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

By Nina Buchmann, Erica Field, Rachel Glennerster, Shahana Nazneen, and Xiao Yu Wang*

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 percent 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 types (socially conservative girls) have lower returns to delaying marriage. Consistent with our theoretical prediction, we observe substantial spillovers of the incentive on untreated nonpreferred types. (*JEL* C93, D91, J12, J13, J16, 012)

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 (Field and Ambrus 2008; Chari et al. 2017). 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

*Buchmann: Stanford University (email: nina.buchmann@stanford.edu); Field: Duke University and NBER (email: field.erica@duke.edu); Glennerster: University of Chicago and NBER (email: rglennerster@uchicago.edu); Nazneen: Bangladesh (email: shahanans@hotmail.com); Wang: Duke University, CRI Foundation, and NBER (email: xiaoyu@crifoundation.org). Rema Hanna was the coeditor for this article. The study was funded by the US National Institutes of Health (1P01HD061315), the International Initiative for Impact Evaluation and Global Development Network (OW.1.12), the Nike Foundation (04-203594 and 013691-001), the International Development Research Center, and GrOW (107821-001), a multifunder partnership with the UK Government's Department for International Development, the William and Flora Hewlett Foundation, and the International Development Research Center, Canada. We thank colleagues at Save the Children and Bangladesh Development Society who were an integral part of designing this evaluation, particularly Jahirul Alam Azad, Winnifride Mwebesa, Veronica Torres, Mary Beth Powers, Elizabeth Pearce, and Barbara Burroughs, as well as Shoraez Shahjahan (IPA) and Diva Dhar (JPAL). Invaluable research assistance was provided by Libby Abbott, Michael Duthie, Rahin Khandker, Elizabeth Linos, Parendi Mehta, Rudmila Rahman, Akshay Dixit, Rizwana Shahnaz, Pauline Shoemaker, Saffana Humaira, Mihir Bhaskar, Jintao Sun, Sara Pratico, Shakil Ayan, and Pulkit Aggarwal. All errors are our own. This project received IRB approval from MIT (0612002074), Harvard (00000109), and IPA (121.11June-007); and was preregistered in the AEA RCT registry with ID 0000204 (https://www.socialscienceregistry.org/trials/204) (Field and Glennerster 2017).

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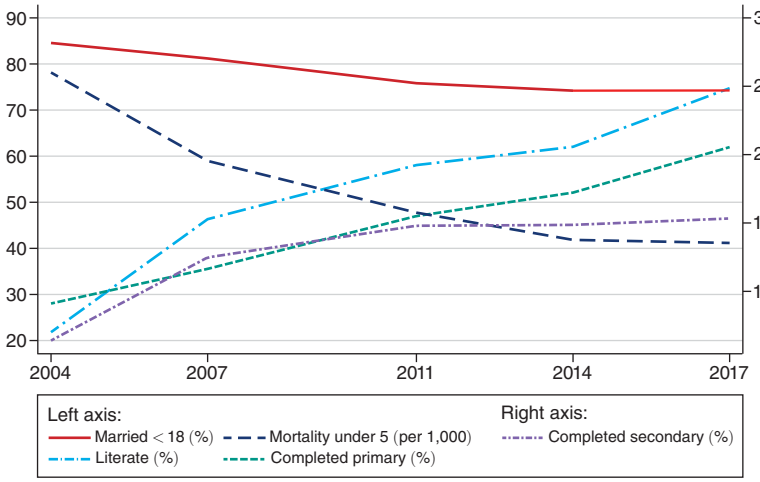

Figure 1. Education, Child Mortality, and Marriage Trends in Bangladesh

Notes: Data from 2004, 2007, 2011, 2014, and 2017 Bangladesh DHS. The samples include ever-married women aged 30–35 for "Married <18," "Completed primary," "Completed secondary," and "Literate." All lines except for "Completed secondary" follow the left y-axis scale. "Married <18" is the percentage of women who report a marriage age under 18. "Literate" is the percentage of women who can read and write in any language easily or with difficulty. "Mortality under 5" is the probability (expressed as a rate per 1,000 live births) of a child exposed in the 5 years preceding the survey year dying before reaching the age of 5.

families to make this choice. 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 percent of women age 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 (NIPORT, Mitra, and ICF International 2016). Not only has Bangladesh achieved gender parity in primary and secondary school enrollment, but poverty fell from 57 percent in 1991–1992 to 32 percent in 2010, and maternal mortality declined by 40 percent between 2001 and 2010. Wage opportunities for women, reproductive health, and child survival also increased steadily over this period (NIPORT, Mitra, and ICF International 2016).

Bangladesh's success in achieving many gender goals, alongside widespread awareness of 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 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).

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 2 years or until they reached the age of consent (18). 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 Save the Children's prototypical adolescent empowerment program, a more standard policy instrument designed to discourage child marriage by promoting more progressive gender norms among adolescent girls. One group of communities received both programs. The setup allows us to directly compare the impact on marriage of a financial incentive and a standard empowerment program in the same context as well as test for complementarities between the programs.

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 2 years are 19 percent less likely to have married under age 18 and 18 percent 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. There is no evidence of complementarities between the programs.

