A Signal to End Child Marriage: Theory and Experimental Evidence from Bangladesh

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Abstract

Child marriage remains common even where female schooling and employment opportunities have grown. We experimentally evaluate a financial incentive to delay marriage alongside a girls' empowerment program in Bangladesh. While girls eligible for two years of incentive are 19% less likely to marry underage, the empowerment program failed to decrease adolescent marriage. We show that these results are consistent with a signaling model in which bride type is imperfectly observed but preferred bride types (socially conservative girls) have lower returns to delaying marriage. Consistent with our theoretical prediction, we observe substantial spillovers of the incentive on untreated non-preferred types.

KEYWORDS: Child marriage, signaling, randomized control trial, financial incentive.

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1 Introduction

Adolescent marriage remains the norm in many parts of the world, with 120 million girls under 18 projected to become brides over the next decade (Unicef, 2017). There is growing evidence that underage marriage is bad for women and their children. Women who marry as adolescents attain less schooling and give birth at a younger age, both of which result in worse outcomes for their children.[1] These welfare concerns have prompted a global campaign to end adolescent and child marriage, and most countries in the world have enacted age of consent legislation banning the practice. Even though marriage is one of the largest financial transactions a household will undertake, there is surprisingly little empirical analysis of the motivations and potential conflicts of interest leading families to make this choice.[2] Understanding why this practice persists is necessary in order to design effective policy approaches to discourage it.

This paper attempts to shed light on this question by investigating marriage behavior in rural Bangladesh, a setting in which female marriage before or shortly after 18 is the norm. Bangladesh has the second-highest adolescent marriage rate in the world: 74% of women aged 26-55 were married before age 18 (Unicef, 2014). More surprisingly, as shown in figure 1, the adolescent marriage rate has changed little in recent years despite large gains in female education and employment, and dramatic reductions in fertility and child mortality. According to the most recent Bangladesh Demographic and Health Survey (BDHS), while the median years of female schooling rose from zero to nearly seven in only two decades, the median age of marriage increased by only one year (Mitra et al., 2016). Not only has Bangladesh achieved gender parity in primary and secondary school enrollment, but poverty fell from 57% in 1991/92 to 32% in 2010, and maternal mortality declined by 40% between 2001 and 2010. Wage opportunities for women, reproductive health, and child survival also increased steadily over this period (Mitra et al., 2016).

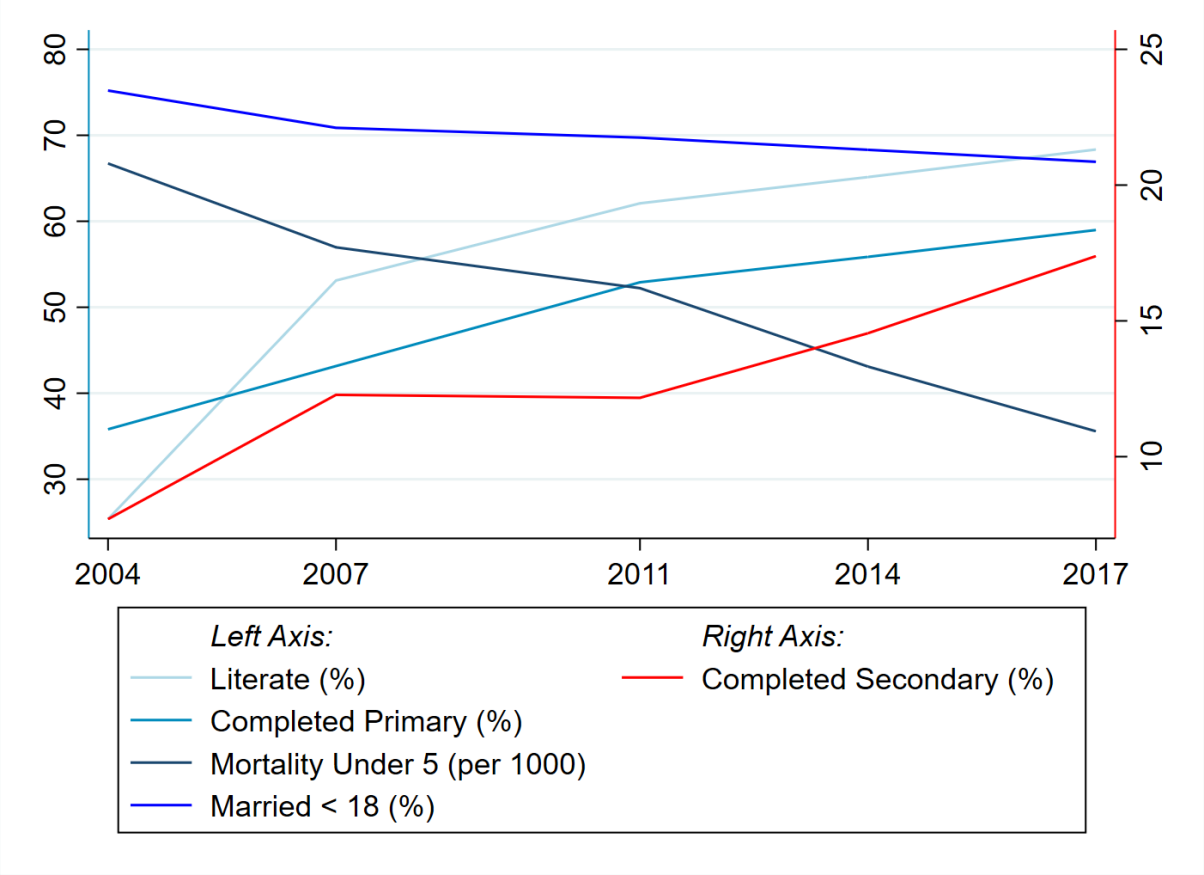
Bangladesh's recent success in achieving many gender development goals, alongside widespread public health campaigns to increase awareness about the negative consequences of underage marriage, raises the question of why it remains so pervasive.As all of the above trends indicate increasing returns to female education, they should exert positive pressure on female marriage age (Becker and Lewis, 1973). Moreover, data from numerous sources, including our own, indicate that the vast majority of female adolescents and their parents no longer favor underage marriage, which makes the stagnation particularly puzzling (see, for example, Loaiza Sr and Wong (2012)).[3]

[1] Much of the literature linking child marriage to poor outcomes is correlational, but there is also evidence that early marriage is *causally* linked to lower schooling attainment and worse outcomes in marriage, including health-seeking behavior (Field and Ambrus, 2008), and worse outcomes for offspring (Chari et al., 2017).

[2] See online appendix OA.1 for a comprehensive literature review of academic studies on child marriage.

[3] In our survey data, 95% of girls aged 15-17 reported a minimum acceptable marriage age at or above 18, and when asked why a girl should not marry younger, 74% said because she would not be physically ready for childbearing.

Figure 1: Education, child mortality, and marriage trends in Bangladesh



Notes: Data from the 2004, 2007, 2011, 2014, and 2017 Bangladesh DHS. The samples include all women aged 18-49. All blue lines follow the left y-a xis scale and the red line follows the right y-axis scale. "Under-5 mortality" includes all deaths of children born in the five years preceding the survey. "Married<18" is the percentage of women who report a marriage age under 18. Because the DHS data only include ever-married women, child marriage rates were calculated by multiplying the share of ever-married women that married under 18 by the share of married women age 18-49 in the household roster.

To provide empirical evidence on the possible drivers of child marriage, we conduct a field experiment in collaboration with Save the Children (USA) that tests the effectiveness of alternative approaches to reducing child marriage in rural Bangladesh. In particular, we introduced a conditional incentive program that offered regular transfers to families of adolescent girls between the ages of 15 and 17 as long as they remained unmarried, for up to two years or until they reached the age of consent (18).[4] The program was implemented as a clustered randomized trial, allowing us to assess the causal impact on marriage age of a small increase in the value of delaying marriage on girls living in communities randomized to receive the program. In parallel, we evaluate the impact of a prototypical adolescent empowerment program, a more

[4]Most conditional cash and non-cash incentives are conditional on girls staying in school and/or maintaining good grades. While they appear to be effective in encouraging girls to stay in school, there is mixed high-quality evidence on whether this leads to reductions in early marriage, teen pregnancy, or cohabitation during teenage years (Angrist et al., 2006; Baird et al., 2011; Duflo et al., 2015). None of these studies are in South Asia or even in countries where parents are the main deciders of marriage age.

standard policy instrument designed to discourage child marriage by promoting more progressive gender norms among adolescent girls.[5] This is the first rigorous evaluation of a financial incentive to delay marriage that is conditional only on marriage age.[6] It is also the first to directly compare the impact on marriage of a financial incentive and a standard empowerment program in the same context.

We find that the conditional incentive program was highly effective in increasing age at marriage and schooling attainment. Four and a half years after program completion, women in treatment communities who were eligible for the incentive for two years are 19% less likely to have married under age 18 and 18% less likely to have married below 16. Back-of-the-envelope calculations suggest extremely high response to the incentive among those at risk of marrying young. Importantly, the delay in marriage resulting from the incentive did not lead to an increase in dowry or a decrease in spouse quality, indicating no marriage market penalty for delaying. Meanwhile, although the empowerment program succeeded in promoting more progressive gender norms, it distinctly failed to encourage later marriage, and there is some evidence that underage marriage actually rose and average dowry payments to grooms increased in this arm, indicative of a marriage market penalty to participation in the program.

These results shed light on the motivations for early marriage. They are inconsistent with two of the main categories of explanations for widespread and persistent rates of early marriage: a culturally entrenched preference for young brides, and an "unraveling" story in which all brides marry early for fear of missing out on the highest quality husbands.[7] First, if early marriage were the result of a marriage market youth premium rooted in beliefs about the benefits of girls marrying young - for instance, physical attractiveness or increased fertility (Goody, 1990)[8] - then delaying marriage in response to a financial incentive would be associated with a dowry penalty or decline in groom quality on the market, which we do not find. In other words, our results indicate that affordability is not the constraint on delaying a girl's marriage age.

[5] Bandiera et al. (2020), Buehren et al. (2015), Edmonds et al. (2019) and Ashraf et al. (2020) evaluate the effect of empowerment or livelihood programs on age of marriage, cohabitation or teenage pregnancy. Only one (Bandiera et al., 2020) found that the program reduced the likelihood of pregnancy but in a setting (Uganda) where marriage and sexual onset are less determined by parents and are more in the control of girls than is the case in Bangladesh.

[6] Krishnan et al. (2014), Nanda et al. (2014) and Sinha and Yoong (2009) conduct non-experimental evaluations of a state-wide program in Haryana that provided a financial incentive to girls at birth who were still unmarried on their 18th birthday. However, these evaluations are subject to potential selection bias given the eligibility criteria for the program (see online appendix OA.1). Erulkar and Muthengi (2009) evaluate Berhane Hewane, a program that provided girls with a combination of group formation and study support and offered parents a financial incentive conditional on the girl still being unmarried at the end of the intervention. The authors find a significant effect on marriage; however, with only one control and one treatment community, it is hard to interpret the results.

[7] Note that even if the factors motivating an original preference for youth have become obsolete, child marriage might persist if norms are sticky due to perceived social sanctions to deviating from norms or incomplete information on higher-order beliefs.

[8] In fact, fertility should be lower if childbearing starts 2-3 years under age 18 relative to 2-3 years above 18 given the health risks of very early childbearing, and survey data indicate that individuals are reasonably aware of the health risks associated with early childbearing.

Nor do we see a differential impact of the incentive by family wealth as this hypothesis would predict since the value of the incentive should be higher for lower-SES households. More generally, the fact that roughly half of marriages take place above age 18 is inconsistent with a very strong cultural pressure for underage marriage , especially given that many households choose early marriage for only a fraction of their daughters (44% of households with more than one daughter chose underage marriage for only some of their daughters in control communities).

Second, the unraveling story is inconsistent with our finding that the impact of the incentive is geographically concentrated within treatment villages, even though the marriage market operates at a much larger level of ten or more villages, encompassing both treatment and control communities. If early marriage were due to unraveling, then delay by some women in incentive villages should spill over onto all treatment and control women within the broader marriage market, which we do not observe.[9] Instead, we observe sharp differences in marriage patterns at the boundary of treatment and control villages.

To explain our results, we propose a new explanation for child marriage that is consistent with our findings: delaying marriage is a negative signal of an unobserved bride type. Specifically, if brides with the lowest economic returns to delaying marriage also differ on an unobservable dimension that grooms desire, these brides might choose to enter the market early to signal they are the preferred type. This is particularly likely to be the case in Bangladesh, and throughout South Asia, where anthropological research has documented high marriage market returns to women's adherence to traditional gender norms of behavior such as docility and obedience to husbands and in-laws. An individual girl's adherence to norms is difficult to observe prior to marriage, especially given that our data suggest that parents' norms (which can be more easily observed) are not a strong predictor of their daughters' beliefs. Conservative gender norms are also very likely to be correlated with the private costs of human capital attainment (e.g., conservative women face greater barriers to attending school, engaging with learning, or behaving competitively in academic settings) and returns to education (e.g., conservative women are also less likely to be employed or participate actively in household decision-making as adults).

As a result, brides who privately know they are conservative ("preferred" types) have an incentive to enter the marriage market earlier than they would in a full-information environment in an attempt to signal their type. This leads less conservative women ("non-preferred" types) who would benefit the most from delaying marriage to pool with preferred types and marry early as well, or face worse marriage prospects as an obvious non-preferred type. In this manner, signaling can lead to an equilibrium in which everyone marries earlier than is optimal, even though everyone (including men) would be weakly better off if all women were required to delay.

[9]Unraveling also requires there to be a fixed supply of grooms on the market, which, as detailed in Anderson (2007b),is unlikely to occur in settings such as ours in which brides can match with grooms over both a large geographic area and large age range.

Moreover, a small incentive that pushes brides to postpone marriage for reasons that are orthogonal to type has the potential to shift the marriage market equilibrium from one in which all brides pool on early marriage to one in which many brides delay without a marriage market penalty. By contrast, policies that attempt to reduce the incidence of child marriage by reducing girls' adherence to traditional gender norms – e.g., traditional empowerment programs – can actually have a perverse effect on marriage age as increasing the perceived fraction of nonpreferred types makes it even more important to signal being the preferred type by entering early.[10]

To more firmly establish that signaling incentives are at play in this setting, the second part of the paper formalizes a theoretical model that generates predictions that are unique to the signaling story, and then tests these predictions using data from our field experiment. Specifically, the signaling model predicts that all treated brides as well as untreated non-preferred types who can plausibly claim to be eligible for treatment should delay marriage. Consistent with our theory, we find that untreated women who live very close to incentive communities and have relatively progressive gender norms are 34% less likely to have married under age 18, despite having received no incentive to do so. This is not true of their socially conservative peers. If "strong preferences" explained child marriage, there should be no immediate effect of the program on anyone who is not eligible for the incentive. In contrast, if child marriage were due to unraveling, then the incentive should lead to spillovers on *all* women.

