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

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

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Abstract

In Malawi, only 5% of parents state that the right age for a woman to marry is below 18, but 42% of girls get married before they reach that legal age. We document that social image concerns are likely an important mechanism behind that wedge: where the prevalence of child marriage is high, those who *do not* marry off their under-age daughters are perceived as *less* altruistic, reciprocal and trustworthy than those who do. We then randomly assign 412 villages to a public donation drive, through which participants could donate maize to be redistributed to the poorest in their village. The idea is that *increasing the visibility* of charitable behavior – which also contributes to social image – might provide a less costly but as visible alternative to child marriage for parents who are only willing to engage in it out of social image concerns. One year after the intervention, we find that girls' marriages and teenage pregnancies *decrease by roughly 30%* in treated villages relative to the control group. Consistent with the social image mechanism, (1) charitable behavior increases the most in villages where child marriage was most prevalent at baseline, and (2) in those villages, parents who do not marry off their under-age daughters are *no longer* perceived as less pro-social than others. We rule out that child marriage is delayed merely because poor families have additional resources due to donations from the drive, and provide evidence that treatment effects increase with the visibility of the intervention. Our findings provide novel evidence on how far individuals might go to protect their social image, and inform new strategies to disrupt arguably inefficient norms when there is a wedge between private and social motives.

Keywords: Child Marriage; Social Norms; Social Image

JEL Classifications: D91, J12, Z10

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1 Introduction

The idea that social image concerns could convert private vices into social virtues has been extensively studied by economists, from tax compliance (e.g. Perez-Truglia and Troiano, 2018) to charitable giving (e.g. Montano-Campos and Perez-Truglia, 2019) to ensuring children get their full course of vaccines (e.g. Karing, 2018; Karing and Karim, 2018). In some instances, however, the opposite holds: social image concerns can lead private virtues to become social vices, from under-investment in one's own education (e.g. Bursztyn, Egorov and Jensen, 2019; Bursztyn, Fujiwara and Pallais, 2017) to barring women from participating in the labor force (e.g. Bursztyn, González and Yanagizawa-Drott, 2018). Along those lines, this paper documents a wedge between private and social motives when it comes to child marriage decisions in Malawi. While very few parents think that a woman should marry before the legal age of 18, more than 4 out of 10 girls marry before that age – a figure that has barely changed over the last 30 years.[1] We show that, in villages where child marriage is prevalent, one's social image is expected to suffer if he were not to marry off his under-age daughter. We then investigate whether manipulating the *production function* of social image can effectively delay girls' marriage decisions in those villages.

We start by documenting an association between households' participation in child marriage traditions and their social image. Concretely, we ask respondents across 412 villages in Malawi to rate villagers who had daughters *at risk* of child marriage in the year prior to the survey – those 17 years old or younger and unmarried at baseline – with respect to their altruism, reciprocity, and trustworthiness, drawing on Falk et al. (2016)'s validated measures.[2] Linking villagers' social image to whether they had recently married off an under-age daughter, we find that those who had *not* done so are perceived as significantly *less pro-social* than others in villages where the practice is high-prevalence. While that association could confound other characteristics of those who defy local norms, we show that social image and support for child marriage are indeed causally related with the help of a vignette experiment.[3]

If social image concerns are an important driver behind child marriage in those villages (as parents seem to privately oppose it), could girls' marriages be delayed by interventions that facilitate building social image in alternative ways? Social image concerns thrive on visibility; individuals cannot reward or punish what they cannot see (Bursztyn, Fujiwara and Pallais, 2017; Bursztyn, Egorov and Jensen, 2019; Bursztyn et al., 2018). While it would be hard to decrease the visibility of child marriage (typically a public ceremony), it is possible to make other socially rewarded behaviors more visible (e.g. Karing, 2018). As such, to study this question, we evaluate the impacts of a public donation drive, randomly assigned across 412 villages, through

[1] Over 650 million women alive today married before they turned 18 years old; see https://data.unicef.org/resources/child-marriage-latest-trends-and-future-prospects.

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

[3] In the vignette experiment, subjects are randomly assigned to a story that features either a Malawian father who supports child marriage, or an otherwise identical father who does not – holding constant observable characteristics such as housing conditions and family structure. We find that the father marries off his under-age daughter is perceived as less pro-social than his 'harmless' version only in villages where prevalence of child marriage is low. In turn, as the local prevalence of child marriage increases, the perceived altruism, reciprocity and trustworthiness of that father increase as well, eventually becoming higher than those of his 'harmless' version at the high-end of the distribution of child marriage.

which villagers could help community members most in need *under the public eye*. Although completely unrelated to child marriage, the idea is that the intervention might encourage some individuals who privately oppose child marriage but were willing to marry off their under-age daughters out of social image concerns to abandon the practice, once they realize they could protect their social image through a visible but less costly alternative.

In each treated village, a *box holder* is selected to coordinate donations, responsible for (i) letting other villagers know that the drive would collect maize - Malawi's staple crop - to be redistributed to the poorest local households, (ii) collecting two kilograms of maize per donor, and (iii) making sure the donation box was *publicly displayed*. While donations had always been available (in fact, 1/3 of villagers report they donate to others in the control group, outside of our experiment), the intervention is meant to make charitable behavior more visible. In treated villages, boxes were introduced during household listing for the baseline survey, which took place 5 weeks later; in control villages, only listing took place at the same time. The drive was meant to be self-organized: box holders were in charge of implementing it; in fact, they were informed that the research team would not come back to redistribute the maize collected through the drive nor to verify whether donations in fact reached the poorest in the village.[4]

We first investigate how the public drive affects charitable behavior in the short and the long runs. Within 5 weeks of the intervention, donations to other households significantly increase in treated villages relative to the control group (by about 26%). Such differences are persistent: 16 months later, while 33% of villagers had donated to others over the course of the previous year in the control group, nearly 55% had done so in the treatment group. Strikingly, the intervention *changes the profile* of those who donate. In control villages, only a minority of those who support child marriage donate to other households; in contrast, in treated villages, those who support child marriage donate to the same extent as those who oppose it. What is more, the higher the local prevalence of child marriage in the control group, the lower is the share of those married before 18 years old who donate; in the treatment group, this difference disappears, as donations sharply increase in high-prevalence villages. Altogether, the evidence suggests that child marriage and donations might indeed be used as alternative social signals.

In fact, 16 months after the intervention, we document that child marriage age-by-age is 1.7 p.p. lower in treated villages relative to the control group – a 30% decrease.[5] To get a sense of the magnitudes involved, if treatment effects persist over time, we estimate that, in control villages, 18% of girls will marry before age 15, and 42% before age 18, compared to only 12% and 34%, respectively, in the treatment group. In line with the evidence that child marriage is intimately linked to childbearing and school dropouts (Field and Ambrus, 2008), the expected prevalence of those outcomes is also significantly affected by the intervention: we estimate that, in control villages, 76% of girls will drop out of school and 42% will have children before 18 years old, compared to 70% and 31%, respectively, in the treatment group. Consistent with a link between social image and traditional practices at large, the effects of the intervention are not confined to child marriage: the expected share of girls who undergo sexual initiations rituals

[4]Box holders were randomly assigned: in 50% of treated villages, donations were assigned to be handled by the village chief; in the other 50%, they were handled by another villager (see Haenni and Lichand, 2021).

[5]Our experiment builds in a series of features to rule out that survey responses about girls' outcomes are driven by experiment demand effects; see Section 4.4.

by age 13 falls by almost 30% in treated villages relative to the control group.[6] In all cases, treatment effects are larger in villages where child marriage was high-prevalence at baseline.

We also access call logs from a local NGO's help lines through which villagers could report violations of girls' rights, including child marriage, to show that results are not an artifact of experimenter demand effects, with the help of independent data. Calls are *not* anonymous; as such, social image concerns definitely play a role when filing a report. Calls are also *consequential*: reports trigger responses outside the community, by the police or by the district's social welfare office. On the one hand, if the intervention decreases child marriage but does not affect reporting decisions, we would expect less calls in villages assigned to the public donation drive; on the other hand, if the intervention makes non-anonymous reporting more socially acceptable, then we might actually find *more* calls in those villages, relative to the control group. Consistent with the latter, we find that villages assigned to the public donation drive feature 11% more reports of child marriage than control villages immediately after the public donation drives. Effects do not decay over time, persisting 2.5 years into the intervention. Treatment effects on reporting behavior are strongly driven by regions where child marriage was high-prevalence at baseline; reports dramatically increase in those villages, by 250% relative to high-prevalence villages in the control group.

Are treatment effects grounded in the social image mechanism? To answer that, we revisit our end-line data on respondents' perceptions about other villagers, linking their perceived social image to whether they had recently married off an under-age daughter. In contrast to control villages, in the treatment group child marriage is no longer needed to protect social image where its baseline prevalence is high. It is insightful that those who defy the local norm are no longer penalized also in low-prevalence treated villages, relative to the control group, confirming that charitable behavior can help defiers act on their private motives. Because most parents privately oppose child marriage, though, the intervention does *not* significantly increase its prevalence in those villages. Such pattern corroborates that treatment effects on behavior arise from an interaction between this wedge – private vs. social motives – and the production function of social image.

Are the effects on child marriage merely the outcome of the intervention's interference with its relative contribution to social image? Or, perhaps to a large extent, do girls delay marriage because of redistribution – as donations help poor families resist the need to sell-off their daughters in exchange for bride prices (Corno and Voena, 2016) –, with subsequent general equilibrium effects on marriage market dynamics? We answer this question in two ways. First, we show that treatment effects are not concentrated in the poorest households in the village. As redistribution in treated villages was in any case relatively small (the drive collected, on average, 60kg of maize, to be redistributed across many poor villagers), this is perhaps not surprising: donations might not have been enough to prevent poor families from marrying under-age daughters in response to pressing needs. Second, we introduce additional arms in our experimental design, cross-randomizing villages to host the public donation drive (or not) and to host the distribution of red rubber bracelets (or not). In villages assigned to hosting both the box and

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

bracelets, the latter are distributed in exchange for two kilograms of maize; in those assigned to host the public donation box only, donors get nothing in exchange for their donations.[7] With the help of the vignette experiment, we document that, as intended, bracelets only contribute to social image in villages were they stand for donations. We find that further increasing the visibility of donations with the help of bracelets magnifies the effects of the public donation drive on girls' outcomes, 16 months after the intervention. These two tests rule out redistribution as the main mechanism behind our results, especially since villages assigned to give away bracelets in exchange for donations actually collected *less* maize for redistribution than other villages.

If a simple donation drive has such large effects on an entrenched tradition privately opposed by most villagers, why is it the case that it was not put in place by villagers themselves? To answer that question, we elicit participants' willingness to pay (WTP) for the donation drive, at the end of the study. WTP increases sharply with prior exposure to treatment in villages where child marriage was high-prevalence at baseline – precisely where treatment effects are the largest. This result suggests that the coordination problem stems from villagers' inability to anticipate that a public donation drive would unravel long-standing social norms before our study.

The main contribution of this paper is two-fold. First, we show that parents might go to incredible heights to *protect their social image* – even when that means destroying their children's human capital in the process. Second, we demonstrate that parents might readily abandon even long-standing traditions when their *relative contribution to social image changes*. This paper adds to a scarce literature documenting that social signaling – linked to beliefs about what others expect one to do – can help explain why certain norms might *inefficiently persist* even when individuals dislike it, face private costs from conforming to it, and do not have biased beliefs.[8,9] While Bursztyn, Fujiwara and Pallais (2017) and Bursztyn, Egorov and Jensen (2019) also provide evidence for that mechanism in different settings, our study not only documents a connection between the arguably inefficient behavior and social signaling, but also evaluates the extent to which an intervention that disrupts this connection can effectively change behavior.

Moreover, our results shed light on why long-standing norms may change rapidly in some settings, crowded out by the emergence of alternative signals of social image.[10,11] They also inform

[7] In villages assigned to bracelets only, those are made available for sale, for a price equivalent to two kilograms of maize. In pure control villages, neither public donations were organized nor bracelets distributed or sold.

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

[9] Other research on the mechanisms behind conformity to inefficient norms focuses on preferences (Kearney and Levine, 2015; Vogt et al., 2016), incentives (Buchmann et al., 2019; Corno, Hildebrandt and Voena, 2020; Corno and Voena, 2016; Vogt et al., 2016; Ashraf et al., 2020) and beliefs about prevalence or higher-order beliefs (Campante and Yanagizawa-Drott, 2015; Bursztyn, González and Yanagizawa-Drott, 2018; Perez-Truglia and Troiano, 2018; Butera et al., 2019).

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

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

new strategies to disrupt inefficient norms: when there is a wedge between private and social motives, increasing the visibility of alternative, more efficient strategies to build social image, may well undermine other inefficient outcomes – from corruption (Tirole, 1996) to inequality (e.g. driven by discrimination, Alesina, Giuliano and Nunn, 2013; Teso, 2019; Fernández, Parsa and Viarengo, 2019) to other norms that destroy children's human capital (e.g. child labor, Basu, 2018, and female genital mutilation, Vogt et al., 2016).

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

2 Social norms and social image in Malawian villages

2.1 Traditional practices in Malawi

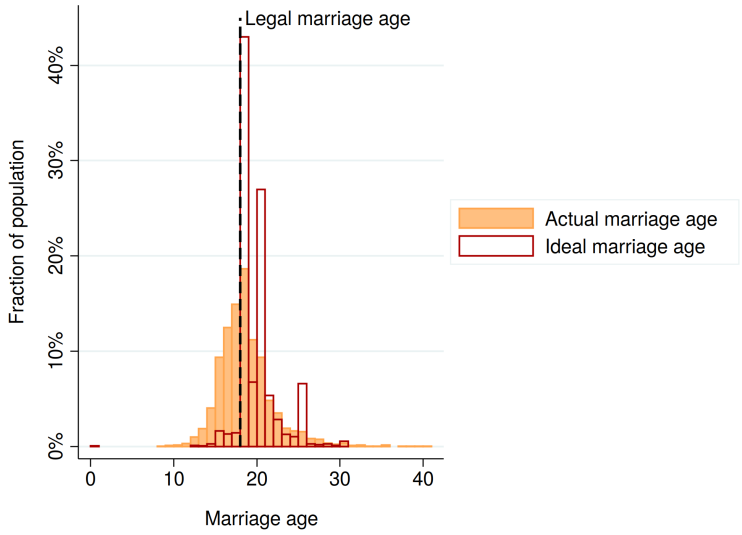
Malawi has a diverse cultural heritage with numerous common traditional practices. In this paper we focus primarily on child marriage, but also provide evidence on other traditional practices that potentially destroy children's human capital, such as sexual initiation rituals.

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

Interestingly, there is a sharp divide between attitudes towards child marriage and its prevalence. Figure 1 shows the distribution of the age at first marriage alongside that of the "ideal age" of marriage for girls, reported by our survey respondents (see Section 3.3). While marriage before age 15 is relatively rare, marriages between 15 and 18 years old are very common. While the distributions of ideal age of marriage and that of actual marriage both have 18 as the modal answer, reported ideal ages below 18 are rare (around 5%) – despite the high prevalence of actual marriages before that age.

