

Harming to Signal*

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Abstract

Is following traditions at least partly driven by social image concerns? If so, could alternative signals substitute for traditions? These questions matter, since a range of norms and traditional practices have been shown to translate into socially sub-optimal outcomes, from corruption to discrimination and violence against children. Studying those questions is hard, as traditions (and compliance with them) are not randomly assigned. This paper overcomes those challenges in the context of initiation rituals and child marriage in Malawi by combining a randomized control trial that exogenously varies the *availability* of strategies to signal pro-sociality with a survey experiment that randomly varies the *adoption* of different signals. We find that subjects judge those who plan to marry their under-age daughters as more pro-social in villages where child marriage prevalence is high. Alternative signals change perceptions about how pro-social those who follow traditional practices are, and decrease favorable attitudes towards child marriage and sexual initiation rituals by 20-30%. Our findings contribute to a booming literature on the drivers of social norms and showcase that social signaling interventions can have implications above and beyond the behaviors they target explicitly.

Keywords: Traditions; Social Norms; Signaling; Children's Human Capital

JEL Classifications: C93, D91, J12, J13, P48

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1 Introduction

Why do people follow traditions? This question lies at the core of a large literature that studies the relationship between culture and a variety of economic outcomes (Guiso, Sapienza and Zingales, 2006; Fernandes, 2008; Alesina and Giuliano, 2015). We hypothesize that individuals follow traditions at least partly out of social image concerns, to convey to others that they can be trusted as *members of the community*. While this signaling mechanism has been entertained theoretically (Bénabou and Tirole, 2006, 2011), empirical evidence remains scarce.

Whether signaling influences compliance with traditions matters because, if that is the case, alternative signals could, under certain conditions, *substitute for* social norms and traditional practices that lead to inefficient outcomes (e.g. corruption, Tirole, 1996), inequality (e.g. discrimination, Basu, 2018), and even to the destruction of children’s human capital (e.g. child labor, Basu, 2018; child marriage, Field and Ambrus, 2008; and violence against children, Vogt et al., 2016).

In our nationally representative survey of Malawi, about 40% of subjects claim that following traditions is a good way to build a great reputation or social image. Does this logic also apply to practices that affect children, such as child marriage and forced sexual initiation rituals? If so, could introducing alternative signals substitute for traditions? This paper tests those hypotheses first-hand, exploring exogenous variation in *how easy* and *how visible* it is to donate to the poorest in one’s community.

Recent papers document that social signaling can trigger behavior change. 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 can significantly increase those investments in children’s human capital, particularly so in villages further from clinics – where such behavior is less visible in the absence of bracelets. Could social signaling also change attitudes and behaviors *outside the domain incentivized by the signal*, substituting for traditions as a way to build a positive social image?

Economic theory suggests that it might. If beliefs are such that harming one’s child as a signal for pro-sociality can be sustained in a separating equilibrium – i.e. costly enough to detain individualistic types and, at the same time, worthwhile for pro-social types to justify avoiding infinitely repeated defection –, introducing an alternative signaling strategy might shift players out of that equilibrium either through the emergence of (1) a *new separating equilibrium*,

in which pro-social types now signal through this alternative strategy, or that of (2) a *pooling equilibrium*, if pro-social types do not consider separating worthwhile anymore. If that logic applies to traditions that harm children this means introducing alternative strategies to build social image could enforce trust-intensive exchanges while no longer destroying children’s human capital.

Taking this model to the data is challenging for two reasons. First, the *availability* of traditions and other signals is not randomly assigned. Second, their *adoption* is also not random: those who decide to signal through alternative signals (e.g. donations) instead of following a local tradition typically differ in many other characteristics, making it impossible to assess the causal effect of following traditions on social image in order to test the mechanisms underlying the signaling model.

We tackle those challenges by combining a randomized control trial (RCT) that exogenously varies the availability of strategies to signal pro-sociality with a survey experiment that randomly varies the adoption of different signals, across 400 villages and 7,400 households in rural Malawi. When it comes to availability, villages are cross-randomized to host public donation boxes to collect food for the poorest local households (or not, in the control group), and to host the distribution of red rubber bracelets (or not, in the control group). In villages assigned to hosting both the box and bracelets, the latter are distributed in exchange for two kilograms of maize, Malawi’s staple crop; 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; last, in pure control villages, neither donations were organized nor bracelets distributed or sold. The idea is that public donation boxes make a familiar alternative strategy to signal pro-sociality more visible and salient; *even more so* when in combination with bracelets.^{1,2}

When it comes to adoption, we resort to a survey experiment, conducted roughly five weeks after the RCT was implemented, as part of a nationally representative survey on traditional practices in Malawi. In the experiment, subjects are randomly assigned to drawings (accompanied by verbal descriptions) either of a Malawian father who supports child marriage and sexual initiation rituals, or of an otherwise identical father who does not. Each subject is shown two

¹About 60% of subjects in our survey point out that “helping others” is also a good way to build a great reputation or social image.

²Of course, bracelets alone could act as an alternative signal (given in-group/out-group biases (e.g. Everett, Faber and Crockett, 2015)) but, presumably, not as strongly as when linked to donations.

versions of the same drawing: the second time around, the only change is that the depicted father is shown wearing a red rubber bracelet. Bracelets were made to look the same as the ones distributed in villages assigned to bracelets in the RCT, but no information was conveyed about what bracelets were supposed to represent. Across all treatment conditions, subjects are requested to assess different dimensions of pro-sociality for the Malawian father depicted in each drawing, relying on survey instruments adapted from Falk et al. (2016)'s social preferences survey module.³ The idea is that, combined with the RCT, the survey experiment randomly assigns the adoption of alternative signals, yielding within-village variation and allowing us to assess the causal effect of signaling on an individual's social image.

Results are as follows. Consistent with the existence of a separating equilibrium at baseline, village chiefs judge those who support child marriage more pro-social in villages where the practice is high-prevalence. Moreover, subjects find fathers randomly assigned to marry off their under-age daughters in survey experiments less pro-social *only* in villages where prevalence of child marriage is low, and perceived pro-sociality increases with the local prevalence of child marriage.

Next, we turn to the question of whether alternative signals can substitute for child marriage. We first document how our RCT affects food donations. In control villages, donations (outside of the experiment) are mainly carried out by pro-social subjects; what is more, only a minority of those who support child marriage engage in donations. In contrast, in treatment villages where public donation boxes are made available, donations become much more prevalent, and neither pro-sociality nor support towards child marriage predict who donates. We then assess whether the signaling intervention affects the predictability of one's *pro-social type*. We ask village chiefs to assess five randomly selected individuals in their village with respect to the dimensions of pro-sociality we measure, and then match these assessments to individuals' self-reported measures. We find that chiefs' prediction accuracy significantly *decreases* in villages where donation boxes are made available, in line with the introduction of a *pooling equilibrium*. Consistent with signaling - rather than with a direct effect of the intervention on subjects' pro-sociality - villagers do not feel more pro-social in treatment villages, even though chiefs believes that to be the case. Going back to the survey experiment, perceived social image improves when the father wears a bracelet *only* in villages where bracelets are linked to donations. Also

³Those pretested survey instruments mimic standard games from experimental economics and have been shown to accurately predict behavior in incentivized choice experiments.

consistent with a pooling equilibrium, the effects of marrying off his under-age daughter on subjects' assessment of his pro-sociality *is dampened* by the bracelet: it becomes less negative where child marriage is low-prevalence, and less positive (or even negative) where its prevalence is high. In sum, the new signal crowds out the signaling value of the long-standing child marriage tradition.

We then investigate whether the new signal *decreases support* towards multiple existing traditional practices. We find that favorable attitudes towards child marriage and forced sexual initiation rituals are reduced by 30% and 20%, respectively. As our intervention only takes place five weeks before the survey, *prevalence* of those practices cannot be affected in such a short period. Thus, using actual prevalence as a placebo, we can rule out that treatment effects are driven by social desirability bias in reporting. The impacts of the intervention on attitudes towards child marriage and forced sexual initiation rituals are concentrated among female respondents and households with girls at the vulnerable ages for each practice – indicating that effects are driven by households at-risk. What is more, the effect is entirely driven by high-prevalence villages, where support for child marriage falls to the same level as that in low-prevalence villages, suggesting that social image concerns might account *entirely* for differences in prevalence across villages.

Last, we study whether local elites matter for changing traditions. Elites have incentives to cling to power (Acemoglu, Reed and Robinson, 2014), often expressed through control of local traditions (Zartman et al., 2000; Henn, 2018). When traditions change, local elites might lose power; does that lead them to block changes in traditions? Conversely, does trying to bypass local elites *backfire*, by strengthening current traditions? We tackle those questions by randomly assigning who is in control of collecting donations and/or distributing bracelets in treatment villages: in half of those villages, we assign control of the new signal to the village chief, in the other half to an ordinary villager, not directly connected to the chief. We analyze what assignment does to chiefs' and villagers' support of traditions, contrasting child marriage (over which chiefs exercise no control, according to survey data) and initiation rituals (for which they fare amongst the most influential decision-makers according to a relevant share of the villagers). The hypothesis is that changes in traditions are *not blocked* when chiefs do not control old traditions, but may be *blocked* otherwise unless chiefs *control* the new signal. Consistent with that hypothesis, we find that the assignment does not matter for the effects of the new signal on child marriage, but significantly matters for initiation rituals: when chiefs control the

new signal, support for initiation falls within at-risk households; otherwise, support actually *increases*, exactly mimicking chiefs' attitudes.

This paper is related to different strands of the literature. We contribute to the literature on the mechanisms behind following social norms⁴, particularly those that potentially harm children's human capital. Past research has focused on intrinsic motivations (Kearney and Levine, 2015; Vogt et al., 2016), extrinsic motivations (Buchmann et al., 2019; Corno, Hildebrandt and Voena, 2016; Corno and Voena, 2016; Vogt et al., 2016; Ashraf et al., forthcoming), and beliefs about prevalence or social expectations, including higher-order beliefs (Mackie and LeJeune, 2009; Bursztyn, González and Yanagizawa-Drott, 2018). This is the first paper to provide rigorous evidence for signaling as potential driver of those behaviors.

What is more, while other drivers tend to motivate interventions that 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 new signals can 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 cost. We also contribute to a rapidly growing literature on field evidence for social signaling mechanisms, outside the domain of traditional practices. Besides Karing and Karim (2018) and Karing (2018), more directly linked to children's human capital, Bursztyn et al. (2018) shows that platinum credit cards are used for social signaling and that social image and self-image are substitutable. We add to this literature by documenting that social signals can be substitutes across multiple domains.

Our findings have direct policy implications. While our intervention is primarily a mechanism experiment, meant to shed light on the drivers behind traditions and the process through which those can be changed, public donations boxes and rubber bracelets are cheap and easily scalable, and could be used to effect change across a range of different behaviors. Most importantly, beyond the specific intervention, our results indicate that, under certain conditions, it is possible to encourage desirable behavior while *at the same time* discouraging undesirable behaviors, even outside the incentivized domain. What is more, we qualify the role of local elites in mediating those effects, shedding light on the conditions under which those should be targeted as facilitators to avoid the risk that the intervention backfires.

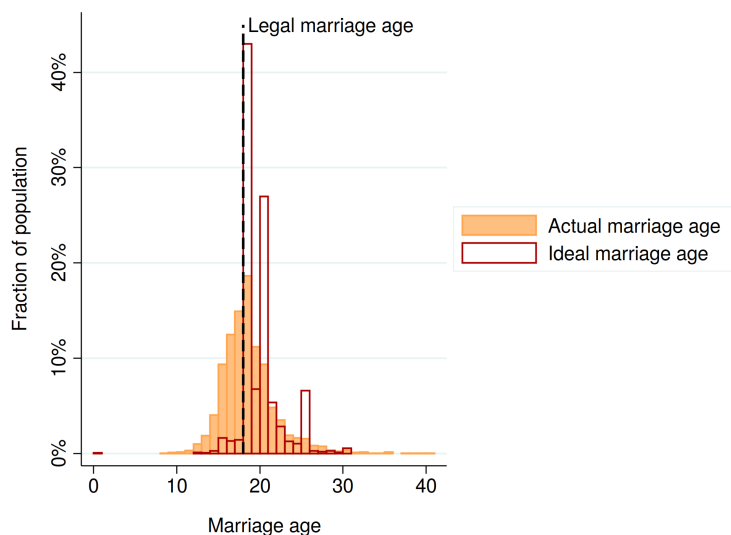
⁴See Fehr and Schurtenberger, 2018 for a recent review of the literature.

2 Background and Data

Malawi has a diverse cultural heritage with numerous common traditional practices. In this paper we focus on two particularly common traditional practices that potentially impair childrens' human capital: child marriage and forced sexual initiation rituals. Both practices predominantly concern girls. Figure B.1 in the appendix shows the prevalence of girls' marriage across the districts of Malawi in Panel A and the prevalence of girls' sexual initiation rituals in Panel B. While child marriage is very common all over the country with rates for women around 50% in many districts, sexual initiation rituals are more concentrated to the populous south of the country.

Attitudes towards traditions and their prevalence do not always coincide. Figure 1 shows the distribution of the age at first marriage alongside the reported ideal age of marriage for girls. Marriage below age 15 is relatively rare but marriages at the ages of 15-18 are very common. Moreover, while ideal age of marriage and actual marriage both have a mode of 18 years, reported ideal ages below 18 are rare, despite its high prevalence. These differences could be explained by changing norms, by external factors influencing marriage decisions – such as financial incentives, peer pressure, and social image concerns – or by reporting biases, influenced by social desirability or cognitive dissonance.

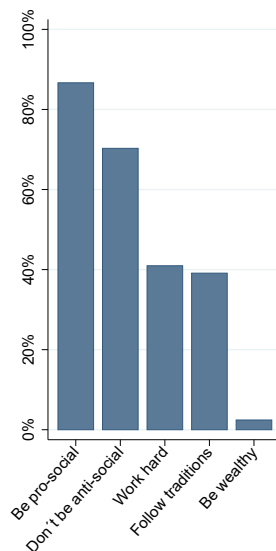
Figure 1: Distribution of actual and ideal marriage age for females



Consequences of early marriage are well documented in the literature, but research on sexual initiation rituals remains scarce. Figure B.2 shows descriptive evidence about typical activities at sexual initiation rituals in Malawi, in Panel A, and the most important decision-makers behind participation in those practices, as stated by survey respondents, in Panel B. Activities range

Figure 2: 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: Answers compiled from open answers. *Be Pro-social* includes being helpful, loving, peaceful, etc; *Don't be anti-social* includes no gossiping, no stealing, etc; *Work hard* also includes working as a farmer; *Follow traditions* includes practicing a specific religion.

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 families in Malawi decide to follow such traditions? In an open question, 40% of respondents state that households follow traditions in order to get a great social image or a great reputation. Further common determinants of a positive social image are shown in Figure 2. Besides following traditions, acting pro-socially and working hard are its most often cited determinants, while being wealthy is *not* seen as an important determinant of social image.

We collected the 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.1 in the appendix shows summary statistics. 7,388 randomly selected eligible households with children from 8-17 were surveyed, for a total of 14,821 interviews. Not every selected individual within a household was selected for every questionnaire. If present, household heads always completed the household questionnaire as well as an individual questionnaire. Their spouses and, if applicable, one minor > 15, also completed an individual questionnaire. Survey experiments were at most conducted by two individuals per household. Villages have, on

average, 116 households, 82% of them are located in rural areas. Average household size is 4.6; respondents are on average 38 years old and 56% are female. Subsection B.5 presents sample characteristics by treatment assignment status.