These results provide novel evidence that non-taste-based factors such as signaling play a substantive role in sustaining the institution of child marriage in Bangladesh. It is important to distinguish between a model in which child marriage exists because youth is valued in the market and a model in which child marriage is influenced by signaling motivations because these models have very distinct policy implications. In particular, a financial incentive has far more potential to be both welfare-improving and cost-effective relative to a similar policy offered in a non-signaling world. This is because, in the signaling model, child marriage is unambiguously inefficient since delaying marriage for all women would be a weak Pareto improvement for society: women increase their education if they enter the marriage market later, and men and children are better off with an educated wife and mother.[11] In contrast, if society has a preference for child marriage, delaying marriage age with a financial incentive does not necessarily make everyone better off. Hence, evidence of signaling provides justification for intervening to

[10] Wahhaj (2018) studies a signaling story where daughters marry early for fear of being suspected to be of bad character, but the model does not consider the relationship between this unobserved type and other attributes such as education, and yields fundamentally different predictions. He shows theoretically that policies that increase women's bargaining power may increase average age of marriage. In our model, adolescent girls have no bargaining power in marriage, which we believe is more realistic in this setting of arranged marriage. Moreover, our signaling model predicts that "empowering" adolescent girls could actually lower their marriage market value and decrease marriage age in the absence of other policy interventions, as docility is the preferred bride type in this environment.

[11] Consistently, 31% of men in our sample reported education as one of the two most desirable characteristics in a bride).

change a practice that may otherwise be seen as reflecting a strong cultural preference.

Likewise, the cost-efficacy of the policy approach depends critically on the underlying model. First, in the signaling environment, a financial incentive has the unique potential to immediately reduce child marriage among those who do not receive the incentive but who face lower stigma from delaying (as long as there is some ambiguity as to who is eligible). Second, the human capital benefits of delaying imply that a lower incentive value is necessary to induce delay among recipients relative to a world in which the only value to postponing marriage is the cash transfer received. That is, because everyone is made better off by postponing marriage in a signaling world, a financial incentive to delay marriage does not need not be large or even universally offered to have an impact on marriage age, only believed to be orthogonal to the preferred type.[12]

Finally, our results also offer an explanation for why existing programs that promote girls' education or attempt to change girls' gender norms directly have not reduced child marriage. Our theory shows that, while these approaches may work if preferences are the sole determinant of marriage age, if signaling concerns drive child marriage, they would have no impact or even increase underage marriage. This is because, in a signaling environment, either policy would exacerbate the adverse selection problem by strengthening grooms' beliefs that a late entrant is the non-preferred type, making it even costlier for women not to pool on early marriage. For the same reason, it is important that incentives are conditioned directly on marriage age rather than indirectly on education.

The rest of the paper proceeds as follows. Section 2 describes the marriage practices in our setting. Section 3 describes the experiment and section 4 presents the results. Section 5 introduces a signaling model of marriage timing to explain the observed patterns. Section 6 presents additional empirical tests generated by the model. Finally, section 7 discusses the cost-effectiveness of the conditional incentive program and section 8 concludes.

2 Setting

Our study takes place in rural Bangladesh in communities that are overwhelmingly Muslim. As a result, most marriages are governed by religious law and follow a standard set of practices. Almost all marriages are arranged by parents, with brides having relatively little control over groom choice or marriage timing. Less than 1% of women in our control group said they could discuss groom choice or timing with their father and only 10% with their mother. Marriages are contracts between families and most (89%) are arranged by third party matchmakers. Moreover, the matchmaking industry is relatively competitive: in our study sample, the vast majority of rural communities (95%) have a professional matchmaker that lives in the community, and

[12]This is easy to accomplish in practice. For example, we achieve orthogonality in our field experiment by randomizing the incentive across locations.

70% have three or more.[13]

While dowry was outlawed in Bangladesh in 1980, most marriages (85% in our sample) continue to involve dowry in the form of a pre-negotiated transfers from the bride's family to the groom's, and the amounts are large: among those who pay it, average dowry in our sample is $786. Dowry serves as the price that equilibrates supply and demand for grooms in a setting in which it is relatively more unattractive for women to stay unmarried than for men, for instance because male individual earning capacity exceeds that of females (Rao, 1993). Data from matchmakers in our study area confirm that the family of a bride with attractive characteristics such as more years of schooling can pay lower dowry (Buchmann et al., 2021).

Every Muslim marriage contract also specifies a *denmeher*, which in Bangladesh is an amount of money to be transferred to the wife in the event of divorce, much in the style of a Western prenuptial agreement (Ambrus et al., 2010). As such, *denmeher* acts to reduce the risk of divorce for the bride, and richer families will pay higher dowries in order to purchase higher *denmeher* for their daughters (Buchmann et al., 2021). Of those marriages in our sample reporting *denmeher* (over 99%), the mean amount is $2,208 with a standard deviation of $1,844.[14]

The average age of marriage for women in our study setting is 18.4, and 41% of women marry under age 18 and 20% under age 16.[15] Husbands are on average 6 years older than their wives, and 90% of married women were married to a husband outside their community and 21% outside the sub-district. In terms of preferences over marriage timing, stated preferences on ideal marriage age reported by parents (20) and women (21) are significantly higher on average than actual marriage age of the same girls, and survey data indicate that both parents and daughters understand that early marriage has health costs.[16] Parents report that both financial (78%) and social (21%) pressure influenced the timing of daughters' marriage, which might explain the discrepancy. 43% of parents reported that a girl should not marry late because she risks getting a bad reputation and 31% because she would not find a good groom, both of which are consistent with (though not unique to) late marriage sending a negative signal of bride quality.

In terms of desired spousal traits, a large body of anthropological work suggests that adherence to traditional norms of behavior is one of the most sought after characteristics of brides throughout South Asia. In multiple qualitative studies, brides in South Asia are said to be valued

[13] We present community summary statistics in online appendix OA.4.1.

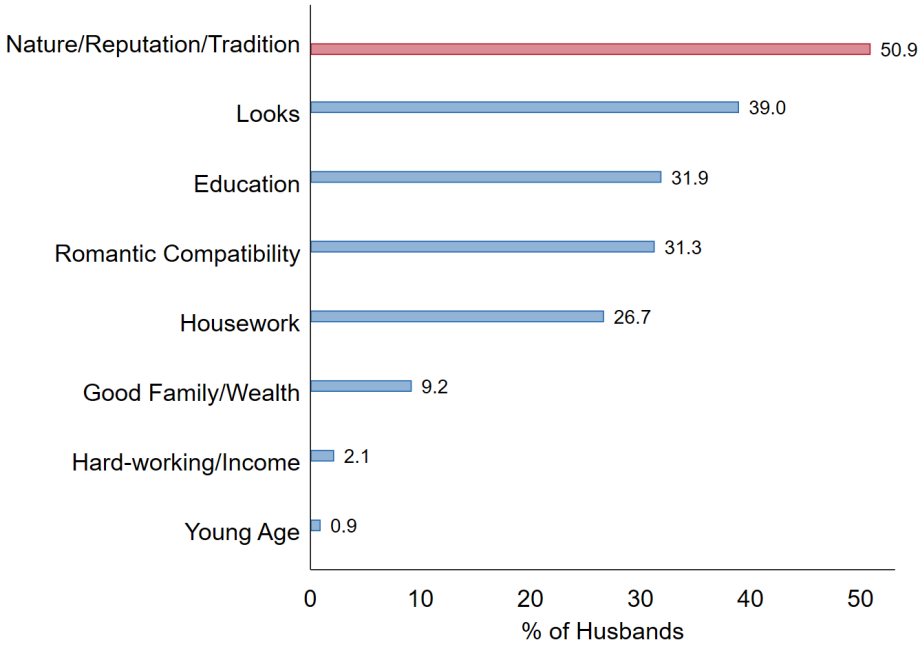
[14] In other Muslim countries, *denmeher* is given to the wife at the time of marriage, or split between an amount given at marriage (prompt dower) and an amount provided upon divorce (deferred dower). See Anderson (2007a) and Ambrus et al. (2010) for more details on Bangladeshi marriage contracts.

[15] Since women in our analysis sample were 15-17 and unmarried at program start, these figures are calculated among older siblings age 18 at program start. Nonetheless, as we oversampled households with unmarried women at baseline, these numbers likely underestimate the true rate of underage marriage in our study area.

[16] Only 5% of women in our sample gave as a minimum age of marriage an age under 18. When asked why girls should not marry below this age, 72% said because a girl would not be physically ready for marriage. In the control group at endline, 90% of women correctly identified at least one health risk associated with early pregnancies.

for being submissive and obedient (Hamid et al., 2010), docile (Goody, 1990), and protecting the family's reputation above all else (Ortner, 1978). Correspondingly, in a subsample survey of 579 husbands of the women in our sample, 51% reported either "Nature", "Character", "Reputation", or "Religion and Tradition" as one of the two most desirable characteristics in a bride (figure 2). Only four husbands reported age as one of the two most important characteristics in a bride, further suggesting that entrenched preferences for young brides are unlikely to be the main driver of underage marriage in our setting. For women, a groom's earnings capacity was the dominant trait: 60% of women reported income as the most desirable characteristic in a husband.

Figure 2: Characteristics desired in a bride (%)



Notes: The figure shows the share of husbands of women in our sample who reported different characteristics as one out of the two most desired characteristics in a bride. "Nature/Reputation/Tradition" is 1 if the husband chose either "Nature", "Character", "Reputation", or "Tradition and Religion". "Good Family/Wealth" is 1 if the husband chose "Good Family" or "Wealth", and "Hard-working/Income" is 1 if the husband chose "Hard-working nature" or "Income potential".

3 A Child Marriage Policy Experiment

In this section, we describe the field experiment designed to test the impact on child marriage of a financial incentive to delay marriage conditional on marriage age as well as the effect of an adolescent girl's empowerment program.

3.1 Experimental Design

Between January 2007 and September 2017, we ran a clustered randomized trial in collaboration with Save the Children (USA) to test whether a conditional incentive or an adolescent

empowerment program would reduce child marriage and increase girls enrollment in school in a cross randomized design.[17] The study was carried out in 460 rural communities within six sub-districts (Daulatkhan, Babuganj, Muladi, Patuakhali Sadar, Bauphal and Bhola Sadar) in south central Bangladesh, where Save the Children was managing a food security program that provided transfers to pregnant and lactating mothers (a map of the study region can be found in online appendix OA.4.2).[18] Out of 610 rural communities in these subdistricts, 460 met eligibility criteria based on size and accessibility.[19]

Using a stratified randomized design in the ratio 1:2:1:2, our sample communities were randomized to receive either i) the conditional incentive to delay marriage, ii) the basic empowerment program, iii) empowerment plus conditional incentive, or iv) the status quo. We stratified by union, an administrative grouping of roughly ten communities, and within union by community size (the randomization procedure is described in detail in online appendix OA.4.3). Sample communities were semi-rural to rural, with an average of 351 households per community, and 16% were more than one hour away from the closest motorable road. About half had a primary school in 2007 while 25% had a secondary school (see online appendix OA.4.1 for full community summary statistics).

In 2007, we conducted a census of all households with adolescent girls in our sample communities that collected data on the marital satus, age, and education of all household members. Our analysis sample includes all unmarried girls in study communities who were age 15-17 at program start, the age range eligible for the incentive treatment. Baseline characteristics were balanced across treatment arms in this sample (appendix A.1.2.1).

3.2 Conditional Incentive and Empowerment Programs

All girls in conditional incentive communities aged 15-17 and unmarried at program start were issued ration cards indicating their eligibility to receive cooking oil every four months until they married or turned 18. Eligibility was based on age as reported in the baseline survey which was collected prior to any announcement of the program, thus minimizing the incentive for misreporting. Every four months, from April 2008 to August 2010, marital status was verified by CHVs and/or independent monitors who conducted unannounced household visits

[17]The trial was registered at the AEA Registry prior to endline data collection, #204 (https://www.socialscienceregistry.org/trials/204). In registering our trial we also included a preanalysis plan for the analysis of detailed data collected on a subsample of young women, which includes variables on health, employment, and empowerment. This paper only uses data from the much shorter survey conducted for every woman in the sample communities (referred to in the PAP as the census). Our PAP states "As the census survey has very few outcomes and is thus less susceptible to data mining, we did not prepare pre-analysis plans for either wave 2 or wave 3 census surveys."

[18]The conditional incentive program that we evaluate used the distribution infrastructure of this existing program, which operated in all treatment and control communities in our study.

[19]In particular, to determine which communities were included in the study, we surveyed households in all 610 communities in the six sub-districts between January and February 2007. Communities were excluded if they were too remote for distribution or had less than 40 or more than 490 adolescent girls, leaving 460 eligible communities.

to verify that the girl was still residing in her parents' home, and interviewed family members, neighbors, and community leaders about her marital status.[20] Those found to be married or who had reached 18 had their names removed from the eligibility list and their cards taken away. Girls themselves collected the oil by presenting their ration card, which was checked against a separate beneficiary list at oil distribution points. The value of the incentive was approximately $16 per year. Cooking oil has to be purchased regularly by every family in Bangladesh and thus is a close substitute to cash (but less susceptible to theft). It also has a high value to volume ratio, which minimized transport costs. A total of 5,734 unmarried adolescent girls received the conditional incentive at least once, or 73% of the girls eligible at baseline and 92% of girls who received an incentive card (online appendix OA.4.4).

In communities randomized to receive the empowerment program, all girls aged 10-19 were invited to participate in the Kishoree Kontha (KK), or "Adolescent Girl's Voice" program for one six-month cycle. To accommodate all eligible girls, communities received up to four cycles of the program between December 2007 and August 2010. Girls met 5 to 6 days per week for 1-3 hours per day in "Safe Spaces", identified at the start of the program as centralized locations where up to 20 girls could meet, socialize, and receive training.[21] Monitoring data show Safe Spaces averaged six meetings, or 7.8 hours, per week, and 41,347 girls, or 93% of girls in target communities, were reached (online appendix OA.4.4).

Two to four girls per safe space were trained to deliver an empowerment curriculum including education support and social competency training. These peer trainers were provided with a structured curriculum containing activities to be completed during each session and visited every few weeks by a KK staff member. The empowerment curriculum was similar in content to many empowerment programs being implemented worldwide, including those designed by BRAC and UNICEF. The social competency component trained girls in life skills, negotiation, legal rights of women, and nutritional and reproductive health knowledge via a curriculum designed by Save the Children USA. The education component aimed to enhance the basic literacy, numeracy, and oral communication of both school-attending and illiterate girls. In randomly selected communities (50%), financial literacy and encouragement to generate own income was added to the curricula.

3.3 Survey Data

To collect data on marriage, schooling and childbearing outcomes, we attempted to resurvey parents in all 22,667 households in two separate rounds of data collection conducted 1 and 4.5 years after program completion (see online appendix OA.4.5 for a timeline of all surveys). Parents were asked both current marital status and age of marriage if their daughter was reported to

[20] Because girls move in with husbands' families upon marriage, they generally relocate outside the community upon marriage; hence, finding them at their parents' home is a reasonable proxy for marital status in this setting.

[21] Safe Space committees with adult community members were organized to support the safe spaces.

have ever married. While parents had no obvious incentive to misreport their daughters' marriage timing given that the program had finished 4.5 years before endline surveying and women were far too old to qualify, we nonetheless carefully assess the quality of the marriage age data collected in light of potential reporting bias. In particular, we assess whether parents appear to *overreport* marriage age of girls using three methods (online appendix OA.5): First, for a subsample of 1,160 young women, we interviewed young women themselves and compared the parents' report to the young woman's report.[22] Second, for a subsample of approximately 100 young women, we collected marriage certificates and compared both the parents' report and the woman's report with the date on the marriage certificate.[23]

Overall, marriage age reported by parents was largely consistent with that reported by women and had no significant bias. On average, parents' reports were 1.8 months higher than young women's reports and 0.3 months higher than the marriage age calculated from certificates. Young women's reports were on average 5.5 months lower than the marriage age calculated from certificates. The differences were balanced across treatment arms for all three comparisons.