Such differences could be explained by multiple factors, including changes in the social norm over time (such that several of those who got married as children no longer support the practice), reporting biases (from social desirability bias to cognitive dissonance), and factors that influence marriage decisions above and beyond individual attitudes (marriage market dynamics, financial incentives linked to bride prices, peer pressure, or beliefs about what others do or expect them to do). This paper tests the hypotheses that this gap at least partly captures social image

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



Notes: Distribution of age at first marriage among female respondents in the baseline survey, along with the distribution of their stated ideal age of marriage for women. In 2017, Malawi determined marriages before 18 years old to be illegal.

concerns, which drive a wedge between private and social motives when it comes to girls' marriage decisions.

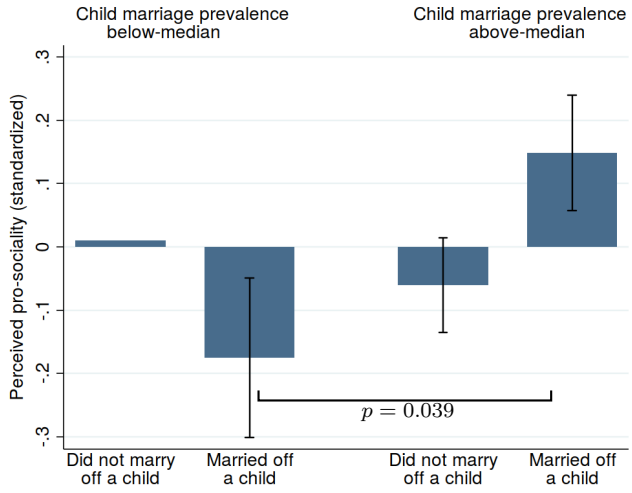
In Malawi, sexual initiation rituals are also particularly relevant for girls. Panel B in Figure A.1 displays its prevalence across Malawian districts. Sexual initiation rituals are mostly concentrated on the populous Southern districts. Figure A.2 displays descriptive evidence on the typical activities conducted as part of initiation rituals in Panel A, and on the most important decision-makers behind participation in those practices in Panel B (as stated by survey respondents; see Section 3.3). Activities range from counseling to different forms of female genital mutilation and forced sexual intercourse. Girls seldom decide themselves to participate in these rituals, but are mostly required to attend by their families or other decision-makers in the village.

2.2 Conformity to local norms vs. social image

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

Irrespective of how those traditions came to be, the question that remains is why they still persist today. After all, there is consistent evidence that early marriages bring about long-term costs for girls (from a higher likelihood of dropping out of school to high-risk teenage pregnancies; Field and Ambrus, 2008) and, as shown, parents seem to privately oppose it. We provide first-hand evidence that persistence is at least partly driven by social image concerns, documenting

Figure 2: Villagers' social image as a function of their engagement in child marriage and prevalence of child marriage in the village



Notes: Villagers' pro-sociality as perceived by the survey respondent in villages where the prevalence of child marriage was low vs high at baseline, according to a median split. The pro-sociality measure is an equally weighted, standardized combination of individual measures for altruism, reciprocity, and trust (see Section 3.3.1). Estimates are from an ordinary least squares regression, including village-level controls (village size, population density, and urban) and district and enumerator fixed-effects. Bars stand for standard errors clustered at the village level. P-values from Wald tests for equality of estimated coefficients.

that the decision to conform or not to local child marriage traditions in Malawian villages is linked to how one is perceived in their community.

Concretely, we ask survey respondents across 412 villages to rate villagers with respect to their altruism, reciprocity, and trustworthiness, drawing on Falk et al. (2016)'s validated measures. We choose to elicit different measures of pro-social behavior because pro-sociality is an important component of social image in general – and, in particular, within a trust-intensive society like Malawi, where transactions often cannot be enforced by formal courts. We then link these perceptions of social image to households' recent participation in the child marriage tradition. To study the correlation between participation in child marriage and social image, we compute a summary measure of social image that averages across its standardized components (Kling, Liebman and Katz, 2007) to deal with family-wise error rates across multiple hypotheses' testing.

Figure 2 shows the correlation between participation in child marriage and our summary measure of social image, separately for villages where under-18 marriage is below-median according to our baseline survey, on the left-hand side, and for those above median, on the right-hand side. While households who married off a girl before she turned 18 are perceived as less pro-social than others in villages where child marriage is relatively rare, the opposite is true within villages where it is more prevalent: in these villages, those who support child marriage are perceived as 0.2 s.d. *more pro-social* than those who do not support child marriage. This correlation is large, roughly half the size of the correlation between the summary measure of social image and charitable giving (Table B.7). The average difference in the social image of those who participated in child marriage in low- vs. high-prevalence villages is statistically significant at the 5% level.

2.2.1 Vignette experiment

The analysis above indicates that complying with local child marriage traditions is strongly correlated with social image. Naturally, conformity to child marriage is not randomly assigned; inference about households' social image could be based on other individual characteristics that correlate with support for local traditions, such as wealth or education.

This subsection provides supporting evidence that such association is causal with the help of a vignette experiment, which randomly assigns support towards child marriage. In the experiment, households are presented with a portrait of a Malawian family, accompanied by their background story, and are then asked to rate the Malawian father featured in the vignette along the same dimensions of social image introduced in the previous section. Participants are randomly assigned to one out of two versions of the vignette. In the *treatment* version, the vignette depicts a Malawian father who supports child marriage: the top left picture in Figure A.3 is displayed, accompanied by the following description:

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

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

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

While marriage before 15 years old is relatively rare in Malawi (its national prevalence is around 10%), focusing on this extreme version of the practice has the advantage of allowing for cleaner spatial heterogeneity analysis: according to our baseline survey, it is completely absent from 38% of the villages in our sample, while it reaches 41% in villages at the upper-half of the distribution.

The design of the vignette experiment is summarized in Figure 3. Randomization is undertaken at the household level within each village. Whenever multiple subjects were interviewed within a household (see Section 3.3), all of them were presented with the same version, to avoid contamination. Balance checks for the vignette experiments are displayed in Table B.3. All

Figure 3: Design of the vignette experiment

Father supports child marriage *(between subjects)*	
Yes	No
3,510 HHs	3,468 HHs

covariates are balanced across treatment conditions, and we cannot reject the hypothesis that subjects' characteristics across different arms of the experiment are statistically identical.

We estimate the following equation:

$$Y_{ihv} = \alpha + \beta_1 \text{ChildMarriage}_h + \beta_2 \left(\text{ChildMarriage}_h \times \text{Prevalence}_v\right) + \gamma \text{X}_{ihv} + \theta_v + \varepsilon_{ihv}, \quad (1)$$

where Y_{ihv} stands for the summary measure of social image rated by subject i at household h in village v; $\text{ChildMarriage}_h = 1$ if household h is presented with the version of the vignette where the father marries off his under-age daughter, and 0 otherwise; Prevalence_v is the prevalence of under-15 marriage in village v at baseline (according to our baseline survey), θ_v are village fixed effects (which absorb the prevalence of child marriage in each village); and ε_{ihv} is an error term.[12] We are interested in testing $\beta_1 \leq 0$ and $\beta_2 \geq 0$, corresponding to the hypothesis that compliance with local child marriage norms is rewarded when it comes to social image. Standard errors are clustered at the village level.

In Table 1, column (1) documents treatment effects on the summary measure, broken down by each of its components in columns (2) to (4).

Table 1: Causal effects of supporting child marriage on social image

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

Notes: The summary measure (1) is an equally weighted, standardized average of standardized individual measures for (2) altruism, (3) reciprocity, and (4) trustworthiness (see Section 3.3.1). Regressions additionally include individual controls (female, age, age^2, age^3, and measures for own pro-sociality) plus a constant. Standard errors clustered at the village level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The table shows that respondents who live in villages without under-15 marriage attribute a significantly lower social image to John when he supports the practice, an effect size of about 0.7 standard deviation (significant at the 1% level; column 1). The interaction term with the share of the village married before age 15 is positive (also significant at the 1% level; column 1),

[12] Results are robust to using child marriage under-18 as our measure of prevalence; see Table B.13.

indicating that social image associated with support for child marriage *increases* with its local prevalence. Such patterns are similarly estimated across all social image components that we measure (columns 2-4). According to theses estimates, supporting child marriage would causally improve social image in villages with prevalence 38.9% or higher – towards the high-end, but still within the range of our sample.[13] That is the case even though under-15 marriages are a more rare and extreme version of the practice, and despite the fact that the control version of the vignette does not rule out that the daughter will eventually marry before 18 years old.

The findings are consistent with a causal interpretation of the correlation between support for child marriage and village chief's ratings of villagers' social image in Figure 2. Altogether, the evidence suggests that defying local child marriage traditions is detrimental to social image.

3 Empirical strategy

This section starts by presenting our experiment design for estimating the causal effects of the public donation drive in subsection 3.1, followed by details on compliance and balance tests in subsection 3.2. Next, subsection 3.3 describes our data sources, and how we elicit subjects' perceptions, attitudes, and conformity to local traditions. Subsection 3.4 then summarizes the timeline of the experiments and surveys. Last, subsection 3.5 introduces the equations that we estimate and discusses our treatment of standard errors.

3.1 Experiment design

All details of the experimental design and a pre-analysis plan were pre-registered at the AEA RCT Registry as trial AEARCTR-0002856. The pre-analysis plan is presented in full in Appendix D.

We randomized the promotion of public donation drives across 412 villages in Malawi. In each treated village, a *box holder* was selected to coordinate donations, with the incumbency of (i) making sure a donation box was publicly displayed, (ii) of letting other villagers know that the donation drive would collect maize (Malawi's staple crop) to be redistributed to the poorest local households, and (iii) of collecting two kilograms of maize per donor. Donation drives were introduced as part of household listings by enumerators from the National Statistical Office of Malawi in preparation for the upcoming national survey (which took place 5 weeks later; see subsection 3.4). In *control* villages, regular listing took place at the same time.

To shed additional light on the social image mechanism, we add additional arms to our experimental design, some of which increase the visibility of the intervention. Concretely, we cross-randomize public donation boxes and the distribution of red rubber bracelets. In villages assigned to hosting both the box and bracelets, the latter were distributed in exchange for two kilograms of maize; in those assigned to host the public donation box only, donors got nothing in exchange for their donations; in villages assigned to bracelets only, those were made available *for sale*, for a price equivalent to two kilograms of maize. In pure control villages, neither public

[13] The maximum prevalence for under-15 marriage in our sample is 41%. The lower bound of the 95% confidence interval for the prevalence above which supporting child marriage improves social image (based on the estimates in column 1) is 18.6%.

donations were organized nor bracelets were distributed or sold. Cross-randomization serves multiple purposes. It allows investigating whether bracelets magnify the treatment effects of the public donation drive in villages where they are distributed in exchange for donations – by making that alternative signal even more visible. Moreover, taking advantage of the fact that bracelets are assigned different meanings across treatment cells (implying pro-sociality only when in exchange for donations), it allows testing whether charitable behavior indeed causally improves social image with the help of the vignette experiment (see Section 5.1.2). Our experiment design is summarized in Figure 4. We assigned a larger share of villages to the *Box* and the *Box & bracelets* conditions because those cells are involved in multiple comparisons.

Figure 4: Experiment design

		Box	
		Yes	No
Bracelets	Yes	117 villages 2,340 HHs	88 villages 1,760 HHs
	No	118 villages 2,360 HHs	89 villages 1,780 HHs

Across all treatment cells, village chiefs were asked during listing to enumerate the ten villagers most likely to support needy households in their village, "for example by giving out food or other important things". We used this set to determine the identity of the *box holder*. Box holders were randomly assigned: in 50% of treated villages, donations were assigned to be handled by the village chief; in the other 50%, they were handled by the last person cited by the local chief as the most likely to support the needy households in the village.[14,15] In this paper, we abstract from the identity of the box holder except in robustness tests; the randomization is explored in a companion paper (Haenni and Lichand, 2021).

We also use the top-10 most pro-social villagers listed by chiefs to kick-off the intervention in villages assigned to host public donation boxes. In those assigned to *Box & bracelets*, village chiefs were informed that the households listed as most likely to help others would be granted a red rubber bracelet to "show everyone that they can be counted on"; those households were handed the bracelets and informed along the same lines. The chief was then told that other households may also want to obtain such bracelets to signal that they can be counted on. For this purpose, the box holder was endowed with a big collection box, a measuring cup, and 80 additional bracelets. The box holder was instructed to hand out two bracelets to every household who donated two kilograms of maize to be later distributed to the neediest households in the village. In villages assigned to *Box*, the procedure was identical, expect that no rubber bracelets were distributed at the beginning of the experiment or in exchange for donations. The box holder was instructed to collect donations of two kilograms of maize by households that would like to "show everyone that they can be counted on", to be redistributed to the neediest households in

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

[15]In villages assigned to have bracelets distributed for sale, we follow the same logic to determine the *bracelet holder*.

the village. In villages assigned to *Bracelets*, we randomly assigned ten households to receive bracelets (rather than sticking to the list of top-10 most pro-social villagers provided by the chief); those households and village chiefs were informed along those lines. The chief was then told that other households may also want to obtain such bracelets. For this purpose, a *bracelet holder* (as in the case of box holders, either the last subject listed by the village chief, or the chief him/herself) was given 80 additional bracelets and instructed to sell them for 200 MWK (the monetary equivalent of two kilograms of maize in local markets). Payments were to be kept by the bracelet holder. Last, in control villages, chiefs were simply asked to list the ten households most likely to support needy households in their village. Even though the last individual listed by the chief plays no role in those village, we still keep track of that *counterfactual box holder* when we collect long-run data on child marriage in all villages (see Section 3.3.3).

The public donation drives (or bracelets' sales) were meant to be self-organized: other than appointing a box (bracelet) holder, distributing boxes (and/or bracelets) and delivering instructions, box holders were in charge of implementing it. Box holders were informed that the research team would not come back to redistribute the maize collected through the drive nor to verify whether donations in fact reached the poorest in the village, only to verify if the actual drive had in fact taken place and to measure how much maize was collected in total. As such, they were told to hold off on redistribution until after enumerators returned. In villages assigned to *Bracelets*, payments were meant to be kept by bracelet holders.

As pre-registered in our pre-analysis plan, throughout the paper we restrict attention to comparisons between villages assigned to the public donation drive and control villages, pooling across those assigned to bracelets or not within each of those groups. We turn to the additional comparisons in Section 5.4, when we test whether making charitable behavior even more visible magnifies the impacts of the intervention.

3.2 Compliance and balance tests

As discussed, during listings for the national survey ran by the National Statistical Office of Malawi, field team supervisors were responsible for implementing the pre-assigned intervention, by delivering the right set of instructions to the village chief, and the right set of materials to the box holder in treated villages. We assess compliance with treatment assignment at the time of the baseline survey. Figure A.4 showcases that compliance with the assignment across villages was very high, although not perfect.[16] Section 3.5 discusses how we deal with imperfect compliance in the estimation of treatment effects.

Table B.2 displays balance tests for the experiment. All covariates are balanced across treatment conditions, and we cannot reject the hypothesis that subjects' characteristics across different cells within each experiment are statistically identical.