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. 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)—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. We would also not expect to find spillovers onto girls who do not receive the incentive, which we do. In other words, our results indicate that affordability is not the constraint on delaying a girl's marriage age. 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.[1]

Our results also cannot be explained by an "unraveling" story in which, in the absence of the incentive, brides marry early for fear of missing out on high-quality husbands, but a few brides deciding to delay (because of the incentive) prevents the unraveling. First, this mechanism works at the level of the marriage market, which in our setting is large and diffuse—90 percent of girls marry outside the village and 38 percent outside the union, which includes roughly 10 villages and is the geographic strata in our experiment. It would therefore equally impact both treatment and control communities within the same union, whereas our results indicate large treatment impacts across villages within the same union. While we do find treatment spillovers, these extend only to households in villages within 500 meters of an incentive village. Hence, they are unlikely to be the result of a general equilibrium unraveling mechanism. Moreover, our spillover effects are only observed among less conservative girls, whereas unraveling implies market-wide effects since our incentive moves marriage outcomes for girls of all types. It is also unclear how an unraveling story is compatible with optimizing grooms: moving earlier to secure a good groom implies grooms are indifferent as to when they marry and do not respond to the "fear of missing out" by increasing dowry rather than marrying younger brides and marrying younger themselves. Finally, unraveling 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 an expansive geographic area and age range.

To explain our results, we propose an explanation for child marriage that is consistent with our findings: delaying marriage is a negative signal of an unobserved bride type that is correlated with returns to education. 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 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

[1] In control communities, 44 percent of households with more than one daughter chose underage marriage for only some of their daughters.

conservative women ("nonpreferred" 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 nonpreferred type. Signaling can therefore 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 delayed.

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.

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 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 nonpreferred 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 41 percent 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 empirical 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 nonsignaling 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.[2] 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 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

[2] Consistently, men in our sample reported education to be the third-most desired characteristic in a bride, below character and looks.

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.[3]

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 nonpreferred 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.

Our paper fits into a growing body of literature documenting how gender norms constrain economic outcomes for women (Fernández, Fogil, and Olivetti 2004; Bertrand, Kamenica, and Pan 2015; Field et al. 2021; Bursztyn, González, and Yanagizawa-Drott 2020). Marriage markets play a key role in this literature, with evidence of a marriage market penalty for economically successful and/or educated women (Brown and Lewis 2004; Greitemeyer 2007; Folke and Rickne 2020; Bertrand et al. 2021) and evidence that women will take a costly action to improve their marriage market outcomes (Bursztyn, Fujiwara, and Pallais 2017; Khalifa 2022). We expand on this literature by considering how norms of female behavior can hinder women's outcomes through costly marriage market signaling even when female economic achievement is understood to be desired by all. In this manner, the paper draws attention to the possibility that signaling considerations may result in female outcomes that are not only bad for women but also socially inefficient.

A subset of the literature on which we build focuses on explaining child marriage as a signaling outcome, including several sociological and anthropological studies that provide qualitative empirical evidence that, in South Asia, early marriage acts as a signal of a bride's obedience (Ortner 1978; Dyson and Moore 1983; Kandiyoti 1988; Heaton 1996; Moghadam 2004). In formalizing the underlying signaling model, our paper is closest to and builds on the work of Wahhaj (2018), who develops a related model to explain why early marriage might happen even when there is no inherent preference for young brides. Both models share the key feature that early marriage is desirable because being unmarried at a later age is a signal of being an undesirable type, such that preferred types have a signaling incentive to enter the market early.

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

There are three key distinctions between our study and Wahhaj (2018). First, we provide rigorous empirical evidence on this general class of signaling models by designing an RCT that tests whether providing an incentive that is independent of type leads to market-wide delays in marriage. Second, while similar in flavor, there are important differences between the two stories that give rise to very different policy predictions. In Wahhaj (2018), the quality of the bride pool is worse at later ages because bride type is sometimes revealed before marriage, leading some nonpreferred types to reenter the market when older. In contrast, age acts as a negative signal of bride quality in our model for a very different reason—because preferred types have lower returns to education and hence gain less from delaying marriage. In considering how desirable qualities in a wife might correlate with her incentives to invest in human capital, our variation of the signaling model illustrates a potential tension inherent in decisions to invest in female schooling in marriage markets that value female submissiveness. In doing so, our model is distinct in offering a potential explanation for why two widespread policy approaches to discouraging child marriage—schooling opportunities for girls and adolescent empowerment programs—have yielded minimal gains in South Asia. Similarly, only in our model does a conditional financial incentive to delay marriage succeed in disrupting the signal that drives all types toward younger marriage, a prediction that we validate in our field experiment.[4] Third, in Wahhaj (2018), early marriage is socially efficient, whereas in our model there is a government incentive to intervene.