There are three sources of missing data from the 2017 endline survey (see consort diagram in online appendix OA.6): 1,340 observations could not be linked across survey waves because of errors by the data entry firm, which lost hard-copy data from 598 individuals and incorrectly entered IDs from 742 individuals; 1,006 young women were living in communities (N=14) that were entirely displaced by cyclone damage;[24] and 3,054 of the 21,749 women we attempted to follow-up could not be tracked either because parents could not be reached or refused to be surveyed (14% attrition, balanced across treatment arms, see table A.1.2.2). Of those with endline data, a further 3,119 are excluded due to incomplete outcome data or marriage before the program start in January 2008.[25] Our final analysis sample thus contains 15,576 women.[26]

Table 1 presents summary statistics on women in our analysis sample from the 2017 endline survey. By age 22-25, 84% are married, including 28% married before the legal age of 18 and 4% before age 16. Moreover, by endline 63% of women in our sample have started childbearing

[22] We are primarily interested in overreporting of marriage age because it could bias our estimates of the key outcomes, whether a woman married under age 18 and 16. It is also possible that parents of girls who marry later than average would have an incentive to underreport marriage age because of stigma from marrying old. However, we assume that this stigma does not kick in until at least age 20, and that women marrying at or above 20 who are ashamed of marrying late would not report a marriage age of less than 18. Under this assumption, these mismeasured underreports would not bias the dummy indicator of underage marriage.

[23] We do not find that either of the treatments is correlated with the probability of having a marriage certificate.

[24] Both data entry errors and cyclone damage are unrelated to treatment, minimizing the risk of bias.

[25] CHVs were instructed to inform communities about the program in January 2008 - four months before the first distribution round.

[26] There is higher attrition in the 2011 survey, which affects only the outcomes "In school at midline" and "Married at midline". Towards the end of the survey, rumors spread in one sub-district (Muladi) that enumerators were abducting or converting girls to Christianity. Survey operations were suspended for several months until confidence could be restored but attrition rates were higher in the affected area due to the greater time lapse and lower willingness to participate in data collection. This impacted 27 communities where data collection had not been completed.

and 24% gave birth before age 20. As women had to be 15-17 and unmarried at program start to be included in the study, these figures greatly underestimate the extent of early marriage and childbearing, and in particular marriage before 16, among this cohort. By comparison, among girls aged 18 at program start, 41% were married under 18 and 20% were married under 16.

Women in our sample have completed an average of 10 years of school at endline and 12% are engaged in income-generating activities with a mean income of $46 per month. Moreover, 22% were still in school at endline, of which 54% were married. At first blush, this figure seems incongruous in Bangladesh, where it is socially unacceptable for married adolescent girls to attend secondary school.[27] However, norms appear to be different for participation in the increasingly common part-time vocational training programs that female students in their 20s are almost exclusively enrolled in.

Table 1: Sample summary statistics

Age at Program Start:	Girls age 15-17 (N=15576)		Girls age 15 (N=5871)	
	Mean	S.D.	Mean	S.D.
Age at Endline	23.4	0.8	22.5	0.3
Ever married (%)	84.1	36.6	82.1	38.3
Married<18 (%)	27.7	44.8	36.6	48.2
Married<16 (%)	4.2	20.1	11.0	31.3
Ever birth (%)	62.9	48.3	59.5	49.1
Birth<20 (%)	24.0	42.7	32.0	46.6
Dowry (USD, conditional on married)	786.3	1327.0	753.5	870.5
Arranged marriage (%)	88.7	31.6	88.9	31.4
Age gap (Husband-Wife)	5.6	4.4	5.9	4.3
Husband from outside village (%)	90.3	29.6	89.7	30.4
Still in school (%)	21.6	41.1	24.0	42.7
Last class passed	9.8	4.3	9.7	4.1
Currently working (%)	11.9	32.3	11.7	32.1

Notes: Sample includes all women in study villages age 15-17 and unmarried at program start and followed-up at endline.

[27] In fact, the government's Girls' Scholarship Program *precludes* married girls from attending school and receiving a stipend. Consistent with this, at midline we find only 7% of married girls aged 17-19 are in secondary school. While those who have gone through the marriage ceremony but have not moved in with their husbands often do continue in school, this period of separation only lasts a matter of months, so is unlikely to pertain to women at endline who are ages 22-25.

3.4 Estimation Strategy

We estimate the impact of the incentive and empowerment programs with the following empirical specification:

$$Y_{icu} = \alpha + \beta_1 I_c + \beta_2 E_c + \beta_3 (I_c \times E_c) + \beta_4' X_{ic} + \epsilon_{icu} \quad (1)$$

where Y_{icu} is outcome Y for person i in community c and union u. I_c is assignment of community c to the incentive program and E_c is assignment of community c to the empowerment program. Since not every eligible girl in the program communities received the incentive or participated in the empowerment program, these are intention-to-treat estimates, although it is worth noting that the vast majority of girls in treatment villages are eligible for the incentive at the start of the program and take-up of both programs is extremely high (73% of eligible girls received the financial incentive and 93% of eligible girls enrolled in the empowerment program, see section 3.2).

Our estimates include a vector of individual and community controls X_{ic} measured at baseline for strata (village population tercile and union fixed effects), age, household size, the presence of an older unmarried sister in the household, school enrollment, mother's level of education, whether the community is accessible via public transport (a proxy for remoteness), and the ratio of unmarried adult boys to unmarried adult girls in the community (a proxy for marriage market conditions). We also test a specification excluding controls other than strata fixed effects. Errors are clustered at the unit of randomization (community).

Since potential program impacts vary widely across girls depending on their age at enrollment, we compare effects on the whole sample with effects on women eligible to receive the incentive for at least two years (aged 15 at distribution start).

4 Results

In this section, we investigate how the availability of the conditional incentive to delay marriage and the empowerment program influenced marriage timing and matching outcomes (i.e., dowry and groom characteristics) of treated girls.

4.1 Direct Effects on Marriage Timing

As shown in table 2, the incentive reduced child marriage by 17% (-4.9ppts, p<0.01) overall and 19% (-7.5pts, p<0.01) for women age 15 at distribution start who received the incentive for two years. The likelihood of being married under 16 fell by 18% (-2.0ppts, p<0.10) among women age 15 at distribution start.[28] These effects hold among both women who were and were not enrolled in school at baseline and among those with and without a father who is employed

[28]Results excluding controls, including women married before program start, and correcting for potential program inclusion errors yield similar results (online appendix OA.7)

full-time (appendix A.1.2.3). The absence of a stronger treatment effect on lower-income girls suggests that the incentive did not impact marriage timing by relieving household liquidity constraints or improving girls' nutrition.[29]

These patterns are also observed in the continuous measure of marriage age. As 16% of our sample is still unmarried, our marriage age data are censored. However, since by endline, marriage rates have converged between treatment arms to statistically indistinguishable levels (columns (6)-(7)), differences in marriage age among the married can be expected to capture an unbiased impact of the program on those who have married. Figure 3 shows the distribution of marriage age of women age 15 at program start by treatment arm, demonstrating a shift in marriages from the two years before 18 to the four years after 18. As shown in columns (8)-(9), the incentive increased average age of marriage by 2.5 months (0.21 years, $p<0.01$) overall and 3.9 months (0.33 years, $p<0.01$) among women age 15 at distribution start.

While this difference may at first appear small in magnitude, two things are important to keep in mind in interpreting the mean effect size: First, the average number of months of program eligibility was only 12 for girls age 16-17 and 24 for girls age 15. In addition, our estimates capture the average program effect across those on the margin of child marriage *as well as on those who would have married after 18 in the absence of the program* (for whom no program effect should be possible).

Based on control group data, a full 61% of women in incentive communities who participated in the program were not at risk of marrying young. Using the control group as a counterfactual and making the assumption that the program did not change the marriage age of those who would have married after 18 without the program, we can calculate the maximal number of months of delay we would observe if all women at risk of early marriage responded to the full duration of the incentive. This back-of-the-envelope calculation implies that if all 39% of control group women age 15 at distribution start who married under 18 were persuaded to wait until age 18, average marriage age would have increased by 6.9 months. Thus, our estimated treatment effect of 3.9 months of delay is the equivalent of more than half (57%) of families at risk responding to the incentive for the duration of the program.

We observe differences in marriage age across incentive and non-incentive communities even beyond the age of 18, despite no incentive being offered to remain unmarried at that age (figure 3). That some marriages were delayed well past 18 by the offer of an incentive at younger ages could be explained by marriage market search frictions. Qualitative interviews (with 116 women and parents) support this view (Field et al., 2018): marriage proposals come at infrequent intervals and parents will often wait many months for the right match for their daughter. Another possible explanation is that delaying marriage may endow women with greater bargaining power in negotiating marriage proposals, which they can then parlay into even further marriage delays once the program is over on account of being older.

[29] We have father's employment for a subset of women in our sample at baseline.

Table 2: Marriage outcomes, women age 15-17 and unmarried at program start

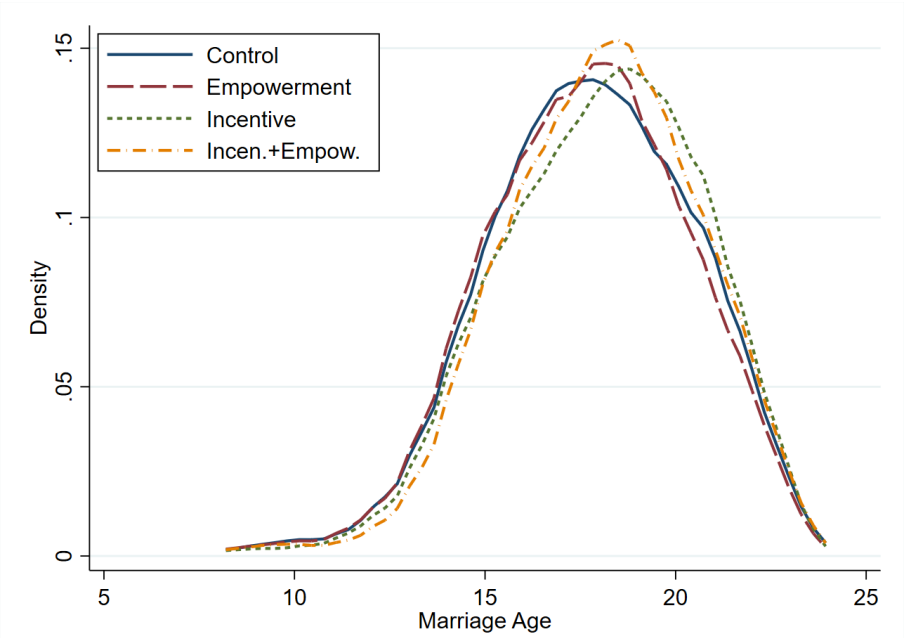
	Married<18		Married<16	Ever married at midline		Ever married at endline		Marriage age		Birth<20	
	Age 15-17	Age 15	Age 15	Age 15-17	Age 15	Age 15-17	Age 15	Age 15-17	Age 15	Age 15-17	Age 15
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Empowerment	-0.007	-0.007	0.006	0.011	0.008	0.006	0.002	0.013	0.013	0.005	0.003
	(0.008)	(0.015)	(0.009)	(0.011)	(0.017)	(0.008)	(0.012)	(0.040)	(0.065)	(0.007)	(0.013)
Incentive	-0.049	-0.075	-0.020	-0.025	-0.054	-0.009	-0.018	0.211	0.328	-0.016	-0.040
	(0.010)	(0.019)	(0.012)	(0.013)	(0.019)	(0.010)	(0.016)	(0.051)	(0.079)	(0.009)	(0.016)
Incen.*Empow.	0.019	0.028	-0.002	-0.011	0.008	-0.002	0.005	-0.052	-0.095	-0.002	0.012
	(0.014)	(0.026)	(0.016)	(0.019)	(0.026)	(0.014)	(0.022)	(0.074)	(0.118)	(0.014)	(0.023)
Control Mean	0.293	0.385	0.113	0.458	0.415	0.837	0.820	18.969	18.293	0.241	0.326
Observations	15549	5861	5861	14891	5604	15562	5864	12993	4773	15494	5847
FE	Union	Union	Union	Union	Union	Union	Union	Union	Union	Union	Union

Notes: The table shows results from OLS regressions with Huber-White robust SEs clustered at the community level in parentheses. The regressions control for strata (union and village size tercile) and a vector of baseline controls, including age, household size, an older unmarried sister in the household, school enrollment, mother's level of education, whether the community is accessible via public transport (a proxy for remoteness), and the ratio of unmarried adult boys to unmarried adult girls in the community (a proxy for marriage market conditions). Columns (1)-(3) and columns (6)-(11) present results from the endline parents' survey and columns (4)-(5) show results from the midline parents' survey. The sample includes all women age 15-17 and unmarried at program start. The sample excludes washedout households as well as households with insufficient tracking data. "Empowerment" is an indicator that is 1 if the woman lived in any of the empowerment communities (empowerment only or empowerment plus incentive) and "Incentive" is an indicator that is 1 if the woman lived in any of the incentive communities (incentive only or empowerment plus incentive).

The decline in child marriage also translated into a 7% (-1.6ppts, p<0.10) decline in teenage childbearing for those receiving the incentive overall and a 12% (-4.0ppts, p<0.05) reduction for women age 15 at distribution start. The childbearing results provide strong evidence that the marriage effects are not driven by underreporting, as bias is much less of a concern in reporting birth histories.

The incentive to delay marriage also has a large positive impact on school enrollment (table 3). We restrict our sample to women that are in school at program start because it is extremely rare for women to return to secondary school once they have unenrolled.[30] Women aged 15 at distribution start and eligible for the incentive were 18% (5.0ppts, p<0.05, column (4)) more likely to be in school at age 22 and had completed 3 months (0.25 years, p>0.10, column (6)) of additional schooling. That is, encouraging girls to delay marriage has a strong indirect effect on schooling attainment even when that education is not directly incentivized.

Figure 3: Distribution of marriage age



Notes: The figure plots the distribution of age at first marriage among women age 15 at program start and unmarried at baseline by community treatment status.

The empowerment program was effective in reducing girls' adherence to traditional gender norms (see appendix A.1.3). In addition, as we discuss in a companion paper (Buchmann et al., 2018), we find long-term impacts of the empowerment program on income-generating behavior. However, we do not observe any effect of empowerment on marriage outcomes or childbearing. For all marriage outcomes, we see null effects of the program, and the point estimate is positive for the majority of outcomes. Moreover, we find a significant *increase* in child

[30] We test this assumption in appendix A.1.2.3 and find no evidence of impact of the incentive on schooling for those women who were out of school at program start.

marriage among the subsample of women we surveyed directly (appendix table A.6). Since the same effects are not observed with precision in the larger sample, we see this as suggestive rather than conclusive evidence of a perverse effect of adolescent empowerment on marriage age. However, because the in-depth survey data are likely to contain substantially less measurement error, it is possible that our small sample results are a more precise estimate of program effects on marriage age than the census results. That is, one interpretation for the difference in empowerment results across samples is that only in the more precisely measured subsample do we have the statistical power to pick up the smaller, negative effect of the empowerment program on marriage age.