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

3.3 Data and outcomes

We collected baseline data on traditional practices in Malawi through a nationally representative survey across 412 randomly selected villages, between July and August 2018, in collaboration with the National Statistical Office of Malawi, the University of Malawi and UNICEF Malawi. Table B.1 shows summary statistics. We surveyed 7,388 households, randomly drawn among all those with children between 8 and 17 years old. Villages in our sample are mostly rural and reasonably large: 82% of them are located in rural areas, with an average of 116 households, each with approximately 5 household members.

The baseline survey contained three modules: (i) a household module, that elicited household characteristics (such as composition, income, and spending); (ii) an individual module, whereby household members were interviewed separately, covering their social preferences, previous participation in initiation rituals, age of marriage, and attitudes towards traditional practices; and (iii) a social signaling module, which included the vignette experiment and questions about charitable behavior. Not every household member had to answer every survey module. If present, household heads always completed the household module as well as the individual module. Their spouses also completed the individual module. Last, if the household also had a minor between 15 and 17 years old, s/he also completed the individual module. In case there was more than one eligible minor, we randomly drew one of them to be part of the survey. Because the social signaling module was significantly lengthy, we conducted it with at most two individuals per household. We randomly drew one adult household member to take part in the experiment. If the household had any 15-17 year-old minors, the one who answered the individual module also participated in the experimental module. We conducted a total of 17,772 interviews, out of which approximately 40% covered the social signaling module. Table B.2 shows that survey respondents are on average 38 years old; 59% of them are female.

End-line data collection took place between September and November 2019, in collaboration with Innovations for Poverty Action (IPA). At this time, we surveyed village chiefs and (counterfactual) box holders about several characteristics of all girls in the village who were 10-17 years old and unmarried at baseline, including their marriage status, childbearing and school enrollment. We also elicited respondents' degree of familiarity with each girl (see Section 3.3.3). We were able to return to 98% of villages, and successfully elicited information for 76% of the girls from sampled households at baseline.[17] Completion rates are balanced across treatment arms (see Table B.4).

3.3.1 Social image

We measure villagers' social image by adapting Falk et al. (2016)'s pre-tested Preference Survey Module on social preferences to the Malawian context.[18] These survey modules mimic stan-

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

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

dard games from experimental economics and have been shown to predict social behavior in incentivized choice experiments very accurately. In our context, this measure correlates with charitable behavior, captured by donations to other villagers outside of our experiment: in control villages, donating over the last 5 weeks is associated with a 0.26 standard deviation higher summary measure of social image (p=0.025; see Table B.5).

We elicit these measures at four different instances: (1) when asking subjects to assess their own pro-sociality; (2) when asking subjects to assess pro-sociality of a hypothetical Malawian father in the vignette experiment; and (3) at end line, once again when asking respondents to assess the pro-sociality of other villagers.

3.3.2 Baseline conformity to traditional practices and attitudes

We measure subjects' history of engagement with child marriage and initiation rituals as well as their attitudes towards these traditional practices. Village-level prevalence of child marriage (under-18 or under-15) is based on subjects' accounts of their own behavior, in response to the question *"How old were you when you started living with your (first) husband/wife?"*. Prevalence of sexual initiation rituals, in turn, is based on subjects' accounts of the village-level behavior – to mitigate social desirability biases –, in response to 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?"*.[19]

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

3.3.3 End-line conformity to traditional practices and other outcomes for girls

16 months after the intervention, we measure child marriage and participation in sexual initiation rituals, as well as childbearing and school enrollment for all girls in the village who were 10-17 years old and unmarried at the time of the baseline survey. We capture those outcomes by surveying village chiefs and (counterfactual) box holders in each village, rather than households directly, to minimize concerns with experimenter demand bias. No question involved references to the girls' age nor to the baseline survey or the intervention, in order to avoid inducing reporting biases.[20] Importantly, alternatives such as checking whether girls under 18 years old still live with their families to infer child marriage through direct observation (as in Buchmann et al., 2019) would not have been informative in our context: Malawi is mostly matrilocal, which means that it is grooms who typically move in with the bride's family, rather than the other way around.

[19] Focus group discussions indicated that self-reported engagement in sexual initiation rituals are extremely sensitive to social desirability bias, while the same is not true for one's own history of child marriage.

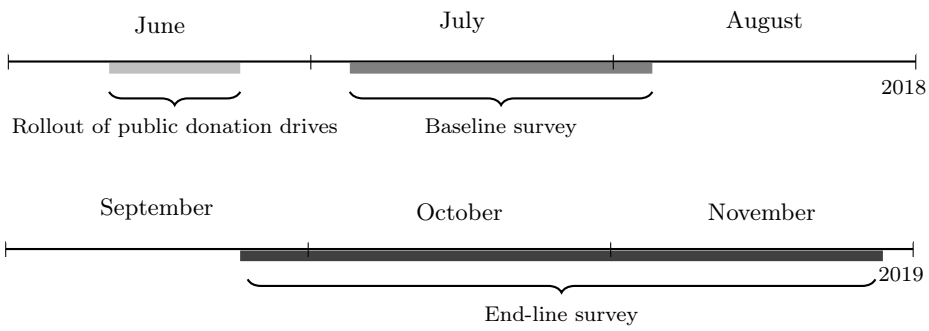
[20] Pilots have shown that respondents are generally bad at guessing the age of a girl, but accurate in determining her marital status.

Survey responses for each girl are weighted by how well respondents claim to know her.[21] We code outcomes as missing for girls whom neither the village chief nor the (counterfactual) box holder states that they know her well enough to answer the questions. We were able to collect at least one response for 76% of girls, and two responses for nearly 60% of the them; for the latter, in only 2% of cases respondents provide conflicting information.

3.4 Timeline

Figure 5 summarizes the timeline of the intervention and surveys. Public donation drives were rolled out in June 2018. About five weeks later (the average time span between the field visits for listing and surveying, across the 412 villages), the baseline survey was conducted as part of a nationally representative household survey about traditional practices in Malawi. Last, about 16 months after the intervention, we conducted the end-line survey.

Figure 5: Timeline



3.5 Estimation

Throughout the paper, we estimate average treatment effects (ATE) based on intention to treat, using OLS regressions. We estimate the following equation:

$$Y_{ihvt} = \alpha + \beta \text{PublicDrive}_v + \theta \text{X}_{ihv,t=0} + \varepsilon_{ihvt}, \tag{2}$$

where Y_{ihvt} is the outcome of interest for individual i in household h and village v at time t; $\text{PublicDrive}_v = 1$ if village v was assigned to host a public donation drive, and 0 otherwise; $X_{ihv,t=0}$ is a vector of individual- and village-level baseline characteristics; and ε_{ihvt} is an error term. For the effects of the public drive on village-level outcomes (such as charitable behavior), we compute $\overline{Y}_v$ by averaging over Y_{ihvt} within village v. We are interested in testing $\beta \leq 0$ for negative outcomes such as child marriage (and the opposite for positive outcomes, such as charitable behavior).

[21] Pre-registered in the update to Trial AEARCTR-0002856 on September 22, 2019, before the start of the end-line data collection.

As pre-registered, we also analyze treatment effects conditional on the baseline prevalence of child marriage, given heterogeneity in local norms – which determines its contribution to social image in different villages. We estimate:

$$\begin{aligned} Y_{ihvt} = \alpha + \beta_H \left(\text{PublicDrive}_v \text{ x High-prevalence}_{v,t=0}\right) + \beta_L \text{PublicDrive}_v + \\ \gamma \text{High-prevalence}_{v,t=0} + \theta \text{X}_{ihv,t=0} + \varepsilon_{ihvt}, \end{aligned} \tag{3}$$

where High-prevalence$_{v,t=0} = 1$ if the prevalence of under-18 marriage in villave v at our baseline survey is above the sample median, and 0 otherwise. We are interested in testing $\beta_H \leq \beta_L$ for negative outcomes such as child marriage (and the opposite for positive outcomes, such as charitable behavior).

We control for district and enumerator fixed effects, and cluster standard errors at the village level across all specifications, allowing errors to be arbitrarily correlated across households within each village.

Given imperfect compliance with the experiment design (Figure A.4), Appendix C estimates average treatment effects on the treated (ATT), instrumenting whether villages actually hosted a public donation drive with treatment assignment.

4 Effects of the public donation drive

This section documents the impacts of the public donation drive. Subsection 4.1 starts by documenting that food donations are indeed perceived as a pro-social activity in our sample, and that donations to fellow villagers indeed contribute to social image, followed by estimates of treatment effects on charitable behavior in subsection 4.2. Next, subsection 4.3 estimates treatment effects on child marriage and other girls' outcomes. Last, subsection 4.4 compiles extensive robustness tests linked to data quality issues and experimenter demand effects.

4.1 Manipulation checks

We start by documenting that survey respondents indeed perceive donations as pro-social activity. For this purpose, we ask subjects in an open question what *food collections* mostly stand for, compiling answers into two categories: *sharing* and *wealth*. Figure A.5 displays the results in Panels A2 and A3. Food donations represent sharing for roughly 40-50% respondents across all treatment conditions, while very few households perceive food donations as a signal of wealth.[22] Most importantly, the figure showcases that the extent to which food donations stand for pro-social behavior does *not* vary systematically across treatment arms.

Next, we document that charitable behavior is positively associated with social image. At the end-line survey, respondents are asked to rate all household heads with 11-17 daughters who were unmarried at baseline with respect to their social image, and then report whether each of them had donated to other villagers over the previous year and over the previous month. Table B.7 documents that donating over the last year and over the previous month is strongly positively correlated with social image. We later show that this association is indeed causal with

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

the help of the vignette experiment (see Section 5.1.2). Most importantly, this relationship is not systematically different across treatment arms.

All in all, those patterns suggest that (1) the intervention in fact has the potential to manipulate the production function of social image, and (2) if we find treatment effects, it is because the intervention made charitable behavior more visible, not because it affected what donations stand for or their marginal contribution to social image, relative to the control group.

4.2 Effects on charitable behavior

Figure A.5 shows that, within 5 weeks, the drives indeed collected substantial donations: villages assigned to the intervention collected on average $\sim$ 50kg of maize.[23] Did the intervention also affect charitable behavior at large, relative to the counterfactual? We assess the short- and long-run effects of the public donation drive on charitable behavior using end-line survey data on donations to other villagers.

Figure A.6 documents that the intervention significantly increased donations. At the end-line survey, the share of villagers who state that they had donated to others over the previous year in treated villages is 10 p.p. higher than that in control villages (significant at the 1% level; Panel A). Treatment effects are even larger when using as dependent variable, instead, the stated share of *other* villagers who donated over that period, a metric presumably less subject to experimenter demand effects. By this metric, the drive nearly doubles the share of donors relative to the control group (significant at the 1% level; Panel B). The figure also suggests that treatment effects on charitable behavior only gradually fade out over time: in Panel A, even 16 months after the intervention, it is still the case that in villages assigned to the drive a higher share of respondents report to have donated over the previous month relative to the control group (albeit lower, the effect size is still significant at the 10% level).

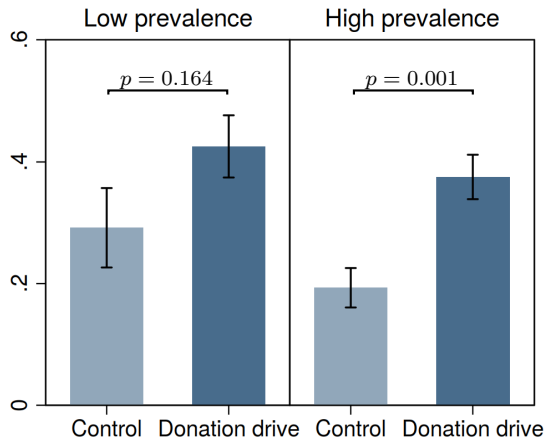
Next, we study selection into charitable behavior: if the intervention systematically increased the number of donors in treated village, did it also change the *profile* of those who donate, relative to the control group? To do that, we contrast the characteristics of those who stated they had donated to other villagers over the previous 5 weeks at our baseline survey, across treatment and control villages.[24] Table B.6 reports summary statistics of those who donate across control (Columns 1-3) and treated villages (Columns 4-6). In control villages, there are considerable differences in the profile of those who recently donated and those who did not. In particular, individuals who donated report to be more altruistic and reciprocal (although not statistically significant), are significantly wealthier (captured by weekly consumption expenditures), and are less likely to engage in and support child marriage. In contrast, in villages assigned to the public donation drive, the composition of those who donate and those who do not is much more similar with respect to all characteristics. This indicates that those who typically do *not* donate *sort into* charitable behavior in treated villages.

[23] As explained to box holders during listing, surveyors would come back $\sim$ 5 weeks later to measure how much maize had been collected. As such, they were instructed not to start redistribution until then. Also in line with those instructions, the survey team was not involved in redistribution nor checked who ultimately received the donations.

[24] We cannot do the same at end line because we did not re-survey subjects directly; see Section 3.3.

Figure 6 focuses on charitable behavior of respondents with a history of child marriage (i.e., who themselves got married before 18), splitting the sample into above- and below-median prevalence of under-18 marriage, according to our baseline survey. While the share of those respondents who donate is lower in high-prevalence control villages than in low-prevalence control villages, the public donation drive has a much larger effect in the former (a nearly 100% increase, significant at the 1% level; on the RHS). In effect, the resulting share of donors among respondents who married as children ends up as roughly the same in treated villages of high and low baseline prevalence.

Figure 6: Share of respondents with a child marriage history that donate in villages with low vs high prevalence of child marriage



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

Together, results indicate that the intervention stimulated charitable behavior on average, but particularly so among those with a history of child marriage, and in high prevalence villages. Did villagers resort to that alternative social signal in those villages at least partly to protect their social image when acting on their private motives, in defiance of local norms? We next turn to treatment effects on child marriage to answer that question.

4.3 Effects on child marriage and girls' outcomes

We estimate treatment effects of the public donation drive on child marriage, teenage pregnancies, school dropouts and participation in sexual initiation rituals, for all girls in our sample who were between 11 and 17 years old and unmarried at baseline. We consider all girls up to 18 years old when estimating treatment effects on child marriage and childbearing, but stop at 17 years old when estimating treatment effects on school dropouts (the right age-for-grade at the end of high school), since we only ask if a girl is still in school – not if she graduated. When it comes to sexual initiation rituals, we consider all girls up to 15 years old, since those typically happen at age 10-15 (see Feng, Haenni and Lichand, 2021).

Table 2 shows that the intervention decreases child marriage age-by-age on average by 1.8 p.p., a 30% reduction relative to the control group.[25] This effect size is comparable to that of conditional incentives (Buchmann et al., 2019), shown to decrease child marriage by 25% in Bangladesh. Similarly, early pregnancies age-by-age decrease on average by 1.6 p.p. in the treatment group (a 29% reduction relative to control), and school dropouts age-by-age decrease on average by 2.5 p.p. in the treatment group (a 15% reduction relative to control). The intervention also decreases sexual initiation rituals (the probability of participation age-by-age decreases on average by 1.7 p.p., a 25% reduction relative to the control group), although that effect is only imprecisely estimated. Figure 7 displays hazard rates over the relevant ages for each outcome on the left-hand side of each panel, documenting that, for all outcomes, the intervention shifts the probability distribution downward over almost the entire age range.