In the policy realm, our paper contributes to the literature evaluating the use of financial incentives to reduce adolescent marriage. In contexts without dowry and where girls have agency over marriage, there is good evidence that transfers conditional on staying in school can delay marriage or childbearing (Alam, Baez, and Del Carpio 2011; Duflo, Dupas, and Kremer 2015; and Austrian et al. 2022, with Baird, McIntosh, and Özler 2011 a notable exception). The (all nonexperimental) evidence from contexts with dowry and arranged marriage is mixed: Alam, Baez, and Del Carpio (2011) and Hong and Sarr (2012) in Pakistan and Hahn et al. (2015) in Bangladesh find that transfers conditional on education and marriage reduce child marriage, while Heath and Mobarak (2015) find no impact of the same Bangladesh program using a different comparison group. The only policy effort to condition financial incentives on marriage age alone was the Apni Beti Apna Dhan (ABAD) program in Haryana, India. Sinha and Yoong (2009) find nonexperimental evidence of positive effects of ABAD on education but do not examine its impact on marriage.[5]

We contribute to this literature by producing rigorous experimental evidence that cash transfers can delay marriage when conditioned on marriage age alone, which also means that conditional transfers have the potential to influence marriage timing for girls who are unconditional school leavers, the subset most vulnerable to child

[4] In our model, unlike that in Wahhaj (2018), early marriage can persist even when the cost of schooling falls because it is known that undesirable types are more likely to take up schooling opportunities. Similarly, because the key bride type that women seek to signal is submissiveness, "empowering" young women has the potential to exert positive pressure on adolescent marriage by changing the distribution of types.

[5] Krishnan et al. (2014) examine marriage effects of ABAD by comparing the age of marriage of brides from Haryana with those from out of state. However, in-state and out-of-state marriages are likely to differ in many dimensions.

marriage. Our study further adds to our understanding of conditional transfer programs by demonstrating evidence of a key mechanism through which they influence marriage timing and are made more efficient: by disrupting the ability of early marriage to signal positive bride attributes.

The rest of the paper proceeds as follows. Section I describes the marriage practices in our setting. Section II describes the experiment, and Section III presents the results. Section IV introduces a signaling model of marriage timing to explain the observed patterns. Section V presents additional empirical tests generated by the model. Finally, Section VI discusses the cost-effectiveness of the conditional incentive program, and Section VII concludes.

I. Setting

Our study takes place in rural Bangladesh in communities that are overwhelmingly Muslim.[6] 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 percent of women in our control group said they could discuss groom choice or timing with their father, and only 10 percent with their mother.[7] Marriages are contracts between families, and most (89 percent) are arranged by third-party matchmakers. Moreover, the matchmaking industry is relatively competitive: in our study sample, the vast majority of rural communities (95 percent) have a professional matchmaker who lives in the community, and 70 percent have 3 or more (online Appendix OA.1.2).

While dowry was outlawed in Bangladesh in 1980, most marriages (85 percent in our sample) continue to involve dowry in the form of a prenegotiated transfer 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 US$1,087. 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). Survey responses to hypothetical vignette questions from matchmakers in our study area confirm that the family of a bride with attractive characteristics such as more years of schooling pay significantly lower dowry (Buchmann, Field, and Glennerster n. d.).

Every Muslim marriage contract also specifies a *denmeher*, which in Bangladesh is a legally binding amount of money to be transferred to the wife in the event of divorce, much in the style of a Western prenuptial agreement (Ambrus, Field, and Torero 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, Field, and Glennerster n. d). Of those marriages in our sample reporting denmeher (over 99 percent), the mean amount is US$2,349, with a standard deviation of US$1,062.[8]

[6] A map of the study region can be found in online Appendix OA.1.1.

[7] Only 51 percent of women in our sample report meeting their husband before the day of marriage.

[8] 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, Field, and Torero (2010) for more details on Bangladeshi marriage contracts.

The average age of marriage for women in our study setting is 18.4, and 41 percent of women marry under age 18 and 20 percent under age 16.[9] Husbands are on average 6 years older than their wives, and 90 percent of married women were married to a husband from outside of their community and 38 percent from outside of the union. 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 the actual marriage age of the same girls, and survey data indicate that both parents and daughters understand that early marriage has health costs.[10] Parents report that both financial (78 percent) and social (21 percent) pressure influenced daughters' marriage timing, which might explain the discrepancy. Overall, 43 percent of girls reported that a girl should not marry late because of reputation concerns and 31 percent 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 for being submissive and obedient (Hamid, Johansson, and Rubenson 2010), docile (Goody 1990), and protecting the family's reputation above all else (Ortner 1978). Correspondingly, in a subsample survey of 328 husbands of women in our sample, 50 percent reported either "nature," "character," "reputation," or "religion and tradition" as one of the two most desirable characteristics in a bride (Figure 2). Only two husbands reported age as one of the two most important characteristics, 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: 59 percent of women reported income as the most desirable characteristic in a husband.

II. A Child Marriage Policy Experiment

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 and an adolescent empowerment program would reduce child marriage.

A. *Experimental Design*

The study was carried out in six subdistricts (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. The conditional incentive program that we evaluate

[9] 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.

[10] Only 5 percent 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 percent said because a girl would not be physically ready for marriage. In the control group at endline, 90 percent of women correctly identified at least one health risk associated with early pregnancies.