Table 3: Education outcomes, unmarried women age 15-17 and in school at program start

	In school at midline		In school at endline		Last class passed		Secondary complete	
	Age 15-17	Age 15	Age 15-17	Age 15	Age 15-17	Age 15	Age 15-17	Age 15
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Empowerment	0.012	0.015	0.014	0.024	0.178	0.174	0.016	0.018
	(0.013)	(0.019)	(0.011)	(0.016)	(0.092)	(0.123)	(0.012)	(0.017)
Incentive	0.030	0.086	0.023	0.050	0.129	0.250	0.020	0.039
	(0.015)	(0.022)	(0.015)	(0.021)	(0.117)	(0.187)	(0.016)	(0.024)
Incen.*Empow.	0.014	-0.005	0.007	-0.018	-0.071	-0.168	-0.006	-0.017
	(0.022)	(0.030)	(0.022)	(0.029)	(0.179)	(0.242)	(0.024)	(0.033)
Control Mean	0.466	0.482	0.280	0.278	11.337	10.833	0.444	0.406
Observations	10226	4272	10930	4545	10857	4518	10857	4518
FE	Union	Union	Union	Union	Union	Union	Union	Union

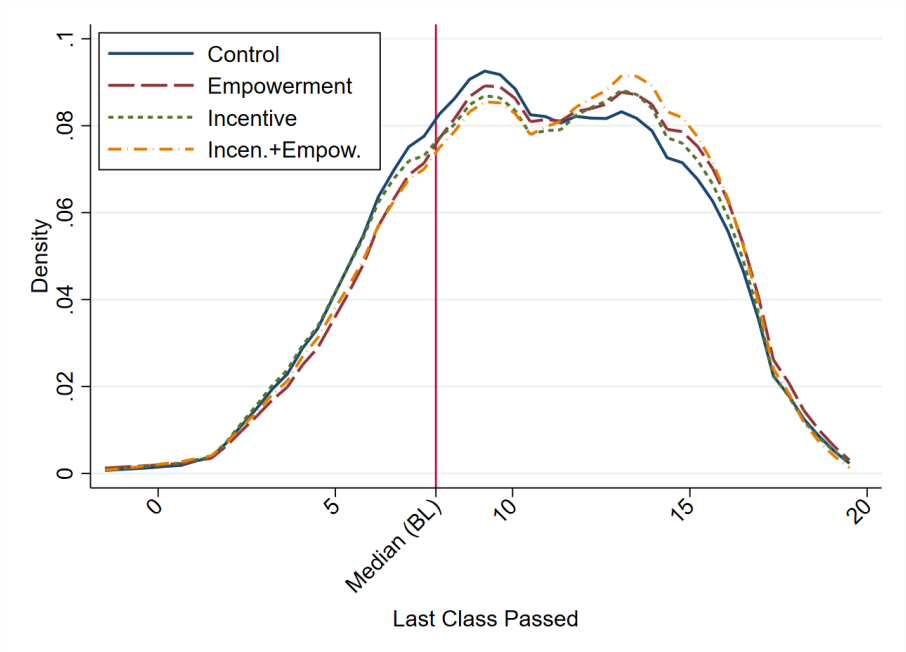
Notes: The table shows results from OLS regressions, adjusted for baseline characteristics and stratification (see notes to table 2), with Huber-White robust SEs clustered at the community level. Columns (1)-(2) present results from the midline parents survey and columns (3)-(8) present results from the endline parents survey. "Secondary completion" is an indicator that is 1 if the woman completed at least secondary school.

There is, however, some evidence that the empowerment program influenced schooling attainment, although to a lesser degree than the incentive, which altered schooling solely through marriage age (table 3). Women eligible for the empowerment program completed 2.1 months (0.18 years, $p<0.10$) of additional schooling relative to those in control communities.[31] As with the marriage results, the coefficient on the interaction term between the incentive and empowerment program is insignificantly different from zero in all specifications.

Figure 4 shows the probability density function of last class passed by treatment arm, demonstrating a shift in education resulting from participation in both programs that extends from the median education level at baseline to secondary and even tertiary education.

[31] Results excluding controls, including women married before program start, and correcting for potential program inclusion errors yield similar results (online appendix OA.7).

Figure 4: Distribution of last class passed



Notes: The figure plots the distribution of last class passed among women age 15 at program start and unmarried and in school at baseline, as reported at endline, by community treatment status. The red line marks the median level of education at baseline.

The positive effects of the empowerment program on schooling do not contradict the absence of a marriage result. They are consistent with the observed impact on girls' norms and beliefs, insofar as they are indicative of participants responding to the curricular components that actively encouraged them to pursue education and aspirations of labor market engagement. They are also in line with the marriage results when considered alongside the timing of educational impacts observed across the two treatment arms. In particular, while the incentive and empowerment programs both increased grade attainment, only the incentive program increased enrollment at midline when women were ages 18-21. This difference in the timing of program effects is also observed in secondary school completion: Women in the incentive program but not the empowerment program are significantly more likely to have completed secondary school (columns (7)-(8) of table 3).

Consistent with the marriage result, this pattern indicates that the empowerment program encouraged girls who were dropping out of school well before marriage to stay in school only up until the time of marriage, which would increase their grade attainment but preclude a large difference in enrollment at higher ages or secondary school completion. In contrast, the incentive, by relaxing the age constraint on schooling attainment, allowed some women to stay in school well past the age of 18 who would have otherwise dropped out to get married before age 18.

Together, this pair of results suggests that, although both programs encouraged girls to stay in school, they should not be viewed as policy substitutes in terms of their potential to increase

female schooling attainment since they have different possible magnitudes of influence. In particular, an empowerment program can only increase schooling insofar as girls in a given environment are dropping out before marriage, whereas a conditional incentive has the potential to influence schooling enrollment well beyond the life of the program.

4.2 Effects on Dowry and Husband Quality

Having found that the incentive and empowerment programs led to changes in bride characteristics (along the dimensions of age, quantity of education, and level of empowerment), we next test whether those characteristics led to different outcomes in the marriage market in terms of price (dowry and denmeher) or match (husband quality).

Table 4: Husband characteristics, unmarried women age 15–17 at program start and married at endline

	Dowry (USD)	Denmeher (USD)	Hus. edu.	Hus. salaried	Outside union
	(1)	(2)	(3)	(4)	(5)
Empowerment	39.415	-28.519	0.022	0.007	0.007
	(14.596)	(70.436)	(0.100)	(0.009)	(0.014)
Incentive	13.896	109.034	-0.163	-0.018	0.017
	(15.956)	(106.294)	(0.120)	(0.013)	(0.017)
Incen.*Empow.	-25.977	38.300	0.180	-0.011	-0.017
	(24.705)	(134.981)	(0.182)	(0.018)	(0.023)
Control Mean	587.106	1643.873	9.668	0.529	0.378
Observations	11842	743	11577	12727	12384
FE	Union	Union	Union	Union	Union

Notes: The table shows results from OLS regressions, adjusted for baseline characteristics and stratification (see notes to table 2), with Huber-White robust SEs clustered at the community level. "Outside union" is an indicator that is 1 if the husband is from outside the woman's union. Dowry and denmeher are trimmed at 3 standard deviations.

Results on dowry, denmeher and observable husband characteristics (a proxy for marriage quality) for women age 15-17 at program start are presented in table 4. Data on dowry and husband characteristics are collected from women's parents where we have a large sample and greater confidence in dowry payment reports. However, this short parental survey did not capture a rich set of husband characteristics. Data on denmeher (payment due to a woman in the event of divorce) are collected from a subset of women directly. On average, women in incentive communities do not pay a dowry penalty for marrying later (column (1)) and the coefficient on denmeher is positive though insignificant (column (2)). There are no statistically significant differences across treatment arms in husband's education (column (3)), the most meaningful indicator of husband quality included in our short survey of parents. Husband's income generation and residence are similarly unchanged (columns (4)-(5)). Together this

suggests no penalty in the marriage market for marrying later in incentive communities.

By contrast, women eligible for the empowerment program saw a 7% (USD 39, $p<0.01$) *increase* in dowry and no compensating change in denmeher or husband quality relative to women in control communities. This combination of results suggests that participation in the empowerment program caused a penalty in the marriage market.

5 A Signaling Model of Marriage Timing

The pattern of program effects on girls' marriage age is inconsistent with a strong preference for young brides, and also with an unraveling story in which women marry early for fear of missing out on the highest quality husbands. In the preferences scenario, the incentive compensates parents for the marriage market penalty for delayed marriage. Yet we do not find that women who marry later as a result of the incentive pay a marriage market penalty in terms of husband quality *or* marriage price (dowry and denmeher). Moreover, although the value of the incentive represents a greater fraction of household income for poorer households, we do not find stronger effects among households whose father was not employed full-time at baseline, a proxy of low income (table A.4). Finally, under a preference-based explanation for early marriage, we would not expect to see a reduction in marriage age in empowerment villages nor an increase in dowry if the empowerment program successfully increased opposition to early marriage by making girls more aware of its dangers.

Under an unraveling story, the incentive persuades marginal women to delay marriage, which reduces the pressure to marry early on inframarginal women as more men become available. However, this mechanism works at the level of the marriage market, which in this setting is much larger than the village (89% of girls marry outside the village). In addition, potential grooms come from a large age range (the standard deviation of age of groom is 4.4 in our data) suggesting a large potential pool of eligible husbands that works against unraveling mechanisms. A marriage delay of a small subset of women in the marriage market within a very narrow age range is thus unlikely to meaningfully change the pool of eligible men, and, in an unraveling story, should not be contained within treatment villages.

In this section, we build a model that seeks to illuminate the results of our field experiment. In particular, we seek to explain the following results: an incentive (which is small relatively to dowry) causes a large fraction of at risk girls to delay marriage; those who respond to the incentive by marrying later do not pay a penalty in terms of higher dowry or worse quality of husband; an exogenous reduction in girls' adherence to traditional gender norms does not increase age of marriage and if anything reduces it; girls in villages where the empowerment program operated pay a higher dowry for an unchanged quality of groom. The model enables us to precisely specify the conditions under which reputation concerns can lead to child marriage. We also use the model to show how the marriage market responds when an incentive to delay marriage is introduced, calculate the optimal structure of an incentive, and show how the market

responds to a change in the distribution of bride types, as occurs as a result of the empowerment program. As the model was written to explain our main results only, we also generate auxiliary predictions that can be tested against our data.

For signaling considerations to influence marriage timing, two conditions must hold: First, it must be the case that a dominant bride characteristic (henceforth, "preferred" type) is not fully observable to potential grooms, and second, it must be the case that this characteristic is correlated with the returns to postponing (the non-preferred types are known to gain more by delaying marriage than the preferred types). If these two conditions hold, men's belief that early marriage signals desirability in this key dimension is sustainable in equilibrium.[32]

As described in section 2, the dominant bride characteristic in our setting is women's adherence to conservative gender norms of behavior, which is highly likely to satisfy both conditions. First, it is arguably difficult for a prospective groom to fully observe a prospective bride's first-order beliefs about gender norms, and our data suggest that family members' beliefs - which may be more observable - provide little information on individual norms. To measure obedience to gender norms, we construct an index based on a suite of baseline survey questions about adherence to conservative gender norms asked of girls and their parents. As shown in online appendix OA.2.1, girls' responses differed substantially from responses of their sisters and mothers, indicating that social conservatism is not well predicted by parents' or older siblings' conservatism in our setting.

In this setting, conservative women are likely to have lower (though positive) returns to education than less conservative women who are willing to challenge gender norms and thus more likely to work outside the home and participate in household decisions. Indeed, even controlling for parents' social conservatism, in our sample socially conservative women in control communities are substantially more likely to marry early and have lower education on average than less socially conservative women (table 5). They are also less likely to work outside the home or have economic decision-making power inside the household once married. Taken together, these results suggest that less conservative women gain more from getting educated.

As we formalize in this section, if these two conditions hold, signaling concerns can lead everyone to marry younger: since dowry can only be conditioned on observables like marriage timing and not on unobservable characteristics like degree of adherence to traditional norms, brides who enter the marriage market later are believed to be non-preferred types because everyone knows they have relatively higher returns from education and therefore a stronger incentive to stay in school.[33] Thus, all brides enter early, even though everyone would gain if

[32]Women may also be heterogeneous in observable dimensions, i.e., physical appearance. The unobservable heterogeneity is within an equivalence class of observable characteristics. To illustrate the idea clearly, we suppose that women cannot send costly signals of their unobservable type (signals are infinitely costly), that is, the case of greatest information asymmetry. As signals become less costly, information becomes less asymmetric. Our signaling model predicts that we should see less pooling on early marriage as type becomes more observable.

[33]We focus on education because there is causal evidence on the impact of early marriage on education and

women delayed marriage and attained more schooling.[34] In this environment, an intervention reduces child marriage induced by reputation concerns if and only if it is believed to strengthen the returns from delaying marriage for sufficiently many preferred types. The rest of this section formalizes these insights and shows how a small but randomly assigned conditional financial incentive can delay marriage.

Table 5: Marriage and education outcomes, unmarried girls age 10-17 at program start in control communities, by women's social conservatism at baseline

	Ever Married	Married<18	Still in school	Last class passed	Currently Working	Economic DM
High Social Conservatism	0.125	0.141	-0.194	-2.290	-0.058	-0.088
	(0.027)	(0.026)	(0.028)	(0.198)	(0.029)	(0.042)
High Parents' Social Conservatism	0.045	0.073	-0.089	-0.891	0.022	-0.077
	(0.022)	(0.028)	(0.025)	(0.202)	(0.026)	(0.039)
Outcome Mean	0.721	0.404	0.417	11.421	0.314	-0.071
Observations	1737	1668	1733	1738	1723	1341
FE	Union	Union	Union	Union	Union	Union

Notes: The table shows results from weighted OLS regressions (see Buchmann et al. (2018)), adjusted for baseline characteristics unrelated to conservatism and stratification (the girl's age, her bmi and an indicator for whether she is stunted (proxies for baseline health), household size and assets and whether her mother is schooled (proxies for per capita wealth), the presence of an older unmarried sister in the household, whether the community is accessible via public transport and distance to closest motorable road (proxies for remoteness), the ratio of unmarried adult boys to unmarried adult girls in the community (a proxy for marriage market conditions) and the distance of the household to the closest incentive community (to control for signaling concerns in non-incentive households)), with Huber-White robust SEs clustered at the community level. "Economic decision-making" is a Kling Mean Effects index of 11 purchase/investment decisions in which the woman could be involved inside the household. The social conservatism indices are formed as described in online appendices OA.2.3 and OA.2.1. "High Social Conservatism" is an indicator that is 1 if the woman had an above median social conservatism at baseline. "High Parents' Social Conservatism" is an indicator that is 1 if the parents of the woman had an above median social conservatism at baseline.