Table 2: Age-by-age treatment effects on girls' outcomes

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

Notes: Regressions additionally include individual controls (age, age^2, and age^3) and village-level controls (village size, population density, urban, and baseline prevalence of child marriage or initiation rituals) and enumerator indicators. Standard errors clustered at the village level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Those estimates allow us to predict the *expected prevalence* of child marriage, teenage pregnancies, school dropouts and sexual initiation rituals in treatment and control villages at the time when all girls in our sample will have turned 18 years old, under the additional assumption that treatment effects persist over time. Because our sample restricts attention to girls who were unmarried at baseline, we compute the expected prevalence of outcome Y^k for girl i (whose age is a_i) at village v by outcome-specific critical age A^k as:

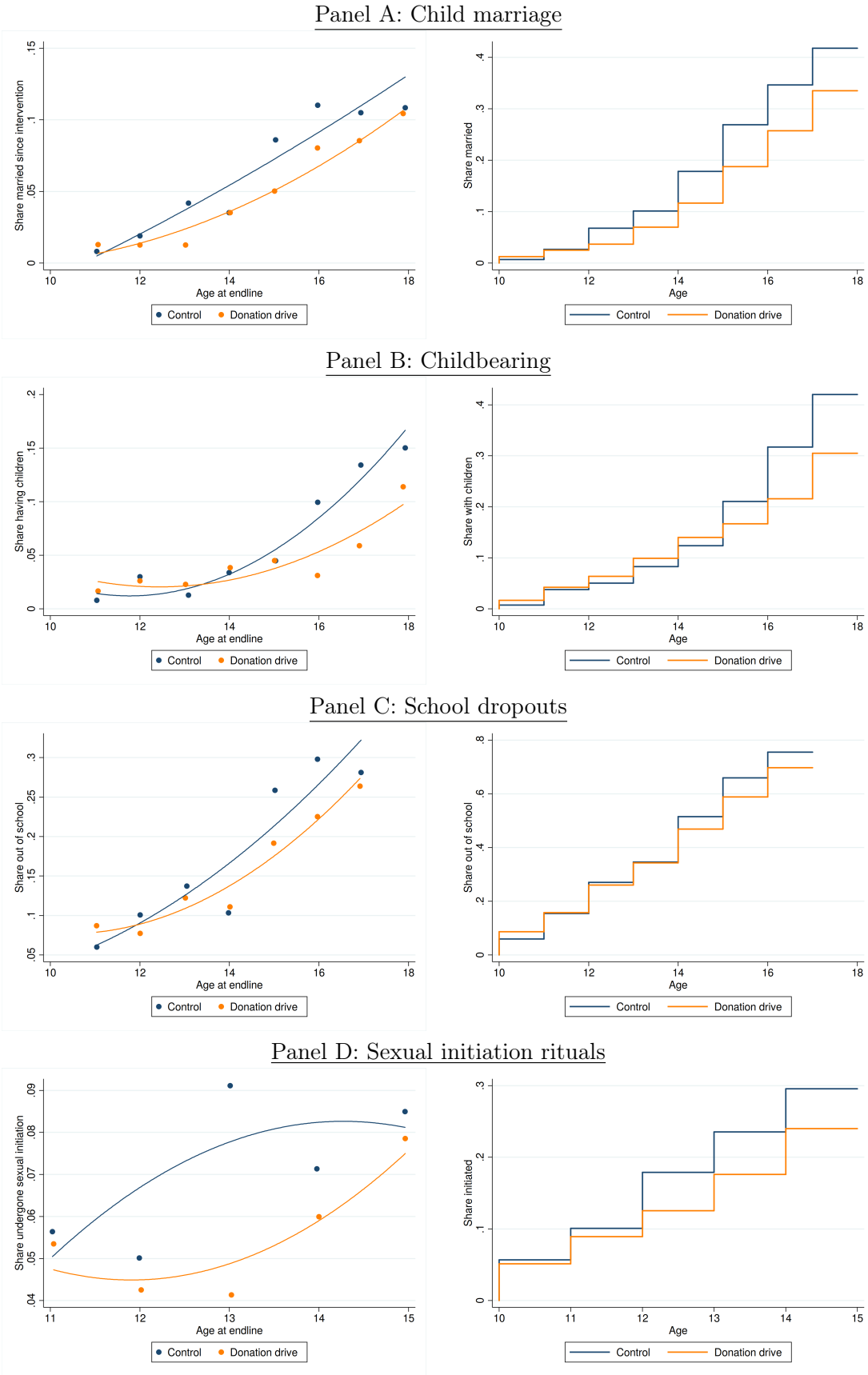
$$E[Y_{iv}^k | a_i = A^k] = \begin{cases} 1 - \prod_{t=a_i}^{A^k}(1 - \mu_t^k), \text{ if PublicDrive}_v = 0; \text{ or} \\ 1 - \prod_{t=a_i}^{A^k}(1 - \mu_t^k - \hat{\beta}_t^k), \text{ if PublicDrive}_v = 1. \end{cases} \tag{4}$$

where μ_t^k is the average prevalence of outcome Y_{iv}^k among girls of age t within control villages; and $\hat{\beta}_t^k$ is the estimated treatment effect on outcome k among girls of age t. We use sample-wide averages rather than village-level ones because we do not have end-line information on girls of *each* age for *every* village, which would be necessary to compute village-specific hazard rates.[26] As such, we do not estimate regressions using those predictions, but rather leverage them to

[25] See Table C.1 for average treatment effects on the treated (ATT) estimates.

[26] Even when we have minimal information to compute village-specific hazard rates, those would still be very imprecisely estimated compared to the sample-wide averages.

Figure 7: Hazard rates of negative outcomes for girls, in treatment vs. control



Notes: Binned scatter plots on the left and cumulative hazard rates based on those estimates on the right. Figures are residualized for village-level controls (village size, population density, rural/urban status, and baseline prevalence of child marriage or initiation rituals) as well as enumerator and district indicators.

provide a sense of the magnitudes of medium-term treatment effects under the assumption of persistence.

Figure 7 showcases the expected prevalence of girls' outcomes by the time all of them will have reached each age, for treatment and control villages, on the right-hand side of each panel. The figure documents an 8 p.p. lower expected prevalence of child marriage in treated villages (a 19% reduction relative to control). Similarly, the expected probability of teenage pregnancies decreases by 11 p.p. (a 27% reduction relative to control), and school dropouts decrease by 6 p.p. (a 8% reduction relative to control). While the expected participation in sexual initiation rituals by age 15 decreases by 6 p.p. (a 20% reduction relative to control), differences in the hazard rates of participation in sexual initiation rituals across the treatment and control groups are more pronounced earlier on (a nearly 30% reduction by age 13).

Last, we estimate heterogeneous treatment effects of the public donation drive. If effects are driven by the social image mechanism, one would expect treatment effects to be concentrated in high-prevalence villages – precisely where one's image is expected to suffer from defying the local norm in the absence of alternative signals (see Section 2.2.1).

Table 3 displays heterogeneous treatment effects on age-by-age child marriage rates. In this subsection, we focus on columns (1) and (2), which estimate treatment effects separately for the sub-sample of villages with above- and below-median prevalence of under-18 marriage at baseline, respectively. We find that the effect size of the intervention for high-prevalence villages is four-fold that for villages where under-18 marriage was low prevalence at baseline (p=0.147).

Table 3: Heterogeneous treatment effects on the expected prevalence of child marriage

	Baseline prevalence of child marriage		Box holder		Baseline spending (village percentile)	
	(1) High	(2) Low	(3) Chief	(4) Other	(5) Poorest 10%	(6) Richest 90%
Public donation drive	-0.0277**	-0.00588	-0.0133	-0.0254**	-0.0121	-0.0168*
	(0.0113)	(0.0127)	(0.0117)	(0.0127)	(0.0171)	(0.00912)
Test of equality (p-val.)	0.147		0.890		0.979	
Control mean	0.062	0.051	0.053	0.061	0.063	0.056
Observations	1,831	1,605	1,641	1,795	438	2,997
District fixed effects	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Notes: Regressions additionally include individual controls (age, age^2, and age^3) and village-level controls (village size, population density, rural/urban status, and baseline prevalence of child marriage or initiation rituals) and enumerator indicators. Indicator for baseline prevalence of child marriage is based on a median split and the median age in that sample is 35 years or ≈ 1 generation before the present data collection. Standard errors, clustered at the village level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

4.4 Robustness checks

This subsection compiles robustness tests of our main results. We start by discussing the sensitivity of results based on our end-line survey data: subsection 4.4.1 tests whether results systematically change when dropping responses from village chiefs or observations with conflicting information. Subsection 4.4.2 then presents direct tests of and bounds for experimenter

demand effects. Last, taking advantage of data from a local NGO's help lines, subsection 4.4.3 estimates treatment effects on an independent, objective measure of behavior.

4.4.1 Sensitivity of results to external sources' reports

Our data on child marriage could not be based on notarized marriage certificates (which largely do not exist out of the main cities in Malawi), neither on home visits to check whether under-age girls still live with their parents (as Malawi is mostly matrilocal, i.e. husbands typically move in with the bride's family in most of the country). Since we can only rely on survey responses, a central concern is whether our estimates conflate experimenter demand effects.

To mitigate those concerns, in our end-line survey, we refrained from asking households directly about whether their under-age daughters got married in the previous year. Instead, we relied on external sources: we surveyed village chiefs and (counterfactual) box holders, asking them to report on several characteristics of each girl – including their marriage status. Respondents could answer "I don't know"; when they chose to answer, they were asked to express their degree of familiarity with each girl to provide a measure of uncertainty. We pre-registered that, whenever we had access to two responses for a girl, we would weight them by respondents' stated degree of familiarity with that girl.

While this strategy presumably limits reporting biases, it could still be the case that the identity of some respondents makes them particularly prone to experimenter demand effects. Chiefs arguably have the strongest motives to manipulate how their village is perceived in the context of a survey on traditional practices – some of which are harmful to girls –, and these motives could be exacerbated if the intervention triggers collective action in the village. Table B.8 assesses the robustness of our findings to omitting village chiefs' responses when computing girls' outcomes. Estimates for treatment effects on child marriage and childbearing are remarkably similar to our main findings in Table 2 (although the latter slightly loses precision; columns 1 and 2). Interestingly, while dropping chiefs' responses decreases somewhat the effect size of the intervention on school dropouts (by 25%, no longer statistically significant; column 3), it greatly increases that on participation in sexual initiation rituals – which becomes 3-fold that in Table 2 (statistically significant at the 10% level; column 4).

A different way to test this hypotheses is by restricting attention to girls with multiple responses (60% of the 10-17 year old girls who were unmarried at baseline) for whom both respondents agree, for each outcome. If chiefs are in fact more prone to experimenter demand effects than other villagers, then dropping observations for girls with conflicting information would minimize any such tendency to manipulate responses. Table B.9 presents the results.[27,28] Estimates of treatment effects on child marriage and participation in sexual initiation rituals are very similar to our main findings in Table 2 (columns 1 and 4). While the effect size of the intervention on school dropouts falls by half (no longer statistically significant; column 3), the control mean also falls by about the same, suggesting this is mostly about how this sub-sample is

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

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

selected rather than about lack of robustness. Last, the effect size of the intervention on teenage pregnancies increases by 2/3 (significant at the 1% level despite the smaller sample size; column 2).

4.4.2 Direct tests of and bounds on experimenter demand effects

This subsection considers direct tests of experimenter demand effects. If the intervention makes subjects more willing to conform to expectations of what surveyors might want them to say, then we should find systematic differences across treatment and control villages even for variables that were determined prior to the baseline survey. We start by investigating whether respondents' self-reported age of marriage and their stated previous participation in sexual initiation rituals are affected by the intervention. Since neither past engagement in initiation rituals or marriage age could have been affected by the intervention – even though self-reports about those are, presumably, equally sensitive to experimenter demand effects – , these provide good candidates for placebo tests. Table B.10 documents no treatment effects on either pre-determined outcome.

Is this really evidence against experimenter demand effects, or could it be that the time elapsed since listing had been insufficient to induce reporting biases, only 5 weeks after the intervention? We rule this out with data on support for child marriage and sexual initiation rituals, also collected at the baseline survey (see Section 3.3.2). Table B.11 documents sizable treatment effects on support for child marriage and forced sexual initiation rituals 5 weeks after the intervention: support for those traditional practices decreases by 30% and 20%, respectively, relative to the control group.[29] Short-run treatment effects on attitudes stated at the baseline survey are consistent with marginal respondents feeling like they can express their private views to a greater extent in treated villages than in the control group, even when those views disagree with local norms; in fact, effects are concentrated in villages with above-median prevalence of under-18 marriage at baseline.[30] This interpretation is also consistent with a short-run spike in calls to report child marriage episodes in treated villages right after the intervention, relative to the control group – an outcome that is not sensitive to experimenter demand effects (see Section 4.4.3).

To provide a final test of how sensitive results might be to reporting biases, we use priming techniques to estimate bounds for experimenter demand effects, following De Quidt, Haushofer and Roth (2018). Concretely, at the very end of the survey, we ask 1/3 of respondents (randomly drawn) whether they agree with the following statement about traditional practices: *"There are common cultural practices in this village that may harm children"*. The last part of the statement makes it a value judgment – not merely a factual claim – and, hence, should reasonably approximate attitudes towards those practices. Before expressing whether they agree with that statement, respondents are primed about surveyors' expectation. We randomly assign subjects to either a "Demand to agree" condition, in which they are told, before reading the statement, that *"We expect participants to whom we ask the following question to agree with it more often*

[29] For local average treatment effects using IV, see Table C.2.

[30] Table B.12 documents that the effects of the intervention on support for child marriage are driven by households with girls at the relevant age as well as by female respondents, and are larger in high-prevalence villages. Notably, treatment effects are concentrated on the sub-sample that states to have married for traditional reasons, while we find no effects among those who married out of emergency situations (although the difference is not statistically significant).

than they normally would"; to a "Demand to disagree" condition, in which we told respondents, before reading the statement, that *"We expect participants to whom we ask the following question to agree with it less often than they normally would"*; or to a control condition, where no priming precedes the statement. The idea is that priming subjects about expected responses should create experimenter demand effects that are arguably much stronger than any implicit effects that could have been triggered over the course of the survey. The reason this question was placed at the end of the survey is to avoid that the priming might also affect other survey responses.

In Table 4, column (1) presents bounds for experimenter demand effects, and column (2) estimates bounds conditional on treatment assignment. We find that priming generates experimenter demand effects that go in the expected direction in each case, but turn out to be rather small (none statistically significant at conventional levels; column 1). The "Demand to agree" condition increases agreement by 1.65 p.p. (a 2% increase compared to the control condition), about the same as the "Demand to disagree" condition (only in the opposite direction). This is not due to floor or ceiling effects: about 77% of respondents agree with the statements in the control condition. Most importantly, experimenter demand effects do not systematically vary with treatment assignment: interaction terms are very small and statistically insignificant (column 2). Results corroborate that treatment effects of the public donation drive are not driven by the desire to conform to surveyors' expectations.

Table 4: Bounding experimenter demand effects

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

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

4.4.3 Treatment effects on help-line calls

Last, we explore data on call logs of a local NGO's help lines, through which villagers could report violations of girls' rights, including child marriage. We use these data to assess whether villagers are prepared to take consequential actions and seek help from outside their communities – an outcome unlikely to be affected by experimenter demand effects.