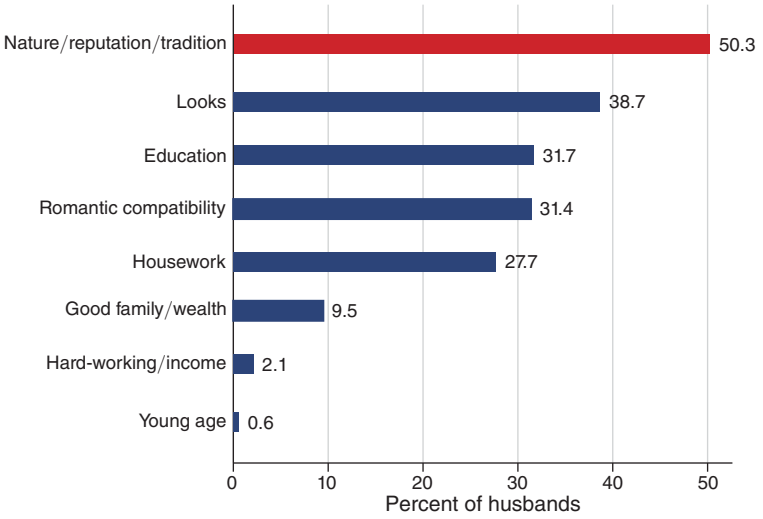

Figure 2. Characteristics Desired in a Bride

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."

used the distribution infrastructure of this existing program, which operated in all treatment and control communities in our study. To determine which communities were included in the study, we conducted a census of all households with adolescent girls in all 610 communities between January and February 2007.[11] Communities were excluded from our study if they were too remote for distribution or had fewer than 40 or more than 490 adolescent girls, leaving 460 eligible communities in 5 subdistricts. Sample communities were semirural to rural, with an average of 351 households per community, and 16 percent were more than 1 hour away from the closest motorable road. About half had a primary school in 2007, while 25 percent had a secondary school (see online Appendix OA.1.2 for full community statistics).

Using a stratified cross-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 designed our study to test for complementarities because of the common perception that, as girls face multiple barriers to avoiding child marriage, it is important to address multiple barriers simultaneously. We describe these interventions in detail in the following two sections. We stratified by union, an administrative grouping of roughly ten communities, and, within

[11] Eligibility for both programs was based on the age reported in this survey, thus minimizing the incentive for misreporting.

union, by community size (the randomization procedure is described in detail in online Appendix OA.1.3).[12]

B. *Conditional Incentive Program*

All girls in conditional incentive communities unmarried and age 15–17 at program start (age 14–16 in the baseline census) were issued ration cards indicating their eligibility to receive cooking oil every 4 months until they married or turned 18. Every four months, from April 2008 to August 2010, marital status was verified by Community Health Volunteers (CHVs) and/or independent monitors who conducted unannounced household visits to verify that the girl was still residing in her parents' home and interviewed family members, neighbors, marriage registrars, and community leaders about her marital status.[13] 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 US$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. Overall, 92 percent of cardholders, encompassing 5,734 unmarried adolescent girls, received the conditional incentive at least once, and the rate is approximately equivalent (93 percent) in communities assigned to both incentive and empowerment programs (online Appendix OA.1.4, Table OA.2). We compare girls who did and did not pick up the incentive on a number of observable baseline characteristics (online Appendix OA.1.4, Table OA.3). Girls who received the incentive at least once are more likely to be in school or to have an older unmarried sister, factors that increase the value of collecting the oil. They are also significantly younger, which suggests that older girls on the cusp of marriage may be forgoing the incentive. They also have more educated mothers, which suggests that parents' human capital may influence program take-up, as is true in other settings.

C. *Empowerment Program*

In communities randomized to receive the empowerment program, all girls age 10–19 at program start (age 9–18 in the baseline census) were invited to participate in the Kishoree Kontha (KK), or "Adolescent Girl's Voice" program for one 6-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.

[12] 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. We discuss this richer dataset in a companion paper.

[13] 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.

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 percent), financial literacy and encouragement to generate own income was added to the curricula. As earlier evaluations did not find any differences between the basic empowerment and empowerment + financial literacy programs, we pool both programs in this study.

Monitoring data show that Safe Spaces averaged 6 meetings, or 7.8 hours, per week and 41,347 girls, or 93 percent of girls in target communities, were reached (online Appendix OA.1.4, Table OA.2). We find higher enrollment in villages assigned to both empowerment and incentive programs (97 percent versus 91 percent). We compare reported KK members and nonmembers on a number of observable baseline characteristics (online Appendix OA.1.4, Table OA.3). KK members were significantly more likely to be in school and lived closer to the Safe Space centers.

D. *Study Timeline*

Our analysis relies on survey data from three waves of data collection and two different samples: the parents' sample and the young women's sample. Figure 3 presents the timeline of data collection (Buchmann et al. 2023).

The first wave of data collection, the 2007 census, which we used as a sample frame to select our 460 study communities, gathered data from a parent (head or spouse of head) on the marital status, age, and education of all household members in all study communities. Our main analysis sample includes all 24,095 unmarried girls listed in the census who were age 15–17 at program start, the age range eligible for the incentive treatment. Throughout the paper, we show results from this sample—the "parents' sample"—whenever available in order to maximize statistical power. Baseline characteristics were balanced across treatment arms in this sample (Appendix Table B1).

In addition, we randomly selected 20 households per community (10 in smaller communities) for detailed interviews with all girls in the household age 10–17 at program start. We present supplemental analyses of outcomes that could not be collected from parents using this much smaller subsample of 2,791 unmarried girls age 15–17 at program start, which we refer to as the "young women's sample." Baseline characteristics were also balanced across treatment arms in this subsample (Appendix Table B1).

