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Can the law affect attitudes and behavior in the absence of strict enforcement? Experimental evidence from a child marriage reform in Bangladesh

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ABSTRACT

In developing countries, one in four girls is married before turning 18, with adverse consequences for themselves and their children. In this article, we investigate whether laws can affect attitudes and behavior toward child marriage—in a context in which the laws are not strictly enforced. We do so by developing a simple theoretical model of marriage age choice which allows us to account for several potential mechanisms through which a change in the formal law may affect attitudes and behavior even when the law is not enforced. We also implement a randomized video-based information intervention that aimed to accelerate knowledge transmission about a new child marriage law in Bangladesh that introduced harsher punishments for facilitating early marriage. Surveys conducted immediately after the intervention document changes in respondents' attitudes while follow-up surveys conducted several months later document an increase in early marriage among treated households—but only if the father or family elders also received the information. The findings allow us to distinguish between several competing theoretical channels underlying the effect of legal change and highlight the risk of backlash against laws that contradict traditional norms and practices. (*JEL* J12, J16, K36)

1. INTRODUCTION

The practice of female early marriage is ubiquitous among women in developing countries, with about one in four marrying before the age of 18, typically in their adolescence (UNFPA 2020). Recent work has shown that the practice has adverse consequences both for the women who experience it and for their families, in the form of lower educational

investments, lower human capital investments in the next generation, adverse health effects from early child bearing and worse social networks.[1]

Most countries have a legal minimum age of marriage although exceptions are usually allowed, typically when parents, a judge or a community elder give consent (UNFPA 2012; Pew Research Center 2016). A number of countries have recently raised the minimum age of marriage and/or introduced harsher penalties for early marriage. But, given the problem of weak law enforcement capacity in developing countries, it is not clear whether such legal changes can be effective, particularly when laws conflict with social norms, depriving them of the support and cooperation of the local population (Platteau and Wahhaj 2014; Acemoglu and Jackson 2017). In South Asia, for example, there are strong social pressures to marry from the onset of puberty (Ortner 1978; Dube 1997) and it is this custom rather than the law which often dictates the age at which a woman marries.

Nevertheless, some legal theorists have argued that the law can influence behavior even in the absence of enforcement. For example, the law may have an "expressive effect" (i.e. it may shape behavior by "sending a message about society's values"), independent of any deterrence effect from legal punishment (Sunstein 1996; McAdams 2000a; Benabou and Tirole 2012). Others have hypothesized that the interaction between formal laws and informal institutions may be even more complex. For example, legal penalties can influence the inferred prosociality of non-compliant behavior and thus affect social behavior in the presence of social image concerns (Benabou and Tirole 2012; Lane et al. 2023). Formal laws which strongly conflict with prevailing social norms may also cause customary authorities in traditional communities to ignore or defy them while more moderate legal reforms may be more effective in changing customary practices (Aldashev et al. 2012a, 2012b; Acemoglu and Jackson 2017).

In this article, we use the case of child marriage to investigate whether the law can influence social attitudes and behavior—in a setting in which enforcement is absent or weak. Specifically, we have three main research questions. First, does the law have an "expressive effect"? Second, can moderating elements of a new law make it more effective in changing attitudes and behavior? Third, how do changes in the formal law affect the attitudes and behavior of the customary authority? We aim to answer these questions by first developing a theoretical model in which families with marriageable daughters make a marriage age choice, taking into account both the customary age of marriage and the legal minimum marriage age. The model takes account of several of the potential mechanisms described above and offers specific predictions regarding the expected effects of a change to child marriage law. Second, we implement a video-based intervention which provided information to adults in rural Bangladesh about a recent change to child marriage law.[2]

The new law in Bangladesh—around which our intervention is based—introduced two key changes: (a) harsher punishments for facilitating underage marriage and (b) a special clause that permitted marriage at any age if a court deemed this to be "in the best interests of the minor". The introduction of harsher punishments increased the contradiction between the formal law and existing practices given that nearly three-fifths of women in Bangladesh still marry below the legal minimum age (further details in Section 2). But the inclusion of a special clause permitting traditional marriage practices under certain (unspecified) circumstances was ostensibly an attempt by legislators to moderate the legal reform.

[1] See Field and Ambrus (2008), Sekhri and Debnath (2014), Chari et al. (2017), Amin et al. (2018), Asadullah and Wahhaj (2019), Sunder (2019).

[2] The law was approved in the national parliament of Bangladesh in March 2017, while our intervention was conducted in June 2018.

The video intervention took the form of a short fictional drama involving the early marriage of an adolescent girl that the study respondents viewed on a handheld electronic device. The control group watched a version of the drama that made reference only to the previous child marriage law. To disentangle the effects of the more severe and moderating elements of the new law on attitudes and behavior, we experimentally varied the legal information provided to treated participants: the first treatment group (henceforth, "Treatment 1") watched a version of the video that referenced the new child marriage law, specifically the introduction of harsher punishments for facilitating early marriage; a second treatment group ("Treatment 2") watched an alternative version of the video that referenced both the harsher punishments and the special clause. Apart from these informational differences, the three versions of the video were, shot by shot, nearly identical. To understand whether and how households' responses to the new law were shaped by concerns about marriage customs and customary authority, we also varied whether the information was given exclusively to mothers of adolescent girls in participating households or, additionally, to family elders—who have primary responsibility within the extended family for ensuring adherence to these customs.

Immediately following the information intervention, we measured a range of outcomes for study participants. These included participants' own views on appropriate marriage customs as well as their beliefs about prevailing attitudes toward early marriage in their communities. We conducted follow-up interviews after five and ten months to collect information on actual marriage outcomes for adolescent girls who were unmarried at the time of the intervention.

Our results are striking. First, in the case of actual marriage outcomes for adolescent girls in treated households, we find that Treatment 1 (the treatment in which participants are only informed about the new harsher punishments for facilitating early marriage) *increased* the probability of marriage by 7 percentage points 10 months after the intervention.[3] The point estimates for Treatment 2 (in which participants were additionally informed about the special clause in the new law) are also positive for this outcome but much smaller in magnitude and statistically insignificant. Estimation of a marriage hazard model reveals that Treatment 2 increased the risk of marriage only after the implementation of the new law while the corresponding estimates for Treatment 1, although statistically insignificant, are larger during the initial period between the intervention and the legal implementation compared to the post-implementation period. We also find that these perverse effects of the information intervention are absent in households in which only the mother of the adolescent girl views the treatment video, but are large and statistically significant when the video is viewed by both the mother and (separately) by the father or a family elder.

Turning to the effects on reported attitudes, we find no effect of either treatment on the appropriate female marriage age stated by female respondents. However, male respondents report a *lower* appropriate marriage age by 8–10 months on average (with no significant difference between the two treatments). Treatment 1 also decreased perceptions that neighbors and family elders would approve of delayed marriage for adolescent girls. The corresponding estimates for Treatment 2 are also negative but are smaller in magnitude and less statistically robust.

These findings are contrary to the "expressive effect" hypothesis which, in our context, would imply that Treatment 1—relative to both Treatment 2 and the control group—should increase perceptions that neighbors and family elders approve of delayed marriage. In

[3] This effect first shows up at our five-month follow-up phone survey, and persists through our second and last follow-up phone survey, 10 months after the intervention.

fact, we find the opposite. We argue that the estimates from the marriage hazard model are consistent with parents rushing to marry their daughters in anticipation of an imminent increase in legal punishments for underage marriage—an expectation produced by the receipt of information about the harsher punishments in the new law. Furthermore, family elders may have responded to the information about the harsher punishments in the new law by reverting to a more traditional position due to the perception that the formal law was too remote from their own preferences and beliefs regarding the appropriate female marriage age (Aldashev et al. 2012a). Such a shift in position by family elders would also have increased pressure on parents to marry off their underage daughters. Information about the special clause aimed at moderating the legal reform appears to have reduced rushing by parents and backlash from family elders but did not produce a positive shift in attitude and behavior compared to the status quo.

This article provides causal evidence that the content of the law matters for shaping attitudes and behavior, even in the absence of enforcement. In this way, it builds on the seminal work of Chen and Yeh (2014), who conduct a lab experiment to show that merely providing individuals with information about the formal law (in their case, having study participants read liberal or conservative court decisions) can be sufficient to shift their reported attitudes toward moral or social norms. Relatedly, Lane et al. (2023) elicit prevailing social norms using vignette-based experiments and provide evidence of discrete changes in perceived social appropriateness of behaviors around legal thresholds, that they attribute to the influence of laws on norms. In contrast to these studies, we conduct a field experiment, measure impacts on actual behaviors (e.g. marriage outcomes) in addition to stated preferences, probe the interaction between the formal law and the customary authority, and additionally investigate the effect of strict versus moderate changes in the law.

Perhaps most importantly—from a policy perspective—our findings demonstrate how legal reforms in a weak institutional setting can have perverse effects on behavior and highlight the potential pitfalls of relying on legal reforms alone to stem the practice of early marriage in low-income countries. They also echo two recent studies on the effects of laws relating to the minimum age of marriage. Bellés-Obrero and Lombardi (2020) finds that a legal reform in Mexico that increased the minimum age of marriage to 18 years merely succeeded in driving marriages underground, and had no effect on the share of births due to women aged below 18. Roy and Tam (2021) show that the 1929 Child Marriage Restraint Act—which fixed the female minimum age of marriage at 14 years in British colonial India—led to a sharp increase in child marriages during the six month period between the announcement and implementation of the law.

Our study also contributes to a growing literature that shows how information-based interventions impact entrenched attitudes and social behavior (e.g. Vogt et al. 2016; Banerjee, La Ferrara, and Orozco-Olvera 2019; Green, Wilke, and Cooper 2020). To our knowledge, ours is the first study to investigate whether providing information about the formal law can affect social attitudes and behavior in a setting with weak legal enforcement.

The remainder of the article is organized as follows. In Section 2, we provide more details on child marriage laws and marriage practices in our study setting, and develop a theoretical model to explain how information about the law can affect perceptions and social behavior. In Section 3, we describe the experimental design and the surveys conducted to collect information on marriage-related attitudes and behavior. We present the results in Section 4 and discuss their interpretation in Section 5. Section 6 concludes.

2. STUDY CONTEXT AND THEORY

2.1 Contextual background

Bangladesh has one of the highest rates of female child marriage in the world: according to a recent survey, 59% of women aged 20–24 years were married before the age of 18 (NIPORT 2016). Based on this measure, only Chad and Niger have a higher incidence (UNFPA 2012).

In the last three decades, there has been a substantial decline in the prevalence of very early marriage among women in Bangladesh: while close to half of women born in the 1970s were married by the age of 15, the proportion was close to 20% for women born in the early 1990s (Wahhaj 2018). However, a significant proportion of adolescent girls continue to marry at 16 or 17, below the legal minimum age. Raj, McDougal, and Rusch (2012) estimate, using data from the Demographic and Health Surveys, that there has been an increase in marriage among girls aged 16–17 years from 15.2% in the early 1990s to 20.6% in the mid-2000s.

Arranged marriages are the norm. Parents, family elders and other members of the extended family play an influential role in the choice of marriage partner, particularly in the case of first marriages and their opposition to a match can give rise to long-term tensions within the family (Dube 1997; White 1992). In the 2014, Bangladesh Women's Life Choices and Attitudes Survey (2014 WiLCAS—described in greater detail below), 83% of married women reported that their marriages had been arranged by their parents or other relatives (Asadullah and Wahhaj 2016).

Until recently, the Child Marriage Restraint Act of 1929 set the legal minimum age of marriage at 18 for women and 21 for men. The law specified that taking part in or facilitating a child marriage was a punishable offense but the punishment itself was relatively mild—imprisonment up to one month or a fine of 1000 taka (USD 12.50).[4] This law was replaced by the Child Marriage Restraint Act of 2017, approved by the Bangladesh National Parliament in February of that year. There were two key changes in the new law. First, the punishment has been made much more severe—2 years' imprisonment or a fine of 100,000 taka (USD 1,250) or both for any adult who marries an under-aged person. On the other hand, an "exception clause" has been introduced that would enable parents or guardians to marry off boys and girls before they reach the legal minimum age if a court rules that this is "in the best interests of the minor". No age limit has been specified for the exception clause.[5] In the debates leading up to the passage of the new law, child rights activists repeatedly argued that the clause would make it more socially acceptable to marry off underage girls, perpetuate gender inequality in child investments and facilitate forced marriages. On the other hand, the Bangladesh government argued that harsher punishments, coupled with the scope of marrying within the law when social pressures make it necessary—for example when a girl's standing within the community has been "compromised" due to a pre-marital relationship—will render the formal institutions more effective (Daily Star 2015, 2016).

2.2 Theoretical framework

How can information about the new child marriage law affect beliefs or behavior? To illustrate the potential mechanisms, we develop a theoretical model in which families with marriageable daughters make a marriage age choice, taking into account both the customary age

[4] The Child Marriage Restraint Act of 1929 is available here: http://bdlaws.minlaw.gov.bd/print_sections_all.php?id=149

[5] Further details about the Child Marriage Restraint Act of 2017 are provided in this article: http://www.thedailystar.net/frontpage/bill-passed-okaying-underage-marriage-special-cases-1368451

of marriage and the legal minimum marriage age. The model is based loosely on Aldashev et al.'s (2012a, 2012b) models of legal pluralism but we adapt them to study the specific case of child marriage law.

2.2.1 Setup

We model strategic interaction between a customary authority (CA) and a unit measure of families that each have one marriageable daughter. Each agent has a preferred age of marriage, denoted by $\mu_c \in [\underline{m}, \overline{m}]$ for the customary authority and $\mu_i \in [\underline{m}, \overline{m}]$ for family i, where $\underline{m}$ is the minimum marriageable age and $\overline{m}$ is the maximum marriageable age. The distribution of μ_i within the relevant population is described by the cumulative distribution function $F(\mu)$ with the properties $F(\underline{m}) = 0, F(\overline{m}) = 1$. In making their marriage decisions, the families also take into account the preferred marriage age of their reference group, μ_r. We discuss the appropriate reference group in more detail below.

There is a legal minimum marriage age given by $m_l \in [\underline{m}, \overline{m}]$. The formal penalty of underage marriage carries a disutility cost $\tilde{P}$. But the law is not strictly enforced. The *perceived* probability of enforcement, shared by all agents in the model is $\pi \in (0, 1)$. For ease of notation we define $P = \pi\tilde{P}$, the expected disutility from the risk of legal punishment for underage marriage. The preferred marriage age of the reference group is not known to the agents with certainty. We denote the expected preference by $\sigma = \mathbf{E}\mu_r$. We assume that π and σ are common knowledge and $m_l > \mu_c, \sigma, \mu_i$ for $i \in [0, 1]$.

The agents make decisions as follows. First, the CA declares a custom $c \in [\underline{m}, \overline{m}]$ which is a prescribed age of marriage. Then each family i chooses an age of marriage $m_i \in [\underline{m}, \overline{m}]$ for their daughter. Following these actions, family i receives the following payoff.

$$U_f(m_i, c; \sigma, m_l, P) = -d(m_i - \mu_i) - \lambda_r d(m_i - \sigma) - \lambda_c d(m_i - c) - \mathbf{1}(m < m_l)P$$

where $\lambda_r, \lambda_c > 0$, $\mathbf{1}(m < m_l)$ is the indicator function that takes a value of 1 if the chosen age of marriage is below the legally defined minimum age, and a value of 0 otherwise; and the function $d : \mathbb{R} \to \mathbb{R}_0^+$ has the following properties: $d(0) = d'(0) = 0, d(x) = d(-x), d''(\cdot) > 0$. Thus, family i incurs disutility when the chosen marriage age (m_i) diverges from its own preferred age of marriage (μ_i), the expected preferred age of marriage of the reference group (σ) and the declared custom (c). In addition, if the chosen age is below the legal minimum, it incurs a disutility equal to the expected penalty from an underage marriage. The function $d(\cdot)$ is a symmetric, convex function with disutility increasing in the difference between the chosen marriage age and each of the reference ages mentioned above. The parameters λ_r, λ_c capture the importance attached to the preferred marriage age of the reference group and that reflected in the declared custom. The payoff to the CA is given by

$$U_c(c, m) = -d(c - \mu_c) + \lambda_f \int_m^{\overline{m}} \max\{1 - d(m - c), 0\} dF(\mu)$$

where $\lambda_f > 0$. For ease of notation, we define $\overline{x} = d^{-1}(1)$. Thus, the CA incurs disutility when the declared custom deviates from his own preference. In addition, he obtains utility whenever a family chooses a marriage age that is sufficiently close to the declared custom as measured by the function $d(m - c)$, reflecting the prestige bestowed upon the CA by a custom-abiding family. The CA gains no utility when the choice of marriage age is sufficiently distant ($\overline{x}$ or more) from the declared custom. Drawing on the terminology of Hirschman (1970), we can interpret such a choice as a family opting to "exit" from the

community such that they are no longer within the purview of the customary authority.[6] An important implication of "exit" is that more extreme deviations from the custom (beyond the threshold $\overline{x}$) will not affect the CA's prestige. As we will see below, this feature of the model will be an important factor behind potential backlash by the CA to legal reforms.