The *Tithandizane Child help line* and the *Gender-Based Violence help line* are nation-wide toll-free services operated by the Malawian NGO YONECO. The help lines aim at providing an easily accessible tool to support victims of child abuse and gender-based violence, and to allow anyone witnessing such abuse and violence to report it. The caller is either directly cared for by a trained counsellor, or referred to government services – the police or district social welfare offices. The organisation also engages in case management and follow-ups. For these purposes, the help line services keep detailed records on each call: the issue reported, actions taken, as well as *personal details of the caller* – including their name and location (village, traditional authority, region, and district). Importantly, since calls are *not* anonymous, social image concerns definitely play a role when filing a report.

It is not clear whether the intervention should increase or decrease child marriage reports. The issue is similar to that behind changes in perceptions about corruption: when perceptions deteriorate, it is not clear whether corruption has become worse or, conversely, if it is actually getting better, e.g. because of a crackdown that exposes it under the public eye (e.g. Olken, 2009). In our case, if the public donation drive decreased child marriage but did not affect reporting decisions, we would expect less calls in villages assigned to the public donation drive. If, conversely, the intervention made non-anonymous reporting more socially acceptable, then we might actually find *more* calls in those villages, relative to the control group. What is more, there are other reasons for why the intervention might lead to higher reporting regardless of changes in actual child marriage. If the donation drive mobilizes villagers for community engagement more broadly, we would find more calls in treated villages even in the absence of actual changes in the prevalence of girls' early marriages.

Despite all those caveats about the call log data, if we find a causal effect of the intervention on the number of calls it would still corroborate the mechanism behind the treatment effects that we document with the help of survey data: because calls are not anonymous, even effects on reporting behavior would confirm that alternative signals of social image allow individuals to voice their private views more freely even when those conflict with local norms.

Call logs are recorded as plain text – a summary paragraph which includes the name and location of the caller as well as the issue reported. YONECO provided us with anonymized child marriage reports, whereby the names of the caller and of the victims were removed from the data. We then used natural language processing tools to code the location of the caller in each report.

We have access to monthly data on calls from February 2017 to December 2020. Since the public donation drive was introduced in June 2018, that gives us almost 1.5 year before and 2.5 years after the intervention. As such, we can use a differences-in-differences strategy to estimate treatment effects on reports accounting for any differences in reporting levels across treatment and control villages before the interventions was rolled out. We can also estimate the extent of persistence or fade-out of treatment effects over time.

Matching locations based on summary descriptions in the call log data is challenging; the more so the more granular the administrative unit. In particular, Malawian villages are often known by the name of the village chief or the group village head, which makes it particularly difficult to uniquely identify them. As such, we are able to match 6,816 calls to our data based

on traditional authority region (TA) names, while we match only 112 calls based on village names. For this reason, we estimate treatment effects at the TA level.

Our sample consists of 412 villages distributed across 181 TAs. In TA-level regressions, the outcome variable is the number of monthly child marriage reports within each TA, and treatment indicators represent the % of villages within each TA that were assigned to the public donation drive. Analogously, when we analyze heterogeneous treatment effects by the baseline prevalence of child marriage, we compute the % of villages in our sample within each TA that featured above-median prevalence of marriages under-18 at our baseline survey.

Calls are relatively rare events. To give a sense of the data, YONECO recorded 1,620 calls reporting child marriage over the course of 2020 (for the whole country, of which our data consists of a small representative sample). As such, roughly 90% of TA-month pairs in our data feature no calls. Prior to our intervention, the probability that a TA featured at least one call in a given month and its average number of monthly calls did not systematically vary with the share of villages in the TA for which child marriage was high prevalence. After June 2018, when the public donation drives were rolled out, the average incidence of reporting and number of monthly calls systematically increased in the aggregate.

Since our dependent variable is count data, we estimate negative binomial regressions for treatment effects on average monthly calls, and OLS regressions when estimating treatment effects on the extensive margin of reporting. We estimate the following equation:

$$\text{Calls}_{jt} = \alpha + \beta\,(\text{PublicDrive}_j \text{ x After}_t) + \gamma\text{PublicDrive}_j + \theta\text{After}_t + \varepsilon_{jt}, \tag{5}$$

where Calls_{jt} is either an indicator variable = 1 if TA j featured any calls reporting child marriage at month t, and 0 otherwise, or the number of calls for TA j at month t; PublicDrive_j stands for the share of villages in TA j assigned to host a public donation drive; $\text{After}_t = 1$ from June 2018, when the public donation drive was rolled out; and ε_{jt} is an error term. We are interested in testing $\beta \neq 0$. We cluster standard errors at the TA level.

Table 5 presents the results. Columns (1) to (4) estimate OLS regressions with the extensive margin of reporting as dependent variable, while columns (5) to (8) estimate negative binomial regressions with the average number of monthly calls. Columns (1) and (5) estimate the differences-in-differences model in equation 5; columns (2) and (6) estimate heterogeneous treatment effects with respect to the share of villages in each TA with above-median baseline prevalence of child marriage; columns (3) and (7) present placebo exercises, controlling for the interaction of PublicDrive_j with an indicator variable = 1 for the 12 months before the public donation drive, and 0 otherwise; and columns (4) and (8) turn to dose-response estimates by adding an interaction of PublicDrive_j with the number of months since the intervention was rolled out. In columns (5) to (8), we report incidence rate ratios and their respective robust standard errors.

The table shows that, while the drive does not systematically affect the extensive margin of reporting (column 1) or the number of monthly calls (column 5) on average, null results mask extensive heterogeneity across space. In effect, the probability of featuring at least one monthly call increases by 9.2 p.p. in TAs where child marriage was high-prevalence everywhere and where all villages were assigned to the public donation drive, relative to otherwise identical

TAs where all villages were assigned to the control group instead (a 92.7% increase relative to the base period, significant at the 5% level; column 2). In those TAs, the average number of calls *increases by 250%* relative to the control group (significant at the 1% level; column 6). This specification also shows that calls are *not* merely the result of collective action triggered by the donation drive: the average number of monthly calls actually *falls by half* in TAs where child marriage was low-prevalence everywhere and where all villages were assigned to the public donation drive, relative to otherwise identical TAs with all villages in the control group (p=0.156; column 7). Together, these patterns are consistent with deviations from local norms becoming more socially acceptable in treated villages (see also Section 5.1). Importantly, columns (3) and (7) document that there were no systematic differential pre-trends across TAs with different shares of treated villages, and that allowing those pre-trends to differ barely affects our previous estimates of treatment effects on reporting behavior.

Last, dose-response treatment effects are also informative: TAs where all villages were assigned to the public donation drive feature 10.8% more reports of child marriage than control ones immediately after the intervention (although not precisely estimated; column 6). Most importantly, this specification shows that treatment effects persist over time: effect sizes on the probability of at least one monthly call (column 4) or on the average number of monthly calls (column 8) are almost exactly zero.

All in all, results in this section confirm that increasing the visibility of an alternative signal of social image allows individuals to act on their private motives to a greater extent, with the help of independent data that is not sensitive to experimenter demand effects.

Table 5: Treatment effects on reporting behavior

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Any monthly calls				Number of monthly calls		
					IRR	IRR	IRR	IRR
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
% Drive x After	0.002	-0.032	-0.045	-0.001	1.131	0.638	0.484	1.108
	[0.015]	[0.023]	[0.030]	[0.016]	[0.241]	[0.202]	[0.248]	[0.236]
% Drive x After x Above-median		0.092**	0.092**			3.567***	3.500***	
		[0.041]	[0.041]			[1.569]	[1.536]	
% Drive x Placebo			-0.017				0.708	
			[0.025]				[0.272]	
% Drive x Months since intervention				0.000				1.001
				[0.001]				[0.009]
% Drive	0.006	0.003	0.016	0.006	1.031	1.063	1.410	1.031
	[0.030]	[0.031]	[0.032]	[0.030]	[0.443]	[0.449]	[0.770]	[0.444]
After	0.010	0.008	0.034**	-0.047***	1.056	1.034	1.718**	0.400***
	[0.011]	[0.011]	[0.016]	[0.015]	[0.180]	[0.176]	[0.467]	[0.075]
% Above-median		-0.029	-0.029			0.704	0.717	
		[0.030]	[0.030]			[0.269]	[0.273]	
Placebo			0.036**				1.884**	
			[0.018]				[0.491]	
Months since intervention				0.002***				1.041***
				[0.001]				[0.006]
Constant	0.084***	0.099***	0.072***	0.061***	0.137***	0.159***	0.095***	0.092***
	[0.020]	[0.028]	[0.025]	[0.019]	[0.041]	[0.057]	[0.036]	[0.029]
Observations	9729	9729	9729	9729	9729	9729	9729	9729
R-Squared	0.000	0.004	0.004	0.005				

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

5 Mechanisms

Are the effects of the public donation drive really grounded on how it impacts the production function of social image? This section turns to the mechanisms behind treatment effects. Subsection 5.1 documents how social image responds to the intervention. Next, subsection 5.2 takes advantage of an additional experiment that randomized the identity of the box holder in treated villages to assess whether treatment effects are driven by the desire to signal to fellow villagers at large, or specifically to the village chief. Subsection 5.3 then estimates heterogeneous treatment effects by household consumption levels, to study whether the impacts of the donation drive could be alternatively explained by redistribution and its effects on marriage market dynamics. Last, subsection 5.4 leverages the cross-randomization of donation boxes and bracelets to test whether making the intervention even more visible magnifies treatment effects on child marriage.

5.1 Effects on social image

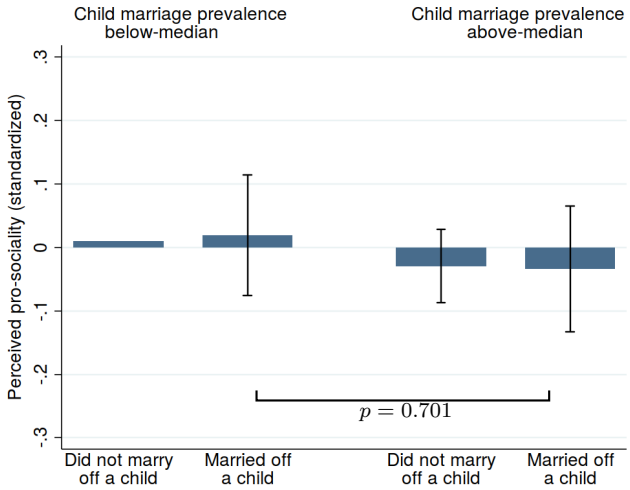
5.1.1 Conformity to local norms vs. social image, revisited

We start by revisiting the relationship between conformity to local norms and social image, previously depicted for control villages in Figure 2, but this time for villages that hosted public donations drives.

In control villages, we saw that those with a recent history of child marriage are perceived as 0.3 standard deviations more pro-social in high-prevalence villages than in low-prevalence villages (p=0.039; Figure 2). In contrast, in villages assigned to the public donation drive, conforming to local child marriage traditions is no longer correlated with social image, regardless of the baseline prevalence of under-18 marriages. Restricting attention to high-prevalence villages, the social image summary measure of those who recently married off an under-age daughter decreases by nearly 0.2 standard deviation when we move from control to treated villages. In fact, those who have recently married a daughter are no longer differentially perceived across low- and high-prevalence villages (p=0.701).

This result provides two key insights. First, it confirms that the intervention protects the social image of those who act on their private motives in defiance of local norms: in high-prevalence villages, it seems now possible *not* to marry off an under-age daughter without being perceived as less pro-social by others. Second, it corroborates that social image concerns induce child marriage *only in the presence of a wedge* between private and social motives; after all, as Section 4.3 shows, child marriage does *not* increase in low-prevalence villages – even though our findings suggest that the intervention allows one to protect their social image in the event they deviate from local norms in these villages as well.

Figure 8: Villagers' social image in villages hosting public donation drives, conditional on their engagement in child marriage and on the prevalence of child marriage in the village



Notes: Villagers' pro-sociality as perceived by the survey respondent in villages where the prevalence of child marriage was low vs high at baseline, according to a median split. The pro-sociality measure is an equally weighted, standardized combination of individual measures for altruism, reciprocity, and trust (see Section 3.3.1). Estimates are from an ordinary least squares regression, including village-level controls (village size, population density, and rural/urban status) and district and enumerator fixed-effects. Bars stand for standard errors, clustered at the village level. P-values from Wald tests for equality of estimated coefficients.

5.1.2 Vignette experiment, revisited

Next, we return to the vignette experiment to investigate (1) whether charitable behavior causally improves social image, and (2) whether the impacts of the intervention on the social image of those who do not marry off under-age daughters in high-prevalence villages are concentrated on those who contribute to the public donation drive.

To study those questions, we take advantage of our experiment design, which cross-randomized the public donation drive with the distribution of bracelets (see Section 3.1). At the end of the social signaling module at our baseline survey, we present respondents with a modified version of the vignette experiment. The only difference between that version and the original vignette experiment described in Section 2.2.1 is that, the second time around, the family is illustrated wearing red rubber bracelets – identical to those distributed in some treatment cells. This allows us to test (1) whether John's social image is higher-rated in villages where bracelets stand for donations, and (2) whether harmless John's social image is no worse than its harmful version in high-prevalence villages assigned to *Box & Bracelets* when he wears a red rubber bracelet, different from everywhere else.

Concretely, at the end of the survey (about 15 minutes after the initial vignette), all respondents are again confronted with the same background story (holding constant the assignment they were presented with the first time around); only, this time, John and his wife are depicted wearing red rubber bracelets (in both versions, depicted in the bottom row of Figure A.3).[31] The difference from the original picture is pointed out to respondents by enumerators, without

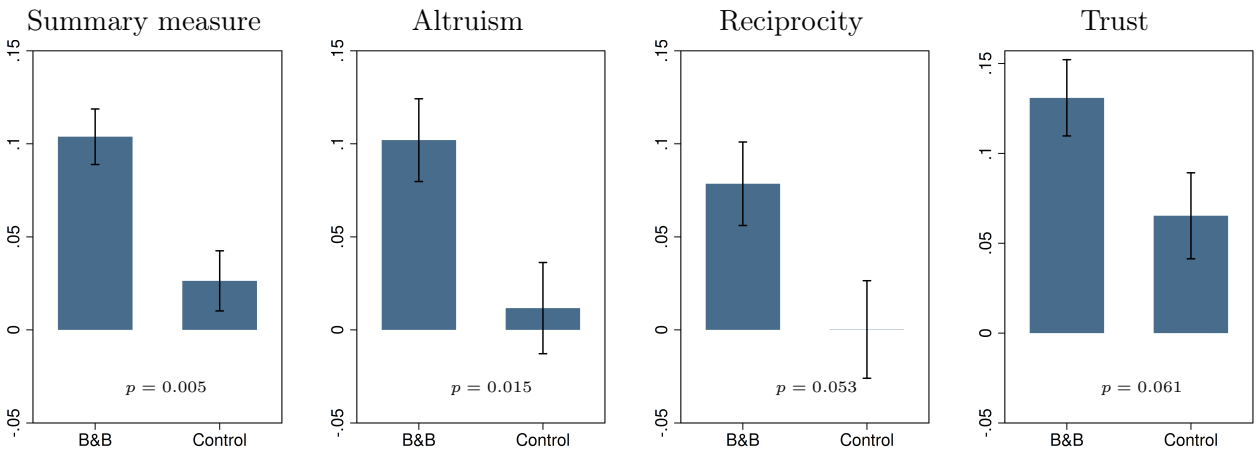
[31] For only 3 respondents the survey ended before they could rate John the second time around; we drop these observations from the analyses.

providing any further context and without mentioning what bracelets are supposed to stand for. Once again, respondents are then asked to rate John's social image.

