infinitely repeated games taking the following form.

- (i) L -types never acquire the signal and only match with other L -types, defecting forever.
- (ii) All H -types acquire the signal and only match with other H -types, cooperating forever.

For (i) to hold, the signal must not be affordable for L -types, i.e.

$$\sum_{t=0}^{\infty} \beta_L^t u(d) > u(c + \zeta) - S + \sum_{t=1}^{\infty} \beta_L^t u(d) \Leftrightarrow S > u(c + \zeta) - u(d) \quad (5)$$

Thus, the signal must be more expensive than the additional benefit from one-time exploiting a H -type.

For (ii) to hold, the separating equilibrium must be attractive for H -types. I.e. the benefit from separating must be worth the costs, compared to pooling. This implies that under risk neutrality and with p denoting the (belief about the) share of H -types in the population:

$$\begin{aligned} \sum_{t=0}^{\infty} \beta_H^t u(c) - S &> p \left[\sum_{t=0}^{\infty} \beta_H^t u(c) \right] + (1-p) \left[u(d - \eta) + \sum_{t=1}^{\infty} \beta_H^t u(d) \right] \\ \Leftrightarrow S &< (1-p) \left\{ \left[\sum_{t=0}^{\infty} \beta_H^t u(c) \right] - \left[u(d - \eta) + \sum_{t=1}^{\infty} \beta_H^t u(d) \right] \right\}. \end{aligned} \quad (6)$$

For this condition to be satisfied, the (belief about the) share of L -types in the population $(1-p)$ must not be too low and the benefit from mutual cooperation compared to being exploited by a L -type has to be sufficiently large.

Appendix E Replacement of social expectations

In this appendix we explain in more detail how we allow treatment effects to differ additionally by whether John is portrayed wearing a bracelet. For this purpose, we stack the two individual responses (for each instance when they rated John’s pro-social preferences, wearing bracelets and not) and re-estimate equation ?? including respondent fixed-effects and adding extra interaction terms:

$$Y_{vhit} = \alpha_i + \eta_1 \text{John}_h \times \text{Share_childmarriage}_v \times \text{Bracelet}_t + \eta_2 \text{John}_h \times \text{Bracelet}_t + \eta_3 \text{Share_childmarriage}_v \times \text{Bracelet}_t + \eta_4 \text{Bracelet}_t + \epsilon_{vhit}, \quad (7)$$

where Y_{vhit} is the summary measure of John’s pro-social preferences, attributed by individual i living in household h and village v at instance t ; John_h captures the version of John households are assigned to, equal to 1 if he marries of his under-age daughter, and 0 otherwise; $\text{Share_childmarriage}_v$ is the share of the sample in village v who married before the age of 15; Bracelet_t is an indicator variable equal to 1 if John is portrayed wearing a bracelet at instance t , and 0 otherwise; and α_i are individual fixed effects. Standard errors are clustered at the village level.

Table E.1 displays the regression results, described in the main text in section 5.1.

Table E.1: Replacement of Social Expectations

Dependent variable:	(1)	(2)	(3)	(4)
Perceived reputation of John (summary measure)	Box&Bracelets	Box	Bracelets	Control
John supports child marriage \times Bracelet	0.314*** (0.0583)	0.0927** (0.0437)	0.0431 (0.0479)	0.0876* (0.0487)
John supports child marriage \times Share married < 15 \times Bracelet	-2.868*** (0.970)	-0.984 (0.938)	1.371 (0.830)	0.294 (0.653)
Share married < 15 \times Bracelet	0.147 (0.625)	0.439 (0.722)	-0.815 (0.555)	0.115 (0.554)
Bracelet	0.0116 (0.0329)	-0.0418 (0.0318)	0.0638* (0.0372)	-0.0265 (0.0370)
Observations	4,626	4,680	3,400	3,300
Individual Fixed Effects	✓	✓	✓	✓

Notes: The summary measure is an equally weighted standardized average of standardized individual measures for altruism, reciprocity, and trust (see Section 3.4.1). Standard errors clustered at the village level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This analysis exploits the within subject design, absorbing individual fixed effects and consequently does not rely on further individual and village-level controls, thus explaining the larger sample size.

Appendix F Effects on perceived pro-social preferences

This appendix provides the details behind Figure 9. To compare John’s perceived pro-social preferences when he wears a bracelet to when he does not, holding constant the framing of his support for child marriage (which is randomized across households), we estimate the following equation:

$$\Delta Y_{vhi} = \beta_0 + \beta_1 \text{Box\&Bracelets}_v + \beta_2 \text{Box}_v + \beta_3 \text{Bracelets}_v + \beta_4 X_{vhi} + \zeta_{vhi}, \quad (8)$$

where ΔY_{vhi} is the difference between the summary measure of John’s perceived pro-sociality assigned by individual i from household h in village v when John is portrayed wearing a bracelet compared to that when he is portrayed without it; Box\&Bracelets_v , Box_v , and Bracelets_v are indicator variables equal to 1 in villages assigned to that treatment condition, and 0 otherwise; and X_{vhi} are individual-level controls. Standard errors are clustered at the village level.