3 Empirical Strategy

This section starts by laying out a simple conceptual framework in subsection 3.1 to organize our empirical strategy. The simple model shows how harmful practices could emerge as a signal within a baseline separating equilibrium and under what conditions alternative signals could destroy that baseline equilibrium, yielding specific predictions about what should happen to perceptions and attitudes under a pooling or a new separating equilibrium. Next, subsection 3.3 presents the details of our research design for randomly assigning both the availability and adoption of different signals, followed by details on compliance with the assignment in subsection 3.4. Last, subsection 3.5 describes how we elicit subjects' perceptions and attitudes.

3.1 Conceptual framework

Motivated by the stylized fact that Malawian households claim to follow traditions at least partly to build a positive social image, we propose a stylized framework – heavily based on standard signaling models (Spence, 1973; Zahavi, 1975) – that helps motivate our empirical strategy and guides the interpretation of the experimental results. In the main text, we summarize the intuition of the simple model in Appendix A.

Suppose there are two types of individuals in society: some that value long-term cooperation to a greater extent, seeking mutually beneficial relationships with one another ("*pro-social*" type), and others that are *not* cooperative, who value short-term benefits to a greater extent ("*individualistic*" type).

Pro-social individuals prefer to interact with others of the same type, to avoid being exploited by individualistic ones. At the same time, the latter try to take advantage of the former in order to maximize their short-term benefits.

It may be difficult for individuals to recognize each others' types without permanent interaction. Thus, it may be worthwhile for pro-social individuals to obtain a costly signal that separates them from individualistic ones. Such a signal needs to be costly enough to prevent the individualistic type from obtaining it, but cheap enough to be attractive for the pro-social

type. This is possible, because pro-social individuals value long-term repeated cooperation to a greater extent, and display a higher willingness to pay for an effective signal than individualistic ones, provided cooperation generates a higher surplus in the long-term.

Sustaining a separating equilibrium depends on beliefs about what others do off-equilibrium path, because even pro-social individuals could benefit in the short-term from defection – a typical prisoners’ dilemma. A subgame perfect separating equilibrium can be sustained by beliefs that deviating from cooperation within pro-social types is punished with eternal defection by peers (and similar for enforcement of this punishment) as long as pro-social types care sufficiently more about future payoffs.

In the context that we study, we entertain the hypothesis that following local traditions stands for a signal in the baseline separating equilibrium, distinguishing those with an interest in mutual long-term cooperation from other villagers.

This simple model suggests two possibilities for how the introduction of a new signal might break down a baseline separating equilibrium. First, if beliefs about the share of pro-social individuals in the society change, signaling could no longer be optimal for the pro-social type. The reason is that, if the risk of being exploited is sufficiently low, the value of an effective signal decreases. In that case, no type would signal, and a pooling equilibrium would emerge in which local traditions formerly used to signal pro-sociality lose support. Alternatively, it might be the case that the new signal is cheap enough that every type adopts it. In that case, it would no longer enforce a separating equilibrium, but, rather, sustain a pooling equilibrium (under certain conditions for beliefs about what individuals do off-equilibrium path) whereby, again, local traditions formerly used to signal pro-sociality lose support.

Second, if the new signal is less costly than the baseline signal but still costly enough to enforce a separating equilibrium, this new signal could replace the old one in a new separating equilibrium.⁵

While both mechanisms might cause societies to abandon the original signal, they yield different predictions for the features of the new equilibrium: in the former case, individuals pool, whereas in the latter, they still separate (just using a different signal). In particular, each mechanism has different predictions for who adopts the new signal and what information can be extracted from different signals. In a pooling equilibrium, different types sort into the new signal and/or pro-social individuals sort out of the original signal. Moreover, in a pooling

⁵The two mechanisms are captured by equations 11-12 in the simple model in the Appendix.

equilibrium it becomes harder to predict who is a pro-social type compared to the baseline separating equilibrium.

We test those predictions through the experimental design outlined in the following subsections.

3.2 Implications for welfare

Within the signaling model, if a new separating equilibrium based on a lower-cost signal replaces the baseline one, then welfare must necessarily improve: gains from cooperation within “pro-social” types are now enforced at lower individual and societal costs.

Conversely, if what emerges is a pooling equilibrium, welfare implications are less straightforward, and depend ultimately on how the new signal affects beliefs about the share of “pro-social” types in society.

On the one hand, when signaling is no longer informative, long-term cooperation within “pro-social” types would no longer be enforced on the equilibrium path if beliefs about the share of “pro-social” types do not change. This situation could emerge if “pro-social” types switch to the new signal believing that other “pro-social” types do the same and cooperate exclusively with senders of the new signal, but “individualistic” types also opt-in, leading to a pooling equilibrium. This would cause cooperation to break down and lead to lower welfare (since there are gains from cooperation). Having said that, there are lower societal costs from enforcement through the original signal, such that net welfare changes will depend on welfare weights assigned to different individuals or on the extent to which future cooperation gains depend on present signals.⁶ On the other hand, if the new signal (accurately) changes beliefs about the share of “pro-social” types, such that “pro-social” types are willing to cooperate regardless of the signal – since the risk of being exploited would be low enough – welfare would improve unambiguously, even in a pooling equilibrium (possibly once everyone has updated their beliefs and stops signaling altogether).⁷

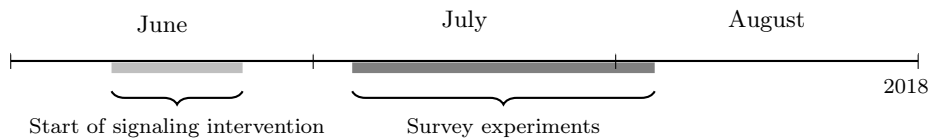
To distinguish between different types of pooling equilibria, one would have to test what happens to cooperation within “pro-social” types – those that harm to signal in the baseline equilibrium – under the new equilibrium.

⁶Concretely, if harming to signal destroy girls’ human capital, decreasing their future productivity and, hence, the surplus associated with any future interactions, then, even though long-term cooperation can no longer be enforced within “pro-social” types in a pooling equilibrium, aggregate welfare might actually increase.

⁷We abstract from elaborated second-order beliefs that would create additional equilibria and ways of norm transformation.

3.3 Experimental design

Our experimental design entails two stages: a randomized control trial (RCT) that randomly assigns the availability of different signals across villages, and a survey experiment that randomizes who adopts them within each village. The village-level RCT was carried out in June 2018. After about five weeks (the average time span across the 412 villages we work with), we conducted the individual-level survey experiment as part of a nationally representative survey on traditional practices in Malawi.

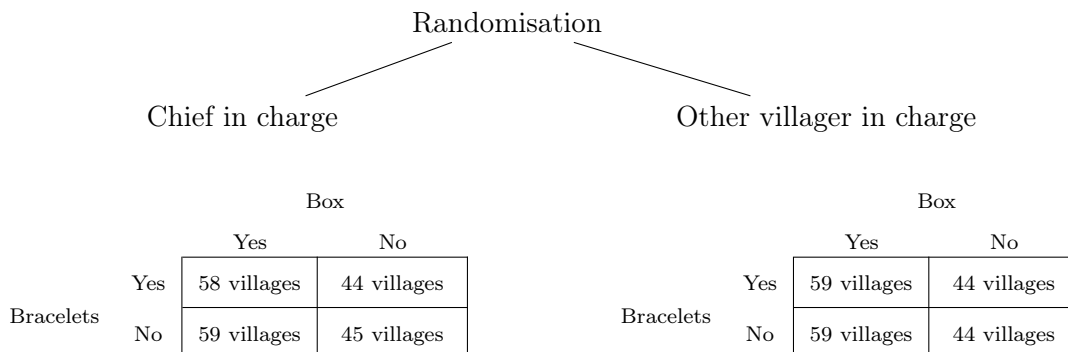


3.3.1 Village-level RCT: availability of different signals

We randomly assigned the availability of alternative signals by cross-randomizing public donation boxes and red rubber bracelets across 412 Malawian villages, in line with Figure 3. Assignment took place between June 10-22, 2018, as part of household listings by enumerators from the National Statistical Office of Malawi in preparation for the upcoming national survey (see subsection 3.3.2).

To kick-off the intervention in treated villages, we distributed bracelets according to different criteria depending on whether public donation boxes were also available. In villages assigned to both bracelets and boxes, village chiefs were asked to list the ten households in the village most likely to support the needy households in their village, “for example by giving food or other important things”. After completing the list, chiefs were informed that the households they mentioned would be granted a red rubber bracelet to “show everyone that they can be counted

Figure 3: Experimental design of the signaling intervention



on”; those households were later informed along those 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/bracelet holder* was endowed with a big collection box, a measuring cup, and 80 additional bracelets.

The box/bracelet holder was randomized to be either the chief him/herself or the last person on the list of households listed as likely to support the needy households in the village.⁸ The box/bracelet 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.⁹

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

In villages assigned to receiving bracelets only, the procedure was again identical except that no public donation box was made available. After listing the ten households, the chief was informed that a study involving bracelets was being conducted in the village. For this purpose, ten households were randomly assigned to receive bracelets, prior to our arrival in the village. Those households were informed about why they were receiving bracelets along the same lines. Because other households in the village might also like to obtain a bracelet after, the *bracelet holder* was given 80 additional bracelets and instructed to sell these bracelets for 200 MWK (the monetary equivalent of two kilograms of maize). Payments were to be kept by the bracelet holder.

Finally, in pure control villages (without bracelets or public donation boxes), village chiefs were simply asked to list the ten households most likely to support the needy households in their village “for example by giving food or other important things”.

3.3.2 Individual-level survey experiment: adoption of different signals

About five weeks after the onset of the signaling intervention, we conducted a survey experiment that randomly assigned the adoption of the new signal, in line with Figure 4.

⁸Pilots suggested that households listed at the bottom were less likely to be the immediate family members or direct peers of the village chief.

⁹In local markets, the price of rubber bracelets is roughly identical to the price of two kilograms of maize.

In the experiment, households have to judge how pro-social a hypothetical Malawian father is. Different households were randomly assigned to different versions of the background story for that father. In the first version, the top left picture in Figure B.3 in the Appendix was shown, 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 second version, the top right picture in appendix Figure B.3 was shown, 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."

Whenever multiple subjects were interviewed within a household, 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.

At the end of the survey (about 15 minutes later), respondents are again confronted with the same background story (the same version as they were presented with at the beginning);

Figure 4: Experimental design of the survey experiment

		Father supports child marriage (between subjects)	
		Yes	No
Father wears bracelet (within-subjects)	No	3,510 HH	3,468 HH
	Yes	3,510 HH	3,465 HH

only, this time, John and his wife are depicted wearing red rubber bracelets (in both versions, depicted in the bottom row of appendix Figure B.3). Bracelets are designed to look exactly like the ones we use in treated villages 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 were then asked to evaluate John’s pro-social preferences.

By cross-randomizing the survey experiment and the RCT, we can test whether differences in how respondents rate John wearing a bracelet or not are caused by whether bracelets are a signal of pro-sociality in their village.

3.4 Compliance

The signaling intervention entailed four different conditions. Team supervisors were responsible for delivering the pre-assigned treatment to the box/bracelet holder in the village. We could check compliance with treatment assignment at the time of the survey. There are two potential sources of non-compliance: either team supervisors did not properly follow the assignment protocol, or box/bracelet holders handed out control of the intervention to another household over the course of the 5 weeks before the survey. Appendix Table B.2 shows that compliance with assigned treatment condition across villages was high, but that there were significant deviations from the intended 50-50 split of box/bracelet holders between chiefs and other villagers. Notably, the control group is perfectly balanced while, for the presumably most powerful signal (Box & Bracelets), chiefs more often took over control – an outcome interesting in itself, to which come back in section 7.

In the econometric analyses, we account for imperfect compliance by restriction attention to intention-to-treat (ITT) and by estimating instrumental variable (IV) regressions using initial treatment assignment as instrument for the actual treatment status (e.g. Angrist and Pischke, 2008).¹⁰

We ensured perfect compliance with the assignment for the survey experiment thanks to pre-assignment of all households to treatment groups and tablet-based implementation of the experiment.

¹⁰One might be concerned about the violation of the monotonicity assumption due "defiers" (Imbens and Wooldridge, 2009). While this hypothesis cannot be tested, we believe this to be unlikely in our setting as a violation of the monotonicity assumption would require some chief to be a *contrarian*, i.e. to always want to give away control of the intervention whenever they were assigned to be in charge and, at the same time, to always take it over whenever another villager was assigned to be in charge.

Balance checks for the RCT and for the survey experiments are displayed in Appendix B.5. All covariates are well balanced across treatment conditions.

3.5 Outcomes

3.5.1 Measuring social image

Capturing the effects of the availability and adoption of different signals requires a comprehensive measure of social image. For this purpose, we adapt Falk et al. (2016)’s pre-tested Preference Survey Module’ on social preferences to the Malawian context.¹¹ We include three dimensions of social image: altruism, reciprocity, and trust. Subjects rate each of these dimensions on a scale from 0 (not at all) to 10 (completely).

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 social image.¹²

We elicit these measures at three different instances: (1) when asking subjects to assess pro-sociality of a hypothetical Malawian father; (2) when asking subjects to assess their own pro-sociality; and (3) when asking village chiefs to assess the pro-sociality of selected subjects in his/her village.

3.5.2 Measuring attitudes towards traditional practices and their prevalence

The second set of outcomes involves prevalence of child marriage and initiation rituals, and subjects’ attitudes towards these traditional practices.

Prevalence of child marriage is based on a measure for marriage below 18, created from 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?”*

Favorable attitudes towards child marriage are inferred from stating the ideal age of marriage as under 18 (in response to the survey question *“I will now ask you for your own, personal opinion on some topics. In your opinion, what is the right age for a woman to get married?”*), while favorable attitudes towards initiation rituals are captured from the answer to the question *“I*

¹¹These survey modules mimic standard games from experimental Economics and have been shown to predict behavior in incentivized choice experiments very accurately.

¹²This was pre-registered as part of trial AEARCTR-0002856 in the AEA RCT Registry on June 11, 2018.

would now like to ask you your personal opinion on some topics. Please tell me whether you agree or disagree with the following statements. Sexual initiation rites for girls should be continued.”.

4 Is Support for Child Marriage a Signal of Pro-sociality?

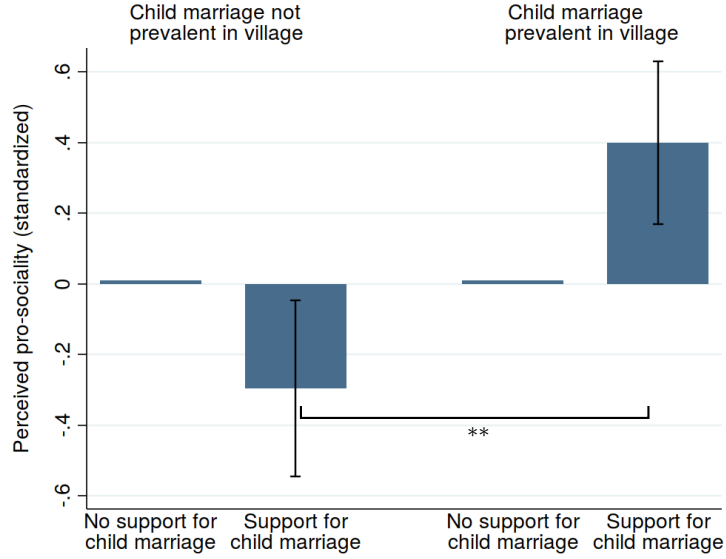
This section investigates whether engagement in traditional practices affects social image. We start with descriptive evidence that shows the correlation between following traditional practices and social image among Malawian villagers in subsection 4.1. Since adoption of different signals is, however, not randomly assigned, in subsection 4.2 we analyze how deviations from local traditions affect the social image of a hypothetical Malawian father with the help of our survey experiment.