2.2.2 Equilibrium and impact of a legal reform

We solve for the equilibrium using backward induction, first determining the marriage timing decisions by families for a given custom c and then determining the optimal custom from the CA's point of view given the best response functions of the families.

2.2.2.1 Marriage timing

Family i solves the following optimization problem:

$$\max_{m\in[\underline{m},\overline{m}]} -d(m-\mu_i)-\lambda_r d(m-\sigma)-\lambda_c d(m-c)-\mathbf{1}(m<m_l)P \tag{1}$$

We solve this problem in two steps. First, we consider the optimal choice if the family opted for underage marriage. In this case, the family faces an expected disutility of P due to the threat of legal punishment.

$$m_u(c,\sigma)=\arg\max_{m\in[\underline{m},\overline{m}]} -d(m-\mu_i)-\lambda_r d(m-\sigma)-\lambda_c d(m-c)-P$$

Assuming an interior solution, $m_u(c,\sigma)$ is given by the following first-order condition:

$$d'(m_u(c,\sigma)-\mu_i)+\lambda_r d'(m_u(c,\sigma)-\sigma)+\lambda_c d'(m_u(c,\sigma)-c)=0 \tag{2}$$

Using Equation (2), we can establish the following:

Proposition 1. *For families that practice underage marriage, a change in the declared custom c, or perceived marriage age preference of the reference group σ, leads to a change in marriage timing in the same direction, by a smaller magnitude than the change in c or σ.*

Next, we consider under what conditions a family will choose a legally appropriate marriage age. Since $m_l>\mu_c,\mu_i,\sigma$ by assumption, the family will never choose a marriage age $m(c,\sigma)>m_l$. Therefore, the only alternative to the choice $m_u(c,\sigma)$ we need to consider is m_l, the legal minimum age of marriage. The utility level obtained from these two possible choices are shown in Table 1.

We define $\overline{\mu}(c,m_l,\sigma,P)$ as the marriage age preference for which a family is indifferent between underage marriage and marriage at the legal minimum age; i.e. $U_f(m_u(\overline{\mu}(c,m_l,\sigma,P)),c)=U_f(m_l,c)$. Using this definition, we can establish the following:

Lemma 2. *Given a customary marriage age c, a family with marriage age preference μ will abide by the law if $\mu>\overline{\mu}(c,m_l,\sigma,P)$ and engage in underage marriage if $\mu<\overline{\mu}(c,m_l,\sigma,P)$. The threshold marriage age preference $\overline{\mu}(c,m_l,\sigma,P)$ is decreasing in the expected punishment P and expected marriage age preference of the reference group σ.*

[6] The notion of exit from the community also appears in the models of legal dualism by Aldashev et al. (2012a, 2012b). Unlike in these models, we do not assume that a family that exits is subject to social sanctions. Introducing social sanctions will reduce the disincentive effect of the legal punishment but will not qualitatively change our theoretical results.

Table 1. Expected utility from underage marriage versus law-abiding marriage age.

Marriage age	Expected utility
$m_u(c,\sigma)$	$U_f(m_u,c) = -d(m_u-\mu_i)-\lambda_r d(m_u-\sigma)-\lambda_c d(m_u-c)-P$
m_l	$U_f(m_l,c) = -d(m_l-\mu_i)-\lambda_r d(m_l-\sigma)-\lambda_c d(m_l-c)$

2.2.2.2. Choice of custom

The optimization problem of the CA is as follows:

$$\max_{c\in[\underline{m},\overline{m}]} -d(c-\mu_c)+\lambda_f\int_m^{\overline{m}}\max\{1-d(m(c)-c),0\}dF(\mu) \tag{3}$$

If there is an interior solution (i.e. $m(c)>c>\mu_c$), it must satisfy the first-order condition:

$$d'(c-\mu_c) = \begin{cases} \lambda_f\{\int_m^{\overline{\mu}} d'(m(c)-c)\{1-m'(c)\}dF(\mu)+\int_{\overline{\mu}}^{\overline{m}} d'(m_l-c)dF(\mu)\} & \text{if } c>m_l-\overline{x} \\ \lambda_f\int_m^{\overline{\mu}} d'(m(c)-c)\{1-m'(c)\}dF(\mu) & \text{if } c<m_l-\overline{x} \end{cases} \tag{4}$$

We can interpret Equation (4) as follows: in declaring a customary age of marriage, the CA equates the marginal cost of deviating from his preferred custom (left-hand side of the equation) and the marginal benefit of bringing the customary age closer to actual marriage timing within the community (right-hand side). The marginal benefit comes from two different types of families: (a) those that practice underage marriage and respond to any increase in the customary age by opting for a higher age of marriage; (b) those that follow the law and, thus, do not respond to an increase in the customary age. If the equilibrium custom is very distant from the law (more precisely, $c<m_l-\overline{x}$), then the CA derives no marginal benefit from the second type of family; hence, the corresponding term is missing for this case, as shown in (4).[7]

2.2.2.3 Impact of a legal reform via the customary authority

Differentiating the right-hand side of (4) with respect to P, we obtain expressions for the effect of increasing the expected cost of punishment for violating the law on the marginal benefits discussed above:

$$\lambda_f\frac{d\overline{\mu}}{dP}[d'(m(c)-c)\{1-m'(c)\}-d'(m_l-c)] \text{ if } c>m_l-\overline{x} \tag{5}$$

$$\lambda_f\frac{d\overline{\mu}}{dP}d'(m(c)-c)\{1-m'(c)\} \text{ if } c\le m_l-\overline{x} \tag{6}$$

By Lemma 2, $\frac{d\overline{\mu}}{dP}<0$ and, by Proposition 1, we have $0<1-m'(c)<1$. By construction, $m(c)-c<m_l-c \iff d'(m(c)-c)<d'(m_l-c)$. Therefore, the expression in (5) is positive but the expression in (6) is negative. Thus, we obtain the following result.

[7] Note that, for an interior solution, we must have $c>m(c)-\overline{x}$. If not, the marginal benefit would be zero for both types of families and the first-order condition could not be satisfied.

Proposition 3. *A legal reform that increases the expected cost of punishment for violating the law will move the customary marriage age in the direction of the law if, in the initial equilibrium, it is sufficiently close to the legal minimum age of marriage* ($c > m_l - \overline{x}$) *and in the opposite direction to the law otherwise* ($c \leq m_l - \overline{x}$).

According to Proposition 3, if the customary practice is initially close to the law, a legal reform that strengthens the perception of legal enforcement will cause the custom to move in the direction of the law. Then, according to Proposition 1, families that, in the initial equilibrium, would have practiced underage marriage will opt to delay the marriage of their daughters.

However, if the customary practice is initially distant from the law, the legal reform will cause the custom to move in the *opposite* direction. The reason is that when the customary marriage age is distant from the legal minimum age, the customary authority derives no "prestige utility" from law-abiding families or moving the custom closer to their marriage practice. As legal enforcement improves, more families fall into this category, and weakens customary authority's incentive to move the custom closer to the law. Then, according to Proposition 1, families that, in the initial equilibrium, would have practiced underage marriage will opt to *hasten* to marry their daughters.

2.2.2.4 Expressive effect of the legal reform

The marriage age legal reform could also influence marriage decisions by "sending a message about society's values" (Benabou and Tirole 2012; see also Sunstein 1996; McAdams 2000a). In the context of child marriage law in Bangladesh, the new law signals to the respondents how the government, legislators, and, potentially, the wider society view the practice of child marriage. Within the present theoretical framework, we can represent this shift by a change in σ, the expected marriage age preference of the peer group. By Proposition 1, an increase in σ would cause families that practiced underage marriage in the initial equilibrium to delay the marriage of their daughters.[8]

2.2.2.5 Perception of future enforcement

Lemma 2 and Proposition 3 can describe how changes in the perceptions of enforcement of the law (as represented by the term π) can influence the custom and marriage behavior. However, if a family believes that the legal reform will be put into practice with a delay, this may increase the perception of *future* enforcement, without altering the current perception. A simple way to capture such effects in the model is to write the perceived probability of enforcement as $\pi_0 + \kappa(m - a_0)$ where π_0 is the perception of the probability of enforcement prior to the legal reform, a_0 is the age of the girl at the time of the legal reform, $m \in (a_0, m_l)$ is the marriage age choice (below the legal minimum), and κ is a constant. If $\kappa > 0$, then delaying marriage (but not complying with the law) increases the risk of punishment. We can represent a legal reform that is expected to be enforced in the future as a change from $\kappa = 0$ to $\kappa > 0$. Following the reasoning of Lemma 2, we can show that such a change would lower the threshold $\overline{\mu}$ below which families practice underage marriage but also *lower* the age of marriage for families that continue to practice underage marriage post-reform (see Proposition 4 in the Theoretical Appendix).[9]

[8] It is worth noting that the customary authorities will anticipate the shift in marriage behaviour and adapt the custom, potentially in the opposite direction (see Corollary to Proposition 3 in the Theoretical Appendix). But as the expected age preference of the reference group σ does not directly affect the utility of the customary authority, the change in the custom cannot entirely undo the direct effect of σ on marriage timing decisions by families.

[9] The observation in the preceding footnote applies here too: customary authorities may also anticipate an increase in future enforcement and adapt the custom. But as future enforcement does not directly affect the utility of the customary authority, it cannot entirely undo the direct effect of κ on marriage timing decisions by families.

Motivated by these theoretical arguments, our experiment is designed specifically to test (a) whether a legal reform has an "expressive effect"; (b) whether moderating elements in the new law make it more or less effective; (c) whether changes in the formal law affect the attitudes and behavior of the customary authority differentially (i.e., whether the stricter elements of the new law—in relation to the custom—can generate a backlash). We measure a variety of outcomes for participants in the experiment, including short-term attitudinal outcomes, beliefs regarding community attitudes, and longer-term marriage-related outcomes. By assessing the relative effects of the information treatments on these outcomes across different sub-groups of respondents, we are able to distinguish between two competing theoretical channels underlying the effect of legal change: the "expressive effect" discussed above versus effects induced by strategic behavior of customary authorities (Aldashev et al. 2012a, 2012b). We describe the direction of change that each theory predicts for our measured outcomes in Section 3.2.3. We discuss whether alternative theories and explanations can account for our results in Section 5.2.

3. DATA AND STUDY DESIGN

3.1 Description of the survey

The 2014 Women's Life Choices and Attitudes Survey (WiLCAS) is a nationally representative survey of women in Bangladesh aged between 20 to 39 years with detailed information about their marital histories, child-related investments, attitudes toward marriage customs and traditional gender roles, access and use of information media, social networks, as well as knowledge about child marriage laws.[10] The survey was conducted immediately before the start of the public discussions that culminated in the Child Marriage Restraint Act of 2017 (CMRA 2017). Therefore, it provides an important (and to our knowledge unique) snapshot of marriage-related social norms before the move to revise child marriage laws was initiated.

To study how the passage of the CMRA 2017 affects social attitudes, a new round of data collection was conducted in a subsample of the WiLCAS households in May–June 2018. We refer to this new survey as CiMLAS (Child Marriage Law and Attitudes Survey). At the time of the new survey, the CMRA 2017 had been approved in parliament but courts were still awaiting instructions from the government on how the new law should be applied in court cases.[11] The new survey (CiMLAS) was conducted in 80 village clusters, selected from the original 391 WiLCAS rural clusters. The selection of survey clusters followed a two-stage randomization process. At the first stage, 24 of the 61 districts covered under WiLCAS were randomly drawn. At the second stage, 80 village clusters were randomly picked from the WiLCAS rural clusters located in these districts. All female respondents from the original WiLCAS survey found in these clusters were selected for individual interviews. This procedure produced a sample of 971 primary respondents.

The survey team also conducted parallel interviews with other members of the extended family who belong to the same household or are living in the same neighborhood. The number of additional interviews per respondent was randomized, with an equal probability of 0, 1, or 2 additional interviews. The additional respondents were chosen from the following list, starting with the first relative present at the time of the interview, and continuing down the list until the required number of additional interviews had been obtained: (a) father-in-

[10] Further information about the 2014 WiLCAS are available at the website www.integgra.org. See also Asadullah and Wahhaj (2019).

[11] The implementation rules for the new law would not be published until November 2018.

law; (b) mother-in-law; (c) eldest brother of father-in-law; (d) uncle-in-law; (e) husband's elder brother; (f) husband; (g) husband's elder brother's wife. The relationships were specified in advance of the intervention according to their importance, in the Bangladesh context, in the marriage decisions of adolescent girls (see Section 2.1). A total of 786 interviews with relatives of the WiLCAS female respondents were conducted during the survey.

At the start of the interview, respondents were informed that (a) the survey was being conducted as part of a study "to understand how much people know about the law in Bangladesh regarding child marriage and their beliefs and attitudes regarding the practice"; (b) the study was not related to any government or NGO program and that their responses would have no direct impact for them.

In all interviews, we began by collecting background information on the respondent. This included information on the respondents' parental background; schooling; own marriage history; exposure to information on child marriage through the media; knowledge of child marriage law. In interviews with female respondents, we also collected marriage-related information on their daughters. Next, we administered a randomized information treatment and collected information on a number of attitudinal and behavioral measures relating to child marriage practices and traditional gender norms (these are described in detail in the next subsection).

For female respondents who had unmarried adolescent daughters at the time of the initial survey, we conducted two rounds of follow-up telephone interviews, five months and 10 months after the initial survey. The purpose of these follow-up interviews was to collect information on any steps taken toward marriage for daughters since the information intervention, including groom search, responses to marriage proposals, engagements and marriages.

3.2 Experimental design and outcome measures

The experiment involved a video-based information intervention randomized across households along two dimensions independently of each other: (a) the video content and (b) whether or not the relevant video was shown to multiple family members. First, we randomized exposure to information about the new child marriage law. Information about the law was conveyed through a short video drama of a hypothetical case of marriage for a girl of 15. There were small variations in the story across different respondents such that some were provided with information about the new law while others were not. Specifically, a control group (C) received information about the minimum age limit for marriage and the punishment for violating the minimum age limit under the old (CMRA 1929) law. A treatment group (T1) received information about the age limit and punishments specified in CMRA 2017 but not the exception clause. A second treatment group (T2) received information about the age limit and new punishments—as well the exception clause. The rationale for arm T2 in our experimental design is two-fold: (a) to provide an empirical test for the theoretical argument that a moderate legal reform is likely to be more effective in changing customary practices than a radical reform (Aldashev et al. 2012a, 2012b; Platteau and Wahhaj 2014); (b) to address the policy question whether, in the context of marriage age legal reform, legal exceptions can undermine the "expressive effect" of the law. The respondents were randomized into the T1, T2, and C groups with an equal probability of being assigned to any one of the groups.

Second, primary respondents were also randomized such that either 0, 1, or 2 other members of the extended family (living in the same household or in the neighborhood) received the same treatment as the primary respondent to whom they were related (the procedure is described in the previous subsection). The videos were displayed on a handheld electronic device that the enumerators used to collect the survey data. For each respondent, the

enumerators initiated the video by tapping on a designated link embedded into the questionnaire. The enumerators were not aware of the treatment/control assignment of the respondents they interviewed and the video behind each designated link. After the videos were administered, respondents were asked a number of questions to check comprehension of the information contained therein, and the video was replayed if comprehension was poor.

3.2.1 Short-term attitudinal outcomes

After the video had been shown, respondents were asked a number of questions to measure their beliefs and attitudes regarding child marriage practices[12] and traditional gender norms[13]. Then, respondents were read out a number of vignettes regarding child marriage where an adolescent girl and her family are faced with a dilemma involving an offer/opportunity of marriage for the girl. In the first vignette, Vignette A, an adolescent girl in grade 9 receives an offer of marriage from a man from a neighboring village. Vignette B describes a similar situation except that the girl's father has passed away, she has younger unmarried sisters, and the offer comes from a man who has good economic prospects (a career in the civil service). The vignettes were followed by questions on what the respondent would do if she/he were the parent of the adolescent girl in the vignette, what other parents in the village would do in the same situation, and what advice they would give to the parents of the adolescent girl in the vignette. The text of the vignettes and the follow-up questions are included in the Appendix.[14]

At the end of the interview, the respondents were provided with a token gift of Taka 200 (approximately 2.50 USD) and the option of contributing all or part of this amount to a charity (NGO) that works on child marriage prevention. The portion of the gift due to the respondent was sent using an existing mobile money transfer service in Bangladesh. The charity in question acts on reports about planned marriages of children and adolescents below the legal minimum age to provide legal counseling to, and mediation between, the parties involved (for example, the prospective groom and bride, their families and the complainant). This counseling takes place against the backdrop that the law enforcement authorities would be informed if the parents decide to go ahead with the marriage.[15] Appendix Table B1 provides a brief description of each short-term attitudinal outcome variable.

3.2.2 Follow-up calls: longer-term marriage outcomes

The study team conducted follow-up telephone interviews in November 2018 and May 2019 respectively, i.e. approximately five and 10 months after the video information intervention. During each follow-up survey, the team attempted to contact all 315 female respondents who had reported, at the time of the survey in May–June 2018, having one or more unmarried daughters aged between 13 and 22. The team were able to contact and

[12] For example: "In your opinion, what is the appropriate age of marriage for a girl?"; "In your opinion, what do most people in this village feel is the appropriate age of marriage for a girl?"; "What do you think is the ideal age gap between a husband and a wife?"