Table F.1 displays the detailed results. We find that John’s perceived pro-social preferences benefits from wearing a bracelet to a significantly greater extent in villages where bracelets stand for donations, compared to all other villages (by about 0.1 standard deviation across all components). Even though bracelets are weakly associated with higher trust in other villages (especially in the treatment arm where bracelets are sold, potentially triggering in- and out-group dynamics), perceived trust is still higher (statistically significant at the 10% level) in the villages where bracelets and public donation boxes were made available. The comparison to other villages helps to rule out experimenter demand effects from eliciting John’s pro-social preferences a second time when he wears a bracelet.

Table F.1: Reputational Benefit of John Wearing a Bracelet

Dependent variable:	Summary Measure	Individual components		
		Altruism	Reciprocity	Trustworthiness
Image improvement	(1)	(2)	(3)	(4)
Box & Bracelets	0.0827*** (0.0277)	0.0930** (0.0380)	0.0866** (0.0418)	0.0707* (0.0360)
Box	-0.0245 (0.0245)	-0.0405 (0.0364)	-0.0508 (0.0365)	0.0199 (0.0344)
Bracelets	0.0451 (0.0276)	0.0395 (0.0403)	0.0330 (0.0431)	0.0649* (0.0380)
Individual controls	✓	✓	✓	✓
Observations	6,975	6,976	6,976	6,975

Notes: The summary measure (1) is an equally weighted standardized average of standardized individual measures for (2) altruism, (3) reciprocity, and (4) trustworthiness (see Section 3.4.1). Regressions additionally include individual controls (female, age, age², age³, and measures for own pro-sociality) plus a constant. Standard errors, clustered at the village level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix G Local average treatment effects using IV

This appendix describes in more detail how we obtain local average treatment effects, given imperfect compliance with the RCT.

We estimate instrumental variables regressions by two-stage-least-squares, using treatment assignment as instrument for treatment implementation:⁴⁸

$$\begin{aligned}
 1^{st} \text{ stage: } & \text{ImplementedDonationDrive}_{vhi} = \beta_0 + \beta_1 \text{AssignedDonationDrive}_v + \beta_2 X_{vhi} + \epsilon_{vhi} \\
 2^{nd} \text{ stage: } & Y_{vhi} = \delta_0 + \delta_1 \overline{\text{ImplementedDonationDrive}_{vhi}} + \delta_2 X_{vhi} + \xi_{vhi},
 \end{aligned}
 \tag{9}$$

where $\overline{\text{ImplementedDonationDrive}_{vhi}}$ are predicted assignments for whether village v ended up hosting a public donation drive. Results for age-by-age girls' outcomes at end-line and for attitudes towards traditional practices at baseline are very similar to ITT results and described in tables G.1-G.2, respectively.

Table G.1: Effect of public donation drive on girls' outcomes age-by-age (IV)

	(1)	(2)	(3)	(4)
	Married <18	Children	School dropout	Initiated
Implemented donation drive	-0.0216** (0.00975)	-0.0198** (0.00888)	-0.0307* (0.0167)	-0.0194 (0.0166)
Control mean	0.054	0.052	0.162	0.058
1 st -stage F-stat of instrument	1457.3	1457.3	1442.8	1425.0
Observations	3,436	3,436	3,153	2,429
District fixed effects	✓	✓	✓	✓
Enumerator fixed effects	✓	✓	✓	✓
Controls	✓	✓	✓	✓

Notes: Regressions additionally include individual controls (age, age², and age³) and village-level controls (village size, population density, urban, and baseline prevalence of child marriage or initiation rituals) and enumerator indicators. Standard errors, clustered at the village level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

⁴⁸See e.g. Angrist and Pischke (2008).

Table G.2: Effect of public donation drive on attitudes towards traditional practices (IV)

	(1)	(2)
	Child Marriage	Initiation
Implemented donation drive	-0.0161*** (0.00583)	-0.0156* (0.00945)
Control mean	0.054	0.077
1 st -stage F-stat of instrument	1057.4	1047.6
Chi ² -test Donation jointly=0, (p-val.)	0.0140	
Observations	11,123	7,243
Individual controls	✓	✓
Village-level controls	✓	✓

Notes: Regressions additionally include individual controls (female, age, age², and age³) and village-level controls (village size, population density, urban, and local prevalence of sexual initiation in columns (iii)-(iv)), plus a constant. Standard errors, clustered at the village level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$