4.1 Chiefs’ perceptions of actual villagers

We start providing evidence that supporting child marriage correlates how villagers are perceived. We asked village chiefs to assess both pro-sociality (according to the dimensions we measure) and support for child marriage of randomly selected villagers. Figure 5 shows the correlation between the two variables in villages where under-15 marriage does not exist (on the left-hand side) and where under-15 marriage is prevalent (on the right-hand side). While those who support child marriage are perceived as less pro-social in villages where child marriage is uncommon, the opposite is true within villages where it is prevalent. Notably, in the latter set of villages, chiefs rate those who support child marriage as 0.4 s.d. more pro-social. This difference is large in magnitude, roughly two-fold the correlation between the summary measure of pro-sociality and charitable giving.

Naturally, support for child marriage (or, in our terms, the adoption of this signal) is not randomly assigned; as such, chiefs could rate different individuals as more or less pro-social based on other characteristics that correlate with attitudes, precluding causal statements merely based on those correlations. For this reason, the next subsection analyzes the effects of random adoption of different signals on perceived social image in the context of our survey experiments.

Figure 5: Villagers' social image 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 village chief for villages where child marriage is not prevalent on the left (0% of marriages < age 15) and villages where child marriage is prevalent on the right (>0% of marriages < age 15). The pro-sociality measure is an equally weighted, standardized combination of individual measures for altruism, reciprocity, and trust (see Section 3.5.1). Estimates are from a linear regression, absorbing village fixed effects, along with standard error bars, clustered at the village level, and a Wald-test for equality of estimated coefficients.

4.2 Results of the survey experiment

We now turn to the results of the survey experiment, in which households are randomly assigned to a vignette featuring John, a Malawian father who either submits his 14-year-old daughter to child marriage (and to initiation rituals, previously) or not.

We estimate the following equation:

$$Y_{vhi} = \alpha_v + \beta_1 \text{John}_h + \beta_2 \text{John}_h \times \text{Share_childmarriage}_v + \beta_3 X_{vhi} + \zeta_{vhi}, \quad (1)$$

where Y_{vhi} is the summary measure of John's perceived social image, attributed by individual i living in household h and village v ; John_h captures the version of John households are assigned to, equal to 1 if he marries off 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; X_{vhi} is a vector of individual-level controls; and α_v are village fixed effects. Standard errors are clustered at the village level.

Table 1: Effect of engagement in traditional practices on perceived social image?

Dependent variable: Social image of John	Summary Measure (1)	Individual components		
		Altruism (2)	Reciprocity (3)	Trustworthiness (4)
John supports child marriage × Share married < 15	1.576*** (0.468)	1.641*** (0.602)	1.080* (0.618)	2.006*** (0.589)
John supports child marriage	-0.612*** (0.0298)	-0.500*** (0.0350)	-0.429*** (0.0382)	-0.909*** (0.0387)
Individual controls	✓	✓	✓	✓
Village fixed effects	✓	✓	✓	✓
Observations	6,978	6,978	6,978	6,978
Clusters	412	412	412	412

Notes: The summary measure (1) is an equally weighted average of standardized individual measures for (2) altruism, (3) reciprocity, and (4) trust (see Section 3.5.1). 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

In Table 1, column (1) displays the results for the summary measure, while columns (2) to (4) report them for 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 assign a significantly worse social image to John if he supports child marriage, an effect size of about 0.6 standard deviation (Column 1; significant at the 1% level). The interaction term with the share of the village married before 15 is positive (also significant at the 1% level), indicating that social image associated with support for child marriage *improves* with the local prevalence of child marriage, consistent with its causal contribution to social image where child marriage is a norm. In column 1, John who supports child marriage would have a better social image than the one who does not in villages with prevalence 38.8% or higher, which is close to the maximum prevalence for child marriage under-15 we observe in our sample (41%).¹³ Findings are consistent with the correlational evidence from chief’s assessments of villagers’ social image (Figure 5). Together, evidence strongly backs the hypothesis that following local traditions affects social image and serves as a signal for pro-sociality.

¹³The lower bound of the 95% confidence interval for the tipping point is 18.6%. In any case, our survey experiment presumably under-estimates the signaling value of child marriage since marriage under-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.

5 Do New Signals Substitute for Child Marriage?

This section assesses whether new signals can break down the baseline separating equilibrium, exploring the RCT introduced in Section 3.3.1. Subsection 5.1 analyzes the take-up of donations and bracelets across treatment conditions, followed by an assessment of whether public donation boxes affect the profile of those who donate in the village (within or outside our experiment), in subsection 5.2. Next, subsection 5.3 showcases the effects of the intervention on the predictability of social image, by contrasting chiefs' assessments of who is pro-social in the village to individuals' self-assessments across treatment and control – an important element to gauge whether the new signal introduces a new pooling or separating equilibrium. Last, subsection 5.4 studies how the new signal affects social image within our survey experiment, by contrasting how subjects perceive John's social image when he wears a bracelet, or not, across villages where bracelets stand for donation and all other villages, and by analyzing whether different signals are substitutes or complements – i.e., whether the social image of John's version who supports child marriage deteriorates or improves when he wears a bracelet in villages where boxes and bracelets are made available.

5.1 Manipulation tests

We start by testing whether assignment indeed had the intended effects on the take-up of donations and bracelets, leading to differential levels of maize donations and bracelet distributions across different treatment conditions. Figure B.4 in the appendix shows the average number of kilograms of maize collected in each village among the four treatment conditions in Panel A1 and the number of bracelets distributed in Panel B1. Indeed, maize was almost exclusively collected (through public donation boxes) and bracelets were almost exclusively distributed in the villages assigned to donation boxes and bracelets distribution, respectively. Interestingly, the number of bracelets distributed (≈ 50) and the amount of maize collected ($\approx 50kg$) almost coincide across the bracelets and box conditions, indicating that households donated roughly the required amount (two kg) on average in exchange for the bracelets.

We also perform manipulation checks to confirm that bracelets and food donations indeed contribute to a positive social image. For this purpose, we analyze an open survey question, asking what *food collections* and *red rubber bracelets* mostly stand for. We categorize open answers into two categories: *sharing* and *wealth*. Figure B.4 in the Appendix shows the results

for food donations in Panels A2 and A3, and for bracelets in Panels B2 and B3. Food donations represent sharing for roughly 40-50% of households among all treatment conditions, while very few households perceive food donations as a signal for wealth.¹⁴ In contrast, there are significant differences regarding perceptions of what *red rubber bracelets* stand for. In the box and bracelets treatment condition, households associate bracelets to sharing to a much greater extent than in any other treatment condition. Conversely, bracelets are nowhere perceived as a signal for wealth.

5.2 Who takes up the new signal?

Having shown that treatment assignment indeed encourages food donations through a public collection scheme, and that donations are indeed recognized as pro-social activity, we investigate who donates (within and outside our experiment) across different treatment conditions.¹⁵

Table B.8 in the appendix reports descriptive statistics for villages without public donation boxes (*Bracelets* and *Control*) in columns (1) to (3), and for those with them (*Box&Bracelets* and *Box*) in columns (4) to (6). In villages without boxes, there are considerable differences in the composition of those who state to have recently donated and those who do not. In particular, individuals who donate report to be more altruistic and reciprocal (not significant), have significantly higher consumption expenditures, and are less likely to engage in and support child marriage. In contrast, in villages assigned to public donation boxes, the composition of those who donate and those who do not is much more similar with respect to *all* characteristics, because those who typically do not donate in the control group *opt into the new signal* by making a food donation.

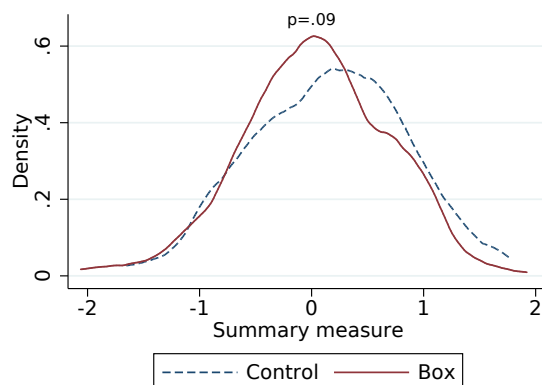
Figure 6 plots the density of the summary measure of pro-sociality for individuals who donate by treatment condition. The *Control* curve lies to the right of the *Box* curve, indicating that, in villages with public donation boxes, those who donate include *less* pro-social individuals than in control villages (statistically significant at the 10% level).

Last, Table B.9 in the appendix documents the joint distribution of favorable attitudes towards child marriage and food donations in villages where boxes were introduced, and in other villages. In villages without boxes, subjects who support child marriage rarely ever donate.

¹⁴This is in contrast to Glazer and Konrad (1996), who argue that charitable giving is driven by the desire to demonstrate wealth.

¹⁵We only surveyed a random subset of households about donations, what explains the smaller sample for the following analysis.

Figure 6: Distribution of self-reported pro-sociality of individuals who donated, by treatment of the signaling intervention



Notes: Graph produced from kernel density estimates using the Epanechnikov kernel and optimal bandwidth, along with the p-value from a Kolmogorov-Smirnov test for equality of the two distributions. The summary measure is an equally weighted average of standardized individual measures for altruism, reciprocity, and trust (see Section 3.5.1).

In contrast, in villages with boxes parents who support child marriage donate considerably more often – even more so than parents without favorable attitudes towards child marriage. Taken together, results suggest that there might indeed be scope for substitution across child marriage and donations as a signal for pro-sociality, as there is no longer segmentation in donation behavior when it comes to support for harmful practices in treatment villages. The next subsection analyzes whether the break down of segmentation makes it more difficult to predict who is pro-social in villages assigned to public donation boxes, which would be consistent with a new pooling equilibrium emerging.

5.3 Separating or pooling?

We turn to the question of whether the new signal affects chiefs’ prediction accuracy of villagers’ self-reported altruism, positive reciprocity, and trust. For this purpose, we ask village chiefs to assess five randomly selected household heads in their village with respect to those pro-sociality measures. We are able to match 787 chief-villager pairs based on unique person names within village.¹⁶

Table 2 displays the effect of the assignment to public donation box on chief’s prediction accuracy, where accuracy stands for the absolute difference between the self-reported measure

¹⁶In many cases, pre-selected household heads were not found by enumerators, and thus no data could be matched.

Table 2: Chiefs' prediction accuracy of altruism and positive reciprocity

Dependent variable: (self reported - chief assessed)	(1) Altruism (0-10)	(2) Reciprocity (0-10)	(3) Trust (0-10)
Box	0.518** (0.259)	0.0874 (0.229)	0.118 (0.209)
Mean dependent variable (Control)	3.241	3.053	3.511
Chi ² -test Box jointly=0, (p-val.)		0.0794	
Village chief controls	✓	✓	✓
Village controls	✓	✓	✓
Observations	787	787	787
Clusters	296	296	296

Notes: Regressions additionally include village chief controls (female, age, age², and age³), village-level controls (village size, population density, urban), plus a constant. Standard errors, clustered at the village level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

and chief's assessment. As can be seen from the mean of the dependent variable, prediction accuracy is low in control villages.¹⁷

In villages where boxes were made available, chiefs become significantly less able to predict who is altruistic, compared to control villages. While prediction accuracy of positive reciprocity and trust are not significantly affected by the treatment, jointly, the three coefficients are significantly different across treatment conditions (at the 10% level). This is not driven by households feeling more pro-social as a result of the interventions (see Table B.10 in the appendix). Instead, it is evidence that the impact of the new signal on segmentation of the composition of those who donate in each village interferes with the signaling value of pre-existing signals. This is consistent with the emergence of a pooling equilibrium.

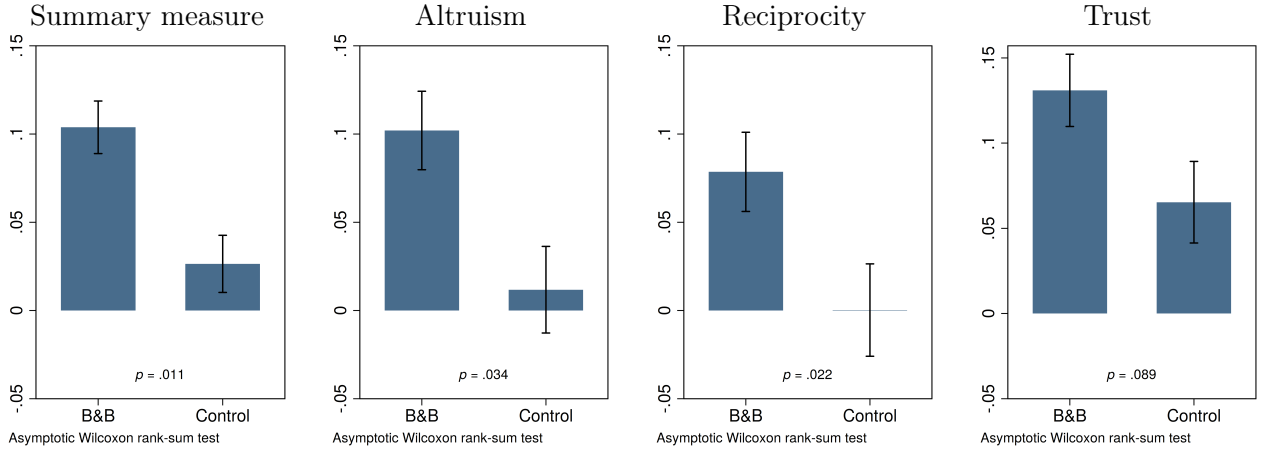
The next subsection tests explicitly whether the new signal substitutes for the reputational effects of supporting child marriage traditions.

5.4 Do new signals substitute for following traditions?

We now return to the survey experiment to analyze how wearing a bracelet affects the reputation of *John* across villages assigned to different treatment conditions. We explore within-subject variation, comparing John's perceived social image when he wears a bracelet to when he does not, holding constant the framing of his support for harmful practices – which is randomized only across households.

¹⁷Different from political brokers in Finan and Schechter (2012), village chiefs are not elected by village members, and presumably do not have to rely on accurate knowledge of who is reciprocal, motivated by vote-buying, for instance.

Figure 7: Reputational Benefit of John Wearing a Bracelet



Notes: Mean improvement of John's reputation from standardized measures along with standard error bars and asymptotic Wilcoxon rank-sum tests. The summary measure is an equally weighted average of standardized individual measures for altruism, reciprocity, and trust (see Section 3.5.1).

We estimate the following equation:

$$\Delta Y_{vhi} = \beta_0 + \beta_1 \text{Box\&Bracelets}_v + \beta_2 \text{Box}_v + \beta_3 \text{Bracelets}_v + \beta_4 X_{vhi} + \zeta_{vhi}, \quad (2)$$

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 B.11 in the Appendix displays the results, summarized in Figure 7. We find that John's social image 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 box bracelets and public donation boxes were made available. The comparison to other villages helps to rule out experimenter demand effects from eliciting John's social image a second time when he wears a bracelet, and results from the survey experiment are consistent with chiefs' perceptions in *Box & Bracelets* villages.