[13] For example: "Boys require more nutrition than girls to be strong and healthy."; "School education is more important for boys than for girls."

[14] A third vignette described a girl who has a secret engagement with a boy from her school, which her parents learn about from a neighbour. However, we do not include the responses to this vignette in our analysis as the wording in the vignette meant that it did not involve opposition to the marriage from the girl in the same manner as Vignettes A and B, making it difficult to interpret the responses.

[15] The exact wording of the information and question addressed to the respondents is as follows: "We have a gift for you at this point. Here is 200 taka as your gift. You can keep this. However, there is a charity organization called … that, among other activities, provides legal counseling to families around Bangladesh to prevent child marriage. They need money to continue with this effort. If you want, you can donate any part or all of this amount to this organization to continue this effort. And we can take this donation from you and send it to them on your behalf. Would you like to make a charitable donation to this organisation that discourages child marriage?"

successfully conduct interviews with 278 respondents in November 2018 (attrition rate of 12%) and 254 respondents in May 2019 (attrition rate of 19%). During each interview, the respondent was asked, for each daughter, whether she had been married since June 2018 and, if not, whether the family had taken any steps related to the marriage process.[16] Appendix Table B1 provides a brief description of each marriage-related outcome variable. The follow-up interviews produced a dataset with marriage-related information on 337 daughters (261 below the age of 18) in November 2018 and 305 daughters (234 below the age of 18) in May 2018.

Two additional interview rounds were conducted in June 2020 and June 2021, i.e. 24 and 36 months after the intervention. As these rounds were conducted after the onset of the Covid-19 pandemic, a potentially important factor in marriage decisions, we do not use them in our main analysis. However, in Section 4.2, we use the additional data to estimate a marriage hazard model based on marital status and marriage timing up to February 2020 (the month preceding the first Covid-19 cases and lockdowns in Bangladesh).[17]

3.2.3 *Mapping theoretical predictions to measured outcomes*

Table 2 provides a mapping between treatments and predicted directions of change for each type of measured outcome under each of the mechanisms described in Section 2.2 through which a legal reform can affect child marriage outcomes: (a) "expressive effect"; (b) effect via the "customary authority"; and 3) effect via a change in the perception of future enforcement of the law. If the new law has an "expressive effect," the treatments should affect the study participants' beliefs regarding the attitudes of other members of the community toward early marriage. More specifically, T1—which provided participants information about the harsher punishments stipulated in the new law—should shift beliefs about others' preferred female marriage age in the direction of later marriage ("+ve effect"); while T2—which provided information about the harsher punishments as well as the exception clause—could have a weaker effect in the same direction, or an effect in the opposite direction (as it is more aligned with traditional marriage practices). These shifts in beliefs regarding others' attitudes could subsequently lead to a corresponding change in the marriage timing of adolescent girls in the study participants' households. If the change in marriage outcomes is due entirely to an "expressive effect," there should be no change in the study participants' own preferences regarding marriage age. Therefore, if they report their preferences truthfully, neither treatment should affect stated preferences. However, if they want to align their *stated* preferences with their beliefs about the preferences of others, the change in beliefs could induce them to adjust their *stated* preferences in the same direction, that is, an increase in reported preferred female age of marriage under T1, and a smaller increase or a decrease under T2.

If the effect of the new law occurs via the "customary authority" channel, the treatments should lead to a shift in the preferences regarding marriage age that family elders express, as captured by their stated preferences. More specifically, knowledge about the new law may lead family elders to strategically choose—and express—a position on female marriage age that is more aligned with the new law. However, if the new law is deemed to be too distant from traditional practices and their own preferences, family elders may revert to a more traditional position. Given that the exception clause is more aligned with traditional practices, the theory implies that a backlash is less likely in the case of T2 compared to T1. These

[16] For example: "Have you had discussions with your family about finding a groom for … ?"; "Have you or your family actively sought a groom for … ?"; "Have you or your family had a marriage offer for … ?".

[17] Further information about the two additional rounds of data are provided in Amirapu, Asadullah, and Wahhaj (2022).

Table 2. Mapping theories to measured outcomes.

Effects on	Theory: 1) Expressive effect	2) Effect via customary authorities	3) Effect via perception of future enforcement
i) Stated preferences	T1: No Effect/ +ve T2: No Effect/ −ve/less +ve	T1: −ve Effect for Elders (if backlash) T2: less −ve Effect for Elders (if backlash)	No effect
ii) Beliefs about others' true preferences	T1: +ve T2: −ve/less +ve	No Effect	No effect
iii) Beliefs about others' stated preferences	T1: No Effect/+ve T2: No Effect./ −ve/less +ve	T1: −ve Effect on Beliefs re. Elders' Preferences (if backlash) T2: less −ve Effect on Beliefs re. Elders' Preferences (if backlash)	No effect
iv) Early marriage practice	T1: −ve T2: +ve/less −ve	T1: +ve Effect (if backlash) T2: less +ve Effect	T1: +ve effect (pre-implementation) T2: less +ve effect (pre-implementation)

Note: This table provides a summary of the relevant measured outcomes and predicted directions of change for each theory that the experiment is designed to test. A "+ve" effect on Stated Preferences or beliefs about others' preferences corresponds to an increase in preferred/appropriate marriage age (or, generally, a more negative view of underage marriage). A "+ve" effect on early marriage practice corresponds to an increase in the practice (i.e. an increase in the rate of child marriages taking place).

shifts in preferences expressed by family elders could subsequently lead to a change in the marriage timing of adolescent girls in the household. If the change in marriage outcomes is due entirely to the customary authorities' shift in position, there need not be any change in the study participants' beliefs about other community members' *true* preferences regarding female age of marriage but, if they anticipate a backlash, it will affect their beliefs about the attitudes that family elders *express* as per the discussion above.[18]

If the effect of the new law occurs via a change in the perception of future enforcement of the law, there need not be any shift in stated preferences regarding marriage age, or beliefs regarding the attitudes of others toward the practice of child marriage. However, as described in Section 2.2, a perception of increased future enforcement should decrease the age of marriage among families that practice child marriage. We hypothesize that if there is such an effect, it should be temporary, limited to the period between the intervention and the implementation of the new law. We further hypothesize that the effect will be stronger under T1 compared to T2 as the latter treatment additionally provided information regarding a pathway for early marriage under the new law.

[18] Similarly, in the case of the "expressive effect", we assume in Table 2 that respondents correctly anticipate the change in the stated preferences of others, i.e. *beliefs* about the stated preferences of others shift in the same manner as actual stated preferences.

Financial contributions to the charity working on child marriage prevention could, arguably, change due to either an "expressive effect" or a strategic response by customary authorities. Although the outcome may not allow distinguishing between the alternative theoretical mechanisms, it is of interest in its own right to the extent that it reflects support for a mode of alternative dispute resolution. As noted in the previous section, the charity aims to prevent marriages of minors through legal counseling and mediation without direct involvement of law enforcement authorities and the formal court.[19] This may be an attractive option for both parties as it reduces the risk of criminal punishment and the involvement of law enforcement authorities (which can harm the social reputation of the families involved even if it does not lead to criminal punishment).

3.3 Description of the data

Table 3 provides descriptive statistics for the primary (female) respondents from the WiLCAS sample as well as for the additional respondents surveyed in those households. From the table we see that the main female respondent is, on average, about 33 years old, with five years of schooling. The vast majority (94%) are married and about two in three married before the age of 18, i.e. below the legal minimum age of marriage. Their parents had little education—on average, three years of schooling among their fathers and less than 1.5 years of schooling among their mothers. About one in three have an adolescent daughter below the age of 18 and thus the change in the minimum marriageable age law is pertinent for them.

Table 3 also shows that the additional respondents are, on average, about 50 years old. About 62% of the sample—which includes the spouses, fathers-in-law and brothers-in-law of the main female respondent—are male. The vast majority (about 86%) are married, and a third of them married below the age of 18. The parents of the additional respondents had little education—on average, 2.23 years of schooling among their fathers and about 0.93 years of schooling among their mothers.

The table also provides a snapshot of the respondents' knowledge about the law prior to the intervention. A large majority of respondents are aware that there is a legal minimum age of marriage and most were able to state it correctly (88% for the female respondents and 83% for the additional respondents). About four out of five respondents were able to state the nature of the punishment for violating the legal minimum age ("the guardian or father would be jailed or fined"). When asked about exceptions to the law, about 10% of the primary respondents (7% of the additional respondents) answered that there was an exception. Only five respondents, however, were able to name the special exemption clause in the 2017 Child Marriage Law, and two other respondents mentioned the possibility of "a court marriage"; 13% of the sample of primary respondents (6.7% of additional respondents) mentioned that an exception was possible "if the parents wanted it" or "if the family wanted it".

Respondents were asked when they had first heard about the current law regarding the minimum age of marriage. About 5% of the primary respondents (4% of additional respondents) reported hearing about it in 2017—the year when the new law came into effect—or later. Another 13% of respondents (both in the sample of primary and additional respondents) reported hearing about it in 2015 or 2016, the two years during which various versions of the new law were widely discussed and debated in the media. These numbers put an

[19] The work of the charity satisfies common definitions of alternative dispute resolution (ADR). For example, according to Shavell (1995), ADR mechanisms "share the feature that a third party is involved who offers an opinion or communicates information about the dispute to the disputants." ADR is much more widely used for civil cases but has been shown to be effective in criminal cases too (see, e.g., Morris 2015).

Table 3. Summary statistics (for primary and additional respondents).

	Primary respondents					Additional respondents				
	mean	**sd**	**min**	**max**	**obs**	**mean**	**sd**	**min**	**max**	**obs**
Age	33.36	6.07	20	58	971	50.37	11.30	24	75	786
Male	0.00	0.00	0	0	971	0.62	0.48	0	1	786
Schooling	4.99	4.04	0	16	971	2.80	4.06	0	19	786
Married	0.94	0.24	0	1	971	0.87	0.34	0	1	786
Married before 18	0.65	0.48	0	1	971	0.33	0.47	0	1	786
Employed	0.15	0.35	0	1	971	0.59	0.49	0	1	786
2014 Norms Index 2	0.00	1.00	−1	1	971					
Father Schooling	3.02	4.06	0	16	971	2.23	3.70	0	19	786
Mother Schooling	1.42	2.53	0	15	971	0.93	2.22	0	19	786
Mother works	0.08	0.27	0	1	971	0.08	0.27	0	1	786
Father low pay	0.23	0.42	0	1	971	0.23	0.42	0	1	786
Half Acre Land	0.51	0.50	0	1	971	0.42	0.49	0	1	786
Adol. girl 13–17	0.34	0.58	0	3	971	0.09	0.33	0	2	295
Knows min age	0.88	0.32	0	1	971	0.83	0.38	0	1	786
Knows punishment	0.81	0.39	0	1	971	0.78	0.41	0	1	786
Knows age exception	0.10	0.29	0	1	971	0.07	0.26	0	1	786
Learnt law after 2014	0.19	0.39	0	1	971	0.20	0.40	0	1	786
Knows CM court case	0.36	0.48	0	1	971	0.35	0.48	0	1	786

Note: This table presents summary statistics of background characteristics for primary and additional respondents, separately. Data for the 2014 Norms Index (a composition of several survey questions that aim to measure traditional gender norms) are not available for additional respondents because those questions were only asked in an earlier survey (WiLCAS) which included interviews with CiMLAS primary respondents only. Source: 2018 CiMLAS.

upper bound of 18% for the proportion of respondents who might have prior knowledge about the 2017 Child Marriage Law.

Based on these responses, we can conclude that the respondents had good knowledge of the pre-2017 law regarding child marriage: specifically, knowledge of existence, the minimum age and the consequences of violating the minimum age law. On the other hand, given that few respondents knew about the exception clause in the new child marriage law, and the fact that most had learnt about the "current" child marriage law before the new law was proposed or legalized, it appears that very few had knowledge of the 2017 Child Marriage Restraint Act before the information intervention. Nearly half of our respondents (47.6% of female respondents and 47.3% of the additional respondents; figures not shown in the tables) report reading/hearing about child marriage issues at least once during the previous 12 months from the radio, television, posters, newspapers or community programs, which suggests that information about the new law is likely to reach them from one or more of these sources in the near future.

The variables included in Table 3 are based on responses to questions addressed to the respondents before they were shown the video on child marriage. Therefore, a comparison of means provides an indication of whether the randomization achieved balance across the three groups. Appendix Table B2 reports p-values for a t-test of equality of means, for the full sample of respondents, between the control group and the first treatment group and between the control group and the second treatment group. In all instances, we find that the variable means are similar across the groups, with p-values above conventional levels for detecting statistical significance, indicating that balance was achieved in assigning the respondent to the control or treatment groups. As information on marriage-related outcomes was obtained only for families with unmarried adolescent daughters at baseline (and because we focus our analysis on this group), we also check whether there is balance across the three treatment/control arms for respondents within

this subsample. These balance tests are shown in Appendix Table B3. Here again we find that the variable means are similar across the groups, with p-values above conventional levels for detecting statistical significance (with the exception of one out of 34 comparisons).

We find substantial differences between the mean values of our respondents' stated beliefs about appropriate marriage rules and their beliefs about these norms in the rest of the community. For example, Appendix Table B4 shows that the mean value of "appropriate marriage age" is 18.7 years for respondents in the control group, while the corresponding mean value for "appropriate marriage age in the village" is 17.3 years. We hypothesize three possible reasons for the disparities between stated views and beliefs about the views of others: (a) individuals have incorrect (biased) beliefs about the overall support within their village for female early marriage; (b) the survey respondents exaggerated their own support for marriage postponement among adolescent girls; (c) the views of the survey respondents are not representative of the views of the wider population within their villages. In previous work, we have shown that beliefs about overall support within the village vary systematically with own age of marriage, which provides suggestive evidence for (a).[20] We argue that (c) is also plausible given that our village samples do not constitute a random sample of the adult village population. Rather, our sample design ensures that the majority of respondents are women in the age range 24–43 years (55% of the overall sample). We address (b) in Section 5.3.

4. RESULTS

4.1 Short-term outcomes: marriage-related beliefs and attitudes

To investigate whether and to what extent the intervention affected beliefs and attitudes relating to child marriage practices, we regress our outcomes of interest against binary treatment indicators and a set of control variables, including parental characteristics, education, and prior knowledge regarding child marriage laws, as well as village fixed-effects. In the Appendix, we also provide results from a specification that excludes control variables (except village fixed-effects) as well as a simple comparison of mean differences across treatment groups.[21]

Our baseline specification takes the following form:

$$y_{ihv} = \alpha + \beta_1 T_{1hv} + \beta_2 T_{2hv} + d_v + X_{ihv} + \varepsilon_{ihv} \quad (7)$$

where y_{ihv} is the outcome variable for respondent i in household h in village v; T_{khv} is a dummy indicating whether household h in village v received treatment k; d_v is a village-level dummy; and X_{ihv} is a vector of individual-level controls. We calculate standard errors using the Eicker–Huber–White method. To address the fact that we are testing multiple hypotheses (by looking at effects of multiple treatments on a number of different dependent variables), our main regression tables include Westfall–Young stepdown adjusted p-values to control the family-wise error rate (FWER) for each family of hypotheses (Westfall and Young 1993).[22]

[20] See Table 17 in an earlier working paper available at https://edi.opml.co.uk/resource/child-marriage-law-gender-norms-marriage-customs-bangladesh/

[21] See Appendix Tables B4 and B13–B15.

[22] When defining families of hypotheses for this purpose we are guided by the conceptual framework developed in Section 3.2.3, which spells out the predicted effects of different theories of legal change. According to this framework, the three different theories have distinct predicted effects on (a) respondents' own stated preferences, (b) respondents' beliefs about others' true preferences, (c) respondents' beliefs about others' stated preferences, and (d) actual early marriage practice. Thus, we group families of hypotheses together according to the above categories. This leads to the following grouping of outcomes: (a) all outcomes pertaining to respondents' own "attitudes towards early marriage" (including "appropriate marriage age" and whether "marriage before 18 [is] appropriate") as well as vignette questions regarding whether the respondent would support their daughter's decision are grouped together; (b) all outcomes pertaining to respondents' "beliefs' re. attitudes in [the] community" toward early marriage (including "appropriate marriage age" and whether "people [would] think worse [of]

The short-term outcome variables are as follows: (a) appropriate age of marriage; (b) respondents' beliefs about what others in the community feel is the appropriate age of marriage; (c) responses to vignette-related questions including own choice regarding hypothetical marriage decisions; (d) beliefs about what choices others would make and approval or disapproval of a particular choice; (e) contribution of money (from a token gift) toward a charitable organization that works to discourage child marriage.[23]

The individual-level controls include age, gender, binary variables for primary school completion, primary school completion by the respondent's mother, parental ownership of half an acre of land or more, experience of marriage before 18, and having one or more daughters between the ages of 13 and 17. We also control for the respondents' prior knowledge of child marriage law by adding binary variables for whether the respondent previously knew of the minimum age law, the punishment for marriage below the legal age, and the exceptions allowed to the legal minimum age under the law; also whether the respondent learnt about the current law after 2014 (when the government first put forward its plans to change the previous child marriage law) and have heard of child marriage cases where the current law has been applied.