We attempted to resurvey all respondents of both samples in two separate rounds of data collection conducted 1 and 4.5 years after program completion, in 2011

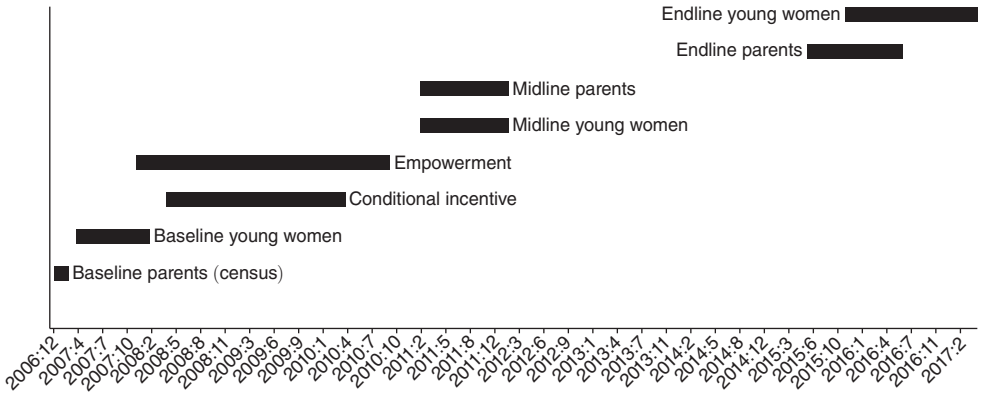

FIGURE 3. PROGRAM TIMELINE

Note: The figure shows the timeline of surveying and interventions.

and 2016–2017.[14] As this paper investigates the impact of the two interventions on child marriage, our analysis relies on outcome data collected 4.5 years after program completion, when substantially more women had entered into marriage and completed schooling. Because of these high rates of censoring on the primary outcomes in the second wave of data collection, 2011 data are only used at one point in the analysis, to investigate current enrollment as an alternative measure of schooling attainment. Current enrollment, which only provides meaningful variation shortly after program completion, is a potentially valuable outcome because it is arguably less subject to measurement error than reports of retrospective outcomes such as completed schooling.

There are three sources of missing data from the 2017 surveys (see consort diagram in online Appendix OA.1.5 for the parent's survey): First, we exclude 1,340 observations from the parents' survey that 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. Second, we exclude 1,006 observations from the parents' survey and 43 observations from the young women's survey who were living in households that were entirely displaced by cyclone damage. Third, 3,054 of the 21,749 women we attempted to follow up with from the parents' survey (14 percent attrition) and 333 of the 2,748 women we attempted to follow up with from the young women's survey (12 percent attrition) could not be tracked either because respondents could not be reached or refused to be surveyed.

Of those with endline data, we further exclude 3,119 observations from the parents' survey and 660 observations from the young women's survey due to marriage before the program start in January 2008 (15 percent) or incomplete outcome data (2 percent).[15] Our final analysis sample thus contains 15,576 women in the parents'

[14] In 2011, we dropped 1 of the 6 subdistricts from our sample (Muladi) because of rumors that emerged in that subdistrict that forced us to suspend data collection activities (2,061 girls in 84 communities excluded).

[15] CHVs informed communities about the program in January 2008—four months before the first distribution round.

Table 1—Sample Summary Statistics

Age at program start:	Girls age 15–17 (N = 15,576)		Girls age 15 (N = 5,871)	
	Mean	SD	Mean	SD
Age at endline	23.4	0.8	22.5	0.3
Ever married (percent)	84.1	36.6	82.1	38.3
Married <18 (percent)	27.7	44.8	36.6	48.2
Married <16 (percent)	4.2	20.1	11.0	31.3
Ever birth (percent)	62.9	48.3	59.5	49.1
Birth <20 (percent)	24.0	42.7	32.0	46.6
Dowry (2016 US$, conditional on married)	1,086.8	922.5	1,046.7	884.8
Arranged marriage (percent)	88.7	31.6	88.9	31.4
Age gap (husband-wife)	5.6	4.4	5.9	4.3
Husband from outside village (percent)	90.3	29.6	89.7	30.4
Still in school (percent)	21.6	41.1	24.0	42.7
Last class passed	9.8	4.3	9.7	4.1
Currently working (percent)	11.9	32.3	11.7	32.1

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

survey and 1,755 women in the young women's survey. Attrition from all sources is balanced across treatment arms (see online Appendix OA.1.6).[16]

E. *Endline Summary Statistics*

Table 1 presents summary statistics on women in our analysis sample from the 2017 endline survey. By age 22–25, 84 percent are married, including 28 percent married before the legal age of 18 and 4 percent before age 16. Moreover, by endline 63 percent of women in our sample have started childbearing, and 24 percent 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 percent were married under 18, and 20 percent were married under 16.