In Figure A.5, Panel B1 shows that bracelets were indeed distributed as was intended and that bracelets are perceived to represent the pro-social activity of sharing significantly more in villages where they were handed out in return for donations. Notably, similar to food, bracelets are not perceived as a signal for wealth in neither of the treatment groups (Panel B3).

To estimate the causal effect of charitable behavior on one's social image, we analyze how wearing a bracelet affects the summary measure and its components across villages assigned to different treatment conditions. We explore within-subject variation, comparing the summary measure of social image when John wears a bracelet to when he does not, across villages where bracelets were exchanged for donations and all other villages (where bracelets are not associated with charitable behavior; see Section 4.1). Figure 9 shows that John is perceived as systematically more pro-social when he wears a bracelet only within villages where bracelets stand for donations – but not in other villages.

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



Notes: Difference between John's pro-sociality summary measure when wears a bracelet, and that when he does not. The summary measure is an equally weighted, standardized average of standardized individual measures for altruism, reciprocity, and trustworthiness (see Section 3.3.1). Standard errors clustered at the village level. P-values from Wald tests for equality of averages.

Next, Table 6 re-estimates equation 1 restricting attention to the second instance when households rate John (when he and his wife are depicted wearing red rubber bracelets). Given our experiment design, bracelets are associated with having contributed to the public donation drive only in villages assigned to *Box & Bracelets*, but not anywhere else.

The table shows that, in villages where under-15 marriage does not exist (the first-row coefficients), supporting child marriage still adversely affects John's perceived pro-sociality, just as in Table 1. While the point estimate is less negative in villages where bracelets are associated with charitable behavior (Column 1) than in control villages (Column 2), it is still very large (significant at the 1% level; column 1). Most importantly, the interaction term with the baseline prevalence of under-15 marriage is *no longer positive* in villages where bracelets imply participation in the public donation drive, while it remains large and statistically significant in control villages. Hence, among individuals who contribute to the public donation drive, there

Table 6: Signal substitution when John wears a bracelet

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

Notes: The summary measure is an equally weighted, standardized average of standardized individual measures for altruism, reciprocity, and trustworthiness (see Section 3.3.1). Regressions additionally include individual controls (female, age, age^2, age^3, and measures for own pro-sociality) plus a constant. Standard errors clustered at the village level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

is no longer a social image differential between those who conform to the local norm and those who do not, in high-prevalence villages.[32]

5.2 Signaling to whom?

One concern with the external validity of our findings is that, where the donation drive was handled by the village chief, villagers could have felt compelled to contribute. In that case, treatment effects could actually be explained by alternative mechanisms unrelated to social image, from liquidity constraints (if donations make families short of resources, since weddings are costly ceremonies; more in Section 5.3) to a desire to please local leaders (as they often privately oppose child marriage; see Henn, 2018).

In this subsection, we take advantage of an additional experiment that randomized who was in charge of the public donation drive: the village chief or the last individual listed by him/her at the baseline survey as most likely to help others in the village. We estimate heterogeneous treatment effects with respect to the identity of the box holder to study whether treatment effects are driven by the desire to signal specifically *to the chief* or to other villagers more generally.

In Table 3, columns (3) and (4) estimate treatment effects separately for the sub-sample of villages where the box holder was the village chief or another villager, respectively. We find no significant differences in treatment effects comparing Columns (3) and (4). If anything, the effect size of the public donation drive on child marriage is actually *larger* when another village was in charge of the intervention. As such, there is no evidence that treatment effects of the public donation drive are driven by a desire to signal to the chief.

[32] Although we could also use respondents' perceptions at end line to estimate a social image production function, in particular to study how donations and child marriage interact in producing social image, those associations would not be causal. Moreover, given heterogeneity by baseline prevalence of child marriage and the need to estimate the relationship separately for treatment and control villages, the model would require a very large number of interaction terms, making it hard to interpret individuals coefficients.

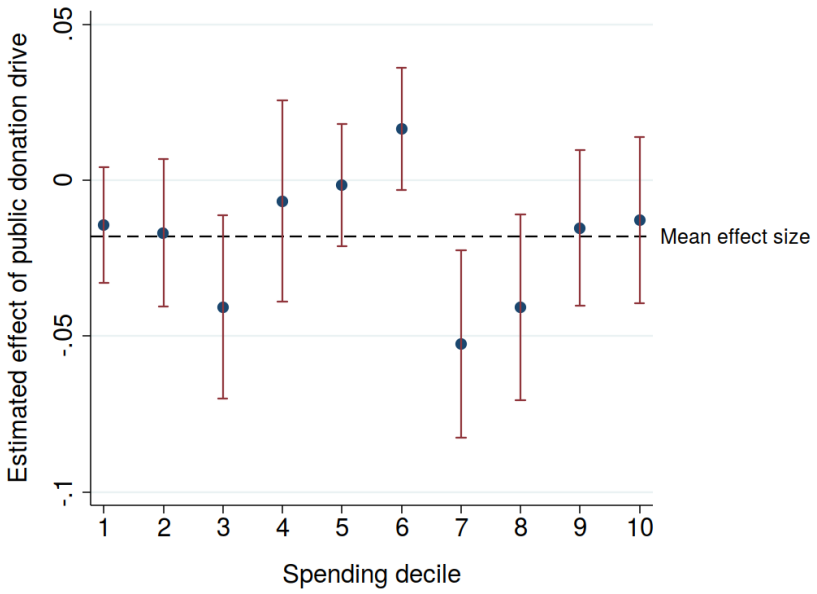
5.3 Selling brides and marriage market dynamics

Another concern with the external validity of our findings is that redistribution of the maize collected through the drive might have prevented the poorest families from having to sell-off their daughters in exchange for bride prices (Corno and Voena, 2016), now that liquidity constraints were somewhat relieved thanks to the intervention. That could have triggered lower marriage rates across the board, as marriage market unraveling is seen as an important driver of early marriages (Zaki, 2015).

To study whether that could be the case, we start by estimating heterogeneous treatment effects by household spending (measured at baseline), contrasting ultra-poor households to all others within each village. In Table 3, columns (5) and (6) estimate treatment effects separately for the 10% poorest households and the remaining 90%, respectively. We find that the effect size of the intervention is very similar for the 10% poorest and the remaining 90% – if anything, it is larger and more precisely estimated for the latter.

Next, Figure 10 estimates treatment effects on child marriage by decile of baseline household spending, based on separate regressions for each sub-sample. Treatment effects for all but one decile are negative, and most effect sizes are very close the average treatment effect in Table 2, including those for the first two deciles – presumably, those targeted by redistribution. The most negative effect sizes are actually for the 7th and 8th deciles, towards the high-end of the income distribution.

Figure 10: Treatment effects on child marriage by decile of household spending at baseline



Notes: Coefficients estimated from separate regressions for each spending decile (measured at baseline).

While we cannot rule out that marriage market dynamics are part of the general equilibrium effects triggered by the intervention, the patters that we document suggest that results are not driven by income effects from redistribution thanks to the donation drive.

5.4 Visibility of the alternative signal

We can take advantage of cross-randomization in our experiment design to test the hypothesis that treatment effects are grounded in the social image mechanism directly. When distributed in exchange for donations, bracelets make charitable behavior *even more visible* in treated villages relative to control ones.

Table 7 replicates Table 2, breaking down villages that hosted the public donation drive into those that were assigned to *Box* only and those that were assigned to *Box & Bracelets*.[33] The table clearly shows that, consistent with the social image mechanism, treatment effects are driven by the visibility of charitable behavior: while treatment effects are still negative and sizeable even in the absence of bracelets, effect sizes are much larger and precisely estimated when those are distributed in exchange for donations. Treatment effects on child marriage are almost 3-fold where the drive distributed bracelets in exchange for donations; in those villages, under-18 marriages age-by-age decrease 45% relative to the control group (significant at the 1% level; the p-value of the difference in effect sizes is 0.128, in column 1). Along the same lines, effects on childbearing are 72% higher (significant at the 5% level; column 2) and those on school dropouts are also nearly 3-fold those in villages assigned to the drive without bracelets (significant at the 5% level; column 3). While treatment effects on participation in sexual initiation rituals are still imprecisely estimated, those are over 40% higher in villages assigned to *Box & Bracelets* relative to *Box* only (column 4).

Table 7: Treatment effects of visibility of charitable behavior on girls' outcomes

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

Notes: Regressions additionally include individual controls (age, age^2, and age^3) and village-level controls (village size, population density, rural/urban status, and baseline prevalence of child marriage or initiation rituals). Standard errors clustered at the village level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Importantly, this is the case even though villages assigned to *Box & Bracelets* actually collected *less* maize than those assigned to *Box* only (Panel A1 in Figure A.5), confirming that treatment effects are driven by the social image mechanism rather than by redistribution induced by the donation drive.

[33] Table shows that baseline characteristics were balanced across these treatment arms, relative to the control group.

6 What prevents harmful traditions from changing?

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

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

In order to test which is the case, we elicit respondents' willingness to pay (WTP) for the intervention. If respondents have a low WTP for the intervention regardless of its treatment effects, that would be consistent with the hypothesis that local leaders oppose change. In turn, if WTP increases with the baseline prevalence of under-18 marriage in villages assigned to the public donation drive, that would corroborate the hypothesis that villagers did not anticipate that the intervention would unravel long-standing traditions – as its effects on child marriage increase with its baseline prevalence.

In the follow-up survey, we asked respondents to state their willingness to pay for the public donation drive through a Becker-DeGroot-Marshak (BDM) elicitation method (Becker, DeGroot and Marschak, 1964; Cornsweet, 1962). To ensure incentive compatibility, subjects were informed that decisions would be eventually implemented in 10 villages, selected through a lottery after the end-line survey.

Table 8 estimates how respondents' WTP varies across the treatment and control groups, allowing it to vary also with the baseline prevalence of child marriage within each group. Column (1) shows that, in villages without child marriage at baseline and with no prior exposure to the public donation drive, respondents are willing to pay 5,690 MWK ($\approx$ 7 USD) on average for the intervention. This is a non-negligible amount, over 1/3 of the average weekly spending by households in our sample. While WTP decreases with the share of under-18 marriages at baseline in the control group, in treated villages, WTP significantly *increases* with it: for every 10 p.p. higher baseline prevalence of under-18 marriage, prior exposure to the public donation drive increases WTP by roughly 380 MWK (significant at the 5% level). Since prior exposure to the donation drive actually leads to a *lower* WTP in villages where under-18 marriage did not exist at baseline (-1,423 MWK, significant at the 5% level), willingness to pay for the intervention increases relative to control villages where the baseline prevalence of under-18 marriage is 37.8% or higher. As the median baseline prevalence of under-18 marriage is 44%, prior exposure to the intervention significantly increases WTP for in above-median villages – precisely those where treatment effects are the largest. Last, Column (2) undertakes a placebo test, replicating the analysis for respondents' WTP for peanuts (instead of the intervention) as dependent variable.

In that case, neither prior exposure to the intervention neither its interaction with baseline prevalence of child marriage affect respondents' WTP, as expected.

Table 8: Willingness to pay for the intervention

Dependent variable:	(1) WTP for donation drive (MWK)	(2) WTP for peanuts (MWK)
Public donation drive	-1,423**	-7.075
	(654.0)	(8.086)
Public donation drive × Share married < age 18	3,764**	25.59
	(1,907)	(24.43)
Share married < age 18	-1,814	-25.66
	(1,369)	(18.58)
Constant	5,690***	75.62***
	(488.9)	(5.863)
Observations	733	733
District fixed effects	✓	✓
Enumerator fixed effects	✓	✓

Notes: 733 out of 736 respondents surveyed were able to pass the test-round WTP elicitation, what explains the smaller sample size. Standard errors clustered at the village level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The evidence is inconsistent with the hypothesis that local leaders purposefully block the change of traditions.[34] Rather, it suggests that villagers privately dislike the traditional practice, but do not anticipate the effects of alternative signals. Prior exposure makes respondents in high-prevalence villages happy to pay for an intervention that successfully disrupts the baseline separating equilibrium based on child marriage while still sustaining long-term cooperation.

7 Concluding Remarks

This paper provides first-hand evidence that individuals engage in traditional practices that effectively destroy children's human capital, like child marriage and sexual initiation rituals, at least partly out of social image concerns. We further document that making an alternative social signal more visible allows villagers to act on their private motives without compromising their social image even when that means defying the local norm. The public donation drives greatly impacted charitable behavior – including the profile of those who donate –, and ultimately allowed individuals to abandon long-standing traditions that they privately opposed.

Bursztyn and Jensen (2017) outline the mechanisms through which interventions might shape the effects of social pressure. One possibility is that our intervention discourages child marriage because it makes it harder to *infer types from behavior*, as donation drives increase the expected share of cooperative individuals in the village – potentially breaking down a baseline separating equilibrium that relied on child marriage as a signal in high-prevalence villages. A different possibility is that our intervention *manipulates second-order beliefs*, as villagers in the treatment group realize that child marriage is no longer needed to sustain a positive social image

[34] See Haenni and Lichand (2021) for a more involved discussion of when local elites matter for social norms' change.

if they can showcase pro-sociality by contributing to the drive. A final possibility is that our intervention affects the extent to which people *care about others' opinions of them*; after all, if perceived charitable behavior increases in the village, the likelihood of receiving unconditional help potentially increases as well. These possibilities are not mutually exclusive; in fact, the impacts of the public donation drive on child marriage likely operate through all of the above.

Our findings might help explain why support for traditional practices (from child marriage to female genital cutting) is decreasing at a fast pace in some parts of the world (see UNICEF's Demographic Health Surveys). Rapid urbanization across Sub-Saharan Africa and Southeast Asia may be giving rise to less trust-intensive societies, in line with the historical trajectories of developed and developing countries (Greif, 1993; Munshi and Rosenzweig, 2006). When there is less need to identify who is pro-social – because there are other mechanisms to enforce cooperation, like legal courts –, the signaling value of conforming to traditional practices decreases, even in the absence of alternative signals. That rationale is also consistent with the claim that urban anonymity causes some institutions to break down (Glaeser, 2014).

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

Having said that, social engineering norms at scale is a challenging endeavour. First, general equilibrium effects might crowd out the value of alternative signals when they are adopted at scale (Butera et al., 2019). Second, the effects of manipulating the production function of social image might cut across multiple dimensions – as we show for the case of child marriage and sexual initiation rituals in Malawi –, possibly crowding out unanticipated (and, perhaps, even desirable) behaviors. The net effect of social engineering might be hard to predict, as it depends on whether different norms are complements or substitutes (Feng, Haenni and Lichand, 2021; Platteau, Camilotti and Auriol, 2018).