Table 3: Substitution of signaling value of child marriage when John wears a bracelet

Dependent variable: Social image of John	Box & Bracelets (1)	Control (2)
John supports child marriage × Share married < 15	-0.285 (1.295)	1.918** (0.737)
John supports child marriage	-0.410*** (0.0655)	-0.523*** (0.0654)
Individual controls	✓	✓
Village fixed effects	✓	✓
Observations	2,020	1,460
Clusters	117	89

Notes: The summary measure (1) is an equally weighted average of standardized individual measures for (2) altruism, (3) reciprocity, and (4) trust (see Section 3.5.1). 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Most importantly, we now assess how the new signal affects the signaling value of child marriage. In line with Section 4, we explore heterogeneity in the prevalence of child marriage across villages in the following analyses.¹⁸ We start by re-estimating equation 1, but this time focusing on the households' assessments of John, when learning that he wears a red rubber bracelet.

Table 3 shows evidence for substitution between signals. In villages where child marriage does not exist, supporting child marriage still hurts John's reputation – in line with Table 1. The estimated coefficient in the second row is less negative in villages where bracelets imply food donations than in control villages. Moreover, the interaction term with the share of child marriage in the first row is no longer positive in villages where bracelets imply donations, but remains large and significant in control villages. Thus, John does not derive a reputational benefit from supporting child marriage anymore, but only if an alternative signal of pro-sociality was available in that village.

As the manipulation of John wearing or not wearing a bracelet was *within subject*, we can additionally stack individual responses for every instance when they evaluate John's social image (wearing bracelets and not) and estimate the following equation, absorbing individual fixed effects:

¹⁸Table B.12 in the Appendix documents that prevalence of child marriage based on our representative survey self-reports is not affected by the RCT, and hence can be considered predetermined for this purpose.

$$\begin{aligned}
Y_{vhit} = & \alpha_i + \eta_1 \text{John}_h \times \text{Share_childmarriage}_v \times \text{Bracelet}_t + \eta_2 \text{John}_h \times \text{Bracelet}_t \\
& + \eta_3 \text{Share_childmarriage}_v \times \text{Bracelet}_t + \eta_4 \text{Bracelet}_t + \epsilon_{vhit},
\end{aligned} \tag{3}$$

where Y_{vhit} is the summary measure of John’s perceived social image, 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 B.13 in the Appendix displays the regression results. In villages where child marriage is not prevalent, wearing a bracelet improves John’s social image when he supports child marriage by 0.2 standard deviations more in villages where bracelets and public donation boxes were made available (significant at the 1% level). In turn, in high prevalence villages – where John previously derived a more positive reputation from supporting child marriage – the opposite happens: the triple interaction term in the first row suggests that the effect of wearing a bracelet gets significantly more negative with increasing prevalence of child marriage in *Box & Bracelets* villages (significant at the 1% level). Other treatment arms of the RCT, where bracelets do not represent food donations, do not show these effects. Both results support the hypothesis that the new signal crowds out the reputational effect of conforming to existing local norms. The signal of child marriage is dampened by bracelets when those stand for pro-social behavior.

Results are again consistent with the hypothesis that the new signal introduces a pooling equilibrium, whereby the informational content of the prior signal – which previously enforced a separating equilibrium – becomes weaker.

6 Do New Signals Change Attitudes and Behavior?

Having shown that the new signal weakens the signaling value of child marriage, this section studies whether the introduction of the alternative signal affects individuals’ attitudes towards traditional practices. Five weeks after boxes and bracelets were made available, do subjects change their views about the right age for a woman to marry or about whether sexual initiation

rites should be continued *differentially* across different treatment conditions? Subsection 6.1 presents effects of our treatment conditions on attitudes, followed by heterogeneity analysis in subsection 6.2 and robustness checks in subsection 6.3. Last, subsection 6.4 presents the results of our 12-month follow-up, assessing whether RCT assignment has long-term effects on perceptions and attitudes, and affects actual engagement in child marriage and sexual initiation rituals [forthcoming].

6.1 Effects on attitudes

In the pre-analysis plan, we pre-specified that we would estimate the effects of signal availability on attitudes by pooling together the two treatment conditions for which public donation boxes were made available (i.e. *Box & Bracelets* and *Box*, through which food donations were collected), on the one hand, and the remaining conditions (*Bracelets* and *Control*), on the other. We also showcase results separately by treatment arm in Appendix C.3.

We estimate the following equation:

$$Y_{vhi} = \alpha_0 + \alpha_1 \text{Donation}_v + \alpha_2 X_{vhi} + \alpha_3 Z_v + \epsilon_{vhi}, \quad (4)$$

where Y_{vhi} is an indicator favorable equal to 1 if individual i living in household h in village v states that the right age for a woman to marry is below 18 (support for child marriage) / that sexual initiation rituals should be continued (support for initiation) / that s/he got married before 18 (prevalence of child marriage) / that s/he took part in initiation rituals with a sexual component (prevalence of initiation), and 0 otherwise (in each case); Donation_v indicates whether village v was assigned to host a public donation box; X_{vhi} is a vector of individual-level controls; and Z_v are village-specific controls. Standard errors are clustered at the village level.

To deal with imperfect compliance (as described in subsection 3.4) we additionally estimate an instrumental variable regression model, whereby actual assignment is instrumented with intended assignment. This captures the effect of the signal had assignment been perfect, under the assumption of monotonicity (Angrist and Pischke, 2008), which we also discuss in subsection 3.4.

$$1^{st} \text{ stage: } \text{Actual-Donation}_v = \beta_0 + \beta_1 \text{Donation}_v + \beta_2 X_i + \beta_3 Z_v + \zeta_{iv} \quad (5)$$

$$2^{nd} \text{ stage: } Y_{iv} = \gamma_0 + \gamma_1 \overline{\text{Actual-Donation}_{iv}} + \gamma_2 X_i + \gamma_3 Z_v + \xi_{iv}, \quad (6)$$

where Actual-Donation_v equals 1 if public donation boxes were actually available in village v at the time of the survey (about 5 weeks after the initial assignment), according to the survey team supervisor, and 0 otherwise. Once again, we cluster standard errors at the village level.

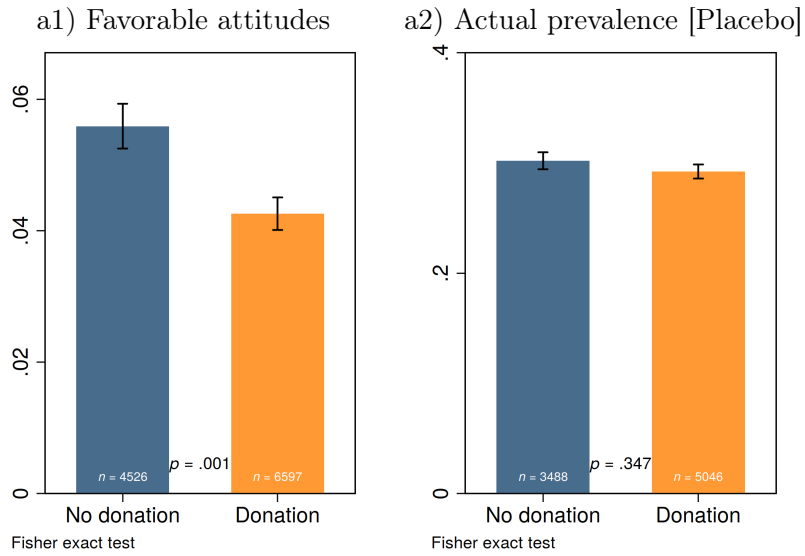
Figure 8 displays the effects of the signaling intervention on *child marriage* (Panel A) and *sexual initiation rituals* (Panel B). In each Panel, the left-hand side sub-panel depicts *support* towards the traditional practice, and that on the right-hand side depicts *prevalence* of that tradition. In the context of the baseline survey, contrasting prevalence across villages assigned to different treatment arms should be thought of as placebo test, given that it could not have been plausibly influenced by the signaling intervention only five weeks before. We look at the effects of the RCT on actual prevalence in the follow-up survey, 12 months after the intervention, in subsection 6.4.

Results are as follows. The new signals significantly decrease support for child marriage (Panel A) by nearly 1/3, from about 6% in control villages to about 4% in those where public donation boxes were made available (significant at the 1% level). This is not driven by experimenter demand effects: actual prevalence of child marriage reported at baseline is the same among all treatment conditions. Along the same lines, the new signals significantly decrease support for sexual initiation rituals (Panel B) by about 20%, from about 8% in control villages to about 6.5% in those with public donations boxes (significant at the 5% level); once again, baseline prevalence based on self-reports is not affected by the treatments.

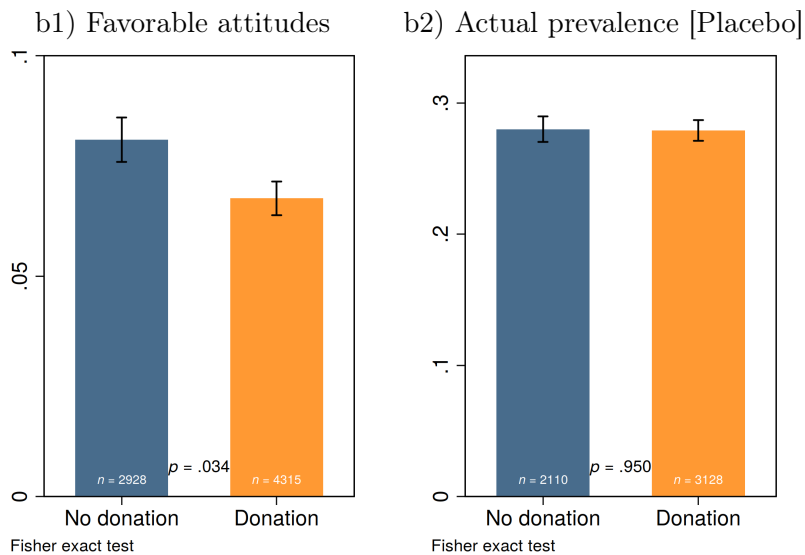
Table B.14 in the Appendix displays the results of the ITT and IV regressions (equations 4-6). Accounting for imperfect compliance with the signaling intervention in column (2), the new signals decrease support towards child marriage by 1.6 percentage points (30% compared to the control group mean). Similarly, column (4) documents that the new signals decrease support for sexual initiation rituals by 1.56 percentage points (20% compared to the control group mean). The estimates are also jointly significant at the 5% level for the two practices ($p=0.014$). Table C.2 in the Appendix confirms the graphical evidence with respect to actual prevalence.

Figure 8: Effect of signaling intervention on traditional practices

Panel A: Child marriage



Panel B: Sexual initiation rituals



Notes: Outcome variables defined according to Section 3.5.2. Means by treatment groups along with standard error bars and p-values from Fisher exact tests.

Appendix C.3 showcases that the intensity of treatment effects matches intuition for the strength of the signals. Effect sizes increase from bracelets alone (which presumably already trigger in-group-out-group dynamics) to boxes alone (which already make donations more visible than in control villages) to boxes and bracelets combined (which make donations very visible).

The fact that public donations affect favorable attitudes towards traditions is consistent with the signaling mechanism. What is more, different from domain-specific interventions like edutainment (e.g. soap operas, as in La Ferrara, Chong and Duryea, 2012, or videos, as in as in Vogt et al., 2016) or information (e.g. Bursztyn, González and Yanagizawa-Drott, 2018), the new signals simultaneously affect *multiple traditions* across different domains.

6.2 Heterogeneity

Child marriage and initiation rituals are influenced by subjects themselves, their parents, the elderly and local leaders (to different extents across practices). Who changes attitudes in response to the new signal? Whether changes in attitudes map into changes in behavior depends critically on the answer to that question. This subsection looks at heterogeneity in treatment effects across different groups. We focus on households with girls *at risk* (at the relevant age for each practice) in contrast to other households, on females in contrast to male respondents, on households that claim to have married for traditional reasons in contrast to those who claim to have done so for emergency reasons (out of pregnancy or economic reasons), and on high-prevalence villages in contrast to low-prevalence. We delay our discussion about village chiefs until section 7 but note that they are not considered influential when it comes to child marriage (according to both villagers and chiefs themselves).

In Figures B.5 and B.6 in the Appendix, which restrict attention to child marriage and sexual initiation rituals, respectively, Panel A contrasts households with and without girls *at risk* for each traditional practice¹⁹; Panel B contrasts female and male respondents; and Panel C contrasts villages above and below the median prevalence of child marriage in our sample. In Figures B.5 only, Panel D contrasts respondents according to the the main reason stated for the own marriage.

We find that the effects of the new signals on support for child marriage are driven by households with girls at the relevant age and by females respondents. Effects are concentrated

¹⁹According to our own data on typical ages of child marriage in Malawi, the relevant age range is 12-17. When it comes to sexual initiation rituals, we assign the relevant age to be 12-14, as rituals typically take place right after menarche.

on high-prevalence villages, as one would expect; when it comes to child marriage, in villages with below-median prevalence of child marriage, there is no treatment effect of the intervention. Notably, support for child marriage in high-prevalence villages with public donation boxes falls to the same level as that in low-prevalence villages, suggesting that social image concerns might completely drive differences in support for child marriage across villages with different prevalence. Last, Panel D shows that the treatment effect is large within respondents that claimed to have married for traditional reasons, while there is no treatment effect within those who married out of an emergency situation (although this difference is not statistically significant, due to a lower sample size as we restrict attention to those ever married and providing relevant reasons). We report the regression analogous results, including IV regressions, in Appendix Table B.15.

While the conditions for changes in attitudes to translate into behavior change might depend on village-specific dynamics (and, ultimately, require assessing effects on the actual prevalence of child marriage and sexual initiation rituals, which we turn to in subsection 6.4), these findings strengthen the interpretation of attitudinal changes in response to the signaling mechanism, since the effects of the new signals on attitudes are uniformly stronger within respondents for whom engagement in each tradition is more likely to influence social image in their village.

6.3 Robustness checks

This subsection assesses whether results are driven by direct beneficiaries of food collection in villages with public donation boxes – what would change the interpretation of our findings –, by experimenter demand effects, or by linearity assumptions in our regression analyses.

We start with the concern that results are driven by the 10% poorest households in each village, the target beneficiaries of food donations collected through our intervention. If that were the case, then that would suggest a different mechanism for the effects of donations on attitudes and behavior: that support for traditions like child marriage is driven by extrinsic motivations (namely, poverty and bride prices) rather than by social image concerns. We can test this hypothesis by contrasting the poorest 10% of households in our sample within each village with all other households, akin to the heterogeneity analysis in Section 6.2. As columns (5) and (9) of Table B.15 show (also, the right-hand side panel of Figures B.5 and B.6), effects are entirely driven by other households (although coefficients are not statistically different due to high variance of the estimates within the 10% poorest households). That not only rules out

that alternative interpretation, but is also reassuringly consistent with the signaling story since those are the households with the ability to donate in the context of the intervention.

Next, we consider experimenter demand effects. Even though boxes and bracelets were distributed about 5 weeks before any survey was undertaken, and even though nothing either at that point or during the survey indicated their connection to traditional practices like child marriage and initiation rituals, one could still worry that the interventions make subjects more willing to comply with their expectations of what surveyors might want them to say. We show that actual prevalence of child marriage and sexual initiation rituals – which should not be affected by the intervention on the absence of those effects – are indeed invariant to treatment assignment; nevertheless, one might worry that attitudes are more sensitive to experimenter demand effects than previous behavior that might have taken place many years prior to 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 subsample of respondents (a random draw of 1/3 of the full sample) 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. We randomly assign subjects to either a "Demand to agree" 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 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.²⁰ The idea is that priming subjects about expected responses should create experimenter demand effects that are arguably much stronger than any implicit experimenter demand effects that could have been triggered over the course of the survey.

In Table 4, column (1) presents general bounds for experimenter demand effects, and column (2) looks at whether bounds depend on the treatment arms of our RCT.