5.1 The Model

A marriage market is populated by women of measure $|W| = 1$, and men of measure $|M| > 1$.[35] Women are heterogeneous in preferred type $\Theta \in \{L, H\}$, which is private information, while men are homogenous.[36] Men desire Θ_H women but cannot learn this type before

the trade-off between early marriage and education is highly salient in Bangladesh, but marrying too young can potentially have negative effects on childbearing and child-rearing for reasons in addition to mother's education (Mathur et al. (2003), Jain and Kurz (2007), EngenderHealth (2003), Nour (2009), Raj (2010)). In our sample, educated women have healthier children (online appendix OA.2.2).

[34] In theory, grooms could also offer a menu of dowries by type. We provide conditions under which this cannot be implemented in equilibrium and focus on this case in our paper, as we do not observe menus of dowries by unobserved type in the data.

[35] This is not necessary for our results, but we assume this so that money given to women is not fully extracted by men in dowry charged, which we do not observe empirically. Partial extraction is likely more realistic but does not affect our results if homogeneous. If heterogeneous, our results do not need to be modified as long as heterogeneity in extraction is not correlated with gains from delaying for the non-preferred unobservable type. Moreover, $|M| > |W|$ is in line with our data, in which the ratio of unmarried men age 21 to 23 to unmarried women age 15-17 is 1.9 (the average marriage gap in our setting is 6 years).

[36] We assume homogeneity of men in our benchmark in order to focus on the signaling strategies of heterogeneous women. Note that both men and women can be heterogeneous in any number of observable ways that matter in the marriage market, without implication for the model. Hence, in reducing men to homogenous types while allowing women to be heterogeneous in type we are assuming that the most important feature of brides in the marriage market is not perfectly observed, while that of men is relatively observable (e.g., income or earnings capacity).

marriage. Not only is social conservatism a difficult trait for suiters to observe directly in this setting of arranged marriages where couples rarely interact one-on-one before their wedding day, but it is arguably easy for Θ_L women to pretend to be conservative when meeting potential grooms.

It is commonly known that the fraction of preferred types is $f \in (0, 1)$. All women have the same outside option, ω_W, and face the same liquidity constraint, a total budget of Y.[37] Women choose between entering the marriage market early at t_1 or delaying until t_2.[38] We assume that if a woman marries in t_1, her education level is E_L, while if she delays until t_2, her education level increases to $E_H > E_L$.[39] A woman's education and her decision about when to enter the marriage market are observable and contractible.

Men have a higher outside option than women: $\omega_M > \omega_W$.[40]

Suppose the total transferable utility generated by a union between a man and a woman W_j, $\mu(\Theta_j, E_j)$, increases in both arguments, and exhibits two additional features:

1. The unobservable type Θ is first-order in marriage desirability: $\mu(\Theta_H, E_H) > \mu(\Theta_H, E_L) > \mu(\Theta_L, E_H) > \mu(\Theta_L, E_L)$.[41]

2. The unobservable type and education are substitutes such that $\mu(\Theta_H, E_H) - \mu(\Theta_H, E_L) < \mu(\Theta_H, E_L) - \mu(\Theta_L, E_H) < \mu(\Theta_L, E_H) - \mu(\Theta_L, E_L)$.[42]

If a woman enters the marriage market, men can decide whether to propose to her. Specifically, they propose a dowry D to be paid by the woman's family as a price for marriage, where D can be conditioned only on observable characteristics.[43] Since there are more men than women, and men are homogeneous, if a woman is such that at least one man is willing to propose to her, then multiple men are willing to propose to her. This implies that men compete for available women with whom marriage would satisfy their participation constraints at some feasible (affordable) dowry.

[37] We do not assume that non-preferred type women have higher outside options because then men would set the dowry such that only preferred type women would be willing to marry, which we do not observe empirically.

[38] We assume that entering the market but not marrying is observable, and the damage to future marriage prospects outweighs any possible gain. This is both realistic and simplifying. Thus, if a woman expects to receive better marriage proposals in t_2, she will wait to enter in t_2.

[39] In our setting, if young women do not get married, they go to school. Thus, marriage and education co-move closely for young women who would be considered child brides. However, it is important to note that our results do not require a one-to-one relationship, merely that there is some desirable societal effect (for example, on human capital) from delaying marriage for young women.

[40] This simply ensures that women are always willing to pay the minimal dowry at which a man is willing to marry them, even if they cannot afford it.

[41] Among husbands of women in our sample, 50.9% reported either nature/character/reputation or religion/tradition as one of their two most desired bride characteristics as compared to 31.9% who reported education, 9.2% who reported family reputation/wealth, 2.1% who reported hard-working/income, and 0.9% who reported age.

[42] In our setting, men who report desiring education in a potential wife are less likely to report desiring gender-normative characteristics in a potential wife (online appendix OA.2.2).

[43] We abstract from denmeher in our model as dowry and denmeher usually move in the same direction in our setting (Buchmann et al., 2021).

The timing of the game is as follows. At the beginning of t_1:

1. An unmarried woman may declare herself available for marriage, and in doing so reveal herself to have low education E_L.
2. Men may make dowry offers to available women.
3. If a woman receives an offer and accepts it, she pays the dowry, gets married, and exits the market.
4. If a woman receives an offer and rejects it, or does not receive an offer, she remains unmarried, and may re-enter in t_2 (but it will be known that she entered in t_1 and remained unmarried).

At the beginning of t_2:

1. Unmarried women may declare availability for marriage. They will be known to have education E_H.
2. Unmarried men may make dowry offers to available women.
3. If a woman receives an offer and accepts it, she pays the dowry, gets married, and exits the market.
4. If a woman receives an offer and rejects it, or does not receive an offer, she stays unmarried.

We study perfect Bayesian equilibria (PBE) that survive the intuitive criterion.[44] Formally, we solve for (i) $Pr(\Theta = H|t_1)$ and $Pr(\Theta = H|t_2)$: a man's beliefs about a woman's unobserved type, conditional on when she enters the marriage market, (ii) $\sigma(\Theta)t_1+(1-\sigma(\Theta))t_2$: a woman's entry strategy, given her type Θ, and (iii) $D|t_1$ and $D|t_2$: the dowries offered by men to women who enter at t_1 and at t_2.

The intuitive criterion disciplines off-equilibrium beliefs and actions. The conditions for on-equilibrium behavior 1.-3. are:

1. Beliefs $Pr(\Theta = H|t_1)$ and $Pr(\Theta = H|t_2)$ must respect Bayes' rule.
2. A woman's entry strategy $\sigma(\Theta)$ maximizes her expected utility, given beliefs, the entry strategies of other women, and the proposal strategies of men.
3. A man's proposed dowry $D|t_1$ to a woman in t_1, or $D|t_2$ to a woman in t_2, maximizes his expected utility, given beliefs, the entry strategies of women, and the proposal strategies of other men.

[44] A PBE violates the intuitive criterion if there exists a type that has a profitable deviation, given beliefs that assign positive weight only to types for whom that deviation is not equilibrium-dominated (Cho and Kreps, 1987).

5.2 Child Marriage

The fact that bride type is not perfectly observed opens up the possibility that, under certain marriage conditions, women of different types pool on a single marriage timing. The pooling scenario is of particular interest because, as we show below, it implies that the unique equilibrium is for all brides to marry young. As a result, there are: (1) an inefficiently large number of women marrying young, and (2) scope for small financial incentives to generate large changes in marriage timing since they have the potential to influence the behavior of all types. Hence, we begin by characterizing conditions under which men are unable to offer a menu of dowries to induce women to reveal their private type by self-selecting into different marriage timing decisions (i.e., separating equilibria).

Result 1. *Suppose women are liquidity-constrained:* $Y < \omega_M + \mu(\Theta_H, E_H) - 2\mu(\Theta_H, E_L)$. *Then non-preferred type women are unable to pay the cheapest dowry* $D|t_2$ *men are willing to offer a later entrant that he knows with certainty is a non-preferred type. Hence, separation cannot be achieved.*

See appendix A.1.1.1 for a formal proof.

First, observe that the substitutability of Θ and education in match quality implies single-crossing: if Θ_H weakly prefers entering in t_2 and paying dowry $D|t_2$, then Θ_L strictly prefers to do so. Thus, if a separating equilibrium exists, it must be that preferred types enter early and non-preferred types delay.

In order to induce this separation, a man must charge a higher dowry for delaying marriage. If he charged a lower dowry in t_2, all women would be strictly better off entering at t_2, as they would be more educated and pay a lower price for marriage. In particular, to induce separation, a man must propose $D|t_2$ that exceeds the preferred type's marginal gain from increasing her education, but does not exceed the non-preferred type's.

The liquidity constraint condition in result 1 ensures that no woman can afford the minimum $D|t_2$ that both induces separation and satisfies the man's participation constraint.[45] Examining the condition, we see that a separating equilibrium is harder to achieve the smaller the gap between preferred and non-preferred types' marginal returns to delaying marriage, and harder to achieve the more men value bride type relative to bride education.

A corollary is that there are no mixed-strategy equilibria.

Corollary 1. *When there is no separating equilibrium, there are no equilibria in mixed strategies.*

The logic is due to single-crossing. A woman mixes between entering at t_1 and entering at t_2 if and only if she is indifferent between entering in either period. Since delaying marriage

[45] If non-preferred types were richer than preferred types, that would make separation more possible. While socially conservative women in our sample come from less wealthy households on average, the difference is very small and unlikely to overcome the information asymmetry (the difference in household income between young women of below and above median social conservatism is less than 1% of average expected dowry at baseline).

affords her the opportunity to increase her education and thus her desirability, a man must ask for a higher dowry for t_2 entrants in order for her to be indifferent between marrying early and delaying.

Non-preferred types do not play a mixed strategy in equilibrium, because if they are indifferent between entering at t_1 and paying dowry $D|t_1$ and entering at t_2 and paying dowry $D|t_2$, then preferred types strictly prefer entering early at t_1. But then men know that t_2 entrants are non-preferred type with certainty, and by our condition that the unobservable type is first-order in marriage desirability, men will not propose to t_2 entrants (the dowry they would require to be willing to marry them exceeds the woman's budget constraint).

On the other hand, if the preferred type is indifferent, the non-preferred type must strictly prefer delaying. Thus, to respect Bayes' rule, men must believe that t_1 entrants are preferred type with certainty.

We show in online appendix OA.3.2 that if a man proposes the minimal $D|t_2$ that satisfies his participation constraint, the period 1 discount he must give in proposing a lower $D|t_1$ violates his participation constraint, and if a man proposes the minimal $D|t_1$ that satisfies his participation constraint, the minimal $D|t_2$ he must charge violates the liquidity constraint. Thus, if there is no separating equilibrium, then there is no semi-separating equilibrium.

Finally, we show that, when there is no separating equilibrium, then the unique equilibrium (absent a financial incentive) is that all women pool on entering the marriage market early, for a sufficiently large difference in the returns to delaying marriage by unobservable type.

Notationally, let the expected utility of a match with a highly educated woman be:

$$\overline{\mu}(E_H) \equiv f\mu(\Theta_H, E_H) + (1-f)\mu(\Theta_L, E_H)$$

Result 2. *Suppose that:*

$$\mu(\Theta_H, E_H) - \mu(\Theta_H, E_L) < \mu(\Theta_H, E_L) - \overline{\mu}(E_H) < \mu(\Theta_L, E_H) - \mu(\Theta_L, E_L)$$

Then the unique equilibrium is:
(i) All women enter the marriage market at t_1.
(ii) Men propose dowries:

$$D|t_1 = \omega_M - [f\mu(\Theta_H, E_L) + (1-f)\mu(\Theta_L, E_L)]$$
$$D|t_2 = \omega_M - \mu(\Theta_L, E_H)$$

(iii) Beliefs are $Pr(\Theta = H|t_1) = f$ *and* $Pr(\Theta = H|t_2) = 0$.

This condition effectively places an upper bound on the fraction of preferred types in the population, relative to the difference in marginal returns to education by type. Appendix A.1.1.2 contains the formal proof.

The intuition for this result is as follows. There are two candidates for a pure-strategy equilibrium, pooling on t_1 and pooling on t_2.

Pooling on t_2 is a PBE but fails the intuitive criterion. To see this, suppose a woman deviates, and enters early at t_1 instead. Then she could credibly send the following message to a man: "I am a docile woman. You should believe this, because a more liberal woman would never deviate and enter early, given the dowry offered to t_2 entrants when all women pool on t_2. This is because, even if you had the most favorable beliefs about t_1 entrants, which are that she is docile for sure, she would still prefer to enter at t_2 and pay the equilibrium dowry $D|t_2$ offered when women are believed to be preferred type with probability f." But, if preferred types can credibly deviate, men should believe that t_1 entrants are Θ_H with certainty. Under these beliefs, preferred types prefer to deviate and enter at t_1 instead. Thus, pooling on t_2 fails the intuitive criterion.

On the other hand, pooling on t_1 is a PBE that survives the intuitive criterion. Suppose a preferred type tried to deviate to t_2. Because of single-crossing, she cannot credibly send a message that convinces a man that women who deviate and delay marriage must be preferred types—if a preferred type prefers to deviate and delay, a non-preferred type has an even stronger preference to do the same thing. A non-preferred type could convince men that she is non-preferred type, but men do not like marrying non-preferred types, so the dowry a man would charge a woman entering at t_2 whom he believes is non-preferred type with certainty exceeds her budget constraint.

Thus, child marriage results as an inefficient consequence of signaling when women with the undesired unobservable type are known to have higher marginal returns (or lower marginal costs) from delaying marriage, for example, as a result of differential returns to staying in school and increasing education.

5.3 Incentive to Delay Marriage

Our first set of results reveals that, if signaling is what is driving child marriage, then a policy that hopes to reduce child marriage must strengthen the preferred woman's incentive to delay marriage at least as much as the non-preferred. In this section we examine how the introduction of a small payment conditional on remaining unmarried changes the equilibrium.

Suppose a conditional incentive C is given randomly and privately to a fraction $\tau \in (0, 1)$ of women, if they enter the marriage market at t_2. Thus, treatment status is unobserved, and orthogonal to unobservable type.

We are interested in small transfers that operate through their effect on signaling, rather than through income effects that alter the intrinsic structure of the marriage market.[46] Thus, suppose that the dowry a man requires to be willing to marry an educated woman who is known to be non-preferred type still exceeds her budget constraint, even if she receives the conditional transfer C.[47]

[46] In our experiment, the annual value of the transfer is approximately 2% of average dowry and 7% of average household income.

[47] Small transfers are also unlikely to change the bargaining power of women.

Now, unobserved type is two-dimensional—treatment status by Θ. A fraction τf of women are treated preferred types, $(1-\tau)f$ are untreated preferred types, $\tau(1-f)$ are treated non-preferred types, and $(1-\tau)(1-f)$ are untreated non-preferred types. Since neither type nor treatment status is observable, men continue to condition dowry only on marriage timing.[48]

Our first result is that a small and random conditional incentive does not enable a separating equilibrium where one was not possible before.

Result 3. *A separating equilibrium cannot be sustained for any τ. That is, men are unable to offer dowries $D|t_1$ and $D|t_2$ such that $\sigma(\Theta_L) \in \{0,1\}$ and $\sigma(\Theta_H) = 1 - \sigma(\Theta_L)$.*

See appendix A.1.1.3 for a formal proof. The key insight is that, because the treatment is random and therefore independent of type, preferred and non-preferred types are equally likely to have the eased liquidity constraint in t_2. Crucially, the conditional incentive does not increase the difference between the non-preferred and preferred type's marginal gains from education, or the relative ability of the non-preferred type to pay (since an equal fraction of preferred types are also more able to pay).