In Tables 4–5, we report results from the specification in Equation (7). From Table 4 we see that the point estimate for the effect of Treatment 1 on the stated appropriate age of marriage is small and insignificant, while the effect of Treatment 2 on the same outcome is actually negative. This is in contrast to what would be expected if the expressive effect of the law were at work. We also find no significant effect of either treatment on beliefs about community attitudes regarding the appropriate age of marriage (Table 5), which is again at odds with an expressive effect of the law.

Turning to the vignette-related questions, we obtain point estimates that are negative or close to zero for the indicator "would support daughter's decision to delay marriage" for both treatments and both vignettes (Table 4).[24] For the questions "would other parents in the village support their daughter's decision to delay marriage" and "would neighbors and extended family approve the parents' decision to support their daughter and delay marriage," we again find point estimates that are negative or close to zero—and mostly insignificant—for both treatments and both vignettes (Table 5).[25] In the case of a second measure of community attitudes, whether respondents believe that people in their community think worse of families whose daughters are still unmarried at 18, Treatment 1 has a small and statistically insignificant estimated effect, but Treatment 2 has a significant effect of −6.1% points (Table 5). Turning to financial contributions, we find that the first treatment increased the amount that respondents were willing to contribute to an NGO working to discourage child marriage by about 6 Taka (Table 4).

These full sample estimates mask considerable heterogeneity across different types of respondents that we report in Section 4.3.

marriage after 18") as well as vignette questions regarding whether the respondent believes that others in the community would support their daughter's decision are grouped together; (c) vignette questions regarding whether the respondent believes that others in the community would approve of a parent's decision to support their daughter in making a potentially controversial decision to postpone or decline a marriage offer are grouped together; (d) actual marriage outcomes as measured in longer term follow-up phone surveys are grouped together. Last, outcomes regarding financial contributions to a child marriage NGO are also placed in their own distinct family for this purpose because these outcomes do not map neatly into any of the other four categories. Our adjusted p-values are implemented in Stata using the command wyoung (Jones, Molitor, and Reif 2019).

[23] Appendix Table B1 provides a list of all relevant short-term outcome variables, together with their definitions.

[24] The effect is statistically significant (and negative) in only one instance (the case of Vignette B for the second treatment).

[25] Again, the effect is statistically significant (and negative) in only one instance (the case of vignette A for the first treatment).

Table 4. Short-term outcomes for full sample (Part a).

	Attitudes toward early marriage		Would others support daughters' decision		Financial contribution to NGO	
	Appropriate marriage age	Marriage before 18 appropriate	Vignette A	Vignette B	Make a contribution	Contribution amount
Treatment 1	0.034	−0.007	0.013	−0.035	0.021	6.434**
	(0.102)	(0.011)	(0.017)	(0.029)	(0.028)	(3.136)
Treatment 2	−0.194**	0.009	−0.001	−0.060**	−0.010	−0.168
	(0.099)	(0.012)	(0.017)	(0.029)	(0.027)	(2.630)
Observations	1757	1757	1757	1757	1757	1757
dep var mean	18.741	0.039	0.912	0.620	0.395	24.189
dep var sd	1.643	0.194	0.284	0.486	0.489	49.400
T1 FWER p-val	0.811	0.811	0.811	0.622	0.419	0.044
T2 FWER p-val	0.149	0.761	0.947	0.149	0.892	0.954
$\beta 1 = \beta 2$	0.024	0.177	0.386	0.413	0.254	0.025

Note: This table presents the results of OLS regressions of short-term outcome variables against treatment status indicators, including individual-level controls and village fixed effects (not shown). The sample consists of all respondents (both primary and additional respondents) in the 2018 CiMLAS survey. Controls include age, gender, primary school completion, primary school completion by the respondent's mother, parental ownership of half an acre of land or more, experience of marriage before 18, and having one or more daughters between the ages of 13 and 17. Standard errors are given in parentheses. The penultimate rows display Westfall–Young stepdown adjusted p-values for the coefficients on T1 and T2. These control the FWER for all tests in a given family of hypotheses (further information is provided in Section 4). The last row reports the p-value from a Wald test for a difference in coefficients between T1 and T2 (β_i corresponds to the coefficient of the term in the ith row). Statistical significance is indicated as follows.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5. Short-term outcomes for full sample (Part b).

	Beliefs re. attitudes in community/village		Would others support daughters' decision		Would others approve parents' decision	
	appropriate marriage age	people think worse marriage a/f 18	Vignette A	Vignette B	Vignette A	Vignette B
Treatment 1	0.024	−0.009	−0.025	−0.012	−0.061**	−0.013
	(0.117)	(0.029)	(0.028)	(0.027)	(0.030)	(0.028)
Treatment 2	0.128	−0.061**	−0.013	0.002	−0.010	−0.007
	(0.109)	(0.028)	(0.027)	(0.027)	(0.029)	(0.027)
Observations	1757	1757	1757	1757	1757	1757
dep var mean	17.300	0.494	0.680	0.315	0.522	0.299
dep var sd	1.911	0.500	0.467	0.465	0.500	0.458
T1 FWER p-val	0.963	0.963	0.820	0.963	0.089	0.652
T2 FWER p-val	0.530	0.114	0.849	0.949	0.926	0.926
$\beta 1 = \beta 2$	0.375	0.070	0.656	0.614	0.086	0.817

Note: This table presents the results of OLS regressions of short term outcome variables against treatment status indicators, including individual-level controls and village fixed effects (not shown). The sample consists of all respondents (both primary and additional respondents) in the 2018 CiMLAS survey. Standard errors are given in parentheses. The penultimate rows display Westfall–Young stepdown adjusted p-values for the coefficients on T1 and T2. These control the FWER for all tests in a given family of hypotheses (further information is provided in Section 4). The last row reports the p-value from a Wald test for a difference in coefficients between T1 and T2 (β_i corresponds to the coefficient of the term in the ith row). Statistical significance is indicated as follows.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.2 Longer-term outcomes: marriage-related outcomes of adolescent daughters

Next, we investigate whether and to what extent the intervention affects child marriage outcomes (actual marriages and steps toward marriage) after five months and 10 months. As before, our primary specification involves regressing the outcome variable of interest on binary variables indicating which treatment, if any, the respondent was exposed to, along with a set of controls including characteristics of the respondent and the respondent's daughter, and the respondent's prior knowledge regarding child marriage laws.[26]

For these longer-term outcome variables, we use the respondent's daughter as the unit of observation, limiting the sample to unmarried daughters aged between 13 and 17 years at the time of the intervention. The regression specification takes the following form:

$$y_{jihv} = \alpha + \beta_1 T_{1hv} + \beta_2 T_{2hv} + X_{jihv} + Z_{ihv} + \varepsilon_{jihv} \tag{8}$$

where y_{jihv} is the outcome variable for daughter j of respondent i in household h in village v; T_{khv} is the treatment status of household h in village v under treatment k; X_{jihv} represents the characteristics of daughter j; and Z_{ihv} the characteristics of respondent i. We do not introduce village dummies in the specifications because of the small number of observations (261 after five months and 234 after 10 months) relative to the number of villages (80). We calculate standard errors using the Eicker–Huber–White method, and include FWER adjusted p-values as before.

The outcome variables indicate (a) whether the daughter is married at the time of the interview; (b) conditional on receiving an offer of marriage, whether it has been accepted; (c) whether any steps have been taken toward the marriage of the daughter, including marriages, acceptance of marriage offers, searching for a groom, discussions within the family about searching for a groom (see Appendix Table B1 for further details).

Estimates from the regression model are shown in Table 6. The estimates indicate that the first treatment *increased* the probability of marriage by 7.2% points relative to the control group (statistically significant at the 5% level) five months after the intervention. For the purpose of comparison, the probability of marriage in the control group is 1.2%. Conditional on an offer, the first treatment also increased the probability that the offer was accepted by 20.3% (significant at the 1% level). We also estimate an 8.6% point increase in the probability of any marriage steps due to the first treatment (statistically significant at the 10% level). In the case of the second treatment, we also obtain positive coefficients for all the marriage-related outcomes but these are smaller in magnitude than the point estimates for the first treatment and are not statistically significant.

To investigate whether the treatment effects on marriage-related outcomes persist over time, we repeat the regressions with outcomes 10 months after the intervention. The estimated effects, shown in the last 3 columns of Table 6, reveal a similar pattern. The first treatment increased the probability of marriage by 7.3% points relative to the control group (significant at the 10% level), and the probability of any marriage steps by 11.2% points (significant at the 5% level). The estimated effects for the second treatment are again smaller and statistically insignificant.[27]

[26] In the Appendix, we also provide results from a specification that excludes control variables as well as a simple comparison of means across the two treatment groups and the control group.

[27] As a robustness check, we redo the estimation using the sample of girls aged 13–16 years at the time of the survey, given that those who were aged 17 may have reached the legal minimum age at the time of the follow-up surveys. In this case, we obtain estimates very similar to those in Table 6. These alternative estimates are not provided in the paper but are available upon request.

Table 6. Marriage outcomes in phone survey.

	5 Months			10 Months		
	Married	**Accepted offer**	**Any marriage steps**	**Married**	**Accepted offer**	**Any marriage steps**
Treatment 1	0.072**	0.203***	0.086*	0.073*	0.162**	0.112**
	(0.031)	(0.074)	(0.044)	(0.042)	(0.074)	(0.054)
Treatment 2	0.024	0.112	0.048	0.039	0.038	0.045
	(0.026)	(0.073)	(0.043)	(0.042)	(0.067)	(0.050)
Observations	261	112	261	234	139	234
dep var mean	0.012	0.032	0.060	0.053	0.100	0.092
dep var sd	0.110	0.180	0.239	0.225	0.304	0.291
T1 FWER p-val	0.046	0.027	0.046	0.088	0.063	0.063
T2 FWER p-val	0.442	0.286	0.442	0.580	0.583	0.580
$\beta 1 = \beta 2$	0.203	0.311	0.451	0.508	0.136	0.259

Note: This table presents the results of OLS regressions of marriage-related outcomes against treatment status indicators for a sample of female children aged 13–17 years at the time of the initial CiMLAS survey. These outcomes were collected from phone surveys conducted 5 and 10 months after the initial CiMLAS survey. A variety of controls were included (but are not shown): age of child, age at child at menarche, whether the primary respondent (PR)/child's mother was married before age 18, PR's primary education completion status, PR's knowledge of the correct legal marriage age, PR's knowledge of the correct punishments for infractions of the child marriage law, PR's awareness of the exception clause, PR's learning of the law after 2014, and PR's knowledge of a child marriage legal case. Standard errors are given in parentheses. The penultimate rows report Westfall–Young stepdown adjusted p-values which control the FWER for each tested hypothesis in the table. The last row reports the p-value from a Wald test for a difference in coefficients between T1 and T2 (β_i corresponds to the coefficient of the term in the ith row). Source: 2018 CiMLAS. Statistical significance is indicated as follows.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

To investigate whether the effect of the information intervention varies over time, we also estimate a marriage hazard model in which the hazard rate is allowed to vary according to the characteristics of the respondent, the age and birth cohort of the daughter, the season in the year, experimental arm and an indicator—for each month in the study period—on whether the law had already been implemented. The implementation rules for the new law were published in November 2018. The period of data used for the hazard model estimation is June 2018 (the month of the intervention) to February 2020. We use the latter date as the cutoff as it is the month before the first detected Covid infections and lockdowns in Bangladesh that may have subsequently affected marriage timing decisions (Makino et al. 2024). For up-to-date information on marital status and marriage dates in this period, we use two additional rounds of data collected through telephone surveys conducted in June 2020 and June 2021.

We use a Cox Proportional Hazard model as per the following equation:

$$\lambda_{jihv}(t) = \lambda_0 \exp\left(\sum_{a=0}^{25}\delta_a(j,t) + \delta_I(t) + X_{jihv} + Z_{ihv} + \cdots \right.$$
$$\left. \sum_{s=1}^{4}\delta_s(t) + \beta_1 T_{1hv} + \beta_2 T_{2hv} + \gamma_1(T_{1hv} \times \delta_I(t)) + \gamma_2(T_{2hv} \times \delta_I(t))\right) \quad (9)$$

where $\lambda_{jihv}(t)$ is the marriage hazard rate of daughter j of respondent i in month t, λ_0 is the base hazard rate[28], $\{\delta_a(j,t)\}_{a=0}^{25}$ are binary variables capturing the age of j in month t, $\{\delta_s(t)\}_{s=1}^{4}$ are

[28] Note that although the base hazard rate is assumed to be constant, the inclusion of the birth cohort and seasonal variables in the equation means that the base hazard rate is allowed to vary over time.

seasonal dummies, and $\delta_I(t)$ is a binary indicator for the implementation status of the law in month t. The terms $\beta_1, \beta_2, \gamma_1, \gamma_2$ are parameters to be estimated and the remaining variables are as defined above.

We report the hazard ratios from the estimation of Equation (9) in Table 7. In column 1, the estimated hazard ratios are 1.196 for treatment 1 and 1.418 for treatment 2, with only the latter being statistically significant. However, the specification in column 1 implicitly assumes that the treatment effect is the same from birth onwards. Given that the law specifically concerns marriage below 18 and marriage before the onset of menarche is rare in Bangladesh (Field and Ambrus 2008; Asadullah and Wahhaj 2019), in column 2 we interact treatment with "exposure" which we define as the daughter being aged between 14 and 17 in month t.[29] The hazard ratios for the interaction terms are 1.393 for treatment 1 and 1.664 for treatment 2, suggesting that information about the law increased the marriage hazard between 39 to 66 percent for girls aged 14–17 years compared to the control group. Using an F-test, we cannot reject the null hypothesis of equality of the coefficients between the treatment arms (p-value = 0.679).

In column 3, we allow the marriage hazard from treatment interacted with "exposure" to differ between the periods prior to the implementation of the new law (June-November 2018) and following implementation (December 2018 to February 2020). We typically obtain a hazard ratio above 1, but the hazard ratio for treatment 1 interacted with "exposure" *falls* somewhat after implementation and that for treatment 2 *rises* sharply after implementation. These ratios suggest that some households provided information about the harsher punishments in the new law rushed to marry off girls aged 14–17 prior to the implementation of the law, but the marriage hazard remained higher than in the control group following implementation. By contrast, households provided information about the harsher punishments as well as the exception clause rushed to marry off girls of this age primarily *after* the implementation of the new law. Nevertheless, we cannot reject equality of the coefficients between the treatment arms either for the pre-implementation period (p-value = 0.233) or the post-implementation period (p-value = 0.321).

4.3 Heterogeneous treatment effects

Next, we investigate whether the information intervention had heterogeneous effects on the short-term outcomes between households with unmarried adolescent girls at baseline, and those without. Specifically, we modify Equation (7) by interacting the treatment dummies with an indicator for the presence/absence of unmarried adolescent girls in the respondent's household. The reason there may be heterogeneous treatment effects along this dimension is that information provided (about child marriage law) during the experiment was potentially more salient if an unmarried adolescent girl was living in the household. This specification also allows a better comparison between estimated effects on short-term outcomes and marriage outcomes, given that the latter are—by construction—only available for households with unmarried adolescent girls at baseline. The estimates based on the modified equation are reported in Tables 8–9.

We find considerable heterogeneity in the effects on beliefs and attitudes toward early marriage elicited through vignettes: For the subsample with unmarried adolescent girls, Treatment 1 has negative effects on beliefs regarding whether "other parents in the village [would] support their daughter's decision to delay marriage" (significant for Vignette B

[29] All columns include an age dummy for the age group 14–17 by itself. We choose 14 as the initial age of exposure as 94% of the daughters in the dataset had attained menarche by this age.

Table 7. Marriage hazard ratios using exponential model.

	Analysis time when record ends	Analysis time when record ends	Analysis time when record ends
Treatment 1	1.196	1.157	1.157
	(0.325)	(0.425)	(0.425)
Treatment 2	1.418**	1.337	1.337
	(0.048)	(0.107)	(0.108)
T1 × Exposed		1.393	
		(0.319)	
T2 × Exposed		1.664	
		(0.160)	
T1 × Pre-Exposed			1.650
			(0.404)
T2 × Pre-Exposed			0.397
			(0.372)
T1 × Post-Exposed			1.296
			(0.515)
T2 × Post-Exposed			2.212**
			(0.045)
Observations	142190	142190	142190
T1 × Exp = T2 × Exp		(0.679)	
T1 × Pre = T2 × Pre			(0.233)
T1 × Post = T2 × Post			(0.321)

Note: The table presents hazard ratios from a marriage hazard model for a sample of female children aged 13–17 at the time of the initial CiMLAS survey. Marriage outcomes were collected from phone surveys conducted in November 2018, May 2019, June 2020 and June 2021. Marriage outcomes between June 2018 and February 2020 are used in the analysis. 'Exposed' is an indicator for age between 14 and 17 in the relevant time period. 'Pre-Exposed' ('Post-Exposed') indicates exposure prior to (after) November 2018. All specifications include period-specific age group dummies, birth cohort dummies and the controls listed in the notes to Table 10. Values in parenthesies denote p-values. The last three rows show p-values from F-tests of the equality of the coefficients between treatment arms. Statistical significance is denoted by
*p <0.10, **p <0.05, ***p <0.01. Source: 2018 CiMLAS.

only) and whether "neighbours and extended family [would] approve the parents' decision to support their daughter and delay marriage" (significant for both vignettes); while Treatment 2 has negative effects on "would support daughter's decision to delay marriage" and beliefs regarding approval by neighbors and extended family (significant for Vignette B only). By contrast, the corresponding point estimates for the subsample without unmarried adolescent girls are close to zero. Treatment 2 has significant effects on beliefs about community attitudes regarding appropriate marriage age, but only for the sample with unmarried adolescent girls. Finally, estimated effects of Treatment 1 on financial contributions are large and positive for the sample *with* unmarried adolescent girls (on both the extensive and intensive margins), with no discernable effects on the sample without.