Are the effects of the public donation drive bound to persist in the long-term? The effect of the intervention on charitable behavior seems to fade-out over time, as differences across treatment and control villages in donations over the previous month are much less pronounced than in donations over the previous year. If differences eventually disappear, it could as well be that old traditions re-emerge in villages that used to be high-prevalence. Having said that, there are reasons to believe that this is unlikely to happen. First, treatment effects on help-line calls do not decay over time, up to 2.5 years after the intervention. Second, treatment effects on willingness to pay for the intervention corroborate that individuals privately dislike child marriage even where it is the local norm, and come to appreciate the effect of the intervention after they have been exposed to it. WTP for the donation box and red rubber bracelets is high, suggesting that local leaders could keep steering community engagement moving forward through new drives or other sources of collective action – offering villagers opportunities to build and protect their social image through means that do not harm girls in the process.

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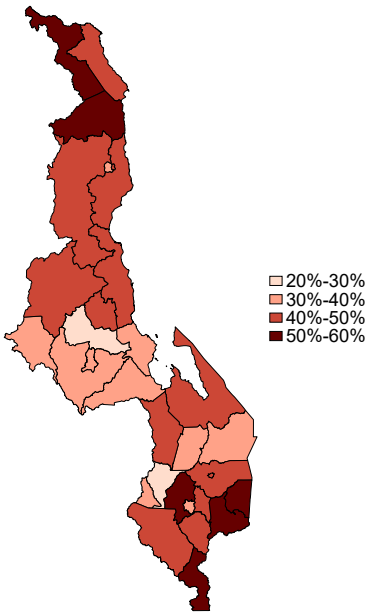
Appendix A Supplementary Figures

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

Panel A: Prevalence of Girls' Marriage

Share of ever married women who married before age 18



Panel B: Prevalence of Sexual Initiation Rituals

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

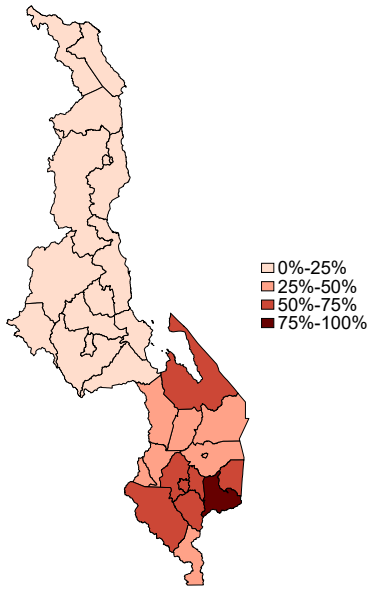
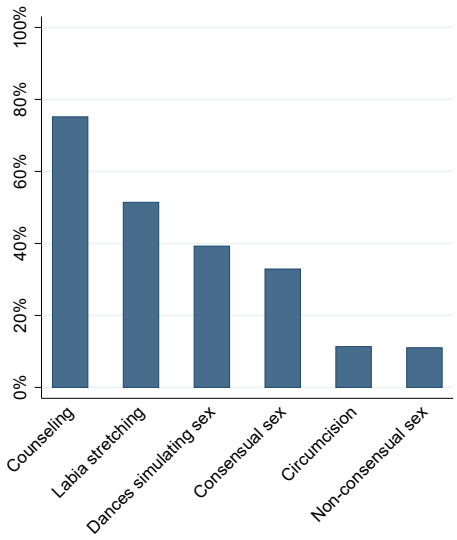


Figure A.2: Activities and decision makers of sexual initiation rituals of girls in Malawi. Share of respondents mentioning each category in multiple-choice questions.

Panel A:
Activities at Sexual Initiation Rituals



Panel B:
Decision Makers of Participation at Rituals

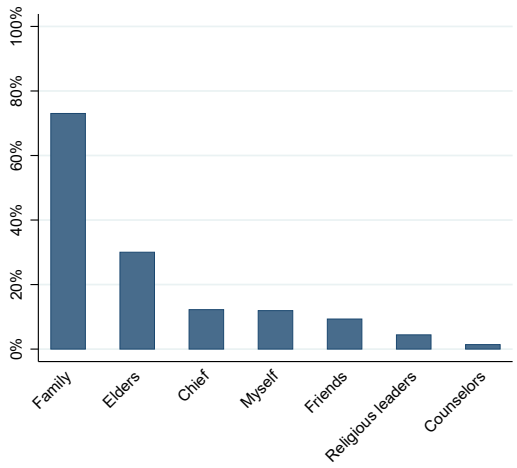


Figure A.3: Framing John as (not) supporting child marriage

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

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

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

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

Figure A.4: Compliance with Experiments

Panel A: Donation drive

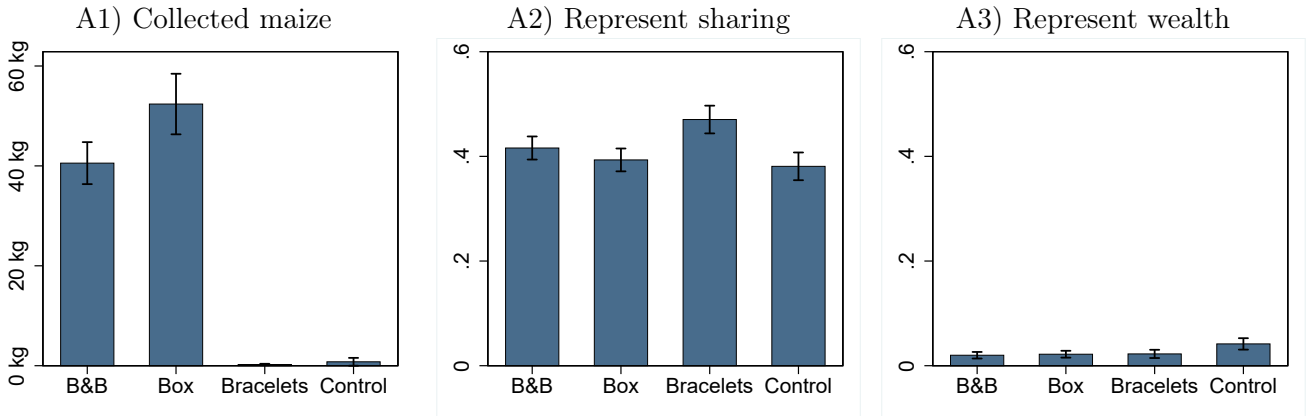
		Box: Yes	Box: No
Bracelets	Yes	89%	87%
	No	92%	96%

Panel B: Vignette experiment

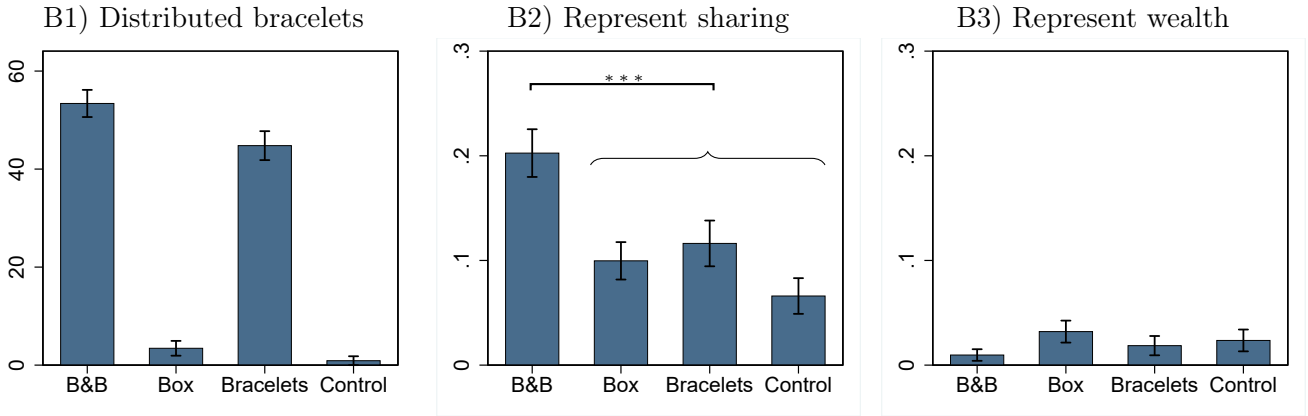
Father supports child marriage: Yes	Father supports child marriage: No
100%	100%

Figure A.5: Manipulation checks after 5 weeks

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

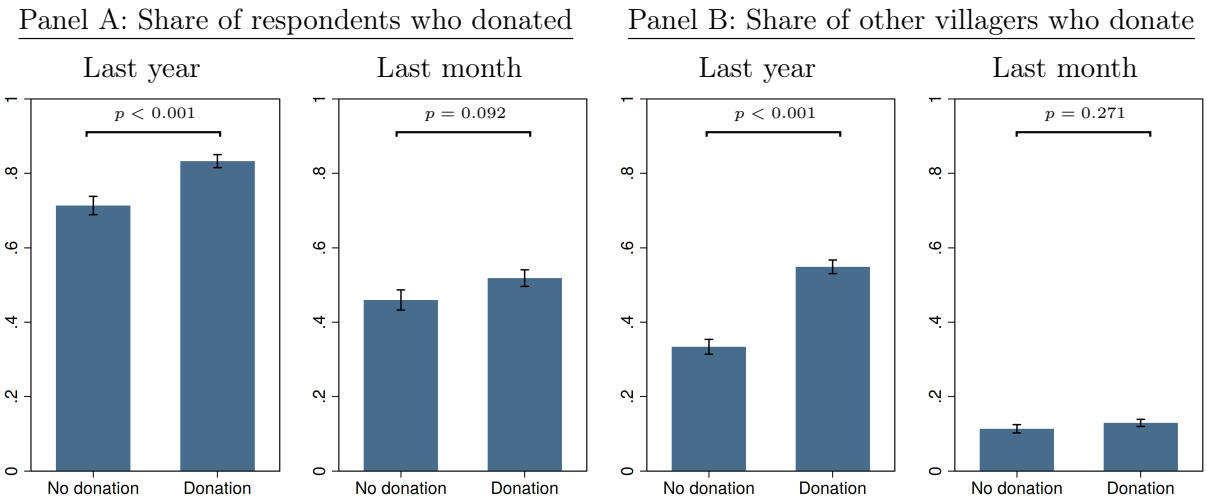


Panel B: Take-up and Public Perception of Red Rubber Bracelets



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

Figure A.6: Treatment effects on charitable behavior, 16 months after the intervention



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

Appendix B Supplementary Tables

Table B.1: Summary statistics of the baseline sample

Number of households	7,388
Household size	4.64
Share of rural households	0.82
Village size (#HH)	116
Villages	412

Table B.2: Balance of baseline covariates in the RCT

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

Notes: Sub-sample means with standard deviations in parentheses. P-values from ANOVA tests of joint significance based on standard errors clustered at the village level.

Table B.3: Balance of covariates in the vignette experiment

	(1)	(2)	(3)
	Supports child marriage		
Variable	No	Yes	Diff.
Urban	0.177	0.181	0.005
	(0.382)	(0.385)	(0.009)
Village size (#HH)	116.360	116.532	0.172
	(31.097)	(30.805)	(0.742)
Number of surveyed HH	18.488	18.501	0.014
	(1.670)	(1.666)	(0.040)
Distance to neighbor (km)	0.024	0.023	-0.001
	(0.019)	(0.018)	(0.000)
Weekly food consumption ($)	11.422	11.222	-0.201
	(8.641)	(8.431)	(0.204)
Weekly non-food consumption ($)	7.249	7.169	-0.080
	(7.296)	(7.112)	(0.172)
Household size	4.943	4.954	0.011
	(2.048)	(1.988)	(0.048)
Female	0.587	0.589	0.001
	(0.492)	(0.492)	(0.012)
Age	35.846	35.353	-0.492
	(16.553)	(16.514)	(0.396)
F-test of joint significance (p-val.)			0.698
Observations	3,468	3,510	6,978

Notes: Sub-sample means with standard deviations in parentheses. P-values based on standard errors clustered at the village level.

Table B.4: Completion rates and balance of covariates in follow-up sample

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

Notes: Sub-sample means with standard deviations in parentheses. P-values based on standard errors clustered at the village level.

Table B.5: Correlation between self-reported pro-social preferences and revealed donations in the control group

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

Notes: The summary measure (column 1) is an equally weighted average of standardized individual measures for altruism (column 2), reciprocity (column 3), and trustworthiness (column 4); see Section 3.3.1. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.6: Summary statistics, by whether one had recently donated to help others

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

Notes: Individual statistics conditional on self-reported donation to recent food collection. Columns (1)-(3) show statistics for villages where no food collection box was distributed (*Bracelets* and *Control*) while columns (4)-(6) show statistics for villages where food collection boxes where distributed (*Box* and *Box&Bracelets*). Number of observations is lower as the question about charitable behavior was only asked to a subset of households.

Table B.7: Correlation between social image and charitable behavior

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

Notes: The summary measure is an equally weighted standardized average of standardized individual measures for altruism, reciprocity, and trust (see Section 3.3.1). Standard errors clustered at the village level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.8: Treatment effects omitting village chiefs' responses

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

Notes: Regressions include individual controls (age, age^2, and age^3) and village-level controls (village size, population density, rural/urban status, and baseline prevalence of child marriage or initiation rituals) and enumerator fixed-effects. Standard errors clustered at the village level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.9: Treatment effects omitting girls with conflicting information

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

Notes: Regressions include individual controls (age, age^2, and age^3) and village-level controls (village size, population density, rural/urban status, and baseline prevalence of child marriage or initiation rituals) and enumerator fixed-effects. Standard errors clustered at the village level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.10: Placebo tests – Treatment effects on pre-determined outcomes

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

Notes: Regressions include individual controls (female, age, age^2, and age^3) and village-level controls (village size, population density, and rural/urban status. Standard errors clustered at the village level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.11: Treatment effects on support for traditional practices

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

Notes: Regressions include individual controls (female, age, age^2, and age^3) and village-level controls (village size, population density, and rural/urban status. Standard errors clustered at the village level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.12: Heterogeneous treatment effects on support for traditional practices (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**					-0.0203			
	(0.00740)					(0.0136)			
HH w/o girls × Donation	-0.0146*					-0.0139			
	(0.00767)					(0.0109)			
HH w/ girls	-0.0133*					-0.00695			
	(0.00761)					(0.0116)			
Female × Donation		-0.0214***					-0.0239*		
		(0.00782)					(0.0123)		
Male × Donation		-0.00869					-0.00411		
		(0.00738)					(0.0133)		
Above median prevalence× Donation			-0.0276***					-0.0208	
			(0.00802)					(0.0195)	
Below median prevalence × Donation			0.000344					-0.0129	
			(0.00750)					(0.0105)	
Above median prevalence			0.0280***					0.0238	
			(0.00854)					(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				0.00429
					(0.0154)				(0.0228)
All other households × Donation					-0.0151**				-0.0172*
					(0.00602)				(0.00988)
10% poorest					0.0127				-0.00275
					(0.0121)				(0.0177)
Control mean			0.054			0.077			
1st-stage F-stat of instrument	491.4	468	223.4	427.4	356.7	472.4	437.4	343.2	329.9
Chi^2-tests (p-val.):									
HH w/ = w/o girls	0.697					0.675			
Female = Male		0.202					0.251		
Above = Below median prev.			0.011					0.722	
Tradition = Emergency				0.384					
Poor=Non-poor					0.623				0.368
Observations	11,123	11,123	11,123	2,577	11,038	7,243	7,243	7,243	7,183
Additional controls	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Regressions include individual controls (female, age, age^2, and age^3) and village-level controls (village size, population density, rural/urban status, and local prevalence of sexual initiation in columns (3) and (4)), plus a constant. Standard errors clustered at the village level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.13: Treatment effects of child marriage on social image in the vignette experiment

		Individual components		
Dependent variable:	Summary Measure	Altruism	Reciprocity	Trustworthiness
Pro-social preferences attributed to John	(1)	(2)	(3)	(4)
John supports child marriage	-0.905***	-0.612***	-0.563***	-1.110***
	(0.0712)	(0.0628)	(0.0744)	(0.0769)
John supports child marriage	1.129***	0.795***	0.796***	1.259***
× Share married < 18	(0.281)	(0.257)	(0.289)	(0.299)
Individual controls	✓	✓	✓	✓
Village fixed effects	✓	✓	✓	✓
Observations	6,978	6,978	6,978	6,978

Notes: The summary measure is an equally weighted, standardized average of standardized individual measures for altruism, reciprocity, and trustworthiness (see Section 3.3.1). Regressions include individual controls (female, age, age^2, age^3, and measures for own pro-sociality). Standard errors clustered at the village level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix C Average Treatment Effects on the Treated (ATT)

This appendix estimates local average treatment effects on the treated (ATT), given imperfect compliance with the RCT.