Parents in the endline survey were asked both current marital status and age of marriage if their daughter was reported to 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 by comparing, for a subsample of 1,222 young

[16] Online Appendix OA.1.6 shows that attrition from all sources, including data entry errors, cyclone damage, endline surveying, and marriage before program start, is balanced across treatment arms. There is higher attrition in the 2011 survey because of the rumors that spread in Muladi, which affects only the outcomes "In school at midline" and "Married at midline." 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. Survey operations in the young women's survey were never resumed.

women, parents' reports to young women's reports (online Appendix OA.1.7).[17, 18] Overall, marriage age reported by parents was largely consistent with that reported by women and indicated no significant treatment bias: on average, parents' reports were 1.5 months higher than young women's reports, and the difference was balanced across treatment arms.

Women in our sample have completed an average of 10 years of school at endline, and 12 percent are engaged in income-generating activities with a mean income of US$46 per month. Moreover, 22 percent were still in school at endline, of which 54 percent were married. At first blush, this figure seems incongruous in Bangladesh, where it is socially unacceptable for married adolescent girls to attend secondary school.[19] 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.

F. *Estimation Strategy*

We estimate the impact of the incentive and empowerment programs on girls' marriage and schooling outcomes. Although both programs—and, particularly, empowerment—may also influence other aspects of girls' long-run well-being, investigating broad-ranging adult outcomes is outside the scope of this paper. This is primarily because we have limited opportunity to observe such outcomes in our 2017 endline data, which were gathered from parents when most girls were newly married and many had yet to begin childbearing. Hence, this paper focuses exclusively on parents' marriage and schooling choices for their daughters, which we believe to be the relevant conceptual framework for child marriage in Bangladesh.[20]

For all outcomes, we employ the following empirical specification:

$$(1) \qquad Y_{icu} = \alpha + \beta_1 I_c + \beta_2 E_c + \beta_3 (I_c \times E_c) + \beta_4' \mathbf{X}_{ic} + \epsilon_{icu},$$

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

[17] 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.

[18] For a subsample of approximately 100 young women, we also collected marriage certificates and compared both the parents' report and the woman's report with the date on the marriage certificate. We do not find that either of the treatments is correlated with the probability of having a marriage certificate. However, marriage certificates appear to systematically overreport age of marriage, so they are not a particularly useful source of age data in this setting.

[19] 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 percent 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 and so is unlikely to pertain to women at endline who are age 22–25.

[20] Meanwhile, a companion paper will examine a more comprehensive set of long-term impacts of the empowerment program on broad-ranging outcomes related to women and their children's well-being collected in a third round of data collection in 2024. These include bargaining power in marriage, labor force participation, life satisfaction, parenting behavior, and children's outcomes.

the empowerment program. Since not every eligible girl in 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 were eligible for the incentive at the start of the program and take-up of both programs was extremely high (92 percent of eligible girls received the financial incentive, and 93 percent of eligible girls enrolled in the empowerment program; see online Appendix OA.1.4).

Our estimates include a vector of individual and community controls $\mathbf{X}_{ic}$ measured at baseline for strata (village population tercile and union fixed effects), age indicators, household size, the presence of an older unmarried sister in the household, school enrollment, mother's level of education, and whether the community is accessible via public transport (a proxy for remoteness). We also estimate a specification excluding controls other than strata fixed effects as a robustness check. Errors are clustered at the unit of randomization (community).

Since potential program impacts vary across girls depending on their age at enrollment, throughout, we compare effects on the whole sample (age 15–17) with effects on girls eligible to receive the incentive for at least 2 years (age 15 at distribution start). Note that the difference across these two samples cannot be interpreted as a simple dose response to the program since girls in the full sample differ from girls who were 15 at enrollment by more than just age at enrollment. In particular, because girls must be unmarried to participate, the younger sample includes girls who would have married at 15 and 16.

Comparing results across these two samples is nonetheless informative because it tells us how changing the age targeting alters the program's cost-efficacy. On the one hand, girls who receive the program beginning at 15 encompass a more vulnerable set of adolescents, yet delivering incentives to girls at ages 15–16 who would unconditionally marry above that age is wasteful in terms of program dollars. Meanwhile, delivering the program to girls 16–17 is substantially less expensive per girl but may have limited impact because it misses the most vulnerable. By comparing the two samples, we learn whether a lighter-touch program in which girls are encouraged to remain unmarried from ages 16–18 has the potential to be more cost-effective than targeting girls at a younger age for longer. In addition, to the extent that program effects are observed among older girls (16–17), pooling the sample increases our ability to detect effects that are statistically noisier or pertain to only a small subsample, such as spillovers onto close neighboring villages.

III. Results

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

A. *Direct Effects on Marriage Timing*

As shown in Table 2, the incentive reduced child marriage by 17 percent (-4.9 ppts, $p < 0.01$) overall and 19 percent (-7.4 ppts, $p < 0.01$) for women