In Appendix Tables B7–B8, we investigate whether the information intervention had heterogeneous effects by the gender of the respondent. The point estimates of the effects of both treatments on the appropriate marriage age reported by women are close to zero and statistically insignificant. But the corresponding effects for men are negative and statistically significant (at the 10% level). The estimates imply that information about the new law induces men to report a lower appropriate marriage age by 8–10 months on average (the difference in the estimated effects of the two treatments is not statistically significant). There is no effect on the proportion who believe marriage before 18 is appropriate for either gender, as the effect on men is due to a shift in stated appropriate marriage age in the part of the age distribution above 18 years of age.

For the other outcomes, the estimated effects for women are similar to those obtained with the original specification, while the interaction terms are small and/or statistically

Table 8. Treatment heterogeneity: short-term outcomes (Part A).

	Attitudes toward early marriage		Would support daughters' decision		Financial contribution to NGO	
	Appropriate marriage age	Marriage before 18 appropriate	Vignette A	Vignette B	Make a contribution	Contribution amount
Treatment 1 (T1)	−0.002	0.001	0.026	−0.072	0.133**	21.128***
	(0.171)	(0.017)	(0.031)	(0.054)	(0.052)	(5.762)
Treatment 2 (T2)	−0.111	0.029	−0.044	−0.160***	0.051	5.894
	(0.203)	(0.023)	(0.036)	(0.059)	(0.052)	(5.085)
No daughter	−0.208	0.037**	−0.025	−0.030	0.098**	10.570***
	(0.143)	(0.017)	(0.027)	(0.045)	(0.043)	(4.091)
T1 × No daughter	0.043	−0.009	−0.019	0.050	−0.152**	−20.160***
	(0.204)	(0.022)	(0.036)	(0.064)	(0.061)	(6.741)
T2 × No daughter	−0.102	−0.027	0.056	0.132*	−0.082	−8.140
	(0.232)	(0.027)	(0.040)	(0.068)	(0.061)	(6.094)
Observations	1757	1757	1757	1757	1757	1757
dep var mean	18.741	0.039	0.912	0.620	0.395	24.189
dep var sd	1.643	0.194	0.284	0.486	0.489	49.400
T1 FWER p-val	1.000	1.000	0.785	0.531	0.006	0.001
T2 FWER p-val	0.585	0.481	0.481	0.024	0.393	0.393
T1 × No daughter (FWER p-val)	0.931	0.931	0.931	0.887	0.012	0.003
T2 × no daughter (FWER p-val)	0.660	0.521	0.414	0.193	0.288	0.288
$\beta1+\beta4=0$	0.738	0.568	0.712	0.526	0.547	0.791
$\beta2+\beta5=0$	0.059	0.890	0.521	0.384	0.316	0.474
$\beta1=\beta2$	0.605	0.180	0.039	0.141	0.127	0.015
$\beta1+\beta4=\beta2+\beta5$	0.026	0.481	0.801	0.836	0.719	0.345

Note: This table presents the results of OLS regressions of short term outcome variables against treatment status indicators interacted with a dummy indicating whether the respondent has no unmarried daughters in the household. The regressions also include village fixed effects and the same controls from Table 4. The sample consists of all respondents (both primary and additional respondents) in the 2018 CiMLAS survey. Standard errors are provided in parentheses, while Westfall–Young stepdown adjusted p-values to control the FWER are reported in the second half of the table. The last four rows report the p-values from a set of Wald tests (β_i corresponds to the coefficient of the term in the ith row of the table). Source: 2018 CiMLAS.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

insignificant. However, it is worth noting that, for men, the two treatments have similar effects on financial contributions on both the extensive and intensive margin (the point estimates are similar and the differences are statistically insignificant), while, for women, the first treatment has a larger effect (the point estimates for the first treatment are larger and the differences are statistically significant).

Next, we investigate whether there are heterogeneous effects of treatment on the marriage-related outcomes of adolescent girls according to *who* in the extended family was exposed to the information intervention. Recall that, together with the primary respondent—a woman aged between 24 and 43 years at the time of the survey in June 2018—a number of additional members of the extended family (0, 1 or 2, depending on a random draw) were also selected for interviews and exposure to the same video-based information (see Section 3.1 for further details). We exploit this variation to investigate whether exposing members of the extended family to the treatment (in addition to the mother) affects the marriage-related outcomes of adolescent girls. Specifically we construct, for each female respondent included in the June 2018 survey, a binary variable indicating whether she alone had received the treatment (binary variable = 1) or her

Table 9. Treatment heterogeneity: short-term outcomes (Part B).

	Beliefs re. attitudes in community/village		Would support daughters' decision		Would others approve parents' decision	
	Appropriate marriage age	People think worse marriage a/f 18	Vignette A	Vignette B	Vignette A	Vignette B
Treatment 1 (T1)	0.145	−0.006	−0.084	−0.092*	−0.142**	−0.150***
	(0.226)	(0.055)	(0.053)	(0.051)	(0.056)	(0.051)
Treatment 2 (T2)	0.557**	−0.102*	−0.035	−0.036	−0.084	−0.098*
	(0.218)	(0.059)	(0.058)	(0.055)	(0.062)	(0.054)
No daughter	0.034	0.018	−0.024	−0.050	−0.069	−0.108**
	(0.175)	(0.047)	(0.043)	(0.044)	(0.048)	(0.044)
T1 × No daughter	−0.169	−0.005	0.081	0.109*	0.110*	0.187***
	(0.261)	(0.064)	(0.062)	(0.061)	(0.066)	(0.061)
T2 × No daughter	−0.558**	0.053	0.029	0.051	0.098	0.120*
	(0.249)	(0.068)	(0.066)	(0.064)	(0.071)	(0.063)
Observations	1757	1757	1757	1757	1757	1757
dep var mean	17.300	0.494	0.680	0.315	0.522	0.299
dep var sd	1.911	0.500	0.467	0.465	0.500	0.458
T1 FWER p-val	0.796	0.938	0.319	0.269	0.014	0.007
T2 FWER p-val	0.052	0.222	0.718	0.718	0.157	0.114
T1 × No daughter (FWER p-val)	0.774	0.944	0.476	0.269	0.100	0.004
T2 × No daughter (FWER p-val)	0.090	0.817	0.817	0.817	0.155	0.095
$\beta1+\beta4=0$	0.858	0.763	0.936	0.589	0.368	0.254
$\beta2+\beta5=0$	0.993	0.126	0.863	0.652	0.681	0.475
$\beta1=\beta2$	0.061	0.094	0.398	0.308	0.331	0.314
$\beta1+\beta4=\beta2+\beta5$	0.866	0.237	0.932	0.920	0.193	0.657

Note: This table presents the results of OLS regressions of short-term outcome variables against treatment status indicators interacted with a dummy indicating whether the respondent has no unmarried daughters in the household. The regressions also include village fixed effects and the same controls from Table 4. The sample consists of all respondents (both primary and additional respondents) in the 2018 CiMLAS survey. Standard errors are provided in parentheses, while Westfall–Young stepdown adjusted p-values to control the FWER are reported in the second half of the table. The last four rows report the p-values from a set of Wald tests (β_i corresponds to the coefficient of the term in the ith row of the table). Source: 2018 CiMLAS.

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

husband or a family "elder" had also been interviewed (and consequently provided the same video-based information; binary variable = 0). For this purpose, we define a family "elder" as the respondent's father-in-law, mother-in-law, husband's elder brother, husband's elder brother's wife, father, mother, elder brother or elder brother's wife.

We modify Equation (8) by interacting the treatment dummies with the binary variable described above. The estimates for this modified equation are reported in Table 10. In the case of Treatment 1, treating the husband and family "elders" in addition to the main respondent yields effects that are statistically significant and larger in magnitude than those reported in Table 6. In the case of Treatment 2, the corresponding effects are smaller and—with the exception of any marriage steps after 10 months—statistically insignificant. In other words, when information about the new child marriage law is provided to the mother as well as other members of the extended family, Treatment 1 has a strong effect on marriage-related outcomes five months and 10 months after the intervention, while Treatment 2 has weaker, typically statistically insignificant, effects. By contrast, if the information intervention

Table 10. Treatment heterogeneity by identity of treated household members: marriage outcomes in phone survey.

	5 Months			10 Months		
	Married	**Accepted offer**	**Any marriage steps**	**Married**	**Accepted offer**	**Any marriage steps**
Treatment 1	0.110**	0.279***	0.146**	0.150**	0.241**	0.188***
	(0.045)	(0.101)	(0.062)	(0.059)	(0.099)	(0.071)
Treatment 2	0.025	0.112	0.045	0.060	0.061	0.106*
	(0.023)	(0.086)	(0.050)	(0.045)	(0.075)	(0.062)
Only wife interviewed	0.024	0.103	0.035	0.063	0.038	0.090
	(0.034)	(0.096)	(0.058)	(0.057)	(0.099)	(0.074)
T1 × Wife only	−0.079	−0.184	−0.124	−0.174**	−0.182	−0.175
	(0.062)	(0.156)	(0.088)	(0.087)	(0.171)	(0.115)
T2 × Wife only	−0.002	0.013	0.007	−0.049	−0.043	−0.140
	(0.049)	(0.183)	(0.084)	(0.085)	(0.135)	(0.104)
Observations	261	112	261	234	139	234
dep var mean	0.012	0.032	0.060	0.036	0.077	0.072
dep var sd	0.110	0.180	0.239	0.188	0.270	0.261
T1 FWER p-val	0.053	0.037	0.053	0.032	0.032	0.029
T2 FWER p-val	0.455	0.432	0.455	0.271	0.399	0.186
T1 × Wife FWER p-val	0.298	0.298	0.298	0.128	0.307	0.222
T2 × Wife FWER p-val	1.000	1.000	1.000	0.708	0.748	0.324
$\beta1+\beta4=0$	0.462	0.407	0.723	0.705	0.646	0.885
$\beta2+\beta5=0$	0.615	0.399	0.455	0.875	0.879	0.687
$\beta1=\beta2$	0.089	0.126	0.123	0.201	0.114	0.324
$\beta1+\beta4=\beta2+\beta5$	0.871	0.849	0.670	0.601	0.729	0.585

Note: This table presents the results of OLS regressions of marriage-related outcomes against treatment status indicators interacted with a dummy variable indicating whether only the primary respondent was interviewed and treated (in the base category, the primary respondent's husband or family elder was also treated). The sample, outcomes, controls, and general specification (apart from the inclusion of interactions) are the same as in Table 6. As in previous tables, standard errors are given in parentheses and Westfall–Young stepdown adjusted p-values are reported in the second half of the table. The last four rows report the p-values from a number of Wald tests (β_i corresponds to the coefficient of the term in the ith row). Source: 2018 CiMLAS.

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

is limited to the mother only, we detect no statistically significant effect of either treatment (the interaction term is negative with the exception of accepted offers and any marriage steps after five months).

In about 14% of cases in which households were randomly assigned for family elders to be treated, no elders were in fact treated because none were living close by. Thus, the differential treatment effects estimated in Table 8 may be due, at least in part, to household characteristics correlated with the *presence* of family elders.[30] To check for this possibility, we estimate a 2SLS model in which the initial elder treatment assignment status—and its interaction with the treatment variables –serve as instruments for the actual treatment status of elders and its interaction with the treatment variables. The 2SLS estimates, shown in Table 11, are broadly similar to the OLS estimates, although the estimated interaction effects are noisier.

[30] We thank an anonymous referee for raising this point.

Table 11. Treatment heterogeneity by identity of treated household members: marriage outcomes in phone survey (IV Analysis).

	5 Months			10 Months		
	Married	Accepted offer	Any marriage steps	Married	Accepted offer	Any marriage steps
Treatment 1	0.085	0.237**	0.027	0.151*	0.313**	0.137
	(0.057)	(0.119)	(0.086)	(0.080)	(0.126)	(0.097)
Treatment 2	0.046	0.148	0.022	0.068	0.070	0.063
	(0.032)	(0.102)	(0.063)	(0.063)	(0.095)	(0.075)
Only wife interviewed	0.080	0.210	0.025	0.108	0.114	0.017
	(0.071)	(0.166)	(0.084)	(0.124)	(0.162)	(0.130)
T1 × Wife only	−0.034	−0.106	0.115	−0.179	−0.350	−0.057
	(0.113)	(0.233)	(0.151)	(0.158)	(0.237)	(0.185)
T2 × Wife only	−0.052	−0.084	0.048	−0.073	−0.054	−0.041
	(0.085)	(0.241)	(0.113)	(0.143)	(0.201)	(0.152)
Observations	261	112	261	234	139	234
dep var mean	0.012	0.032	0.060	0.036	0.077	0.072
dep var sd	0.110	0.180	0.239	0.188	0.270	0.261
$\beta1+\beta4=0$	0.476	0.401	0.114	0.773	0.804	0.491
$\beta2+\beta5=0$	0.918	0.716	0.362	0.966	0.913	0.833
$\beta1=\beta2$	0.492	0.440	0.956	0.269	0.049	0.429
$\beta1+\beta4=\beta2+\beta5$	0.407	0.704	0.455	0.765	0.689	0.580

Note: This table presents the results from two stage least squares estimation of marriage-related outcomes against treatment status indicators interacted with a dummy variable indicating whether only the primary respondent was interviewed and treated (in the base category, the primary respondent's husband or family elder was also treated). Although treatment of additional family members was subject to experimental variation, additional family members were not always able to be interviewed in practice. Therefore, actual treatment status of additional family members is instrumented by allocation to the experimental group (i.e. intention to treat). The sample, outcomes, controls, and general specification (apart from the inclusion of interactions and instruments) are the same as in Table 6. As in previous tables, standard errors are given in parentheses and the last four rows report the p-values from a number of Wald tests (β_i corresponds to the coefficient of the term in the ith row). Source: 2018 CiMLAS. Statistical significance is indicated as follows.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

5. INTERPRETATION OF RESULTS

5.1 Explaining treatment effects

Next we consider the results described above in light of our three main questions: (a) whether a legal reform has an "expressive effect"; (b) whether moderating elements in the new law make it more or less effective; (c) whether changes in the law affect customary authorities differentially (e.g. by generating a backlash). We also consider whether and to what extent the results support any of the theories discussed in Section 3.2.3.

Our results on measured beliefs and attitudes in Section 4.1 and 4.3 do not support the notion that the legal reform has an "expressive effect". In particular, contrary to the "expressive effect" theory, we find that informing participants about the harsher punishments in the new law (Treatment 1) *reduced* beliefs (for respondents in households with unmarried adolescent girls) that other parents would support their daughter's decision to delay marriage, and that neighbors and the extended family would approve a decision to support, while the theory predicts the opposite (Table 9). The theory also predicts that informing respondents about the more moderating elements in the new law (Treatment 2) would negatively affect beliefs that other parents would support and approve delayed marriage relative to the first treatment. We find no evidence of this and, in some instances, we find the

opposite, i.e. that Treatment 1 has a more negative effect on these beliefs (Table 9).[31] A possible explanation for the absence of an expressive effect in the data is that the vignettes and survey questions are concerned with the beliefs of the community and extended family (arguably the relevant reference group where marriage norms are concerned) for which the child marriage legal reform—initiated by the central government and approved in the national parliament—may not serve as an informative signal.[32]

While our estimated effects on beliefs and attitudes are not consistent with the law having an "expressive effect," they are consistent with what would be expected under the customary authority theory when there is a backlash. Both treatments *lowered* the appropriate age of marriage stated by men (Appendix Table B7), consistent with the notion that male family elders reverted to a more traditional position in response to a progressive law, as discussed in Section 2.2. We also find that respondents exposed to the first treatment were less likely to believe that neighbors and extended family members would approve if parents turned down a marriage proposal for an adolescent daughter (Tables 5 and 9). This suggests that, consistent with the notion of a backlash effect of the new law, those exposed to information about harsher punishments in the new law (Treatment 1) expected greater pressure of early marriage from neighbors and extended family members. Additionally informing respondents about the moderating elements in the new law (Treatment 2) led to smaller effect sizes, although the differences with Treatment 1 are typically not statistically significant.