Our second result is that the conditional incentive *does* enable a semi-separating equilibrium in unobservable type. In particular, untreated preferred types continue to enter early at t_1, but all other women delay until t_2. Bayes' rule implies that beliefs are:

$$Pr(\Theta = H|t_1) = 1$$
$$Pr(\Theta = H|t_2) = \frac{\tau f}{\tau f + (1-f)} \equiv f'$$

Note that $f' < f$—as treatment coverage τ increases, f' approaches f. Let $\overline{\overline{\mu}}(E_H)$ denote the expected quality of a match with a highly-educated woman when the fraction of preferred types is f'. This leads us to our main result.

Result 4. *Suppose the size of the conditional transfer C satisfies:*

$$C > \mu(\Theta_H, E_L) - \overline{\overline{\mu}}(E_H) - [\mu(\Theta_H, E_H) - \mu(\Theta_H, E_L)]$$

Then a semi-separating equilibrium in unobservable type exists, and Pareto-dominates the equilibrium where all women enter at t_1:
(i) Untreated preferred types marry at t_1, while untreated non-preferred types, treated preferred types, and treated non-preferred types marry at t_2.
(ii) Men propose dowries:

$$D|t_1 = \omega_M - \mu(\Theta_H, E_L)$$
$$D|t_2 = \omega_M - \overline{\overline{\mu}}(E_H)$$

(iii) Beliefs are $Pr(\Theta = H|t_1) = 1$ and $Pr(\Theta = H|t_2) = f'$.

[48] We discuss the observability of the treatment status in section 6.

See appendix A.1.1.4 for a formal proof. Note that as treatment coverage τ increases, the size of the transfer C needed decreases. We discuss this further in the next subsection.

As delaying marriage is no longer a certain signal of non-preferred type, untreated non-preferred types also prefer to delay marriage until t_2, even though they do not receive a conditional transfer for doing so. In other words, the treatment generates spillovers. However, untreated preferred types will prefer to enter at t_1 and pay the cheaper dowry that results from men knowing that t_1 entrants are preferred type with certainty.

Because men compete for women, men receive their outside options in equilibrium. Hence, men are indifferent between marrying untreated preferred types early and receiving a lower dowry $D|t_1$, and waiting to marry a woman who may be a treated preferred type, a treated non-preferred type, or an untreated non-preferred type, and receiving a higher dowry $D|t_2$. The equilibrium dowries are determined by this competition.

Thus, this semi-separating equilibrium candidate is a PBE that survives the intuitive criterion, although it is not unique. Pooling on t_1 continues to be an equilibrium as well. However, it is Pareto-dominated by the semi-separating equilibrium. All women are strictly better off. Untreated preferred types still enter at t_1, but they pay a lower dowry, since men now know they are preferred type with certainty (non-preferred types no longer pool with them). The remaining women are better off because their education is increased, and the returns to education are not extracted through dowry, because men compete for women. In fact, because increased education increases marriage utility (increases the total pie to be divided), women pay a lower dowry than they would in the absence of the conditional incentive.[49]

Men are indifferent, because competition for women drives them to receive their outside options, which are not affected by the small conditional incentive. However, since the total pie has increased through increased education, it is easy to see that slightly relaxing perfect competition for women would result in a strict improvement for every individual, including men.

This set of results shows why a random conditional incentive might effectively reduce signaling-driven child marriage and lead to a Pareto improvement. In the next subsection, we think about the optimal way to structure this type of treatment. That is, we shed light on how to maximize the impact of the treatment (including positive spillovers), given the cost of the transfer.

5.4 Optimal Coverage of the Incentive

First, we observe that, if it is the case that under treatment coverage τ, every treated woman delays marriage (fixing transfer size), then it is the case that under treatment coverage $T > \tau$, every treated woman delays. This is because increasing treatment coverage increases the beliefs that t_2 entrants are preferred types (since untreated preferred types continue to enter at t_1).

[49] The more imperfect the competition for women, the smaller the decrease in dowry.

Thus, giving every woman the conditional incentive would be likeliest to cause all women to delay marriage.

However, we show that this is not the policy that yields the most benefit for cost, given linear benefits and costs. This is because treating every woman mechanically eliminates the scope for positive spillovers.

We formalize this in the following result. Suppose that delaying marriage for a woman yields unit benefit B_Θ, and that total cost of the treatment is the transfer C multiplied by the number of women receiving the treatment (the monetary cost of the treatment). Suppose that the conditional incentive is given randomly. Then:

Result 5. *(i) Interior treatment coverage maximizes total social welfare when benefits and costs are linear in the number of women treated. (ii) The minimal treatment coverage needed to induce treated preferred types to delay if the size of the transfer is C is:*

$$\tau_{\min}(C) = \frac{\frac{2\mu(\Theta_H,E_L)-[\mu(\Theta_H,E_H)+\mu(\Theta_L,E_H)]-C}{[\mu(\Theta_H,E_H)-\mu(\Theta_L,E_H)]}}{\left[1-\frac{2\mu(\Theta_H,E_L)-[\mu(\Theta_H,E_H)+\mu(\Theta_L,E_H)]-C}{[\mu(\Theta_H,E_H)-\mu(\Theta_L,E_H)]}\right]}\frac{(1-f)}{f}$$

Observe that the more widespread the coverage, the smaller the transfer that is needed.

Thus, if child marriage persists due to signaling motives, the most cost-effective random conditional incentive is one with lower coverage and larger transfers if non-preferred types are believed to be prevalent (to maximize spillovers), while greater coverage and smaller transfers are preferred if non-preferred types are believed to be less prevalent.[50] We discuss the optimal policy with random treatment in online appendix OA.3.3.[51]

5.5 Changing Distribution of Bride Types

We next use our model to examine the impact, in a signaling environment, of an empowerment intervention that decreases, or is believed to decrease, the fraction of potential brides who adhere to traditional gender norms. We find that it weakly increases the likelihood of pooling on early marriage. The intuition is akin to the above result on optimal transfer amounts. As the likelihood grows that a girl in a given marriage market is a non-preferred type, there is greater incentive for preferred types to deviate from marriage delay. We formalize this in the following corollary:

Corollary 2. *The smaller f is – that is, the larger the share of non-preferred types – the more likely it is that women will pool on early marriage and low education.*

See appendix A.1.1.5 for a formal proof. The key insight is that, given a mix of preferred and non-preferred types such that there is still reasonable uncertainty about type in the popula-

[50] Of course, non-preferred types must be a significant presence in the population for there to be a signaling story in the first place.

[51] We discuss non-random treatment in online appendix OA.3.4.

tion, a decrease in f will cause men to face a higher risk of marrying a non-preferred type, and thus will strengthen women's desire to signal they are preferred by entering the marriage market early and foregoing education, which everyone knows yields differentially higher returns to non-preferred women.

5.6 Testable Predictions

In the previous subsections, we formally described the conditions of the environment under which reputation concerns lead to child marriage as well as the effects of a financial incentive to delay marriage and an intervention that decreases the fraction of docile brides in this environment.

We show that the three key empirical findings from our experiment are compatible with a reputation signaling model. In particular, our model generates the following three predictions which align with our main empirical findings:

Prediction 1. *A conditional incentive to delay marriage offered to a randomly chosen subset of women delays marriage timing and increases education among treated women of all types.*

Prediction 2. *Treated women pay weakly smaller dowry and do not have lower quality marriages than untreated women.*

Prediction 3. *An exogenous increase in the fraction of non-preferred types weakly decreases marriage age and increases dowry among all types.*

In addition, the model generates an auxiliary prediction on the nature and location of spillovers from the incentive, which helps distinguish a signaling model of child marriage from alternative hypotheses:

Prediction 4. *A conditional incentive to delay marriage offered to a randomly chosen subset of women delays marriage among untreated non-preferred types whose treatment status is not fully observable.*

In the following section we test prediction 4 by analyzing the effects of the financial incentive on untreated non-preferred type women.

6 Spillover Effects on Marriage Timing

According to theoretical prediction 4, if social conservatism is first-order in men's marriage preferences and if less conservative women gain more from getting educated, then untreated non-preferred types should also delay marriage. To test this prediction, we analyze whether the conditional incentive led to spillovers among untreated women whose treatment status is observed with noise, and whether spillovers are stronger among untreated non-preferred, or less socially conservative, women.

First, we find that knowledge about the incentive program spread to untreated households: 79% of untreated women in incentive communities and 25% of untreated women in non-incentive communities had heard about the incentive program at midline (online appendix OA.2.4). Furthermore, at midline, 8% of in-laws of women who did not receive the treatment incorrectly believed that their daughter-in-law received the treatment, suggesting that not only was knowledge about the treatment widespread, but also that we are correct in assuming the treatment was observed with noise.

If the treatment was observed with noise, then we should see spillovers precisely to those untreated women who are hardest to distinguish from the treated women. Indeed, women who lived close to incentive communities were almost 50% more likely to have heard about the incentive program (35% of women living within 500 meters vs. 24% of women living farther than 500 meters from the closest incentive community center). Hence, using communities' geo-locations, we test for spillovers among women who live in untreated communities that are close to treated communities. Reports of in-laws at midline support that treatment status was observed with noise in these communities: 23% of in-laws of women from these communities incorrectly believed that their daughter-in-law had received the treatment compared to <1% of in-laws of women living in farther-away communities. Our regression estimates of program spillovers in columns (1)-(2) in table 6 thus include all households in non-incentive communities at endline and compare women in communities within 500 meters to women in communities farther than 500 meters from the closest incentive community center. To ensure that distance from the community to the closest incentive community does not simply capture urbanicity, we control for distance of the community center to the geographic center of the closest community.

As shown in column (1), women age 15-17 at program start who lived close to incentive communities are 10% (-2.9ppts, $p<0.10$) less likely to have married under the age of 18 than women who did not live close to an incentive community. As a placebo test, we estimate the same regression for women aged 7-14 who are thus observably not eligible for the incentive in any community and find no spillover effect (column (2)). It does not appear that treatment effect spillovers to nearby non-treatment communities are due to a general change in the acceptability of later marriage, but rather operate through a more specific channel that only benefits those women that have the potential to be mistaken for incentive program participants.

According to predictions 1 and 4, if child marriage persists due to signaling motives, then all treated women as well as less socially conservative (i.e.non-preferred type), untreated women should delay marriage. As women's social conservatism is not fully observable from household conservatism (online appendix OA.2.2), we test these predictions among the subsample of program participants who were tracked and surveyed directly.[52] Consistent with our theoretical prediction, we observe spillovers only on less socially conservative women (columns

[52]These women were randomly sampled and fully tracked to all parts of the country with a 10% attrition rate (see Buchmann et al. (2018), in progress for details of the subsample survey.

(3)-(4)): Less socially conservative women who lived close to incentive communities are 34% (-12.1ppts, $p<0.05$) less likely to have married under the age of 18 compared to less socially conservative women who did not live close to an incentive community. We find no such effects on socially conservative women who lived close to incentive communities. Reports from in-laws are intriguingly consistent with untreated non-preferred types using the pretence of treatment (or at least the ambiguity over treatment) to justify their later marriage: 34% of in-laws of less socially conservative women living close to treatment communities incorrectly believed that their daughter-in-law had received the treatment – as opposed to 12% of in-laws of socially conservative women living close to treatment communities. Meanwhile, consistent with our theory, we find no heterogeneity in social conservatism among eligible women (see appendix table A.7).

Table 6: Market spillovers on child marriage (married<18) in non-incentive communities, by girl's social conservatism (SC)

	Parents' Survey		Young Women's Survey	
	Married<18		Married<18 (Age 15-17)	
	Age 15-17	Age 7-14	Low SC	High SC
	(1)	(2)	(3)	(4)
Close to incentive village	-0.029	-0.009	-0.121	0.092
	(0.016)	(0.015)	(0.061)	(0.094)
Control Mean	0.288	0.390	0.360	0.479
Observations	10544	40136	682	417
FE	Union	Union	Union	Union

Notes: The table shows results from weighted OLS regressions (see Buchmann et al. (2018)), adjusted for stratification and baseline characteristics (see notes to table 5), with Huber-White robust SEs clustered at the community level. "High Social Conservatism" is an indicator that is 1 if the woman has an above median social conservatism. "Close to incentive village" is an indicator that is 1 if the community is less than 500 meters away from the closest incentive community center. The regressions also control for distance to the closest community center and safe space to ensure that distance to closest incentive community is not simply proxying for urbanity or participation in the empowerment program (we control for both distances from the village center and from the household in the subsample for which we have geo-locations).

These cross-community spillovers on less socially conservative women provide strong empirical support for our signaling model of marriage delay and help distinguish it from alternatives. If the incentive merely compensated families for a utility loss from marriage delay, we would not anticipate a corresponding delay among those not receiving the incentive. If spillovers were driven by norms changes in the acceptability of later marriage driven by the direct program effects, we would see as large or larger an effect on younger women, since norms changes generally have a delayed impact on behavior. If child marriage had persisted

due to "unraveling" that leads all brides to marry early for fear of missing out on the highest quality husbands, then we should find spillovers on all women and not only less socially conservative women who lived close to incentive villages(note that in the presence of professional matchmakers and a fear of unraveling, information is likely to spread quite quickly throughout the marriage market). In addition, the presence of cross-community spillovers implies that the direct impacts of the program we measure in tables 2-3 are likely to substantially underestimate the full program impact on marriage and schooling, and thus the potential of a scaled up incentive policy to delay marriage.

7 Cost Effectiveness

As our model predicts, the program effects we estimate are high relative to the size of the conditional marriage incentives, suggesting that these incentives could be a highly cost-effective approach to reducing underage marriage in settings in which child marriage persists due to signaling motives. We show in a companion paper that the conditional incentive translates into 4.9 years of delayed marriage, 1.1 averted child marriages, and 3.7 years of schooling for every $1,000 invested by the implementer (Buchmann et al., 2021). We also calculate the benefits of delayed marriage based on the cumulative education wage premium by assuming that, absent the study, study participants would have started engaging in productive activity at age 17.6 (the mean marriage age in the control group among all women age 15-17 at program start) and continued until age 60.[53] This analysis suggests that the conditional incentive generated $1,010 in Net Present Value for every $1,000 spent (costs to implementer and beneficiary) – the highest impact among rigorously evaluated interventions affecting marriage age in a comprehensive cost-efficacy analysis. These estimates do not account for the effects on untreated girls discussed above, the importance of which would depend on how comprehensively the program is implemented. They therefore underestimate the program's full cost-effectiveness if replicated at anything less than full coverage.

However, while our experience was that the conditional incentive program was straightforward to implement and highly effective on a relatively small scale, scalability of the program depends on the feasibility of monitoring marriage status in a larger sample. While the risk of monitoring collusion may increase with scale, the rising rate of digitization of identification in Bangladesh and elsewhere will greatly reduce the costs of monitoring over time, making it more feasible to implement a similar program at scale. Moreover, the delivery costs will also fall as digital payment systems become more widespread.