Results are as follows. Priming generates experimenter demand effects that go in the expected direction in each case, but turn out to be rather small (the "Demand to agree" condition increases

²⁰This question is the very last in the survey, to avoid that the priming contaminates other survey responses.

Table 4: Bounding experimenter demand effects

Dependent variable:	(1)	(2)
<i>"Harmful practices are common"</i>	Baseline	Interaction with signaling intervention
Demand to agree	0.0165 (0.0152)	0.0233 (0.0246)
Demand to disagree	-0.0166 (0.0147)	-0.00798 (0.0231)
Donation box		0.0127 (0.0248)
Agree \times Donation box		-0.0119 (0.0312)
Disagree \times Donation box		-0.0151 (0.0299)
Constant	0.773*** (0.0122)	0.765*** (0.0190)
Observations	4,865	4,865
R-squared	0.001	0.001

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

agreement by 1.65 percentage points, a 2-percentage point increase compared to the control condition) and statistically insignificant (Column 1). 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 please experimenters or enumerators.

Last, we consider whether linearity assumptions in our regression specifications matter for our results, since our measures of attitudes are binary variables and since the linear probability model does not bound predicted effects between 0 and 1. Table C.1 in the Appendix estimates those regressions with Probit instead of OLS, displaying marginal effects. All results are virtually identical.

6.4 Effects on behavior

We return to the villages 12 months after the intervention to investigate whether RCT assignment has effects on actual engagement in child marriage and sexual initiation rituals.

[RESULTS ARE FORTHCOMING]

6.5 What 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.

[RESULTS ARE FORTHCOMING]

7 Do Local Elites Matter?

This section investigates whether local elites try to block social norms' change and whether trying to bypass them in the process can backfire, taking advantage of an additional layer in our experiment: the randomization of the identity of the box/bracelet holder in each treatment arm. Subsection 7.1 states the hypotheses, motivated by the literature on political elites and social norms, followed by our empirical strategy and results in subsection 7.2. Last, subsection 7.3 presents evidence on the mechanism underlying our findings by exploring an additional survey experiment with both villagers and chiefs.

7.1 Hypotheses

Local elites exert influence over many critical dimensions of society, from how resources are taxed (extracting rents from those who are not in power; Acemoglu and Robinson, 2006) to how they are redistributed (often with better ability to target; Alatas et al., 2012; Basurto, Dupas and Robinson, 2017) to the extent to which cooperation can be enforced (Acemoglu and Jackson, 2014).

When it comes to traditions, local elites may be both shaped by them (as a source of their power; see Zartman et al., 2000; Henn, 2018) and able to shape them (as *prominent* figures that influence the frames of reference individuals use to interpret the outcomes of past interactions when deciding whether to be cooperative in the future; Acemoglu and Jackson, 2014).

In face of that, when will local elites support social norms' change? In Acemoglu and Jackson (2014), leaders *always* favor change to the extent that it fosters cooperation (because they directly benefit from it). But what does that imply in the context of our model, since the baseline separating equilibrium already enforces cooperation on the equilibrium path – only at high costs? Also, how would that change if the baseline equilibrium allowed leaders to extract rents, as in the case where power stems from traditions (Zartman et al., 2000; Henn, 2018)?

We hypothesize that, when leaders' power does not stem from a particular tradition, they will never oppose a new norm that enforces cooperation at lower costs, closely mimicking Acemoglu and Jackson (2014). However, when it does, we hypothesize that whether they oppose it or not depends critically on whether they can also extract power from the new norm. If there is no change in power, once again the setting closely mimics Acemoglu and Jackson (2014) and leaders will never oppose a new norm that enforces cooperation at lower cost. Conversely, if they lose power under social norms' change, then a trade-off emerges: they will oppose it if their utility loss from losing power is greater than or equal to their utility gain from having cooperation enforced at lower cost. Importantly, taking chiefs to be *prominent* in the spirit of their model, the extent to which they oppose or block social norms' change should *directly reflect into villagers' beliefs and behavior*.

Our setting is particularly conducive to test those hypotheses because the two traditional practices that we focus on differ strikingly in the extent to which chiefs extract power from them. As we show in Appendix Figure B.7, while village chiefs have no say in child marriage decisions (Panel A), they are perceived by a relevant share of villagers to feature among the most influential actors when it comes to sexual initiation rituals (Panel B). Appendix Figure B.7 also shows that these differences are common knowledge: such perceptions are equally shared by villagers (blue bars) and chiefs themselves (orange bars).

Concretely, within the context of our experiment, our hypotheses are as follows. The effects of the new signal on chiefs' and villagers' support towards *child marriage* should not depend on whether chiefs are the box/bracelet holders: we predict that public donation boxes should decrease support in all cases. Conversely, the effects of the new signal on chiefs' and villagers' support towards *sexual initiation rituals* should depend on whether chiefs are the box/bracelet holders: when they are, we predict that public donation boxes should decrease support; when they are not, we predict that public donation boxes should either decrease support towards the traditional practice *to a lesser extent*, or even *weakly increase it* – reflecting the trade-off between utility gains from enforcing cooperation at lower cost and the utility losses from losing control over traditions.

7.2 Empirical strategy and results

To test those hypotheses, we explore the randomization of box/bracelet holders in our RCT, as described in subsection 3.3.1.

Figure B.8 in the Appendix showcases the take-up of bracelets and the amount of maize collected within the experiment, conditional on whether the village chief or some other villager was in charge of the new signal. While the number of bracelets distributed was the same across conditions, other villagers collected somewhat less maize in the Bracelets&Box condition than in the Box condition. This pattern is likely explained by imperfect compliance with treatment assignment (subsection 3.4): whenever other villages were assigned to handle box and bracelets together, chiefs were way more likely to take over – apparently being much more lax about the donation requirement when distributing bracelets.

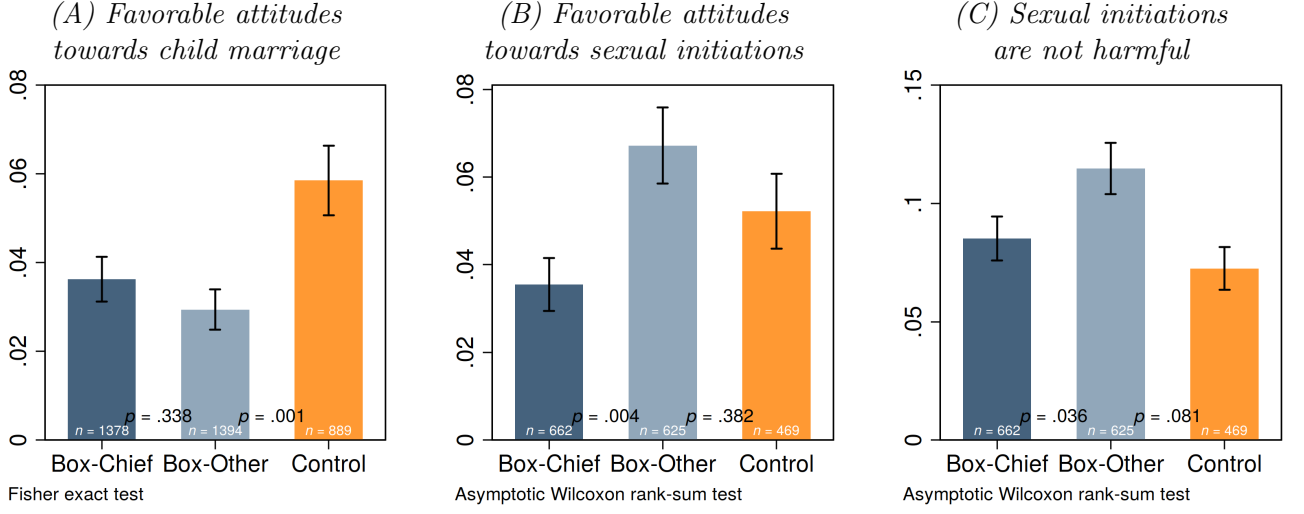
The fact that chiefs are less willing to comply with assignment when they are not in control of the most powerful new signal suggests they might indeed be concerned with losing power under that scenario, hinting at the trade-off we have introduced in the previous section. Does that mean that, ignoring selection into the treatment (by restricting attention to intention to treat), assigning other villagers as box/bracelet holders fails to change attitudes to the same extent – or even backfires? If so, does that only hold within sexual initiation rituals – a tradition from which village chiefs’ power at least partly stems from?

To answer those questions, we turn to households’ attitudes towards child marriage and initiation rituals, estimating how those are affected by who was in charge of the new signal. When it comes to ITT, the equation we estimate is as follows:

$$Y_{vhi} = \alpha_0 + \alpha_1 \text{Box-Chief}_v + \alpha_2 \text{Box-Other}_v + \alpha_3 X_{vhi} + \epsilon_{vhi}, \quad (7)$$

where Y_{vhi} is an indicator equal to 1 if individual i living in household h in village v states that the right age for a woman to marry is below 18 (support towards child marriage) / that sexual initiation rituals should be continued (support towards initiation); Box-Chief_v indicates whether village v was assigned to host a public donation box handled by the village chief; Box-Other_v indicates whether village v was assigned to host a public donation box handled by another villager; and X_{vhi} is a vector of individual-level controls. Standard errors are clustered at the village level.

Figure 9: Attitudes towards traditional practices of households with girls



Notes: Outcome variables defined according to Section 3.5.2. Means by treatment groups along with standard error bars and p-values from Fisher exact tests.

As before, to account for imperfect compliance, we also estimate an IV model. The equation we estimate is as follows:

$$1^{st} \text{ stages: } \text{Actual-Box-Chief}_v = \beta_0 + \beta_1 \text{Box-Chief}_v + \beta_2 \text{Box-Other}_v + \beta_3 X_{vhi} + \epsilon_{vhi} \quad (8)$$

$$\text{Actual-Box-Other}_v = \gamma_0 + \gamma_1 \text{Box-Chief}_v + \gamma_2 \text{Box-Other}_v + \gamma_3 X_{vhi} + \zeta_{vhi} \quad (9)$$

$$2^{nd} \text{ stage: } Y_{vhi} = \delta_0 + \delta_1 \overbrace{\text{Actual-Box-Chief}_{vhi}} + \delta_2 \overbrace{\text{Actual-Box-Other}_{vhi}} + \delta_3 X_{vhi} + \xi_{vhi}, \quad (10)$$

where $\overbrace{\text{Actual-Box-Chief}_{vhi}}$ and $\overbrace{\text{Actual-Box-Other}_{vhi}}$ are predicted assignments (at the individual-level, because the first stage includes individual-level controls) for whether village v hosts a public donation box handled by the village chief or by another villager, respectively.

Figure 9 illustrates the results, displaying in Panel A the share of households with favorable attitudes towards child marriage, in Panel B, the share of households with favorable attitudes towards sexual initiation rituals and, in Panel C, the share of households stating that initiation rituals are *not harmful*. All panels focus on households *at-risk* – those with girls at the relevant age for each tradition.²¹

²¹Figure B.9 in the Appendix showcases results for the full sample.

We find that support for child marriage is significantly lower in villages with public donations, regardless of whether the chief is control or not. In line with our hypotheses, chiefs prefer a lower-cost norm to enforce cooperation to the extent that it does not affect his/her ability to extract rents. In contrast, we observe significant differences in support for sexual initiation rituals depending on who is in charge of the new signal. Also consistent with our hypotheses, support decreases significantly more if the chief is the box/bracelet holder: in Panels B and C, support for the traditional practice is way lower (by almost 50% in Panel B, significant at the 1% level, and by about 25% in Panel C, significant at the 5% level) in those villages compared to those where another villager is the box/bracelet holder. Strikingly, in Panel C, it is also the case that targeting other villagers *backfires*: support for the traditional practice in villages where another villager handled the new signal is even *higher* than in control villages (by nearly 100%, significant at the 10% level). These patterns are consistent with chiefs extracting power from their influence over sexual initiation rituals, and with the trade-off between chief's private gains, on the one hand, and social gains, on the other, overturning the effects of the new signal on equilibrium selection.

Table B.16 in the Appendix presents the graphical results in regression form. IV and ITT estimates are very similar across all specifications. For child marriage, public donation boxes have the same effect regardless of who is in charge, reducing support by 2 percentage points (roughly 30% compared to the control group mean). Regarding initiation rituals, Columns (7)-(8) and (11)-(12) confirm that support for initiation rituals is significantly lower when the chief is the box/bracelet holder, compared to villages where another villager was in charge. Those differences only exist for households at-risk, consistent with those being specifically targeted by the village chief.

7.3 Chiefs' attitudes and desire for control

While we have claimed that villagers' attitudes should reflect those of village chiefs, as the latter are "prominent" agents in the spirit of Acemoglu and Jackson (2014), there are alternative explanations for why results might look this way. First, it could be that chiefs do not care about whether they are in charge or not, but that the credibility of the new signal depends on the extent to which the one handling it is influential in this domain. While the effects on support for child marriage seem to rule out this explanation, it could still be the case that child marriage and

initiation rituals are different in many other ways. To address this concern, in this subsection we elicit chiefs' attitudes directly, and evaluate how they respond to treatment assignment.

Second, even if chiefs' attitudes change in the same way as villagers', it could be that normative statements about behavior emerge bottom-up instead of top-down in those societies, such that it is chiefs' attitudes that mirror those of villagers', and not the other way around. To address this concern, we resort to a survey experiment. Motivated by prior literature that has found a close relationship between self-esteem and desire for control (Burger, 1995; Kernis, 1995), we prime chiefs on their self-esteem to assess whether their support for sexual initiation rituals increases – consistent with this mechanism – and, most importantly, whether it does so differentially depending on whether chiefs' were in charge of the new signal.^{22,23}

We first analyze if chiefs' attitudes towards child marriage and initiation rituals depend on who is in charge of the new signal. Figure B.10 in the Appendix shows, in Panel A, the share of chiefs who support child marriage and, in Panel B, that who support sexual initiation rituals. In line with our findings for households at-risk, chiefs support for child marriage is indistinguishable across conditions with different box/bracelet holders. Even though the Figure makes it seem like chiefs' support for those practices increases with the intervention, effects on their attitudes towards child marriage are very imprecisely estimated because of the extremely low share of chiefs who support the tradition; in fact, support is statistically identical across all conditions. In contrast, chiefs displays significantly lower support for sexual initiation rituals if they are the box/bracelet holders than if other villagers are in charge of the new signal (by about 50%, significant at the 5% level). What is more, support in the latter group is even higher than in the control group, although the small sample size prevents results from being more precisely estimated ($p = 0.175$). Corresponding regression results are reported in Table B.17 in the Appendix. All in all, villagers' attitudes match those of chiefs, particularly when it comes to sexual initiation rituals – the domain over which the latter exert influence.

Are mirroring attitudes a result of top-down normative prescriptions, as we claim? Or, instead, of chiefs mimicking bottom-up attitudinal changes? To tackle that question, we turn to the self-esteem priming, in order to uncover the relationship between chiefs attitudes and desire for control. We randomly assigned half the chiefs to a self-esteem condition, in which they are

²²We did not pre-register this hypothesis as, ex-ante, we were primarily interested in testing the effects of self-esteem on the substitutability between public and self-image, as in Bursztyn et al. (2018).