In Section 4.2, we found that informing adults about the harsher punishments for child marriage stipulated in the new law accelerated marriages of adolescent girls within the household, contrary to the direction of change implied by the expressive effect (see Table 2). The hazard model analysis indicates that the marriage risk of adolescent girls in Treatment 2 increased only after the implementation of the new law, while the estimated coefficients for Treatment 1 are larger prior to implementation, although statistically insignificant in both the pre- and post-implementation periods under this specification. These patterns are consistent with the hypothesis—discussed in Section 2.2—that the information intervention affected marriage timing decisions by changing perceptions about the *future* enforcement of the law (see also Table 2).

This interpretation is similar to an explanation in the literature for an observed spike in early marriages in the 1931 Indian Census. Demographers have argued that in the late 1920s, parents had rushed to marry off their daughters before the 1929 Child Marriage Act came into effect—a law which set the minimum age of marriage at 14 for girls—believing that they had only a short window to continue with their traditional marriage practices (See Caldwell, Reddy and Caldwell 1983 and the references within). A recent study by Roy and Tam (2021) uses a difference-in-differences strategy to estimate that the law had an "announcement" effect that increased the proportion of girls married at ages 5–10 by 20–29% in British India relative to the princely states.[33]

The heterogeneity analysis in Section 4.3 showed that, while the effect is absent when only the mothers of the adolescent girls are informed about the new law, it is large and

[31] Our estimates also imply that Treatment 2 positively affects beliefs about the appropriate marriage age in the community (Table 9), which is contrary to the "expressive effect" theory and, indeed, all of the theories we consider. It is also inconsistent with the estimated effects on beliefs elicited through the vignette-based questions. We leave to future research a consistent explanation of these results.

[32] We thank an anonymous referee for proposing this explanation. It is conceivable that there was an expressive effect via changes in beliefs regarding the preferences of the members of the central government and/or national parliament that we did not capture in our survey data. However, if so, then we would expect to see an effect on longer-term outcomes in the direction implied by the expressive effect, but, as we discuss below, we do not.

[33] In a different but related context, Camilotti (2016) finds that legal sanctions against female genital cutting in Senegal lowered the age of cutting; and attributes the change in age to de-ritualization and individualisation of FGC to lower the risk of detection and legal prosecution.

statistically significant when the father or a male elder within the extended family is informed about the law alongside the mother. Again, these patterns are consistent with the idea of a backlash from family elders in response to the first treatment, as discussed in Section 2.2. Additionally, informing adults about the moderating elements in the new law appears to have mitigated the backlash: the effect on actual marriages of informing extended family members about the new law is smaller in the case of Treatment 2 compared to Treatment 1, although, again, the differences in point estimates are typically not statistically significant.

This last set of results also imply that when the mother is the only person within the household to be informed, she withholds this information from other members of the family (if not, the marriage outcomes would not depend on who in the household received the information).[34] Such behavior makes sense if, as implied by the estimated effects on responses to the vignette-related questions, the information about the harsher punishments in the new law does not affect a mother's support for a daughter's desire to delay marriage but negatively affects her belief about whether neighbors and extended family members would approve such support.

It is worth noting that the estimated effects on marriage outcomes are based on a subsample of households with unmarried adolescent daughters (aged 13–17 years) during the information intervention in 2018. Recall that the primary respondents were drawn from a sample of women aged 20–39 years in 2014 (see Section 3.1), who were thus 24–43 years of age in 2018. Respondents in the full sample who married late were, mechanically, less likely to have children who had reached adolescence by the time of the intervention.[35] Therefore, we explore whether our estimated effects hold for girls whose mothers experienced later marriage. Specifically, we estimate an alternative equation in which we interact the treatment variables with a binary indicator for whether the mother married before reaching 18 years of age. The estimates are shown in Appendix Table B9. The estimates indicate that the treatments lead to increased marriage only if the mother had experienced early marriage. The corresponding estimates for adolescents whose mothers married at 18 or above are closer to zero and, in the case of Treatment 2, negative. Thus, the perverse effects of the intervention are driven by households in which the previous generation had experienced early marriage.[36]

The results on financial contributions (Tables 4 and 8) imply that information about the harsher punishments in the new law increased support for the activities of the charity working on child marriage prevention. Given the estimated effects on actual marriage outcomes over the longer term, the impact on financial contributions may appear counter-intuitive. But it is plausible that, in a new environment in which traditional marriage practices have more severe consequences within the formal legal system, the alternative dispute resolution mechanism offered by the charity—with its possibility of compromise without the involvement of law enforcement authorities and the risk of criminal punishment—would appeal to

[34] This echoes findings in the existing literature which provides evidence on lack of information-sharing within the household. See Baland and Ziparo (2018) for a recent review of this literature.

[35] We find that a chi-square test of independence between indicators for the presence of unmarried adolescent girls and early marriage of the primary respondent rejects the null hypothesis. Similarly, the former variable is not independent of the respondent's schooling (which is correlated with marriage age) but the null hypothesis cannot be rejected for other household or respondent characteristics such as the land ownership, awareness of child marriage court cases, employment status of the mother, and number of elders interviewed.

[36] If the mother's age of marriage is considered a proxy for the existing marriage age norm within the extended family, then these heterogeneous treatment effects are broadly consistent with the theoretical model (Proposition 3). They also echo existing empirical evidence on the effects of the threat of sanctions in a different type of community: Casaburi and Machiavello (2015) show that the announcement of sanctions against members of a dairy cooperative that failed to comply with the co-op's by-laws regarding milk sales, had heterogeneous effects, leading to improved compliance by the most engaged farmers as well as increased non-compliance by others.

both traditionalists and progressives.[37] The absence of an effect on financial contributions in the case of the second treatment also makes sense if we interpret the exception clause as signaling the possibility of compromise within the formal legal system.

5.2 Alternative explanations

Next, we consider possible alternative explanations for the backlash effect of the law on attitudes and behavior drawing on alternative theories of the social effects of legal reforms and interventions in the existing literature.

Chen and Yeh (2014) argue that providing information about a new law can produce a backlash if the law creates the perception that the behavior it prohibits is more widespread than previously believed. In the present context, we find that the information treatment did not change beliefs about the prevalence of child marriage within the community (results shown in Appendix Table B10).

Acemoglu and Jackson (2017) show theoretically, in a setting where detection of law-breaking behavior and enforcement of the law relies at least in part on whistleblowing by members of the public, that a legal change that expands the range of behavior that falls outside of the law can lead to reduced whistleblowing and an increase in the behavior that is legally prohibited.[38] Most of our respondents already knew the minimum age of marriage for girls at the time of the intervention (88% among the primary respondents and 83% among the additional respondents). Bearing in mind that the reform consisted not of modifying the minimum age at marriage but of increasing the penalty for its violation, the first treatment would not have shifted people's perceptions about the range of behavior that falls outside of the law for this subsample. Yet, when we re-estimate the equations for the longer-term marriage outcomes with this subsample, we still find that the treatment increased the probability of early marriage (the results, shown in Appendix Tables B11–B12, are very similar to those obtained for the full sample, although the estimate of marriage status at 10 months is noisier and insignificant).[39]

The information intervention may have affected perceptions about the likelihood of enforcement of the minimum age law. Specifically, if the first treatment—information about the harsher punishment only—led to the belief that enforcement would be weaker under the new law, then

[37] A large-scale survey conducted in 2017 highlighted lack of public confidence in formal legal institutions in Bangladesh and, among low income groups, a preference for consulting with community leaders to resolve disputes (Kind et al., 2018). Legal aid NGOs in Bangladesh engage in initiatives aimed at reforming the traditional justice system or *shalish*, which refers to a "community-based … informal process through which influential local people help resolve community members' disputes" (Golub 2013). These initiatives include organizing *shalish* panels, training community members to conduct *shalish*, supplementing *shalish* panel with individuals with less traditional perspectives, and training citizens to "persuade, educate and otherwise influence traditional *shalish* bodies" (Golub 2013; see also Begum 2006). In this context, the NGO for which financial contributions were sought may have been perceived as moderating rather than undermining the customary authority in resolving disputes relating to marriage timing. Additionally, the charity trains young paralegals to provide legal advice to parents within their communities on child marriage issues. In a focus group discussion with a number of these paralegals conducted in 2023 for a related project (https://www.globalinnovation.fund/investments/blast), they reported that parents in their communities not only look to them for advice but also see them as role models for their daughters, guiding them "on the right path". An important factor that drives parents toward early marriage of their daughters is the fear is they would otherwise engage in premarital relations or acquire the reputation for doing so. If these parents believe that the legal reform would make the option of early marriage more remote, it is plausible that they look to the charities for further guidance and protection of their daughters and, consequently, become more supportive of their activities.

[38] Two key assumptions for this result are that (a) only members of the public who comply with the law can engage in whistle-blowing; and (b) the negative externality inflicted by law-breaking behaviour by others is not too large. In the Bangladesh context, helplines have been used successfully by members of the community to report on imminent weddings involving minors to law enforcement authorities. For example, calls to the national emergency helpline led to the prevention of 7,304 child marriages between 2018 and 2020, including "calls from neighbours, sisters, friends, boyfriends, teachers, and even from grooms' relatives as well" (Hossain 2020).

[39] Acemoglu and Jackson (2017) show that a legal change that increases the penalty for behaviour outside of the law can, under certain scenarios, also generate a backlash effect in a subset of the population. But this mechanism involves increased compliance in another subset of the population. Therefore, it is unlikely that this mechanism could account for the negative and large average effects we observe.

this could explain why the treatment led to an increase in early marriages.[40] However, such a mechanism is unlikely to account for our findings given that the law was rarely enforced even before the change in child marriage law in 2017 (see Section 2.1).

5.3 Truthful reporting by survey respondents

Before concluding, we consider the possibility that survey respondents were not truthful in their answers and ask whether biased reporting may affect the interpretation of the findings. First, we note that the study participants' responses regarding preferred age of marriage may not reflect their true preferences but some strategic motive, for example certain social advantages from aligning their stated preferences with the formal law or the prevalent attitudes within the community. Although we cannot distinguish between true preferences and strategy-driven responses, the stated preferences are of interest in their own right because, as noted in Section 3.2.3, the two theories make different predictions about stated preferences regarding female age of marriage.

Next, we consider two other reasons that respondents may have withheld their true opinions or actual behavior regarding traditional marriage practices: (a) experimenter demand effects (Zizzo 2010; de Quidt, Haushofer and Roth 2018) and (b) social desirability bias. We discuss each in turn. Experimenter demand effects may have occurred if respondents interpreted the information provided in the videos as a signal of the objectives of the study and the type of answers expected of them. However, this is unlikely in our context because (a) enumerators were not aware of the treatment status of study participants,[41] and (b) differences in information provided across the videos were very subtle. In particular, both treatment groups received the same information as the control group about the purpose of the study and the legal minimum age of marriage (18 years). The videos shown to the participants in the treatment groups were almost identical—shot by shot—to that shown to the control group. The only differences in information content across the three groups related to the severity of the punishment and the exceptions permitted—but this additional information does not map readily to specific answers to the questions subsequently asked. Therefore, experimenter demand effects are unlikely to explain any differences in responses or behavior between the treatment and control groups.

Next we turn to the issue of social desirability bias, which, in this context, refers to the possibility that respondents' responses may have been biased by fear or discomfort in reporting behavior contrary to the law. Respondents to the 2018 survey were asked to provide information about the marital status and marriage age of their own daughters. Of the marriages reported by the primary respondents, the marriage age was below the legal minimum age (18 years) for 69% (N = 159). The median age of marriage for daughters aged 20–24 years was 17 years (N = 69), which is close to the national figure of 17.2 years obtained from the 2014 Bangladesh Demographic and Health Survey (NIPORT 2016). The high frequency of underage marriage reported among their own daughters, similar to rates obtained from other sources, suggests that the respondents had no reservations about reporting behavior contrary to the law.

6. CONCLUSION

In this article, we addressed the question of whether a change in the formal law regarding child marriage can influence social attitudes and behavior in a situation characterized by weak law enforcement. In addition, we tested whether moderating elements in the new law made it more or less effective. For this purpose, we made use of a new child marriage law in Bangladesh which was recently

[40] Aldashev et al. (2012a) argue that a legal reform that moves the formal law further from the custom may lead to weaker enforcement if it increases the likelihood of deviation from the written law by the police, prosecutors, and judges.
[41] See Section 3.2 for details.

approved by the national parliament and conducted a randomized information treatment aimed at accelerating knowledge transmission about the new law in rural areas. The new law introduced two key changes: (a) harsher punishments for facilitating underage marriage and (b) a special clause that permitted marriage at any age if a court deemed this to be "in the best interests of the minor". To disentangle the effects of the more severe and moderating elements of the new law on attitudes and behavior, we experimentally varied the legal information that study participants received.

We find that adolescent girls in households that were informed about the harsher punishments stipulated in the new child marriage law were more likely to experience early marriage. These perverse effects are absent in households where only the mother of the adolescent girl receives the information treatment, but are large and statistically significant when the information is received both by the mother and (separately) by other members of the extended family.

Thus, the intervention had a "backlash" effect against the new law, causing an acceleration of marriages for adolescent girls, the very behavior that the law was meant to discourage. The empirical patterns are consistent with two theoretical mechanisms: (a) a rush to marry off adolescent girls in anticipation of a future increase in punishment or enforcement of the new law; (b) family elders—who have primary responsibility within the extended family for ensuring adherence to marriage customs—reverting to a more traditional position in response to a legal reform that made the formal law too remote from their own preferences and beliefs regarding the appropriate female marriage age, thereby increasing pressures of early marriage within the extended family. Information about the special clause aimed at moderating the legal reform but did not produce a positive shift in attitude and behavior compared to the status quo. Rather, it led to an increase in the marriage hazard after the implementation of the new law.

The perverse effects are limited to households in which the previous generation had experienced early marriage. Nevertheless, they constitute an important finding as the majority of women in Bangladesh—and consequently the majority of mothers—experience early marriage. More generally, they carry an important message for the design of future interventions and programs that make use of formal laws to bring about social change on issues where tradition and custom have hitherto played a dominant role. If the laws are perceived as being contradictory to the custom, then the population may respond to a significant reform in ways that aim to defy or circumvent the state authority, with unintended consequences for the intended beneficiaries of the program.

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APPENDIX A: PROOFS OF THEORETICAL RESULTS

Proof. (Proposition 1): We use the notation $m_{u1} = \frac{\partial m_u(c,\sigma)}{\partial c}$, $m_{u2} = \frac{\partial m_u(c,\sigma)}{\partial \sigma}$. Differentiating throughout Equation (2) w.r.t. c, we obtain

$$d''(m_u(c,\sigma)-\mu_i)m_{u1} + \lambda_r d''(m_u(c,\sigma)-\sigma)m_{u1} + \lambda_c d''(m_u(c,\sigma)-c)(m_{u1}-1) = 0$$

$$\Rightarrow m_{u1}\{\lambda_r d''(m_u(c,\sigma)-\sigma) + \lambda_c d''(m_u(c,\sigma)-c) + d''(m_u(c,\sigma)-\mu_i)\} = \lambda_c d''(m_u(c,\sigma)-c)$$

$$\Rightarrow m_{u1} = \frac{\lambda_c d''(m_u(c,\sigma)-c)}{\lambda_r d''(m_u(c,\sigma)-\sigma) + \lambda_c d''(m_u(c,\sigma)-c) + d''(m_u(c,\sigma)-\mu_i)} \tag{A1}$$

Since $d''(.) > 0$, we have $m_{u1} > 0$. Since the denominator in Equation (A1) is greater than the numerator, $m_{u1} < 1$. Similarly, differentiating throughout Equation (2) w.r.t. σ and arranging terms as above, we obtain $m_{u2} \in (0,1)$. □

Proof. (Lemma 2): Using the Envelope Theorem,

$$\frac{d}{d\mu}\{U_f(m_u(\mu),c) - U_f(m_l,c)\} = d'(m_u(\mu)-\mu) - d'(m_l-\mu)$$

By construction, $m_u(\mu) < m_l$. Therefore, $d'(m_u(\mu)-\mu) < d'(m_l-\mu)$. It follows that

$$\frac{d}{d\mu}\{U_f(m_u(\mu),c) - U_f(m_l,c)\} < 0 \tag{A2}$$

Thus, the difference in expected utility from underage marriage relative to marriage at the legal minimum age is decreasing in the family's own marriage age preference μ. By definition, a family with marriage age preference $\overline{\mu}(c,m_l,\sigma,P)$ is indifferent between the two choices. Therefore, a family opts for underage marriage if $\mu < \overline{\mu}(c,m_l,\sigma,P)$ and abides by the law if $\mu > \overline{\mu}(c,m_l,\sigma,P)$.