Table 2—Marriage Outcomes, Women Unmarried at Program Start

	Married <18		Married <16	Married at midline		Marriage age		Birth <20	
	Age 15–17 (1)	Age 15 (2)	Age 15 (3)	Age 15–17 (4)	Age 15 (5)	Age 15–17 (6)	Age 15 (7)	Age 15–17 (8)	Age 15 (9)
Empowerment	−0.007	−0.005	0.006	0.011	0.009	0.012	0.003	0.006	0.005
	(0.008)	(0.015)	(0.009)	(0.011)	(0.017)	(0.040)	(0.065)	(0.007)	(0.013)
Incentive	−0.049	−0.074	−0.020	−0.025	−0.054	0.210	0.323	−0.016	−0.039
	(0.010)	(0.019)	(0.012)	(0.013)	(0.019)	(0.051)	(0.079)	(0.009)	(0.016)
Incen. × Empow.	0.019	0.028	−0.002	−0.011	0.007	−0.051	−0.090	−0.003	0.011
	(0.014)	(0.026)	(0.016)	(0.019)	(0.026)	(0.074)	(0.118)	(0.014)	(0.023)
Control mean	0.293	0.385	0.113	0.458	0.415	18.969	18.293	0.241	0.326
Observations	15,549	5,861	5,861	14,891	5,604	12,993	4,773	15,494	5,847
FE	Union	Union	Union	Union	Union	Union	Union	Union	Union
Age 15–17 versus 15									
Empowerment		0.865			0.869		0.858		0.986
Incentive		0.082			0.034		0.068		0.039
Incen. × Empow.		0.646			0.334		0.652		0.402

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 fixed effects, household size, the presence of an older unmarried sister in the household, school enrollment, mother's level of education, and whether the community is accessible via public transport (a proxy for remoteness). Columns 1–3 and columns 6–9 present results from the endline parents' survey, and columns 4 and 5 show results from the midline parents' survey. The sample includes all women age 15–17 and unmarried at program start. The sample excludes washed out 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). "Married at midline" is equal to one if ever married. The bottom three rows present *p*-values from cross-equation equality tests of the coefficients for girls age 15–17 and girls age 15 at program start for each of the interventions.

age 15 at distribution start who were eligible for the incentive for 2 years. The likelihood of being married under 16 fell by 18 percent (-2.0 ppts, $p < 0.10$) among women age 15 at distribution start.[21] These effects hold among both women who were and were not enrolled in school at baseline and among those with and without a mother who has received schooling (online Appendix OA.2.2). 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.

These patterns are also observed in the continuous measure of marriage age. As 16 percent of our sample is still unmarried (Table 1), our marriage age data are censored. However, since by endline marriage rates have converged between treatment arms to statistically indistinguishable levels (Appendix Table B4, columns 1 and 2), differences in marriage age among the married can be expected to capture an unbiased impact of the program on those who have married. Figure 4 shows the distribution of marriage age of women age 15–17 and unmarried at program start in control and incentive only communities, demonstrating a shift in marriages from the 2 years before 18 to the 4 years after 18. As shown in columns 6–7 of Appendix Table 2, the incentive increased average age of marriage by 2.5 months (0.21 years,

[21] Results excluding controls, including women married before program start, and correcting for potential program inclusion errors yield similar results (online Appendix Table OA.16)

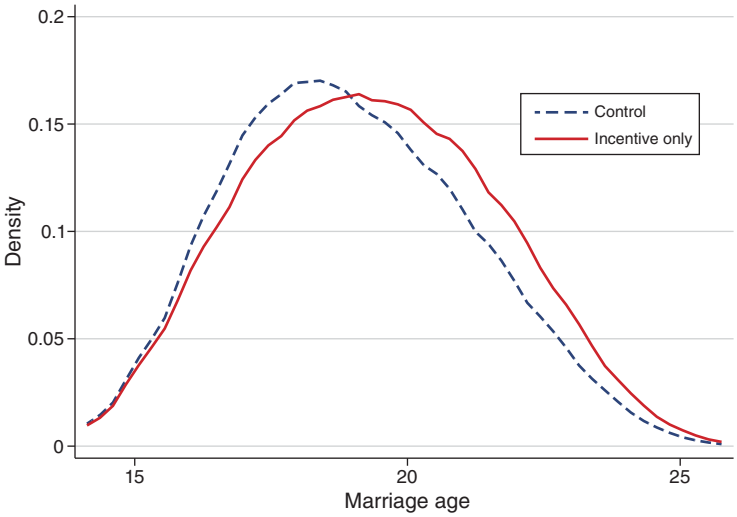

FIGURE 4. DISTRIBUTION OF MARRIAGE AGE

Note: The figure plots the distribution of age at first marriage among women age 15–17 and unmarried at program start by community treatment status.

$p < 0.01$) overall and 3.9 months (0.32 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 percent 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 percent 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 percent) of families at risk responding to the incentive for the duration of the program.

We observe differences in marriage age across incentive and nonincentive communities even beyond the age of 18, despite no incentive being offered to remain unmarried at that age (Figure 4). 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, Glennerster, and Nazneen 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 (for example, through increased education and income prospects), which they can then parlay into even further marriage delays once the program is over on account of being older.

The decline in child marriage also translated into a 7 percent (-1.6 ppts, $p < 0.10$) decline in teenage childbearing for those receiving the incentive overall and a 12 percent (-3.9 ppts, $p < 0.05$) 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 who are in school at program start because it is extremely rare for women to return to secondary school once they have unenrolled.[22] Women age 15 at distribution start and eligible for the incentive were 18 percent more likely to be in school at age 18 (8.6 ppts, $p < 0.01$, column 2) and 22 (5.0 ppts, $p < 0.05$, column 4) 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, consistent with existing estimates in the literature (Field and Ambrus 2008).