²³We cannot do the same analysis for child marriage because self-esteem priming was only undertaken at a later point in the survey, before we elicited support for sexual initiation rituals.

asked “*Can you please describe a specific incident in your life, something you did or achieved, that made you feel successful or proud of yourself? It could be from any aspect of your life, whether family related, education, or something related to your work.*”. In the control group, chiefs are asked “*Can you please describe your favorite dish/food and tell me what you particularly like about this dish?*”, which should not prime them with respect to either self-esteem or desire for control. Common answers from those assigned to the the self-esteem priming include *built a nice new house, got married and started a family, earned a lot of money, managed to buy a cow, invested in education, became chief, stopped drinking, and made peace in the village.*

We also undertake the same self-esteem priming with a sub-sample of villagers. We can use those as a placebo, to parse direct effects (if any) of self-esteem on attitudes towards sexual initiation rituals.²⁴

Figure B.11 in the Appendix shows, in Panel A, the results of the self-esteem priming on villagers’ attitudes and, in Panel B, those of the priming on chiefs’ attitudes. Villagers support for sexual initiation rituals is not affected by the priming; in particular, the effects of assignment to public donation boxes, conditional on the box/bracelet holder, are completely unaffected by it. In contrast, priming chiefs on self-esteem magnifies differences in support across conditions when the chief handles the new signal or not (differences in support scale by about 100%; in fact, such difference is only statistically significant within the primed sub-sample, despite the small sample size). Interestingly, chiefs’ support for sexual initiation rituals increase in all conditions *except when they are in charge of the new signal*. This suggests that chiefs indeed prefer the lower-cost signal, as long as societal gains from adopting the new signal do not impose a trade-off with respect to his/her personal gains from holding power. Section B.11.1 in the Appendix shows corresponding regression results, augmenting the ITT and IV estimates from the previous subsection with interactions of treatment assignment with the self-esteem priming.

Taken together, results are consistent with the hypotheses that village chiefs are prominent actors who set top-down normative behavioral prescriptions to other villagers, at least within some domains. They favor lower-cost mechanisms for enforcing cooperation in society as long as those do not threaten their ability to exercise power and, presumably, extract private rents.

²⁴Bursztyn et al. (2018) find that increasing self-esteem reduces individuals’ demand for status goods. This could be interpreted as a general reduction of social image concerns.

7.4 Long-term effects

We return to the villages one year after the intervention.

[RESULTS ARE FORTHCOMING]

8 Concluding Remarks

This paper provides first-hand evidence that individuals may engage in traditional practices that destroy children’s human capital, like child marriage and sexual initiation rituals, at least partly out of social image concerns. We document that individuals assign a more positive social image to those who comply with local norms, consistent with a separating equilibrium, whereby those who value future payoffs most undertake costly actions to ensure long-term cooperation in a trust-intensive society.

Incidentally, this finding might help explain why support for traditional practices (from child marriage to female genital cutting) seems to be decreasing at fast rates worldwide (see UNICEF’s Demographic Health Surveys). Rapid urbanization taking place in Africa and Southeast Asia may be leading to less trust-intensive societies, consistent with the claim that urban anonymity causes some institutions to break down (Glaeser, 2014) and in line with the historical trajectories of 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, the signaling value of harmful traditions decreases even in the absence of alternative signals.

Our findings for the effects of alternative signals have important 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 attractive. Having said that, we show that caution must be exercised in making sure local elites are engaged in social norms’ change, particularly when they exercise control over standing traditions, at the risk that the intervention backfires – rather strengthening harmful practices.

Our results shed light on important evidence gaps, but leave several open questions. What are minimal sufficient conditions for a new signal to break down a standing separating equi-

librium? Could other *positive* behaviors be unintentionally substituted by the a signal? In particular, under a pooling equilibrium, can cooperation break down, decreasing overall welfare? Are there trade-offs between short- and long-term outcomes as local elite’s power is strengthened or weakened by social norms’ change? These are promising avenues for future research.

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Appendix A 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.

$$\sum_{t=0}^{\infty} \beta_L^t u(d) > u(c + \zeta) - S + \sum_{t=1}^{\infty} \beta_L^t u(d) \Leftrightarrow S > u(c + \zeta) - u(d) \quad (11)$$

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:

$$\begin{aligned} \sum_{t=0}^{\infty} \beta_H^t u(c) - S > p \left[\sum_{t=0}^{\infty} \beta_H^t u(c) \right] + (1-p) \left[u(d - \eta) + \sum_{t=1}^{\infty} \beta_H^t u(d) \right] \\ \Leftrightarrow S < (1-p) \left\{ \left[\sum_{t=0}^{\infty} \beta_H^t u(c) \right] - \left[u(d - \eta) + \sum_{t=1}^{\infty} \beta_H^t u(d) \right] \right\}. \end{aligned} \quad (12)$$

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.

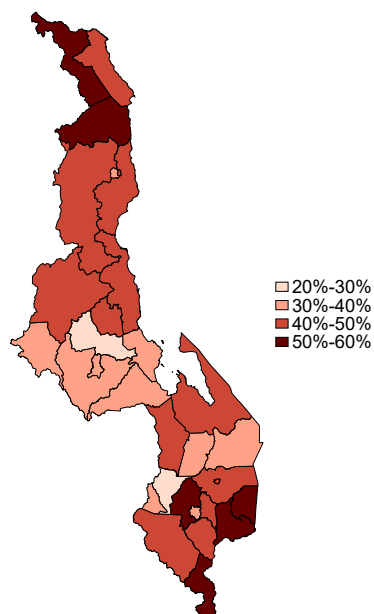
Appendix B Tables and figures

B.1 Prevalence of child marriage and sexual initiation rituals in Malawi

Figure B.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 < 18*



Panel B: Prevalence of Sexual Initiation Rituals

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

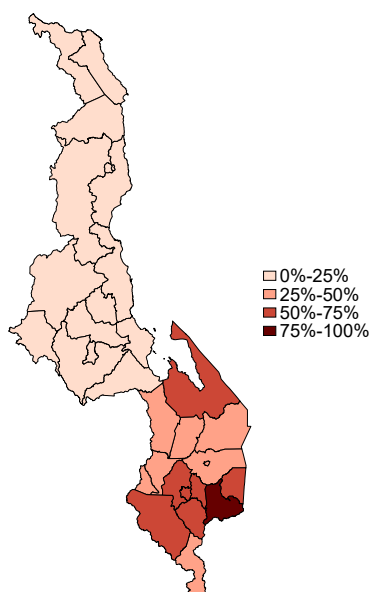
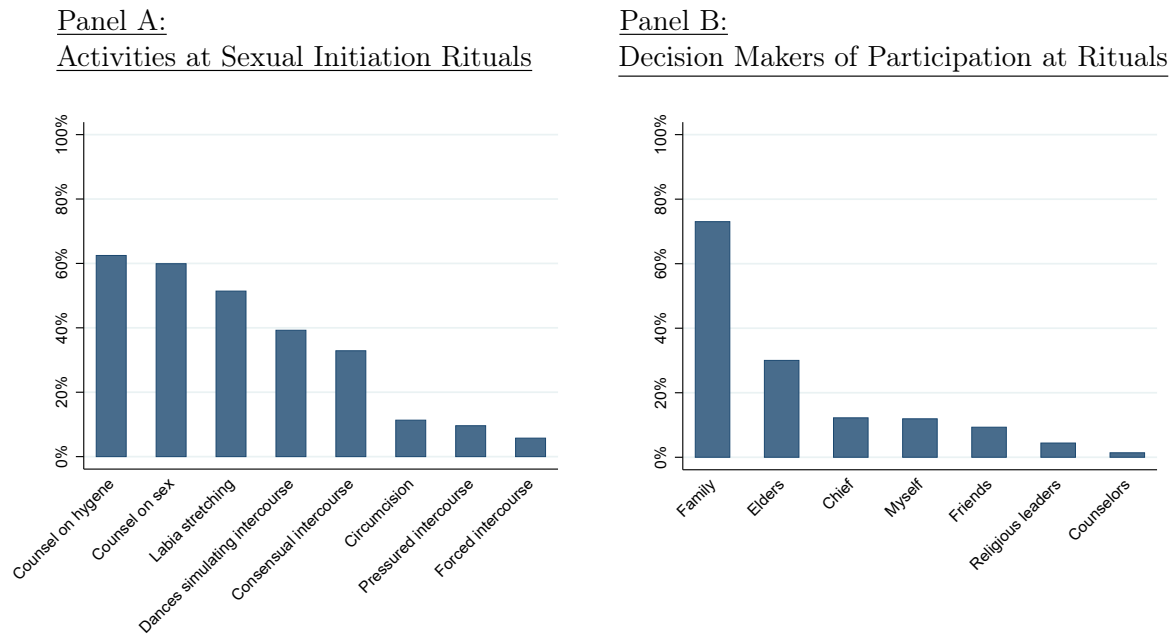


Figure B.2: Activities and decision makers of sexual initiation rituals of girls in Malawi. Share of respondents mentioning each category in multiple-choice questions.



B.2 Summary statistics

Table B.1: Summary Statistics

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

B.3 Design of survey experiment

Figure B.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.



B.4 Compliance

Table B.2: Compliance with signaling intervention

	Overall (100%)	Box/bracelet holder	
		Chief (50%)	Other (50%)
Box & Bracelets	89%	63%	37%
Box	92%	45%	55%
Bracelets	87%	46%	54%
Control	96%	50%	50%

B.5 Balance checks

Table B.3: Balance of covariates (framing of John)

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 (m)	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)
Observations	3,468	3,510	6,978

Means with standard deviations in parentheses.

Table B.4: Balance of covariates (public signaling intervention)

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

Means with standard deviations in parentheses.

Table B.5: Balance of covariates (box/bracelet holders)

Variable	(1) Box-chief	(2) Box-other	(3) Control	(4) Placebo-brac.
Urban	0.191 (0.393)	0.190 (0.392)	0.185 (0.388)	0.187 (0.390)
Village size	114.123 (29.628)	118.027 (32.489)	116.593 (30.468)	115.619 (32.133)
Number of surveyed HH	18.613 (1.503)	18.352 (1.538)	18.160 (2.133)	18.438 (1.854)
Distance to neighbor (km)	0.023 (0.016)	0.024 (0.017)	0.021 (0.016)	0.025 (0.022)
Female	0.579 (0.494)	0.597 (0.490)	0.589 (0.492)	0.568 (0.495)
Age	37.395 (16.185)	37.940 (16.184)	37.721 (16.010)	37.888 (16.698)
Weekly \$ food spending	10.997 (8.580)	11.443 (8.612)	11.748 (8.554)	11.230 (8.421)
Weekly \$ non-food spending	6.914 (7.131)	7.681 (7.626)	7.060 (7.046)	7.420 (7.376)
Household size	5.152 (2.162)	5.068 (2.019)	5.098 (2.121)	4.969 (1.967)
Observations	5,148	5,100	3,722	3,802

Means with standard deviations in parentheses.

Table B.6: Balance of covariates (self-esteem priming of households with girls)

Variable	(1) SE=0	(2) SE=1	(3) Diff.
Urban	0.164 (0.371)	0.172 (0.378)	0.008 (0.020)
Village size	114.294 (29.648)	116.457 (30.163)	2.163 (1.385)
Number of surveyed HH	18.506 (1.616)	18.411 (1.667)	-0.095 (0.087)
Distance to neighbor (km)	0.023 (0.015)	0.023 (0.016)	0.000 (0.001)
Female	0.636 (0.482)	0.601 (0.490)	-0.034 (0.025)
Age	35.260 (16.229)	34.380 (16.398)	-0.879 (0.736)
Weekly \$ food spending	10.459 (8.292)	11.774 (9.198)	1.316 (0.616)**
Weekly \$ non-food spending	6.511 (6.873)	7.513 (7.875)	1.002 (0.482)**
Household size	5.932 (1.948)	6.142 (2.137)	0.210 (0.130)
Observations	755	878	1,633

Means with standard deviations in parentheses.

Table B.7: Balance of covariates (self-esteem priming of chiefs)

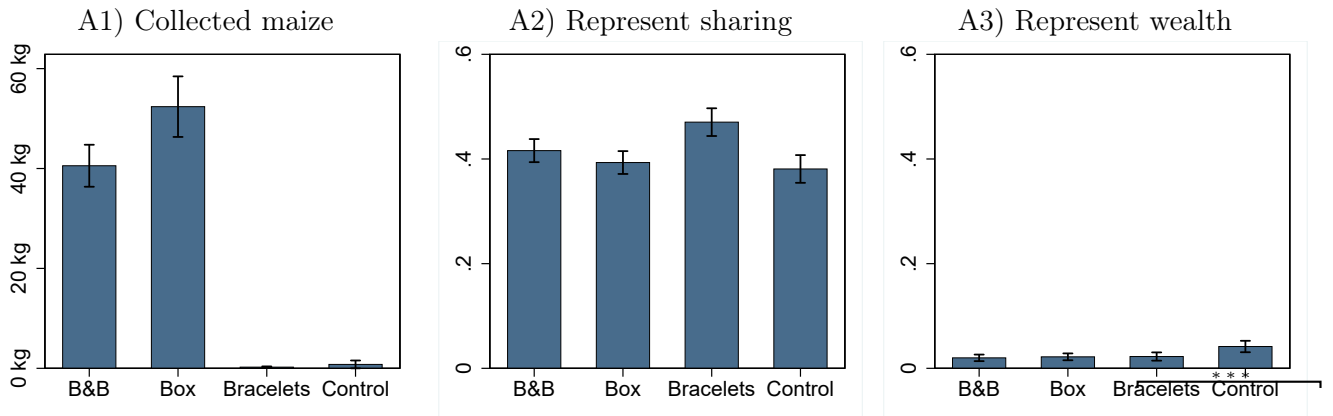
Variable	(1) SE=0	(2) SE=1	(3) Diff.
Urban	0.170 (0.377)	0.199 (0.400)	0.029 (0.040)
Village size	117.357 (33.290)	114.369 (29.600)	-2.988 (3.240)
Number of surveyed HH	18.319 (1.897)	18.276 (1.822)	-0.043 (0.191)
Distance to neighbor	0.024 (0.020)	0.022 (0.015)	-0.002 (0.002)
Female	0.159 (0.367)	0.138 (0.346)	-0.022 (0.037)
Age	56.559 (14.335)	56.850 (13.015)	0.290 (1.422)
Weekly \$ food spending	13.190 (9.941)	13.385 (9.915)	0.195 (1.022)
Weekly \$ non-food spending	8.538 (8.905)	8.485 (8.906)	-0.054 (0.917)
Household size	5.291 (2.266)	5.403 (2.632)	0.112 (0.254)
Observations	182	196	378

Means with standard deviations in parentheses.

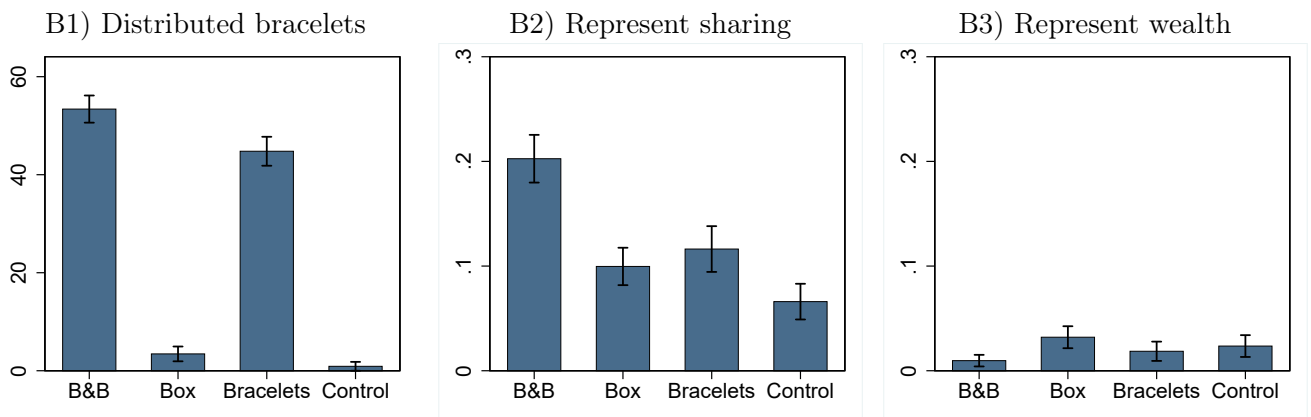
B.6 Manipulation checks

Figure B.4: Manipulation checks

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*.