By definition, the threshold marriage age preference satisfies the following equation:

$$U_f(m_u(\overline{\mu}(c,m_l,\sigma,P)),c) - U_f(m_l,c) = 0 \tag{A3}$$

Differentiating throughout Equation (A3) w.r.t. P, we obtain

$$\frac{\partial}{\partial P}U_f(m_u(\overline{\mu}(c,m_l,\sigma,P)),c) + \frac{\partial \overline{\mu}}{\partial P}\frac{d}{d\mu}\{U_f(m_u(\mu),c) - U_f(m_l,c)\} = 0$$

$$\Rightarrow \frac{\partial \overline{\mu}}{\partial P} = -\frac{\frac{\partial}{\partial P}U_f(m_u(\overline{\mu}(c,m_l,\sigma,P)),c)}{\frac{d}{d\mu}\{U_f(m_u(\mu),c) - U_f(m_l,c)\}} \tag{A4}$$

Using the Envelope Theorem, $\frac{\partial}{\partial P}U_f(m_u(\overline{\mu}(c,m_l,\sigma,P)),c) = -1$. And, we have shown above that the denominator in Equation (A4) is negative. Therefore, $\frac{\partial \overline{\mu}}{\partial P} < 0$. Differentiating throughout (A3) w.r.t. σ, we obtain

$$\frac{\partial}{\partial\sigma}\{U_f(m_u(\overline{\mu}(c,m_l,\sigma,P)),c)-U_f(m_l,c)\}+\frac{\partial\overline{\mu}}{\partial\sigma}\frac{d}{d\mu}\{U_f(m_u(\mu),c)-U_f(m_l,c)\}=0$$

$$\Rightarrow\frac{\partial\overline{\mu}}{\partial\sigma}=-\frac{\frac{\partial}{\partial\sigma}\{U_f(m_u(\overline{\mu}(c,m_l,\sigma,P)),c)-U_f(m_l,c)\}}{\frac{d}{d\mu}\{U_f(m_u(\mu),c)-U_f(m_l,c)\}} \tag{A5}$$

Using the Envelope Theorem, $\frac{\partial}{\partial\sigma}U_f(m_u(\overline{\mu}(c,m_l,\sigma,P)),c)=d'(m_u(\overline{\mu}(c,m_l,\sigma,P))-\sigma)$ and $\frac{\partial}{\partial\sigma}U_f(m_l,c)=d'(m_l-\sigma)$. By construction, $m_l>m_u(\overline{\mu}(c,m_l,\sigma,P))$. Therefore, since $d''(.)>0$, we have $d'(m_l-\sigma)>d'(m_u(\overline{\mu}(c,m_l,\sigma,P))-\sigma)$. Therefore, $\frac{\partial}{\partial\sigma}\{U_f(m_u(\overline{\mu}(c,m_l,\sigma,P)),c)-U_f(m_l,c)\}<0$. And we have shown above that the denominator in Equation (A5) is negative. Therefore, $\frac{\partial\overline{\mu}}{\partial\sigma}<0$.□

Proof. (Proposition 3): Suppose that the customary marriage age in the initial equilibrium satisfies the condition $c>m_l-\overline{x}$. Therefore, it satisfies the first-order condition in the first line of (4). Differentiating throughout the equation with respect to P and applying the Leibniz Integral Rule, we obtain

$$\frac{\partial c}{\partial P}d''(c-\mu_c)=\lambda_f\frac{d\overline{\mu}}{dP}[d'(m(c)-c)\{1-m'(c)\}-d'(m_l-c)]$$
$$-\lambda_f\frac{\partial c}{\partial P}\int_{\overline{\mu}}^{\overline{m}}d''(m_l-c)dF(\mu)$$

Rearranging terms, we obtain

$$\frac{\partial c}{\partial P}=\frac{\lambda_f\frac{d\overline{\mu}}{dP}[d'(m(c)-c)\{1-m'(c)\}-d'(m_l-c)]}{d''(c-\mu_c)+\lambda_f\int_{\overline{\mu}}^{\overline{m}}d''(m_l-c)dF(\mu)} \tag{A6}$$

An increase in P lowers the marriage age preference, $\overline{\mu}$, at which a family is indifferent between following or breaking the law, i.e. $\frac{d\overline{\mu}}{dP}<0$. By Lemma 1, we have $0<1-m'(c)<1$. And, by construction, $m(c)-c<m_l-c\Rightarrow d'(m(c)-c)<d'(m_l-c)$. Therefore, the numerator in Equation (A6) is positive. Since $d''(.)>0$ by assumption, the denominator is also positive. Therefore, $\frac{\partial c}{\partial P}>0$. Thus, the custom moves in the direction of the law. On the other hand, if $c\leq m_l-\overline{x}$, then the initial equilibrium satisfies the first-order condition in the second line of Equation (4). Differentiating throughout the equation with respect to P and rearranging terms, we obtain

$$\frac{\partial c}{\partial P}=\frac{\lambda_f\frac{d\overline{\mu}}{dP}d'(m(c)-c)\{1-m'(c)\}}{d''(c-\mu_c)+\lambda_f\int_{\overline{\mu}}^{\overline{m}}d''(m_l-c)dF(\mu)}<0$$

Thus, the custom moves in the opposite direction to the law. □

Corollary. *(to Proposition 3): An exogenous increase in the expected marriage age preference of the reference group (σ) will move the customary marriage age in the direction of the law if, in the initial equilibrium, it is sufficiently close to the legal minimum age of marriage ($c>m_l-\overline{x}$); and in the opposite direction to the law otherwise ($c\leq m_l-\overline{x}$).*

Proof. We obtain the stated results by differentiating throughout (4) w.r.t. to σ and following the same steps as in the proof of Proposition 3. □

Proposition 4. *Suppose that the probability of legal punishment for underage marriage is equal to* $\pi_0 + \kappa(m - a_0)$ *where* $a_0 < m_l$ *is the current age and* $m \in [a_0, m_l)$. *An increase in* κ *lowers the threshold* $\overline{\mu}\left(c, m_l, \sigma, \pi, \kappa, \tilde{P}\right)$ *below which families practice underage marriage, and also lowers the age of marriage among families that practice underage marriage.*

Proof. We obtain the first part of the result following the same steps as in the proof of Lemma 2. The threshold marriage age preference satisfies the following equation:

$$U_f\left(m_u\left(\overline{\mu}\left(c, m_l, \sigma, \pi, \kappa, \tilde{P}\right)\right), c\right) - U_f(m_l, c) = 0 \tag{A7}$$

Differentiating throughout (A3) w.r.t. κ, we obtain

$$\begin{aligned} &\frac{\partial}{\partial \kappa} U_f\left(m_u\left(\overline{\mu}\left(c, m_l, \sigma, \pi, \kappa, \tilde{P}\right)\right), c\right) + \frac{\partial \overline{\mu}}{\partial \kappa} \frac{d}{d\mu}\left\{U_f(m_u(\mu), c) - U_f(m_l, c)\right\} = 0 \\ &\Rightarrow \frac{\partial \overline{\mu}}{\partial \kappa} = -\frac{\frac{\partial}{\partial \kappa} U_f\left(m_u\left(\overline{\mu}\left(c, m_l, \sigma, \pi, \kappa, \tilde{P}\right)\right), c\right)}{\frac{d}{d\mu}\left\{U_f(m_u(\mu), c) - U_f(m_l, c)\right\}} \end{aligned} \tag{A8}$$

Using the Envelope Theorem, $\frac{\partial}{\partial \kappa} U_f\left(m_u\left(\overline{\mu}\left(c, m_l, \sigma, \pi, \kappa, \tilde{P}\right)\right), c\right) = -(m_u - a_0)\tilde{P}$. And, we have shown above that the denominator in Equation (A4) is negative. Therefore, $\frac{\partial \overline{\mu}}{\partial \kappa} < 0$. For families that choose underage marriage, we have

$$m_u\left(c, \sigma, \pi, \kappa, \tilde{P}\right) = \arg \max_{m \in [a_0, \overline{m}]} -d(m - \mu_i) - \lambda_r d(m - \sigma) - \lambda_c d(m - c) - \{\pi_0 + \kappa(m - a_0)\}\tilde{P} \tag{A9}$$

The first-order condition to this optimization problem is given by:

$$d'(m_u - \mu_i) + \lambda_r d'(m_u - \sigma) + \lambda_c d'(m_u - c) = \kappa \tilde{P} \tag{A10}$$

An increase in κ increases the cost of delaying marriage on the right-hand side of Equation (A10) without affecting the left-hand side. Given that the maximand in the optimization problem in Equation (A9) is globally concave, it follows that an increase in κ lowers the optimal choice $m_u\left(c, \sigma, \pi, \kappa, \tilde{P}\right)$. □

APPENDIX B: ADDITIONAL TABLES

Table B1. Description of outcomes.

Variable	Description
Stated own preferences	
Appropriate marriage age	Appropriate age of marriage for girls/women reported by respondent
Marriage before 18 appropriate	= 1 if Approp. marriage age < 18, 0 otherwise
Beliefs regarding others' true preferences	
Village: appropriate marriage age	Respondent's belief about appropriate age of marriage for girls/women within his/her village
Village: people think worse marriage a/f 18	= 1 if respondent believes his/her village thinks worse of girls who marry above age 18, 0 otherwise
Beliefs regarding others' stated preferences	
Vignette A support	= 1 if respondent supports marriage postponement in Vignette A, 0 otherwise
Vign. A others support	= 1 if respondent believes other parents in village would support marriage postponement in Vignette A, 0 otherwise
Vign. A oth. approve support	= 1 if respondent approves decision to postpone marriage in Vignette A
Vignette B support	= 1 if respondent supports marriage postponement in Vignette B, 0 otherwise
Vign. B others support	= 1 if respondent believes other parents in village would support marriage postponement in Vignette B, 0 otherwise
Vign. B oth. approve support	= 1 if respondent approves decision to postpone marriage in Vignette B
Actual marriage outcomes	
Married	= 1 if adolescent is married at the time of the telephone interview
Accepted Offer	= 1 if an offer of marriage was accepted after the June 2018 survey
Any Marr. Steps	= 1 if any steps toward marriage of the adolescent were taken after the June 2018 survey
Miscellaneous outcomes	
Make a contribution	= 1 if respondent make positive contribution to charity, 0 otherwise
Contribution amount	Contribution amount in Bangladesh Taka

Note: This table provides a brief description of the main outcome variables.

Table B2. Baseline comparison: control vs T1 and T2, full sample.

	control (C)	treatment 1 (T1)	p-val of diff (C−T1)	treatment 2 (T2)	p-val of diff (C−T2)
Age	41.36	40.71	(0.36)	40.82	(0.46)
Male	0.29	0.28	(0.72)	0.26	(0.27)
Schooling	4.12	4.01	(0.68)	3.90	(0.36)
Married	0.90	0.91	(0.72)	0.91	(0.57)
Married before 18	0.52	0.48	(0.23)	0.52	(1.00)
Employed	0.35	0.35	(0.99)	0.34	(0.70)
2014 Norms Index 2	0.01	−0.01	(0.74)	0.01	(0.97)
Father Schooling	2.68	2.62	(0.80)	2.70	(0.94)
Mother Schooling	1.24	1.25	(0.95)	1.12	(0.39)
Mother works	0.09	0.07	(0.23)	0.07	(0.17)
Father low pay	0.23	0.24	(0.48)	0.23	(0.80)
Half Acre Land	0.46	0.49	(0.24)	0.46	(0.93)
Adol. girl 13–17	0.26	0.30	(0.30)	0.29	(0.52)
Knows min age	0.85	0.87	(0.21)	0.86	(0.67)
Knows punishment	0.79	0.81	(0.43)	0.79	(0.88)
Knows age exception	0.09	0.07	(0.23)	0.09	(0.63)
Learnt law after 2014	0.18	0.20	(0.43)	0.20	(0.25)
Knows CM court case	0.38	0.34	(0.17)	0.36	(0.48)
Observations	613	556	1169	588	1201

Note: This table presents average values of baseline characteristics by treatment status. The sample consists of all respondents (both primary and additional respondents) in the 2018 CiMLAS survey. Columns 3 and 5 display p-values from a t-test of the difference in means between the control group and either the Treatment 1 (T1) or Treatment 2 (T2) group. Source: 2018 CiMLAS.

Table B3. Baseline comparison: sample of respondents with unmarried girls (13–17) in extended family.

	Control (C)	Treatment 1 (T1)	p-val of diff (C−T1)	Treatment 2 (T2)	p-val of diff (C−T2)
Age	44.31	43.84	(0.62)	44.69	(0.70)
Schooling	4.13	3.61	(0.19)	3.26	(0.02)
Married	0.96	0.97	(0.73)	0.96	(0.98)
Married before 18	0.26	0.28	(0.74)	0.32	(0.18)
Employed	0.65	0.63	(0.60)	0.62	(0.38)
2014 Norms Index 2	0.00	−0.02	(0.76)	0.01	(0.94)
Father Schooling	2.31	2.75	(0.21)	2.57	(0.44)
Mother Schooling	1.14	1.31	(0.44)	0.88	(0.18)
Mother works	0.11	0.07	(0.18)	0.08	(0.29)
Father low pay	0.24	0.27	(0.41)	0.26	(0.49)
Half Acre Land	0.43	0.45	(0.65)	0.43	(0.92)
Adol. girl 13–17	1.19	1.19	(0.98)	1.26	(0.31)
Knows min age	0.88	0.91	(0.23)	0.87	(0.80)
Knows punishment	0.81	0.84	(0.34)	0.82	(0.75)
Knows age exception	0.09	0.08	(0.67)	0.11	(0.43)
Learnt law after 2014	0.21	0.19	(0.59)	0.21	(0.98)
Knows CM court case	0.41	0.38	(0.55)	0.38	(0.59)
Observations	262	246	508	239	501

Note: This table presents average values of baseline characteristics by treatment status for all respondents from families with unmarried girls aged 13–17 at the time of the survey. Columns 3 and 5 display p-values from a t-test of the difference in means between the control group and either the Treatment 1 (T1) or Treatment 2 (T2) group. Source: 2018 CiMLAS.

Comparison of mean outcomes

Table B4. Comparison of mean (short-term stated preference) outcomes by treatment status: control vs T1 and T2.

	Control (C)	Treatment 1 (T1)	P-val of diff (C−T1)	treatment 2 (T2)	p-val of diff (C−T2)
Approp marriage age	18.74	18.79	(0.61)	18.57	(0.06)
Approp. marry b/f 18	0.04	0.03	(0.42)	0.05	(0.32)
Vill. approp. marr. age	17.30	17.32	(0.85)	17.37	(0.51)
Vill marr a/f 18 bad	0.49	0.49	(0.77)	0.45	(0.12)
Vignette A support	0.91	0.92	(0.83)	0.91	(0.74)
Vignette A others support	0.68	0.68	(0.96)	0.68	(0.95)
VA oth. approve support	0.52	0.47	(0.07)	0.51	(0.77)
Vignette B support	0.62	0.58	(0.17)	0.54	(0.01)
Vignette B others support	0.31	0.31	(0.94)	0.31	(0.99)
VB oth. approve support	0.30	0.30	(1.00)	0.30	(0.98)
Make Contribution	0.39	0.43	(0.20)	0.40	(0.77)
Contribution Amount	24.19	30.07	(0.06)	22.14	(0.45)
Observations	613	556	1169	588	1201

Note: This table presents average values for short term outcome variables by treatment status. The sample consists of all respondents (both primary and additional respondents) in the 2018 CiMLAS survey. Columns 3 and 5 display p-values from a t-test of the difference in means between the control group and either the Treatment 1 (T1) or Treatment 2 (T2) group. Source: 2018 CiMLAS.

Table B5. Comparison of mean outcomes in phone survey (five months) by treatment status.

	Control (C)	Treatment 1 (T1)	p-val of diff (C−T1)	treatment 2 (T2)	p-val of diff (C−T2)
Married	0.01	0.08	(0.04)	0.05	(0.18)
Accepted Offer	0.03	0.21	(0.01)	0.16	(0.07)
Any Marr. Steps	0.06	0.13	(0.11)	0.12	(0.20)
Observations	83	92	175	86	169

Note: This table presents average values for long term outcome variables by treatment status. The data were collected in a phone survey conducted 5 months after the initial CiMLAS survey and information treatment. The sample consists of unmarried daughters of primary respondents aged 13 to 17 at the time of the 2018 CiMLAS survey. Columns 3 and 5 display p-values from a t-test of the difference in means between the control group and either the Treatment 1 (T1) or Treatment 2 (T2) group. Source: 2018 CiMLAS.

Table B6. Comparison of mean outcomes in phone survey (10 months) by treatment status.

	Control (C)	Treatment 1 (T1)	p-val of diff (C−T1)	Treatment 2 (T2)	p-val of diff (C−T2)
Married	0.05	0.10	(0.25)	0.11	(0.17)
Accepted Offer	0.10	0.24	(0.10)	0.18	(0.28)
Any Marr. Steps	0.09	0.18	(0.11)	0.16	(0.19)
Observations	76	78	154	80	156

Note: This table presents average values for long-term outcome variables by treatment status. The data were collected in a phone survey conducted 10 months after the initial CiMLAS survey and information treatment. The sample consists of unmarried daughters of primary respondents aged 13–17 at the time of the 2018 CiMLAS survey. Columns 3 and 5 display p-values from a t-test of the difference in means between the control group and either the Treatment 1 (T1) or Treatment 2 (T2) group. Source: 2018 CiMLAS.