[53] We assume that wage returns to education are constant across their working life and that the returns to years of secondary education are equal for women in and out of the workforce. We further assume that extra education delays girls' entry into the workforce, and that they begin working immediately after finishing their studies, provided they are older than the median age of marriage.

8 Conclusion

To understand why progress against child marriage in Bangladesh has been slow despite declines in fertility and increasing education and work opportunities, we randomized communities to a financial incentive to parents conditional on girls remaining unmarried and a traditional adolescent empowerment program. Our results demonstrate that a relatively small transfer not only significantly delays marriage for participating families, but also delays marriage for women who live nearby but were ineligible for the transfer. Meanwhile, the empowerment program failed to generate a reduction in adolescent marriage rates and imposed a marriage penalty despite increasing education prior to marriage. We develop a model of the marriage market in which women remain in a child marriage equilibrium because delaying marriage is seen as a signal of low adherence to traditional gender norms, which is undesirable to grooms. In such a setting, a small conditional transfer has the potential to generate a significant delay in marriage while an intervention that reduces women's adherence to traditional gender norms might actually increase early marriage. The model also predicts that untreated nonpreferred type women, but not untreated preferred type women, will delay marriage if they can credibly claim to have been treated, explaining the particular pattern of spillovers we find in our data.

The results provide novel evidence that child marriage is not a deeply-held preference that is hard to move in Bangladesh. Rather, the theoretical model and corresponding empirical results indicate that underage marriage and female school dropout are a consequence of adverse selection based on a hidden desirability type that is correlated with returns to education. Women more likely to adhere to conservative gender norms of behavior are both desirable in the marriage market and get relatively lower returns to education than less socially conservative women who are more likely to work after marriage. The results are *not* consistent with child marriage persisting as a result of a strong cultural preference for underage brides. Because age and physical appearance are both observable, a conditional transfer could not generate spillovers if the latter two mechanisms were at play.

This set of findings has important implications for policy surrounding child marriage. Child marriage driven by signaling is inefficient – everyone, including men and parents, would be happier with collective delay, but cannot coordinate on this in the status quo. Hence, although the steady rates of child marriage in Bangladesh over the past two decades might have suggested that a large cultural shift is needed, our work demonstrates that policies aimed at changing preferences may be misdirected. Instead, relatively modest economic incentives can be highly effective in reducing the number of underage brides. Small conditional transfers are a potential cost-effective policy approach to child marriage that can be implemented at any scale, which is important given that many governments have demonstrated limited political will to enforce legal mandates.

More generally, our results highlight the primacy of understanding the underlying determinants of child marriage in identifying the most cost-effective policy strategies to combat it.

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A.1 Appendix

A.1.1 Theory Appendix

A.1.1.1 Proof of Result 1: Liquidity Constraints Prevent Separating Equilibrium in Control Communities

Observe that we have **single-crossing**: for any dowries charged in t_1 and t_2, if the preferred type weakly prefers entering in t_2, then the non-preferred type strictly prefers entering in t_2 (because of higher marginal gains from education).

In order to screen, M has to exploit that Θ_L gets higher marginal gains from education, by charging a higher dowry for t_2 entrants, where the gap between the dowry for t_1 entrants and t_2 entrants exceeds $\mu(\Theta_H, E_H) - \mu(\Theta_H, E_L)$ but is less than $\mu(\Theta_L, E_H) - \mu(\Theta_L, E_L)$ (such that non-preferred type women would prefer to enter late while preferred type women would not).

The minimum dowry M can charge without violating his participation constraint in t_1 is $\omega_M - \mu(\Theta_H, E_L)$. Hence, $D|t_2 = \omega_M - \mu(\Theta_H, E_L) + \Delta$, where $\Delta \in (\mu(\Theta_H, E_H) - \mu(\Theta_H, E_L), \mu(\Theta_L, E_H) - \mu(\Theta_L, E_L))$. Thus, the minimum separating dowry for later entrants is $D|t_2 = \omega_M + \mu(\Theta_H, E_H) - 2\mu(\Theta_H, E_L)$.

Hence, if $\omega_M + \mu(\Theta_H, E_H) - 2\mu(\Theta_H, E_L) > Y$, then the cheapest separating equilibrium is not feasible at this $D|t_2$ (Θ_L is willing but unable to enter at t_2).

A.1.1.2 Proof of Result 2: The Unique Equilibrium in Control Communities is Pooling on t_1

A.1.1.2.1 Pooling on t_2 does not survive the Intuitive Criterion

As the separating equilibrium is not feasible, and there are no mixed-strategy equilibria (see online appendix OA.3.2), the candidate equilibria are "pooling on t_1" and "pooling on t_2".

First consider "pooling on t_2". If all women enter at t_2, then Bayes' rule implies that men must have beliefs $\Pr(\Theta = H|t_2) = f$. Since men compete for women, this implies that the dowry offered in t_2 is:

$$D|t_2 = \omega_M - [f\mu(\Theta_H, E_H) + (1-f)\mu(\Theta_L, E_H)]$$

Since entry at t_1 is a probability 0 event, Bayes' rule does not impose any restriction on the off-equilibrium beliefs $\Pr(\Theta = H|t_1) = \gamma \in [0, 1]$. It should be clear that if $\gamma = 0$ (to give an example), then this can sustain "all women enter at t_2" as a sequential equilibrium, since given these beliefs, the preferred type clearly prefers to get a higher utility and pay a lower dowry by entering at t_2:

$$\mu(\Theta_H, E_H) - [\omega_M - [f\mu(\Theta_H, E_H) + (1-f)\mu(\Theta_L, E_H)]] > \mu(\Theta_H, E_L) - [\omega_M - \mu(\Theta_L, E_L)]$$

By single-crossing, the non-preferred type also prefers to enter at t_2. So, we ask whether this equilibrium survives the Intuitive Criterion (IC).

Suppose Θ_H deviated to t_1 and received the most favorable treatment possible from M. That is, suppose she receives the lowest dowry possible, which is the dowry M would offer if he believed she were preferred type for sure:

$$D^{best}|t_1 = \omega_M - \mu(\Theta_H, E_L)$$

Then Θ_H prefers deviating and entering at t_1 over entering at t_2 iff:

$$\begin{aligned}\mu(\Theta_H, E_L) - [\omega_M - \mu(\Theta_H, E_L)] &> \mu(\Theta_H, E_H) \\ &\quad - [\omega_M - [f\mu(\Theta_H, E_H) + (1-f)\mu(\Theta_L, E_H)]] \Leftrightarrow \\ \mu(\Theta_H, E_L) - [f\mu(\Theta_H, E_H) + (1-f)\mu(\Theta_L, E_H)] &> \mu(\Theta_H, E_H) - \mu(\Theta_H, E_L)\end{aligned}$$

On the other hand, deviating to t_1 is equilibrium-dominated for Θ_L iff:

$$\begin{aligned}\mu(\Theta_L, E_L) - [\omega_M - \mu(\Theta_H, E_L)] &< \mu(\Theta_L, E_H) \\ &\quad - [\omega_M - [f\mu(\Theta_H, E_H) + (1-f)\mu(\Theta_L, E_H)]] \Leftrightarrow \\ \mu(\Theta_H, E_L) - [f\mu(\Theta_H, E_H) + (1-f)\mu(\Theta_L, E_H)] &< \mu(\Theta_L, E_H) - \mu(\Theta_L, E_L)\end{aligned}$$

Thus, "pooling on t_2" fails the IC iff:

$$\mu(\Theta_H, E_H) - \mu(\Theta_H, E_L) < \mu(\Theta_H, E_L) - [f\mu(\Theta_H, E_H) + (1-f)\mu(\Theta_L, E_H)] < \mu(\Theta_L, E_H) - \mu(\Theta_L, E_L)$$

This condition reflects that: (i) "type" is first-order in marriage, (ii) education has more marriage value-added for a non-preferred type than a preferred type, and (iii) the population fraction of preferred types is not too large.

If these features are true in the environment, then "pooling on t_2" fails the IC because the preferred type should be able to deviate to entering at t_1 and convince men that she is preferred type because a non-preferred type would still prefer to enter at t_2. Men would then offer her the best possible terms, which she would accept over delaying marriage and entering at t_2, as prescribed by the equilibrium.

A.1.1.2.2 Pooling on t_1 does Survive the Intuitive Criterion

Now consider "pooling on t_1". Then $\Pr(\Theta = H|t_1) = f$ by Bayes' rule, while the off-equilibrium belief $\Pr(\Theta = H|t_2)$ could be anything. M offers dowry in t_1:

$$D|t_1 = \omega_M - [f\mu(\Theta_H, E_L) + (1-f)\mu(\Theta_L, E_L)]$$

If $\Pr(\Theta = H|t_2) = 0$, then $D|t_2 = \omega_M - \mu(\Theta_L, E_H)$. Θ_H prefers entering at t_1 iff:

$$\mu(\Theta_H, E_L) - [\omega_M - [f\mu(\Theta_H, E_L) + (1-f)\mu(\Theta_L, E_L)]] > \mu(\Theta_H, E_H) - [\omega_M - \mu(\Theta_L, E_H)] \Leftrightarrow$$
$$[f\mu(\Theta_H, E_L) + (1-f)\mu(\Theta_L, E_L)] - \mu(\Theta_L, E_H) > \mu(\Theta_H, E_H) - \mu(\Theta_H, E_L)$$

which we have by assumption. (Intuitively, this is saying that type is first-order in marriage and thus preferred types do not gain much from education.)

This is a sequential equilibrium. But does it survive the intuitive criterion?

Suppose Θ_H deviated and entered at t_2. The best possible terms she could receive for this is the dowry that M offers if he believes she is preferred type for sure:

$$D|t_2 = \omega_M - \mu(\Theta_H, E_H)$$

Clearly, $D|t_2 < D|t_1$ since $\mu(\Theta_H, E_H) > \mu(\Theta_H, E_L) > \mu(\Theta_L, E_L)$. Then Θ_H clearly prefers to delay marriage, receive a higher marriage utility (she gets educated) and pay a lower dowry. Since we have shown the single-crossing property holds in our setting, Θ_L also strictly prefers deviating to t_2 at the best possible terms.

But then, since both types prefer to deviate under the best possible terms, deviation is not informative to men about type. Thus, "pooling on t_1" is the unique sequential equilibrium that survives the intuitive criterion in control communities.

A.1.1.3 Proof of Result 3: Liquidity Constraints Prevent Separating Equilibrium in Treatment Communities

Does easing liquidity constraints for some randomly chosen women, conditional on delaying marriage, enable a screening menu of dowries?

Note that there are now two dimensions of unobserved type: type and treatment. Since untreated Θ_L still cannot afford the higher dowry for entering later (under our original liquidity constraint), it's clear that the treatment does not enable men to induce all Θ_H to enter at t_1 and all Θ_L to enter at t_2.

Does the treatment enable men to achieve semi-separation by charging a $D|t_2$ that induces only treated Θ_L to enter at t_2? $D|t_1$ and $D|t_2$ would have to satisfy:

$$D|t_2 - D|t_1 < C + \mu(\Theta_L, E_H) - \mu(\Theta_L, E_L)$$
$$D|t_2 - D|t_1 > C + \mu(\Theta_H, E_H) - \mu(\Theta_H, E_L)$$

If treated non-preferred types entered at t_2 and everyone else entered at t_1, Bayes' rule dictates that $\Pr(\Theta = H|t_1) = \frac{f}{f+(1-\tau)(1-f)} \equiv f'$, where $f' > f$. Then to satisfy his participation constraint, the minimum dowry M charges is $D|t_1 = \omega_M - (f'\mu(\Theta_H, E_L) + (1-f')\mu(\Theta_L, E_L))$.

But:

$$\begin{aligned} D|t_2 &= \omega_M - (f'\mu(\Theta_H, E_L) + (1-f')\mu(\Theta_L, E_L)) + C + \mu(\Theta_H, E_H) - \mu(\Theta_H, E_L) \\ &> \omega_M - 2\mu(\Theta_H, E_L) + \mu(\Theta_H, E_H) + C \\ &> Y + C. \end{aligned}$$

So, the treatment does not make the separating contract feasible–although some women have been enriched, the very fact that this enrichment is random in type means that there is no way to separate treated preferred types from treated non-preferred types, for the same reason separation is not feasible in control communities. A separating contract would have to fully extract C and charge a rent on top of that for those who delay marriage, where this rent is the same as in the analysis for control communities, and therefore unaffordable.

A.1.1.4 Proof of Result 4: Equilibria in Treatment Communities

A.1.1.4.1 Pooling on t_2 does not Survive the IC

Because men compete for women, the transfer given to treated women is not extracted from them. Thus, if all women enter at t_2, the dowry that men charge is the same as the dowry they would charge if they thought all women entered at t_2 in the absence of any treatment. Clearly, this is still a sequential equilibrium. By the same reasoning as in the control section above, the untreated preferred type prefers to deviate at t_1 under the best possible terms than to abide by the equilibrium and enter at t_2. That is, she can make a credible speech to men that she is a preferred type if she enters at t_1. Hence, this sequential equilibrium continues to fail the IC.

A.1.1.4.2 Pooling on t_1 Survives the IC

We showed that this sequential equilibrium survives the IC in the control setting. It continues to survive the IC in the treatment setting, since the treatment strengthens the treated women's preference to enter at t_2 and doesn't change anything else. Thus, since untreated non-preferred and preferred types would rather deviate to t_2 at the best possible terms in the control, the untreated and treated non-preferred and preferred types would rather deviate to t_2 at the best possible terms in the treatment. Therefore, this is still an equilibrium. However, we will show that the treatment creates a new, semi-separating equilibrium, in which every agent's welfare is higher.

A.1.1.4.3 There is a Semi-Separating Equilibrium, and it Pareto Dominates the Pooling Equilibrium

There is no separating equilibrium in the control communities because there were only two types of women, non-preferred and preferred type, and the only way for men to generate

separation would be to charge a high enough dowry such that non-preferred types prefer to pay the higher dowry and delay marriage (because they get relatively large gains from further education), while preferred types prefer to enter early and pay the smaller dowry (because unobservable type is first-order and they do not have as much value-added from education, relative to non-preferred types). Because we do not observe separation in our setting, we infer that the extra dowry that men would need to charge for women who delay marriage is too much for non-preferred types to afford.

There is no semi-separating equilibrium in the control communities because again, there are only two types of women, and due to single-crossing, only one type (the preferred type) could possibly be mixing. We showed that there is no lower dowry $D|t_1$ that M can charge that simultaneously keeps preferred types indifferent between entering at t_1 and t_2, and satisfies M's participation constraint.

Without assuming anything different about liquidity constraints, the treatment makes a semi-separating equilibrium possible because it generates a second dimension of type, and in particular, this second dimension is orthogonal to type. Now, we have treated preferred, treated non-preferred, untreated preferred, and untreated non-preferred types, where crucially treatment is not informative about the unobservable type.

Consider the following equilibrium candidate: untreated preferred types enter at t_1, and everyone else enters at t_2.