B.7 Take-up of new signal

Table B.8: Characteristics of Individuals by Donation Status

Variable	Control			Box		
	(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 < 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	1.923 (0.267)	1.934 (0.249)	0.011 (0.017)	1.934 (0.249)	1.933 (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.9: Joint Distribution of Signaling Strategies

	Control			Box		
	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

B.8 Separating or pooling

Table B.10: Individuals' self-reported altruism, positive reciprocity, and trust

Dependent variable:	(1)	(2)	(3)
Self reported pro-sociality	Altruism	Reciprocity	Trust
Box	-0.178 (0.246)	0.0175 (0.217)	-0.0153 (0.223)
Mean dependent variable (Control)	7.559	7.931	3.518
Chi ² -test Box jointly=0, (p-val.)		0.686	
Village chief controls	✓	✓	✓
Village controls	✓	✓	✓
Observations	787	787	787
Clusters	296	296	296

Notes: Regressions additionally include village chief controls (female, age, age², and age³), village-level controls (village size, population density, urban), plus a constant. Standard errors, clustered at the village level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

B.9 Replacement of social expectations

Table B.11: 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
Clusters	412	412	412	412

Notes: 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 B.12: Predetermination of Child Marriage Norm in Village

Dependent variable:	Share married < 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
Clusters	412

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

Table B.13: Replacement of Social Expectations

Dependent variable:	(1)	(2)	(3)	(4)
Perceived reputation of John (joint measure)	Box&Bracelets	Box	Bracelets	Control
John supports child marriage \times Share married < 15 \times Bracelet	-2.469*** (0.835)	-0.847 (0.808)	1.180 (0.714)	0.253 (0.562)
John supports child marriage \times Bracelet	0.270*** (0.0502)	0.0798** (0.0376)	0.0371 (0.0412)	0.0754* (0.0419)
Share married < 15 \times Bracelet	0.126 (0.538)	0.378 (0.621)	-0.702 (0.478)	0.0986 (0.477)
Bracelet	0.00995 (0.0283)	-0.0360 (0.0274)	0.0549* (0.0320)	-0.0228 (0.0318)
Observations	4,626	4,680	3,400	3,300
Clusters	117	118	88	89
Individual Fixed Effects	✓	✓	✓	✓

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

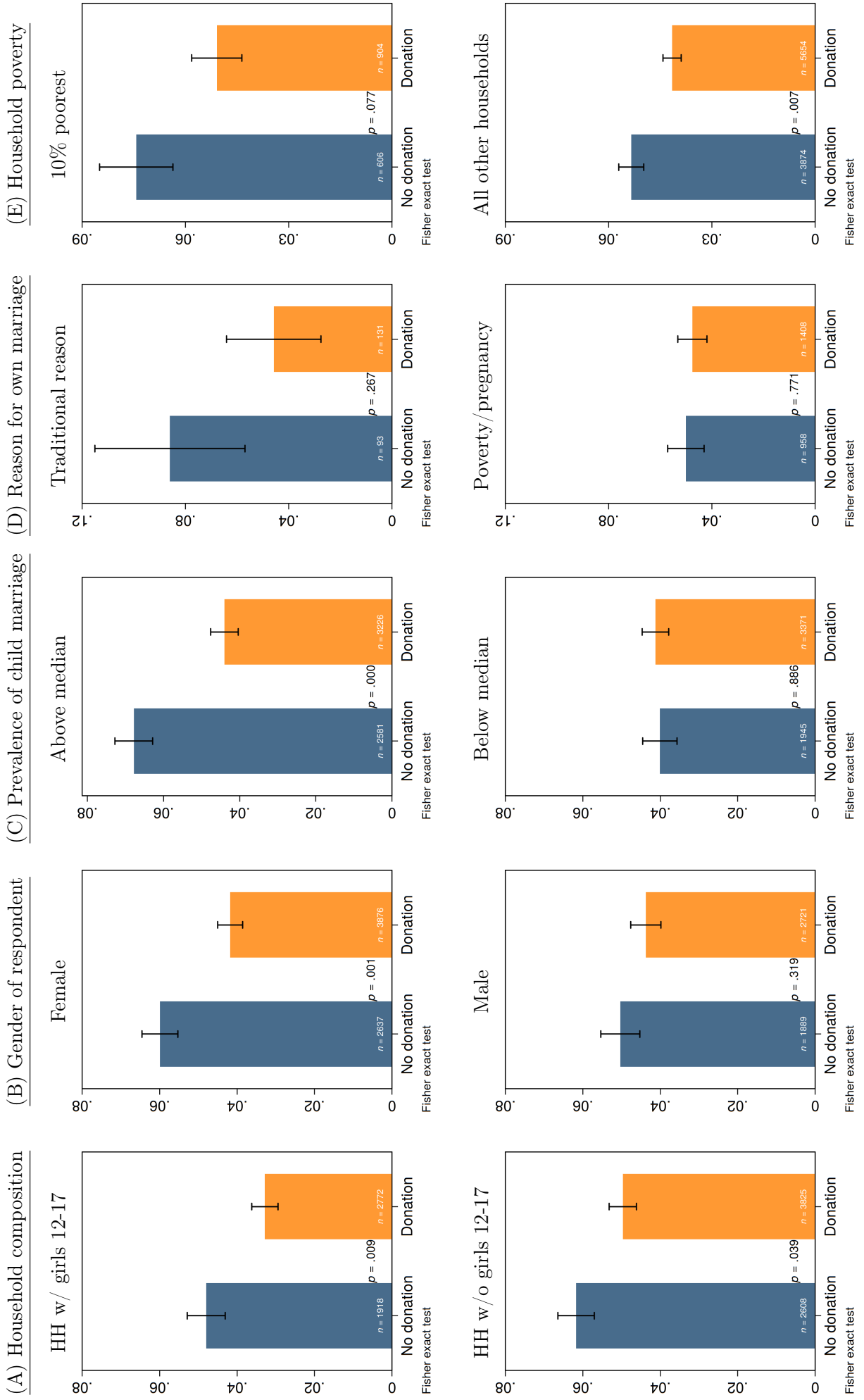
B.10 Effect of new signal on attitudes towards traditional practices

Table B.14: Effect of Signaling Intervention on Attitudes Towards Traditional Practices

	(1)	(2)	(3)	(4)
	Child Marriage	Child Marriage (IV)	Initiation	Initiation (IV)
Donation	-0.0140*** (0.00504)	-0.0161*** (0.00583)	-0.0136* (0.00820)	-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.)		0.0135		
IV: Chi ² -test Donation jointly=0, (p-val.)		0.0140		
Observations	11,123	11,123	7,243	7,243
Clusters	412	412	412	412
Individual controls	✓	✓	✓	✓
Village-level controls	✓	✓	✓	✓

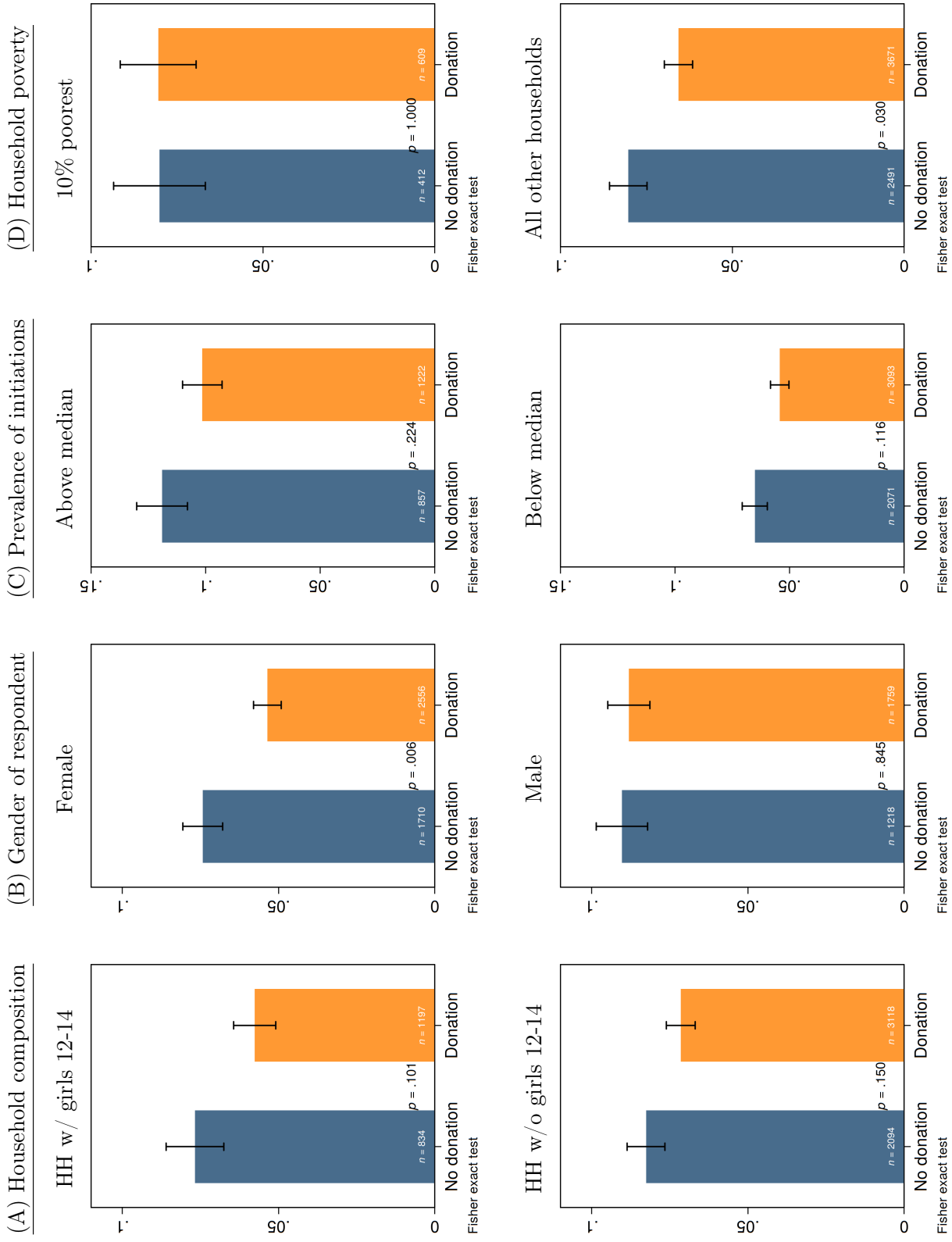
Notes: Regressions additionally include individual controls (female, age, age², and age³) and village-level controls (village size, population density, urban, and local prevalence of sexual initiation in columns (iii)-(iv)), plus a constant. Standard errors, clustered at the village level, in parentheses.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

Figure B.5: Effect of signaling intervention on attitudes towards child marriage (heterogeneities)



Notes: Outcome variables defined according to Section 3.5.2. Group means along with standard error bars and p-values from Fisher exact tests.

Figure B.6: Effect of signaling intervention on sexual initiation rituals (heterogeneities)



Notes: Outcome variables defined according to Section 3.5.2. Group means along with standard error bars and p-values from Fisher exact tests.

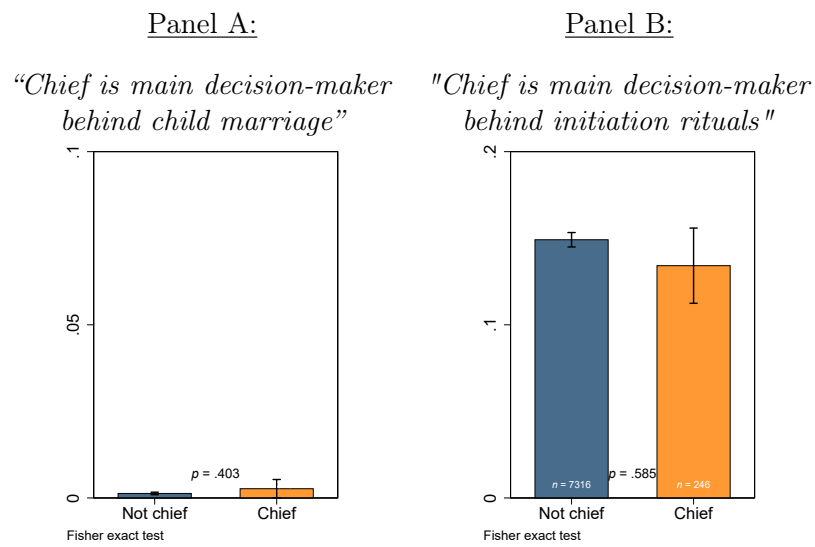
Table B.15: Effect of signaling intervention on attitudes towards traditional practices, heterogeneities (2nd stages from IV)

	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
Clusters	412	412	412	412	412	412	412	412	412
Additional 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

B.11 Do local elites matter?

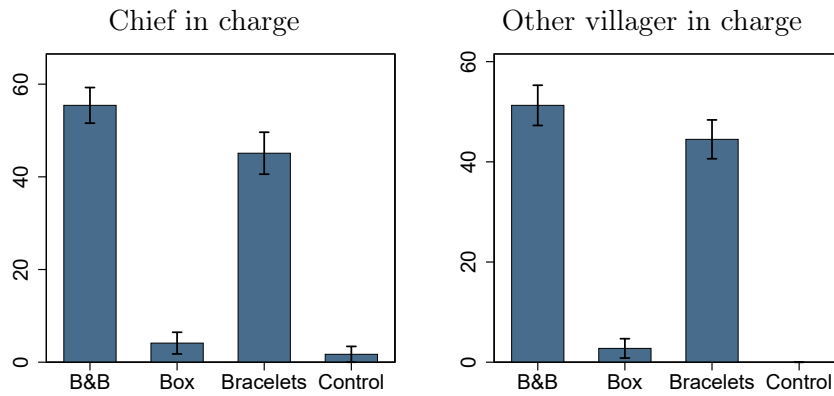
Figure B.7: Chiefs' domain of influence



Notes: Share of chiefs (orange bars) and other villagers (blue bars) stating that chiefs are main decision-makers when it comes to child marriage (Panel A) and sexual initiation rituals (Panel B). Means by treatment groups along with standard error bars and p-values from Fisher exact tests.