Treatment heterogeneity: female vs male respondents (full sample)

Table B7. Treatment heterogeneity by gender: short-term outcomes for full sample (Part A).

	Attitudes toward early marriage		Would support daughters' decision		Financial contribution to NGO	
	appropriate marriage age	marriage before 18 appropriate	Vignette A	Vignette B	make a contribution	contribution amount
Treatment 1	0.161	−0.016	0.009	−0.037	0.005	4.146
	(0.107)	(0.013)	(0.021)	(0.034)	(0.032)	(3.582)
Treatment 2	−0.042	0.006	0.003	−0.045	−0.021	−2.501
	(0.110)	(0.015)	(0.021)	(0.034)	(0.031)	(3.051)
Male	0.527***	−0.015	0.058**	0.038	0.013	0.599
	(0.167)	(0.019)	(0.027)	(0.047)	(0.044)	(4.633)
Treatment 1 × Male	−0.447*	0.033	0.016	0.007	0.058	8.086
	(0.230)	(0.025)	(0.033)	(0.061)	(0.058)	(6.502)
Treatment 2 × Male	−0.552**	0.008	−0.016	−0.056	0.036	8.419
	(0.214)	(0.027)	(0.036)	(0.061)	(0.059)	(5.610)
Observations	1757	1757	1757	1757	1757	1757
dep var mean	18.741	0.039	0.912	0.620	0.395	24.189
dep var sd	1.643	0.194	0.284	0.486	0.489	49.400
T1 FWER p-val	0.438	0.480	0.663	0.496	0.885	0.376
T2 FWER p-val	0.967	0.967	0.967	0.554	0.613	0.613
T1 × Male FWER p-val	0.202	0.416	0.844	0.907	0.338	0.338
T2 × Male FWER p-val	0.052	0.895	0.895	0.751	0.555	0.221
$\beta1+\beta4=0$	0.180	0.438	0.347	0.553	0.201	0.032
$\beta2+\beta5=0$	0.002	0.533	0.656	0.053	0.759	0.222
$\beta1=\beta2$	0.066	0.081	0.767	0.825	0.421	0.050
$\beta1+\beta4=\beta2+\beta5$	0.134	0.910	0.190	0.200	0.374	0.254

Note: This table presents the results of OLS regressions of short-term outcome variables against treatment status indicators interacted with the gender of the respondent. The regressions also include village fixed effects and the same controls from Table 4. The sample consists of all respondents (both primary and additional respondents) in the 2018 CiMLAS survey. Standard errors are provided in parentheses, while Westfall–Young stepdown adjusted p-values to control the FWER are reported in the second half of the table. The last four rows report the p-values from a set of Wald tests (β_i corresponds to the coefficient of the term in the ith row of the table). Source: 2018 CiMLAS.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B8. Treatment heterogeneity by gender: short-term outcomes for full sample (Part B).

	Beliefs re. attitudes in community/village		Would support daughters' decision		Would others approve parents' decision	
	Appropriate marriage age	People think worse marriage a/f 18	Vignette A	Vignette B	Vignette A	Vignette B
Treatment 1	0.047	0.009	−0.040	−0.034	−0.064*	−0.024
	(0.137)	(0.033)	(0.033)	(0.031)	(0.035)	(0.032)
Treatment 2	0.138	−0.057*	−0.017	−0.017	−0.018	−0.005
	(0.123)	(0.032)	(0.032)	(0.031)	(0.034)	(0.031)
Male	0.084	−0.002	0.012	−0.040	0.001	−0.028
	(0.178)	(0.048)	(0.043)	(0.047)	(0.048)	(0.046)
Treatment 1 × male	−0.081	−0.065	0.051	0.076	0.011	0.038
	(0.239)	(0.061)	(0.056)	(0.060)	(0.062)	(0.059)
Treatment 2 × male	−0.035	−0.012	0.014	0.068	0.030	−0.010
	(0.231)	(0.062)	(0.057)	(0.060)	(0.062)	(0.058)
Observations	1757	1757	1757	1757	1757	1757
dep var mean	17.300	0.494	0.680	0.315	0.522	0.299
dep var sd	1.911	0.500	0.467	0.465	0.500	0.458
T1 FWER p-val	0.928	0.928	0.592	0.594	0.125	0.443
T2 FWER p-val	0.577	0.242	0.798	0.798	0.799	0.869
T1 × male FWER p-val	0.705	0.660	0.660	0.591	0.860	0.751
T2 × male FWER p-val	0.992	0.992	0.992	0.686	0.863	0.863
$\beta 1+\beta 4=0$	0.868	0.286	0.805	0.419	0.314	0.785
$\beta 2+\beta 5=0$	0.613	0.195	0.957	0.335	0.823	0.764
$\beta 1=\beta 2$	0.498	0.046	0.493	0.601	0.180	0.537
$\beta 1+\beta 4=\beta 2+\beta 5$	0.519	0.805	0.781	0.873	0.238	0.580

Note: This table presents the results of OLS regressions of short term outcome variables against treatment status indicators interacted with the gender of the respondent. The regressions also include village fixed effects and the same controls from Table 4. The sample consists of all respondents (both primary and additional respondents) in the 2018 CiMLAS survey. Standard errors are provided in parentheses, while Westfall–Young stepdown adjusted p-values to control the FWER are reported in the second half of the table. The last four rows report the p-values from a set of Wald tests (β_i corresponds to the coefficient of the term in the ith row of the table). Source: 2018 CiMLAS. Statistical significance is indicated as follows.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$..

Treatment heterogeneity: treatment effects on marriage outcomes by age of marriage of primary respondent

Table B9. Treatment heterogeneity by age of marriage of primary respondent: marriage outcomes.

	5 Months			10 Months		
	Married	**Accepted offer**	**Any marriage steps**	**Married**	**Accepted offer**	**Any marriage steps**
Treatment 1	0.108**	0.231**	0.140**	0.150**	0.226**	0.155**
	(0.046)	(0.092)	(0.060)	(0.060)	(0.095)	(0.070)
Treatment 2	0.047	0.159*	0.116**	0.101*	0.121	0.115*
	(0.036)	(0.090)	(0.055)	(0.052)	(0.077)	(0.061)
Resp. Late Marr.	0.000	0.006	0.066	0.082	0.118	0.080
	(0.023)	(0.057)	(0.055)	(0.056)	(0.095)	(0.072)
Treatment 1	−0.094*	−0.106	−0.142	−0.201**	−0.175	−0.119
× Resp. Late Marr.	(0.049)	(0.143)	(0.089)	(0.080)	(0.146)	(0.114)
Treatment 2	−0.061	−0.236*	−0.222***	−0.193**	−0.299**	−0.253***
× Resp. Late Marr.	(0.043)	(0.125)	(0.079)	(0.077)	(0.131)	(0.093)
Observations	261	112	261	234	139	234
$\beta1+\beta4=0$	0.510	0.259	0.979	0.338	0.650	0.682
$\beta2+\beta5=0$	0.543	0.326	0.063	0.108	0.098	0.050
$\beta1=\beta2$	0.226	0.512	0.701	0.474	0.312	0.605
$\beta1+\beta4=\beta2+\beta5$	0.370	0.121	0.075	0.338	0.027	0.021

Note: This table presents the results of OLS regressions of marriage-related outcomes against treatment status indicators interacted with a dummy variable indicating whether the primary respondent was at least 18 at time of marriage or not (in the base category, the primary respondent was younger than 18 when married). The sample, outcomes, controls, and general specification (apart from the inclusion of interactions) are the same as in Table 6. As in previous tables, standard errors are given in parentheses. The last four rows report the p-values from a number of Wald tests (β_i corresponds to the coefficient of the term in the ith row). Source: 2018 CiMLAS.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Perception of incidence of early marriage (with controls)

Table B10. Perception of incidence of early marriage.

	Early marriage common	**Early marriage not common**
Treatment 1	−0.000	0.011
	(0.028)	(0.027)
Treatment 2	−0.016	0.020
	(0.028)	(0.027)
Observations	1757	1757
p-value	0.572	0.745

Note: The sample consists of all respondents (both primary and additional respondents) in the 2018 CiMLAS survey. Specifications includes controls for respondent characteristics (not shown). Standard errors in parentheses. The last row reports the p-value from a Wald test for a difference in coefficients between T1 and T2.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Marriage-related outcomes in subsample where minimum legal age is known (with controls)

Table B11. Marriage outcomes from June 18 to November 18 for girls < 18.

	Married	Received offer	Accepted offer
treatment==1	0.070**	0.202***	0.083*
	(0.032)	(0.074)	(0.049)
treatment==2	0.015	0.114	0.040
	(0.026)	(0.076)	(0.045)
Observations	237	104	237
dep var mean	0.071	0.071	0.071
dep var sd	0.259	0.259	0.259

Note: The sample consists of unmarried daughters of primary respondents aged 13–17 at the time of the 2018 CiMLAS survey. Outcomes were collected from phone surveys conducted five months after the initial CiMLAS survey. The specifications include controls for child characteristics. Standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B12. Marriage outcomes from June 18 to May 19 for girls < 18.

	Married	Received offer	Accepted offer
Treatment==1	0.074	0.151*	0.122**
	(0.046)	(0.077)	(0.057)
Treatment==2	0.034	0.038	0.055
	(0.044)	(0.070)	(0.053)
Observations	210	128	210
dep var mean	0.125	0.125	0.125
dep var sd	0.332	0.332	0.332

Note: The sample consists of unmarried daughters of primary respondents aged 13–17 at the time of the 2018 CiMLAS survey. Outcomes were collected from phone surveys conducted 10 months after the initial CiMLAS survey. The specifications include controls for child characteristics. Standard errors in parentheses. Statistical significance is indicated as follows.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Short-term outcomes for the full sample (no controls)

Table B13. Short-term outcomes for full sample (Part A): no controls.

	Attitudes toward early marriage		Would others support daughters' decision		Financial contribution to NGO	
	Appropriate marriage age	Marriage before 18 appropriate	Vignette A	Vignette B	Make a contribution	Contribution amount
Treatment 1	0.012	−0.007	0.013	−0.038	0.017	5.851*
	(0.103)	(0.011)	(0.017)	(0.029)	(0.028)	(3.158)
Treatment 2	−0.226**	0.009	−0.005	−0.063**	−0.018	−1.191
	(0.100)	(0.012)	(0.017)	(0.029)	(0.027)	(2.666)
Observations	1757	1757	1757	1757	1757	1757
dep var mean	18.741	0.039	0.912	0.620	0.395	24.189
dep var sd	1.643	0.194	0.284	0.486	0.489	49.400
$\beta 1 = \beta 2$	0.021	0.152	0.320	0.400	0.212	0.018

Note: This table presents the results of OLS regressions of short term outcome variables against treatment status indicators. The sample consists of all respondents (both primary and additional respondents) in the 2018 CiMLAS survey. No controls are included except village fixed effects. Standard errors are given in parentheses. The last row reports the p-value from a Wald test for a difference in coefficients between T1 and T2 (β_i corresponds to the coefficient of the term in the ith row). Source: 2018 CiMLAS. Statistical significance is indicated as follows.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B14. Short-term outcomes for full sample (Part B): no controls.

	Beliefs re. attitudes in community/village		Would others support daughters' decision		Would others approve parents' decision	
	appropriate marriage age	people think worse marriage a/f 18	Vignette A	Vignette B	Vignette A	Vignette B
Treatment 1	0.039	−0.011	−0.022	−0.011	−0.056*	−0.008
	(0.117)	(0.029)	(0.028)	(0.027)	(0.030)	(0.028)
Treatment 2	0.133	−0.059**	−0.011	0.002	−0.007	−0.002
	(0.109)	(0.028)	(0.027)	(0.027)	(0.030)	(0.027)
Observations	1757	1757	1757	1757	1757	1757
dep var mean	17.300	0.494	0.680	0.315	0.522	0.299
dep var sd	1.911	0.500	0.467	0.465	0.500	0.458
$\beta 1 = \beta 2$	0.427	0.095	0.709	0.620	0.100	0.852

Note: This table presents the results of OLS regressions of short term outcome variables against treatment status indicators. The sample consists of all respondents (both primary and additional respondents) in the 2018 CiMLAS survey. No controls are included except village fixed effects. Standard errors are given in parentheses. The last row reports the p-value from a Wald test for a difference in coefficients between T1 and T2 (β_i corresponds to the coefficient of the term in the ith row). Source: 2018 CiMLAS.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Marriage-related outcomes (no controls)

Table B15. Marriage outcomes in phone survey (no controls).

	5 Months			10 Months		
	Married	Accepted offer	Any marriage steps	Married	Accepted offer	Any marriage steps
Treatment 1	0.064**	0.177**	0.070	0.050	0.138*	0.087
	(0.030)	(0.071)	(0.044)	(0.043)	(0.082)	(0.055)
Treatment 2	0.034	0.126*	0.056	0.060	0.075	0.070
	(0.026)	(0.068)	(0.044)	(0.044)	(0.070)	(0.053)
Observations	261	112	261	234	139	234
dep var mean	0.012	0.032	0.060	0.036	0.077	0.072
dep var sd	0.110	0.180	0.239	0.188	0.270	0.261
T1 FWER p-val	0.063	0.038	0.120	0.261	0.169	0.171
T2 FWER p-val	0.312	0.156	0.312	0.304	0.304	0.304
$\beta 1 = \beta 2$	0.412	0.555	0.775	0.841	0.455	0.778

Note: This table presents the results of OLS regressions of marriage-related outcomes against treatment status indicators for a sample of female children aged 13–17 at the time of the initial CiMLAS survey. These outcomes were collected from phone surveys conducted 5 and 10 months after the initial CiMLAS survey. No controls were included in this specification. Standard errors are given in parentheses. The penultimate rows report Westfall–Young stepdown adjusted p-values which control the FWER for each tested hypothesis in the table. The last row reports the p-value from a Wald test for a difference in coefficients between T1 and T2 (β_i corresponds to the coefficient of the term in the ith row). Source: 2018 CiMLAS. Statistical significance is indicated as follows.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

APPENDIX C: VIGNETTES

Introduction to vignettes

Next I will tell you several stories about people living in villages similar to this one. I would like you to listen to the stories carefully and answer the questions that follow each one. Some of the questions will ask you to agree or disagree with a statement.

Vignette A

Jesmin is a 14-year-old girl attending grade 9 in secondary school. She lives with her mother, father, and two older brothers. Two months ago, her parents received a marriage proposal for Jesmin. The groom is a 32-year-old man from a neighboring village. Jesmin told her parents that she would like to finish her schooling before getting married, but her uncles are pressuring her to accept the marriage offer immediately.

SL	Questions	Answer	Answer code
VA_01	What would you do if you were Jesmin's parent?		support your daughter's decision to delay marriage ..... 1 seek more information about the groom,with the goal of arranging the marriage 2
VA_02	What do you think most other parents in this village would do if they were in this situation?		support their daughter's decision to delay marriage ..... 1 seek more information about the groom,with the goal of arranging the marriage 2
VA_03	What do you think most other parents would advise Jesmin's parents to do regarding their daughter's request to postpone marriage?		support their daughter's request ..... 1 seek more information about the groom,with the goal of arranging the marriage 2

Let's return to the story. Imagine that Jesmin's parents listen to her and refuse the marriage proposal so that Jesmin can finish school before marrying.

SL	Questions	strongly approve	somewhat approve	somewhat disapprove	strongly disapprove	Response Code
VB_04	To what extent would you approve or disapprove of Rokeya's mother's decision?	1	2	3	4	
VB_05	To what extent do you think the neighbours and extended family would approve or disapprove of Rokeya's mother's decision?	1	2	3	4	

Vignette B

Rokeya, aged 15, is the eldest of three sisters. She is enrolled in class 10 in secondary school and lives in a village like this with her mother. Her father passed away a year ago. One day her paternal uncles speak to her mother about an offer of marriage from a young BCS officer. Rokeya firmly announces that she is not interested in marrying any time soon.

SL	Questions	Answer	Answer code
VB_01	What would you do if you were Rokeya's mother?		Support your daughter's desire to delay marriage ..... 1 Request Rokeya's uncles to seek more information about the prospective groom, with the goal of arranging the marriage 2
VB_02	What do you think most other mothers in this village would do in this situation?		Support their daughter's desire to delay marriage ..... 1 Request Rokeya's uncles to seek more information about the prospective groom, with the goal of arranging the marriage 2
VB_03	What do you think most other parents in this village would advise Rokeya's mother to do regarding her daughter's refusal to the proposal?		Support her daughter's desire to delay marriage ..... 1 Request Rokeya's uncles to seek more information about the prospective groom, with the goal of arranging the marriage 2

Let's return to the story. Imagine that Rokeya's mother listens to her daughter and supports her desire to delay the marriage.

SL	Questions	strongly approve	somewhat approve	somewhat disapprove	strongly disapprove	Response Code
VB_04	To what extent would you approve or disapprove of Rokeya's mother's decision?	1	2	3	4	
VB_05	To what extent do you think the neighbours and extended family would approve or disapprove of Rokeya's mother's decision?	1	2	3	4	

DATA AVAILABILITY

The data underlying this article are available in the online supplementary material.

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