We estimate two-stage least squares regressions, using treatment assignment as an instrument for actual treatment implementation (Angrist and Pischke, 2008):

$$\begin{aligned} &1^{st} \text{ stage: } \text{ActualDrive}_{ihv} = \eta_0 + \eta_1 \text{AssignedDrive}_v + \eta_2 \text{X}_{ihv} + \xi_{ihv} \\ &2^{nd} \text{ stage: } Y_{ihv} = \beta_0 + \beta_1 \widehat{\text{ActualDrive}_{ihv}} + \beta_2 \text{X}_{ihv} + \varepsilon_{ihv}, \end{aligned} \tag{6}$$

where $\widehat{\text{ActualDrive}_{ihv}}$ are predicted assignments for whether individual i in household h at village v was actually exposed to a public donation drive. Results for age-by-age girls' outcomes at end-line and for support for traditional practices at baseline, presented in tables C.1 and C.2, respectively, are very similar to average treatment effects based on intention to treat (ITT).

Table C.1: Treatment effects on girls' outcomes age-by-age (IV)

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

Notes: Regressions include individual controls (age, age^2, and age^3) and village-level controls (village size, population density, rural/urban status, and baseline prevalence of child marriage or initiation rituals) and enumerator fixed-effects. Standard errors clustered at the village level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.2: Treatment effects on support for traditional practices (IV)

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

Notes: Regressions additionally include individual controls (female, age, age^2, and age^3) and village-level controls (village size, population density, and rural/urban status). Standard errors clustered at the village level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix D Pre-analysis Plans

This paper is part of a larger research project that was jointly pre-registered as trial AEARCTR-0002856 in the AEA RCT Registry. We pre-registered one analysis plan before the roll-out of the intervention and baseline data collection, and another one before the end-line survey. In this appendix, we present those pre-analysis plans in full, in subsections D.2 and D.3, preceded by a discussion of significant deviations from pre-registration in subsection D.1.

D.1 Deviations from the pre-analysis plans

At baseline, we managed to visit 412 villages out of the 413 that had been randomly selected from an existing sampling frame. In the 1 missing village, there were ongoing disputes about chieftaincy that made it impossible to conduct listings. At end line, we tried to revisit 411 of those, as 2 villages had merged since (being subsequently counted as 1 village).

We did not pre-specify our analyses of treatment effects on charitable behavior. Having said that, these analyses are useful in documenting that the public donation drives actually collected donations and spurred charitable behavior relative to the counterfactual as intended, and that treatment effects persisted even over a year into the intervention. We also did not pre-specify that we would compute cumulative treatment effects on child marriage and girls' outcomes under the assumption that effect sizes persist over time. These analyses are, nevertheless, only included to help benchmark the economic relevance of our estimates, and are computed directly from regression estimates that were pre-specified. Next, we did not pre-specify that we would analyze heterogeneous treatment effects by baseline spending. Having said that, these analyses are useful in helping us rule out that treatment effects were merely driven by redistribution of maize through the public drives. We also did not pre-specify some of the robustness checks that we undertake. Specifically, we did not pre-register how we would drop observations to assess the sensitivity of the end-line data to the quality of third-party reports, and did not pre-register that we would analyze treatment effects on help-line calls. We reached out to the local NGO about their call logs only later, in an attempt to address concerns with experimenter demand effects with the help of independent data.

Conversely, some analyses that we had pre-registered are not covered in this paper, but rather shown in Haenni and Lichand (2021). That paper studies the role of local elites for social norms' change; as such, we defer additional analyses of heterogeneous treatment effects by box holder's identity to the companion paper. We also analyze the effects of a self-esteem priming that was pre-registered in that paper, in the context of estimating treatment effects on chiefs' short-run attitudes towards child marriage and sexual initiation rituals.

D.2 Pre-analysis plan registed before baseline

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

1 Interventions and Experiments

1.1 Signaling Intervention

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

1.1.1 Hypotheses

With this intervention we test the following main hypotheses:

- Does introducing a new signaling language, i.e. a less harmful signaling opportunity, decrease support for other, more harmful signals?

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

- Do chiefs enforce harmful social norms as a signaling mechanism because they exercise control over it?

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

1.1.2 Experimental Design

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

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

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

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

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

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

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

Cross-randomize

Managed by village chief (C_c =1)	**Managed by most pro-social person** (C_c =0).
E: $^1/_2$N (207 villages)	F: $^1/_2$N (206 villages)

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

1.2 Self-esteem Intervention (E_h)

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

1.2.1 Hypotheses

With this intervention we test the following main hypotheses:

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

- Does self-esteem affect social desirability bias?

1.2.2 Experimental Design

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

Treatment (E_h =1)	**Control** (E_h =0)
$^1/_2$N (4130 households)	$^1/_2$N (4130 households)

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

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

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

1.3.1 Hypotheses

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

1.3.2 Experimental Design

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

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

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

Condition 1 ($J_h^1 = 1$)	**Condition 2** ($J_h^1 = 0$)
$^1/_2$N (4130 households)	$^1/_2$N (4130 households)

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

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

1.4 List Experiments

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

1.4.1 Hypotheses

- Is there social desirability bias involved in reporting of attitudes and (planned) engagement in harmful traditional practices?

- Can we measure individual-level susceptibility to social pressure in list experiments?

- Does self-esteem affect attitudes towards harmful traditional practices or exclusively reporting thereof?

1.4.2 Experimental Design

Condition 1	**Condition 2**
$^1/_2$N (4130 households)	$^1/_2$N (4130 households)

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

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

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

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

1.4.3 Validation Measures

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

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

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

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

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

2 Outcomes

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

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

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

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

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

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

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

(iii) Prevalence of harmful traditional practices: *Y*

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

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

3 Analysis Plan

3.1 Harming to Signal

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

$$Y_i = \beta_0 + \beta_1 Prosociality_i + e_i$$

$$A_i = \beta_0 + \beta_1 Prosociality_i + e_i$$

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

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

$$Y_{iv} = \beta_0 + \beta_1 Prosociality_i + \beta_2 Harmful\_Norm_v + \beta_3 Prosociality_i Harmful\_Norm_v + e_{iv}$$

$$A_{iv} = \beta_0 + \beta_1 Prosociality_i + \beta_2 Harmful\_Norm_v + \beta_3 Prosociality_i Harmful\_Norm_v + e_{iv}$$

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

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

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

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

(iii) IV:

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

We will come back one year after the baseline data collection and look at actual change in behavior. The pre-analysis plan will be updated accordingly at this position at a later stage.

3.2 Authority vs Norms

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

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

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

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

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

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

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

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

3.2.3 Perceived powers

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

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

3.2.4 Reputational concerns by the chiefs

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

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

3.3 Effects of liquidity constraints and marriageability concerns

3.3.1 Effect of liquidity constraints on harmful traditional practices

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

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

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

3.3.2 Are child marriage and initiation rituals complements or substitutes?

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

1. Stage:

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

2. Stage:

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

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

D.3 Pre-analysis plan registed before end line

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

1 Sampling

Respondents: 411 village chiefs and 411 other villagers.[1]

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

2 Interventions and Experiments

2.1 Signaling Intervention

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

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

2.2 Willingness-to-pay for signaling intervention

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

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

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

3 Outcomes

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

(i) Emergence of alternative signaling strategies

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

Specific outcomes:

- Frequency of food collections for needy households (answer intervals of 3 month averaged over 2 respondents in village): FC_v

- Share of households in village contributing to food collections in last year/ last month (answer intervals of 10% averaged over 2 respondents in village): $ShareFC_v^M, ShareFC_v^Y$

- Contributions to food collections of households with girls at risk (contributed to food collections in last month/year? → binary): FC_{hv}^M, FC_{hv}^Y

- *Importance of traditions in village (scale 0-10):* $ImportanceTradition_{rv}$

- *Publicity of traditions: scale 0-3 → create dummy for >=2.:* $PubliclyTradition_{rv}$

- Change of importance of local traditions (more important, less important, equally important): $ChangeTradition_{rv}$

- Willingness to pay for public donation intervention (0-10,000 MWK). We remove observations if the enumerator states that respondent did not understand WTP instructions, even after repeated explanations and a practice round: WP_{rv}

(ii) participation in traditional practices

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

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

- Change of child marriage (<15, <18) /sexual initiation frequency, as perceived by respondent (more common, less common, equally common): $\left(CM^{R}_{rv}, SI^{R}_{rv}\right)$

(iii) social image of household heads

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

Main measure of social image (adapted versions from Falk et al's (2016) social preference module)
- Altruism (scale 0-10): *"How willing is **(head)** to give to good causes without expecting anything in return?"*
- Reciprocity (scale 0-10): *"When someone does **(head)** a favour, **(head)** is willing to return it."*
- Trust(scale 0-10): "**(head)** assumes that people have only the best intentions."
→ Here, we are interested on the joint measure of pro-sociality, containing altruism, reciprocity and trustworthiness: $Image_{hrv}$

Other household characteristics

- Support for child marriage (scale 0-10): $Support^{CM}_{hrv}$

- Religious attendance (Never, weekly, monthly, yearly, less than yearly): $Church_{hrv}$

- Support for sexual initiation rituals (scale 0-10): $Support^{SI}_{hrv}$

- Perceived to follow local traditions even if they harm children (scale 0-10): $SupportTradition_{hrv}$

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

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

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

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

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

4 Analysis Plan

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

4.1 Social signaling

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

Main specifications:

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

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

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

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

$$\rightarrow \beta_2 > \gamma_2\,?$$

Alternative specifications:

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

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

- Alternative dependent variable: $ReceiveHelp_{hrv}$

4.2 Substituteability of signaling strategies

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

Manipulation check: long-term compliance with the treatment

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

$$FC_v = \beta_0 + \beta_1 DonationTreatment_v + \epsilon_v \rightarrow \beta_1 > 0\,? \quad (4)$$

$$ShareFC_v = \beta_0 + \beta_1 DonationTreatment_v + \epsilon_v \rightarrow \beta_1 > 0\,? \quad (5)$$

ITT analysis

Main:

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

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

Additional:

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

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

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

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

IV analysis

1st stage: $FC_v = \alpha_0 + \alpha_1 DonationTreatment_v + \alpha_2 X_{ihv} + \alpha_3 Z_v + \epsilon_{ihv}$ (12)

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

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

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

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

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

ITT analysis

$$CM^{A}_{hrv} = \beta_0 + \beta_1 DonationTreatment_v + \beta_2 X_{hv} + \beta_3 Z_v + \beta_4 W_{rv} + \epsilon_{hrv} \rightarrow \beta_1 < 0\,? \quad (14)$$

$$SI^{A}_{hrv} = \beta_0 + \beta_1 DonationTreatment_v + \beta_2 X_{hv} + \beta_3 Z_v + \beta_4 W_{rv} + \epsilon_{hrv} \rightarrow \beta_1 < 0\,? \quad (15)$$

IV analysis

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

2nd stage: $Y_{hrv} = \beta_0 + \beta_1 \widehat{FC_{hrv}} + \beta_2 X_{hv} + \beta_3 Z_v + \alpha_4 W_{rv} + \epsilon'_{hrv} \;\; for \;\; Y_{hrv} \in \left[CM^{A}_{hrv}, SI^{A}_{hrv}\right] \rightarrow \beta_1 < 0\,?$ (17)

Alternative endogenous variables if first stage is weak with FC_v: $ShareFC^{M}_{v}, ShareFC^{Y}_{v}$

Heterogeneity:

Low vs high prevalence villages at baseline (median split)

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

Main:

$$ImportanceTradition_{rv} = \beta_0 + \beta_1 DonationTreatment_v + \beta_2 X_{rv} + \beta_3 Z_v + \epsilon_{rv} \rightarrow \beta_1 \neq 0\,? \quad (18)$$

$$ChangeTradition_{rv} = \beta_0 + \beta_1 DonationTreatment_v + \beta_2 X_{rv} + \beta_3 Z_v + \epsilon_{rv} \rightarrow \beta_1 \neq 0\,? \quad (19)$$

Additional:

$$PubliclyTradition_{rv} = \beta_0 + \beta_1 DonationTreatment_v + \beta_2 X_{rv} + \beta_3 Z_v + \epsilon_{rv} \rightarrow \beta_1 \neq 0\,? \quad (20)$$

Heterogeneity:

Low vs high prevalence villages at baseline (median split)

4.2.4 Type of pooling equilibrium

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

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

$$\rightarrow \beta_1 \neq 0\,? \qquad (21)$$

4.3 Targeting elites for social norms change

4.3.1 Willingness to pay for social norms change

Main analyses:

Does WTP depend on the DonationTreatment implemented at baseline?

$$\mathrm{WP}_{rv} = \beta_0 + \beta_1 DonationTreatment_v + \beta_2 X_{rv} + \beta_3 Z_v + \epsilon_{rv} \rightarrow \beta_1 \neq 0\,? \qquad (22)$$

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

$$\mathrm{WP}_{rv} = \beta_0 + \beta_1 Chief_v + \beta_2 X_{rv} + \beta_3 Z_v + \epsilon_{rv} \rightarrow \beta_1 \neq 0\,? \qquad (23)$$

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

$$\mathrm{WP}_{rv} = \beta_0 + \beta_1 DonationTreatment_v + \beta_2 Chief_v + \beta_3 DonationTreatment_v * Chief_v + \beta_2 X_{rv} + \beta_3 Z_v + \epsilon_{rv}$$

$$\rightarrow \beta_3 \neq 0\,? \qquad (24)$$

Additional analyses:

- Heterogeneous treatment effects, depending on rate of child marriage and sexual initiation rituals at baseline and at endline.

- Analyze chief and others separately. Do they differ?

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

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

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

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

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

Main:

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

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

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

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

Additional:

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

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

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

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

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

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

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

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

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