Figure B.8: Take-up of new signal by box/bracelet holder

Panel A: Number of bracelets distributed



Panel B: Kg of maize collected

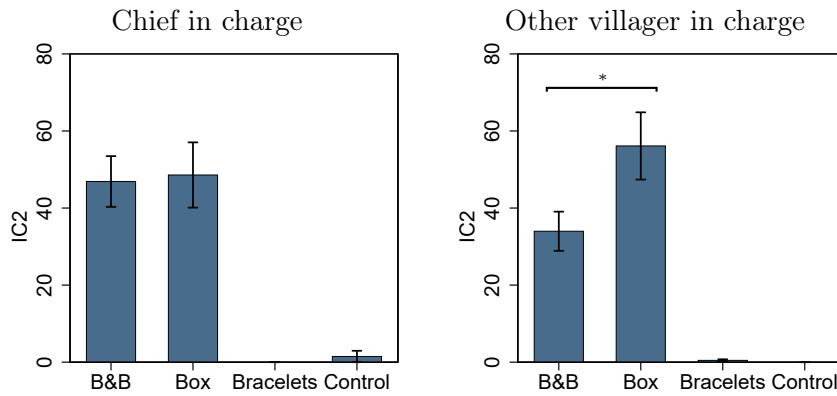
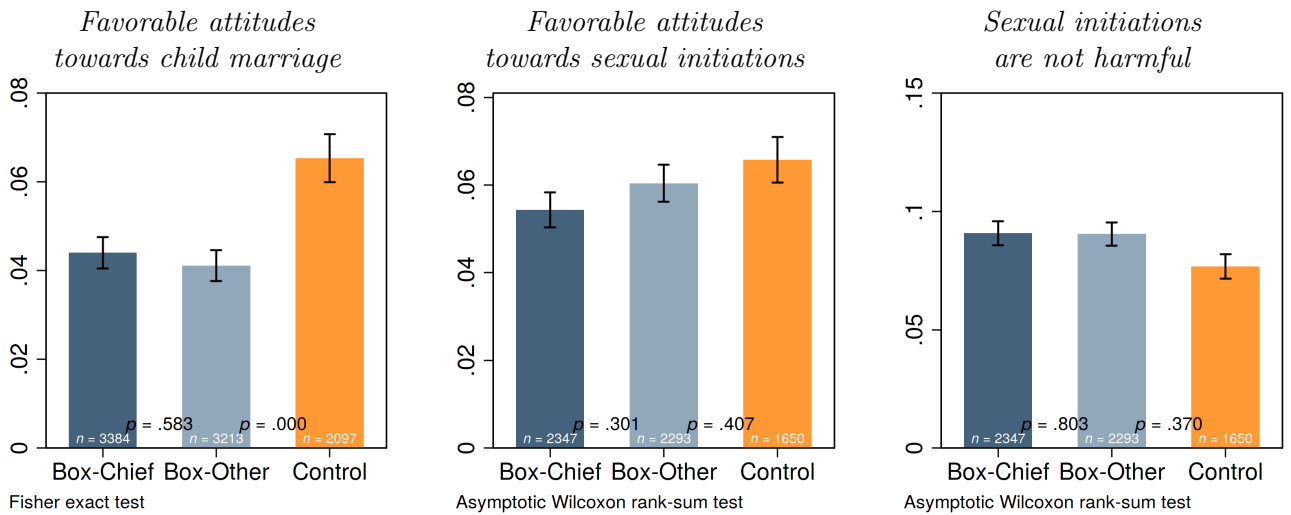
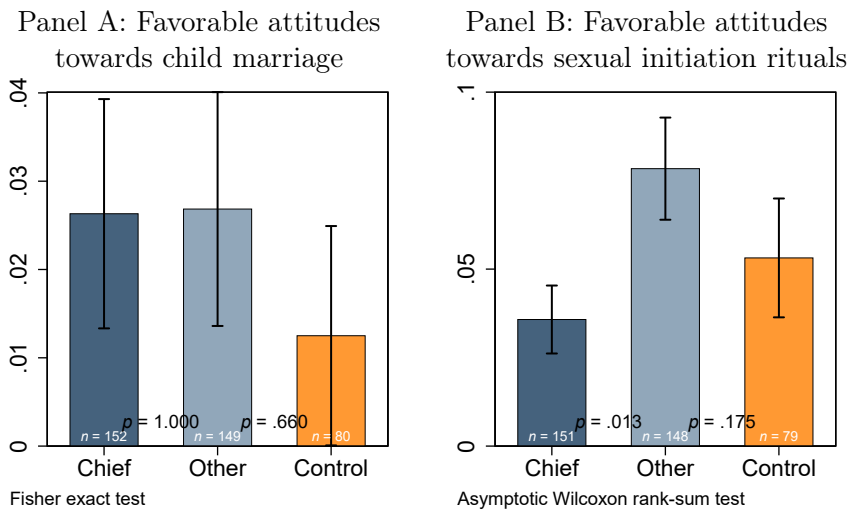


Figure B.9: Attitudes towards traditional practices of all households



Notes: Outcome variables defined according to Section 3.5.2. Means by treatment groups along with standard error bars and p-values from Fisher exact tests.

Figure B.10: Chiefs' attitudes towards traditional practices



Notes: Outcome variables defined according to Section 3.5.2. Means by treatment groups along with standard error bars and p-values from Fisher exact tests.

Table B.16: Households' favorable attitudes towards traditional practices

	Favorable attitudes towards child marriage			Favorable attitudes towards initiations			Initiation rituals are not harmful					
	All households	HHs with girls 12-17	All households	HHs with girls 12-14	All households	HHs with girls 12-14	All households	HHs with girls 12-14				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	ITT	IV	ITT	IV	ITT	IV	ITT	IV	ITT	IV	ITT	IV
Box-Chief	-0.0181** (0.00787)	-0.0186** (0.00897)	-0.0198** (0.00973)	-0.0195* (0.0112)	-0.0131 (0.0107)	-0.0157 (0.0124)	-0.0187 (0.0122)	-0.0267* (0.0142)	-0.0131 (0.0107)	0.0114 (0.0148)	-0.0187 (0.0122)	0.00373 (0.0197)
Box-Other	-0.0212*** (0.00797)	-0.0242** (0.00982)	-0.0247** (0.00975)	-0.0282** (0.0119)	-0.00594 (0.0108)	-0.00375 (0.0135)	0.0135 (0.0140)	0.0274 (0.0183)	-0.00594 (0.0108)	0.0172 (0.0169)	0.0135 (0.0140)	0.0604** (0.0257)
Test Chief=Other (p)	0.61	0.57	0.54	0.51	0.41	0.41	0.011	0.0097	0.41	0.79	0.011	0.083
Control mean	0.063	0.063	0.054	0.054	0.066	0.066	0.052	0.052	0.077	0.077	0.072	0.072
1 st -stage F-stat		62.9		56.5		61.0		51.8		61.0		51.8
Observations	8,892	8,892	3,770	3,770	5,861	5,861	1,633	1,633	5,861	5,861	1,633	1,633
R-squared	0.015	0.014	0.015	0.013	0.003	0.003	0.015	0.014	0.003	0.005	0.015	0.016
Individual controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Regressions additionally include individual controls (female, age) plus a constant. Standard errors clustered at the village level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.17: Regressions: chiefs' favorable attitudes

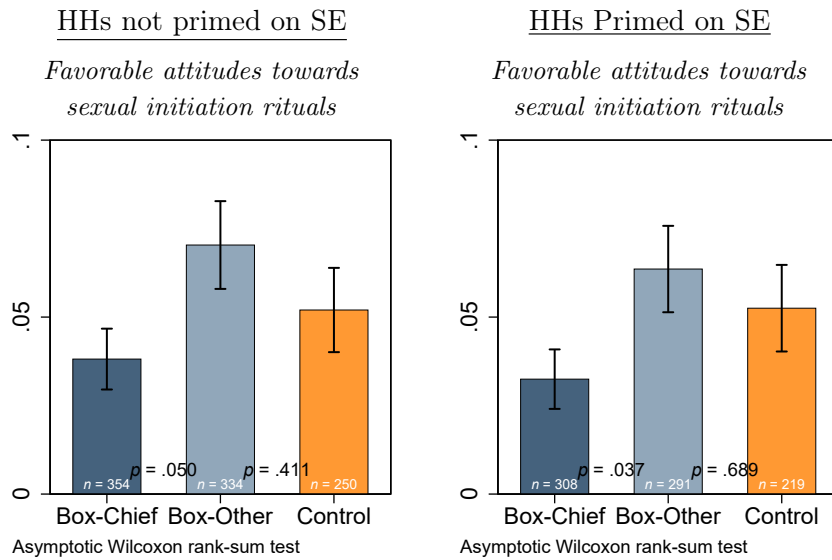
	Child marriage		Initiation rituals	
	(1) ITT	(2) IV	(3) ITT	(4) IV
Chief in charge	0.0137 (0.0185)	0.0144 (0.0216)	-0.0189 (0.0199)	-0.0276 (0.0220)
Other in charge	0.0144 (0.0184)	0.0160 (0.0233)	0.0211 (0.0224)	0.0350 (0.0276)
F-test Chief=Other (p-val.)	0.97	0.96	0.019	0.019
Control mean	0.013	0.013	0.053	0.053
1 st -stage F-stat		107.4		105.0
Observations	369	369	368	368
R-squared	0.006	0.006	0.015	-0.003
Individual controls	✓	✓	✓	✓

Regressions additionally include individual controls (female, age) plus a constant. Standard errors clustered at the village level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

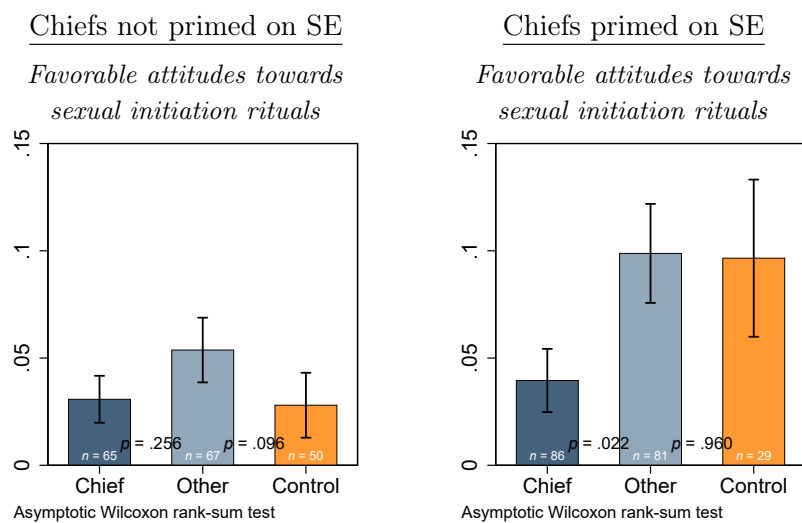
B.11.1 Self-esteem priming

Figure B.11: Self-esteem priming of households and chiefs

Panel A: Households with girls



Panel B: Chiefs



Notes: Outcome variables defined according to Section 3.5.2. Means by treatment groups along with standard error bars and p-values from Fisher exact tests.

Regression models of self-esteem priming

ITT:

$$Y_{iv} = \alpha_0 + \alpha_1 SE_i + \alpha_2 \text{Box-Chief}_v + \alpha_3 \text{Box-Chief}_v \times SE_i + \alpha_4 \text{Box-Other}_v + \alpha_5 \text{Box-Other}_v \times SE_i + \alpha_6 X_i + \epsilon_{iv}, \quad (13)$$

IV:

$$1^{st} \text{ stages for } Z_{iv}^a = \begin{bmatrix} \text{Actual-Box-Chief}_v \\ \text{Actual-Box-Chief}_v \times SE_i \\ \text{Actual-Box-Other}_v \\ \text{Actual-Box-Other}_v \times SE_i \end{bmatrix} :$$

$$Z_{iv}^a = \beta_0^a + \beta_1^a SE_i + \beta_2^a \text{Box-Chief}_v + \beta_3^a \text{Box-Chief}_v \times SE_i + \beta_4^a \text{Box-Other}_v + \beta_5^a \text{Box-Other}_v \times SE_i + \beta_6^a X_i + \zeta_{iv} \quad (14)$$

2nd stage:

$$Y_{iv} = \eta_0 + \eta_1 SE_i + \eta_2 \overline{\text{Actual-Box-Chief}_{iv}} + \eta_3 \overline{\text{Actual-Box-Other} \times SE_{iv}} + \eta_4 \overline{\text{Actual-Box-Other}_{iv}} + \eta_5 \overline{\text{Actual-Box-Other} \times SE_{iv}} + \eta_6 X_i + \xi_{iv} \quad (15)$$

Table B.18: Regressions: self-esteem priming

Dependent variable:	HHs with girls		Chiefs	
	(1)	(2)	(3)	(4)
Favorable attitudes towards initiations	ITT	IV	ITT	IV
SE	-0.00583 (0.0185)	-0.00722 (0.0193)	0.0694* (0.0397)	0.0699* (0.0409)
Chief in charge	-0.0264 (0.0167)	-0.0350* (0.0192)	-0.00193 (0.0188)	-0.00511 (0.0205)
Chief in charge × SE	0.0143 (0.0236)	0.0154 (0.0276)	-0.0540 (0.0436)	-0.0667 (0.0480)
Other in charge	0.00765 (0.0195)	0.0235 (0.0268)	0.0237 (0.0221)	0.0328 (0.0271)
Other in charge × SE	0.0107 (0.0275)	0.00708 (0.0367)	-0.0267 (0.0473)	-0.0180 (0.0567)
Test Chief=Other if SE=0 (p-val.)	0.035	0.040	0.16	0.16
Test Chief=Other if SE=1 (p-val.)	0.11	0.10	0.048	0.052
Control mean	0.053	0.053	0.028	0.028
1 st -stage F-stat		20.9		24.5
Observations	1,633	1,633	368	368
R-squared	0.015	0.013	0.035	0.008
Individual controls	✓	✓	✓	✓

Regressions additionally include individual controls (female, age) plus a constant. Standard errors clustered at the village level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix C Supplementary tables and figures

C.1 Effect of Signaling Intervention on Favorable Attitudes Towards Child Marriage/Sexual Initiation (Marginal Effects from Probit)

Table C.1: Effect of Signaling Intervention on Favorable Attitudes Towards Traditional Practices (Probit Marginal Effects)

	(1) Child Marriage	(2) Child Marriage (IV)	(3) Initiation	(4) Initiation (IV)
Donation	-0.0135*** (0.00487)	-0.0156*** (0.00570)	-0.0138* (0.00798)	-0.0159* (0.00926)
Control mean	0.054		0.077	
Chi ² -test Donation jointly=0, (p-value)			9.003 (0.0111)	
IV: Chi ² -test Donation jointly=0, (p-value)			8.883 (0.0118)	
Observations	11,123	11,123	7,243	7,243
Clusters	412	412	412	412
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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

C.2 Signaling Intervention (Placebo)

Table C.2: Effect of Signaling Intervention on Engagement in Traditional Practices (Placebo)

	(1) Child Marriage	(2) Child Marriage (IV)	(3) Initiation	(4) Initiation (IV)
Donation	-0.00940 (0.0110)	-0.0108 (0.0127)	-0.00105 (0.00159)	-0.00118 (0.00179)
Control mean	0.30		0.27	
Chi ² -test Donation jointly=0, (p-value)			1.229 (0.541)	
IV: Chi ² -test Donation jointly=0, (p-value)			1.227 (0.541)	
Observations	8,534	8,534	5,238	5,238
Clusters	412	412	412	412
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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

C.3 Effects of Signaling Intervention for Individual Treatment Arms

Table C.3: Effect of Signaling Intervention on Attitudes Towards Traditional Practices

	(1) Child Marriage	(2) Child Marriage (IV)	(3) Initiation	(4) Initiation (IV)
Bracelets	-0.0176** (0.00847)	-0.0180* (0.00939)	0.00259 (0.0138)	0.00431 (0.0153)
Box	-0.0216*** (0.00790)	-0.0233*** (0.00871)	-0.0107 (0.0133)	-0.0116 (0.0147)
Box & Bracelets	-0.0253*** (0.00792)	-0.0278*** (0.00874)	-0.0137 (0.0133)	-0.0156 (0.0147)
Control mean	0.063		0.075	
Observations	11,123	11,123	7,243	7,243
Clusters	412	412	412	412
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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$