Then Bayes' rule implies that M have the following beliefs:

$$\begin{aligned}
&\Pr(\Theta = H|t_1) = 1 \\
&\Pr(\Theta = H|t_2) = \frac{\tau f}{\tau f + (1-f)} \equiv f'
\end{aligned}$$

Note that $f' \to_{\tau \to 1} f$. Thus, dowries charged are:

$$\begin{aligned}
D|t_2 &= \omega_M - f'\mu(\Theta_H, E_H) - (1-f')\mu(\Theta_L, E_H) \\
D|t_1 &= \omega_M - \mu(\Theta_H, E_L)
\end{aligned}$$

Treated preferred types prefer t_2 to t_1 if and only if (Condition 1):

$$\mu(\Theta_H, E_H) - [\omega_M - f'\mu(\Theta_H, E_H) - (1-f')\mu(\Theta_L, E_H)] + C > \mu(\Theta_H, E_L) - [\omega_M - \mu(\Theta_H, E_L)]$$

If treated preferred types prefer t_2 to t_1, then so do treated non-preferred types.

Untreated non-preferred types prefer t_2 to t_1 if and only if (Condition 2):

$$\mu(\Theta_L, E_H) - [\omega_M - f'\mu(\Theta_H, E_H) - (1-f')\mu(\Theta_L, E_H)] > \mu(\Theta_L, E_L) - [\omega_M - \mu(\Theta_H, E_L)]$$

Untreated preferred types prefer t_1 to t_2 if and only if (Condition 3):

$$\mu(\Theta_H, E_L) - [\omega_M - \mu(\Theta_H, E_L)] > \mu(\Theta_H, E_H) - [\omega_M - f'\mu(\Theta_H, E_H) - (1 - f')\mu(\Theta_L, E_H)]$$

Condition 3 holds by assumptions (we showed in the control analysis that preferred types prefer t_1 under the belief that t_1 entrant is preferred type for sure over t_2 under the belief that t_2 entrant is preferred type with probability f. Here, the belief that t_2 entrant is preferred type is $f' < f$, so Condition 3 holds as an implication).

Condition 2 is the least likely to hold for $f' = 0$, in which case it holds by the substitutes condition 2.

Condition 1 holds by assumption as long as the transfer is non-trivial.

A key observation is that the treatment's impact is all about the common knowledge that treated preferred types now have a stronger incentive to delay marriage and not at all about women getting richer.

Recall that the dowry charged in the "pooling on t_1" case is $\omega_M - f\mu(\Theta_H, E_L) - (1 - f)\mu(\Theta_L, E_L)$. Note that *every* woman is strictly better off in this semi-separating equilibrium, and no man is worse off (since men compete for women, they always get their outside option ω_M in every equilibrium).

Untreated preferred types are strictly better off, because they still enter at t_1 and are uneducated, but pay a strictly lower dowry than they do in the "pooling on t_1" equilibrium, because now men believe they are preferred type for sure. Everyone else is strictly better off because they do even better by entering at t_2 than entering at t_1 and paying that strictly lower dowry. So, even though "pooling on t_1" continues to be a sequential equilibrium that survives the IC, this semi-separating equilibrium is also a sequential equilibrium, and **Pareto dominates**.

A.1.1.5 Proof of Corollary 2: Changing Distribution of Bride Types

From result 2, we know that, if liquidity constraints preclude a separating equilibrium, then pooling on child marriage happens when:

$$\begin{aligned}\mu(\Theta_H, E_H) - \mu(\Theta_H, E_L) &< \mu(\Theta_H, E_L) - \mu(\Theta_L, E_H) - f(\mu(\Theta_H, E_H) - \mu(\Theta_L, E_H)) \\ &< \mu(\Theta_L, E_H) - \mu(\Theta_L, E_L).\end{aligned}$$

The left-hand side is more likely to hold as f decreases, since $\mu(\Theta_H, E_H) - \mu(\Theta_L, E_H) > 0$. The right-hand side always holds, since it holds for $f = 0$ by condition 2 of our model:

$$\mu(\Theta_H, E_H) - \mu(\Theta_H, E_L) < \mu(\Theta_H, E_L) - \mu(\Theta_L, E_H) < \mu(\Theta_L, E_H) - \mu(\Theta_L, E_L).$$

Hence, pooling on early marriage and low education grows more likely as f decreases.

A.1.2 Empirical Appendix

A.1.2.1 Baseline Balance

Table A.1: Baseline characteristics in the parents' survey, women age 15-17 at program start

	Empowerment			Incentive			Empow.+Incen.			Control		Total	
Married & Unmarried at Baseline													
N	8,739			4,176			4,503			8,990		26,408	
	Mean	S.D.	Diff.	Mean	S.D.	Diff.	Mean	S.D.	Diff.	Mean	S.D.	Mean	S.D.
Ever Married (%)	8.5	28.0	-0.1	9.4	29.1	0.7	8.8	28.4	0.2	8.7	28.1	8.8	28.3
Still in-school (%)	60.2	48.9	-0.9	59.2	49.2	-2.0	60.2	48.9	-0.9	61.2	48.7	60.4	48.9
Highest Class Passed	6.2	2.6	0.0	6.1	2.7	-0.1	6.3	2.7	0.1	6.2	2.7	6.2	2.6
Unmarried at Baseline													
N	7,992			3,785			4,106			8,212		24,095	
	Mean	S.D.	Diff.	Mean	S.D.	Diff.	Mean	S.D.	Diff.	Mean	S.D.	Mean	S.D.
Still in-school (%)	64.6	47.8	-1.2	64.1	48.0	-1.7	65.1	47.7	-0.8	65.8	47.4	65.0	47.7
Highest Class Passed	6.4	2.6	0.0	6.3	2.6	-0.1	6.4	2.6	0.1	6.4	2.6	6.4	2.6
Age	14.9	0.8	-0.0	14.9	0.8	0.0	14.9	0.8	-0.0	14.9	0.8	14.9	0.8
Father Education (0-17)	4.1	4.4	0.2	3.8	4.1	-0.1	4.0	4.2	0.0	3.9	4.2	4.0	4.2
Mother Education (0-17)	3.2	3.3	0.1	3.0	3.3	-0.1	3.0	3.1	-0.0	3.1	3.3	3.1	3.3
HH Size (members)	6.0	1.9	0.0	6.1	2.0	0.1	6.0	2.0	0.0	6.0	2.0	6.0	2.0
Unmarried older sister in HH (%)	18.9	39.1	0.3	18.0	38.5	-0.5	18.0	38.4	-0.5	18.5	38.9	18.5	38.8
Community Boys/Girls Ratio	1.9	0.7	-0.1	2.0	0.8	0.0	1.9	0.8	-0.1	2.0	0.9	1.9	0.8
Community size (girls age 10 to 19)	265.9	121.3	-9.2	251.2	119.4	-23.9	261.3	118.6	-13.7	275.1	126.2	265.9	122.5

Notes: The table shows baseline characteristics by treatment arm of women age 15-17 at program start. The differences are calculated from OLS regressions with modified Huber-White SEs clustered at the community level.

A.1.2.2 Attrition

Table A.2: Outcome: Attritted, unmarried women age 15-17 at program start (excluding prefill errors and washed-out households)

	(1)	(2)
Empowerment	-0.019	-0.019
	(0.012)	(0.012)
Incentive	-0.011	-0.011
	(0.014)	(0.014)
Incen.+Empow.	-0.021	-0.021
	(0.015)	(0.015)
In school (BL)		-0.030
		(0.006)
Mother's education (BL)		0.003
		(0.001)
Unmarried older sister		-0.008
		(0.006)
Village remote		0.001
		(0.010)
Age (BL)		-0.002
		(0.003)
Control Mean	0.152	0.152
Observations	21749	21749

Notes: The table shows results from OLS regressions with modified Huber-White SEs clustered at the community level. The sample includes women age 15-17 at program start and unmarried at baseline and excludes women for which tracking data was lost at baseline as well as washed-out households.

A.1.2.3 Heterogeneity

Table A.3: Marriage and education outcomes, unmarried women age 15-17 at program start, by whether woman was in or out of school at baseline

	Out of school						In school					
	Married<18		Still in school		Last class passed		Married<18		Still in school		Last class passed	
	Age 15-17	Age 15	Age 15-17	Age 15	Age 15-17	Age 15	Age 15-17	Age 15	Age 15-17	Age 15	Age 15-17	Age 15
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Empowerment	-0.036	-0.018	0.025	-0.010	0.479	0.435	-0.029	-0.012	0.013	0.022	0.240	0.369
	(0.029)	(0.053)	(0.037)	(0.055)	(0.316)	(0.444)	(0.018)	(0.031)	(0.023)	(0.034)	(0.160)	(0.248)
Incentive	-0.095	-0.095	0.080	0.169	0.594	0.781	-0.039	-0.055	0.013	-0.001	0.214	0.339
	(0.034)	(0.057)	(0.042)	(0.067)	(0.371)	(0.584)	(0.025)	(0.041)	(0.031)	(0.040)	(0.259)	(0.354)
Incen.*Empow.	0.127	0.123	-0.034	-0.034	-0.186	-0.511	0.006	-0.004	-0.031	-0.035	-0.105	-0.526
	(0.049)	(0.082)	(0.066)	(0.088)	(0.513)	(0.729)	(0.037)	(0.059)	(0.042)	(0.058)	(0.345)	(0.455)
Control Mean	0.344	0.474	0.183	0.171	10.206	10.000	0.339	0.411	0.289	0.301	11.198	10.594
Observations	1212	528	852	399	847	395	2998	1324	2152	1036	2135	1028
FE	Union	Union	Union	Union	Union	Union	Union	Union	Union	Union	Union	Union

Notes: The table shows results from OLS regressions, adjusted for baseline characteristics and stratification (see notes to table 2), with Huber-White robust SEs clustered at the community level.

Table A.4: Marriage and education outcomes, unmarried women age 15-17 at program start, by whether father was working full-time at baseline

	Not full-time						Full-time					
	Married<18		Still in school		Last class passed		Married<18		Still in school		Last class passed	
	Age 15-17	Age 15	Age 15-17	Age 15	Age 15-17	Age 15	Age 15-17	Age 15	Age 15-17	Age 15	Age 15-17	Age 15
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Empowerment	-0.036	-0.018	0.025	-0.010	0.479	0.435	-0.029	-0.012	0.013	0.022	0.240	0.369
	(0.029)	(0.053)	(0.037)	(0.055)	(0.316)	(0.444)	(0.018)	(0.031)	(0.023)	(0.034)	(0.160)	(0.248)
Incentive	-0.095	-0.095	0.080	0.169	0.594	0.781	-0.039	-0.055	0.013	-0.001	0.214	0.339
	(0.034)	(0.057)	(0.042)	(0.067)	(0.371)	(0.584)	(0.025)	(0.041)	(0.031)	(0.040)	(0.259)	(0.354)
Incen.*Empow.	0.127	0.123	-0.034	-0.034	-0.186	-0.511	0.006	-0.004	-0.031	-0.035	-0.105	-0.526
	(0.049)	(0.082)	(0.066)	(0.088)	(0.513)	(0.729)	(0.037)	(0.059)	(0.042)	(0.058)	(0.345)	(0.455)
Control Mean	0.34351	0.47399	0.18345	0.17054	10.20578	10.00000	0.33852	0.41129	0.28894	0.30052	11.19772	10.59424
Observations	1212	528	852	399	847	395	2998	1324	2152	1036	2135	1028
FE	Union	Union	Union	Union	Union	Union	Union	Union	Union	Union	Union	Union

Notes: The table shows results from OLS regressions, adjusted for baseline characteristics and stratification (see notes to table 2), with Huber-White robust SEs clustered at the community level.

A.1.3 Treatment Effects on Girls' Social Conservatism

Table A.5: Social Conservatism Index, unmarried girls age 10-17 at program start

	Age 10-17	Age 15-17
	(1)	(2)
Empowerment	-0.051	-0.026
	(0.018)	(0.029)
Incentive	0.004	0.028
	(0.022)	(0.036)
Incen.*Empow.	0.060	0.028
	(0.031)	(0.046)
Control Mean	0.000	0.000
Observations	5205	1933
FE	Union	Union

Notes: The table shows results from OLS regressions, adjusted for baseline characteristics and stratification (see notes to table 5), with Huber-White robust SEs clustered at the community level in parentheses. The "girls' social conservatism index" is a Kling mean effects index of whether the girl believes wives should be less educated than men, girls should be allowed to wear what they want (entering negatively), boys should be given more education resources than men, she stops activities when she menstruates, and the highest age the girl finds acceptable for marriage (entering the index negatively).

Table A.6: Marriage and childbearing outcomes, unmarried girls age 15-17 at program start in the young women's subsample survey

	Married<18		Married<16	Ever married		Marriage age		Birth<20	
	Age 15-17	Age 15	Age 15	Age 15-17	Age 15	Age 15-17	Age 15	Age 15-17	Age 15
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Empowerment	0.041	0.055	0.001	-0.013	-0.036	-0.291	-0.415	0.035	0.037
	(0.028)	(0.049)	(0.036)	(0.023)	(0.033)	(0.114)	(0.201)	(0.025)	(0.044)
Incentive	-0.048	-0.119	-0.092	0.009	-0.041	0.074	0.312	-0.049	-0.146
	(0.037)	(0.060)	(0.042)	(0.025)	(0.040)	(0.148)	(0.241)	(0.032)	(0.050)
Incen.*Empow.	0.042	0.034	0.025	0.007	0.091	0.128	0.314	0.012	0.036
	(0.051)	(0.086)	(0.058)	(0.036)	(0.058)	(0.208)	(0.342)	(0.042)	(0.067)
Control Mean	0.379	0.478	0.155	0.847	0.834	18.175	17.620	0.215	0.295
Observations	1737	755	755	1742	757	1464	629	1742	757
FE	Union	Union	Union	Union	Union	Union	Union	Union	Union

Notes: The table shows results from weighted OLS regressions (see Buchmann et al. (2018)), adjusted for baseline characteristics and stratification (the girl's age, her bmi and an indicator for whether she is stunted (proxies for baseline health), her school enrollment and education, her desired marriage age and ideal age at first birth (proxies for her marriage and childbearing preferences), household size and assets and whether her mother is schooled (proxies for per capita wealth), the presence of an older unmarried sister in the household, whether the community is accessible via public transport and distance to closest motorable road (proxies for remoteness), the ratio of unmarried adult boys to unmarried adult girls in the community (a proxy for marriage market conditions) and the distance of the household to the closest incentive community (to control for signaling concerns in non-incentive households), with Huber-White robust SEs clustered at the community level.

A.1.4 Treatment Effect Heterogeneity by Girls' Social Conservatism

Table A.7: Child marriage, unmarried girls age 15-17 at program start in the young women's subsample survey, by girl's social conservatism (SC)

	Low SC		High SC	
	Age 15-17	Age 15	Age 15-17	Age 15
	(1)	(2)	(3)	(4)
Empowerment	0.038	0.002	0.080	0.135
	(0.038)	(0.065)	(0.049)	(0.075)
Incentive	-0.036	-0.009	-0.012	-0.120
	(0.044)	(0.075)	(0.065)	(0.097)
Incen.*Empow.	0.127	0.031	-0.139	-0.104
	(0.064)	(0.108)	(0.091)	(0.140)
Control Mean	0.331	0.444	0.453	0.523
Observations	1062	437	675	318
FE	Union	Union	Union	Union

Notes: The table shows results from weighted OLS regressions, adjusted for baseline characteristics and stratification (see notes to table 5), with Huber-White robust SEs clustered at the community level in parentheses. "High Social Conservatism" is an indicator that is 1 if the woman has an above median social conservatism.