Meanwhile, although the empowerment program was effective in reducing girls' adherence to traditional gender norms (see Appendix Table B2), 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 marriage among the subsample of women we surveyed directly (Appendix Table B3). For instance, among girls age 15 and unmarried at program start, the empowerment program increased the share of girls married at midline by 23 percent ($p < 0.10$) and reduced average age of marriage by nearly half a year. 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 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.

There is, however, some weak evidence that the empowerment program influenced schooling attainment, although the result is only significant with respect to 1 schooling measure (last class passed) and in 1 subsample (girls 15–17), and only significant at the 10 percent level, so should be treated as highly speculative. Women eligible for the empowerment program completed 2.0 months

[22] We test this assumption in online Appendix OA.2.2 and find no evidence of impact of the incentive on schooling for those women who were out of school at program start.

Table 3—Education Outcomes, Unmarried Women in School at Program Start

	In school at midline		In school at endline		Last class passed		Secondary complete	
	Age 15–17 (1)	Age 15 (2)	Age 15–17 (3)	Age 15 (4)	Age 15–17 (5)	Age 15 (6)	Age 15–17 (7)	Age 15 (8)
Empowerment	0.011	0.013	0.014	0.023	0.174	0.158	0.016	0.015
	(0.013)	(0.019)	(0.011)	(0.016)	(0.091)	(0.123)	(0.012)	(0.018)
Incentive	0.030	0.086	0.023	0.050	0.129	0.252	0.020	0.040
	(0.015)	(0.022)	(0.015)	(0.021)	(0.117)	(0.188)	(0.016)	(0.024)
Incen. × *Empow.*	0.014	−0.005	0.007	−0.018	−0.071	−0.166	−0.006	−0.017
	(0.022)	(0.030)	(0.022)	(0.029)	(0.179)	(0.243)	(0.024)	(0.033)
Control mean	0.466	0.482	0.280	0.278	11.337	10.833	0.444	0.406
Observations	10,226	4,272	10,930	4,545	10,857	4,518	10,857	4,518
FE	Union	Union	Union	Union	Union	Union	Union	Union
Age 15–17 versus 15								
Empowerment		0.907		0.412		0.847		0.943
Incentive		0.000		0.062		0.290		0.169
Incen. × *Empow.*		0.382		0.184		0.518		0.585

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 and 2 present results from the midline parents survey, and columns 3–8 present results from the endline parents survey. "Secondary complete" is an indicator that is 1 if the woman completed at least secondary school. The bottom three rows present *p*-values from cross-equation equality tests of the coefficients for girls age 15–17 and girls age 15 at program start for each of the interventions.

(0.17 years, $p < 0.10$, column 5, Table 3) of additional schooling relative to those in control communities.[23] Note that positive effects of the empowerment program on schooling do not contradict the absence of a marriage result, only indicate that they increased schooling only up until the point of marriage. They are also 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.

Finally, we find no evidence of complementarity between the incentive and empowerment interventions on the age of marriage. Where the empowerment and incentive coefficients move in the same direction (Table 2, columns 1, 2, 8, and 9), the interaction term while insignificant is of the opposite sign, suggesting if anything that any effect is less than additive. This would be the case if it becomes progressively harder to reduce child marriage once those closest to the margin have changed. 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 for education (Table 3). This is especially surprising in light of the fact that attendance was higher in this treatment arm. In the case of last class passed (column 6, Table 3), when both the incentive and empowerment programs have a positive impact on education, the interaction term is negative and over half the magnitude of the coefficient on the incentive. This implies, if anything, that the impact of the two programs may be less than additive. This would be the case if

[23] Results excluding controls, including women married before program start, and correcting for potential program inclusion errors yield similar results (online Appendix OA.2.1).

it becomes progressively harder to increase last class passed as more and more girls stay longer in school.

Together, these results suggest that, although both programs might have 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 appear to have different magnitudes of influence and only incentives also change underage marriage. In that sense, an empowerment program can only increase schooling insofar as girls in a given environment are dropping out before marriage, whereas a conditional incentive—because it also relaxes the marriage constraint—has the potential to influence schooling enrollment well beyond the life of the program.

B. *Effects on Marriage Price 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).

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) were not included in the parents' survey and hence are only available from the young women's survey.

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 also 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 and 5). Together, this pattern indicates no penalty in the marriage market for marrying later as a result of the incentive program, i.e., no net penalty for the combined behavior change of marrying later and acquiring more education. By contrast, women eligible for the empowerment program saw a 6 percent (US$57, $p < 0.01$) *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.

The fact that we observe a near zero net effect on dowry as a result of an increase in both education and age tells us only that the education premium and age penalty are comparable in magnitude and says nothing about whether both are small or both are large. Hence, in order to say something more conclusive about whether the theoretical predictions bear out in our experimental results, we also estimate marriage market effects of program participation among the subsample of girls who were out of school at baseline, among whom age effects but no significant schooling effects of the incentive program are observed. In this subsample, we similarly see no

Table 4—Husband Characteristics, Unmarried Women Age 15–17 at Program Start and Married at Endline

	Dowry (2016 US$) (1)	Denmeher (2016 US$) (2)	Husband's education (3)	Husband is salaried (4)	Outside union (5)
Empowerment	57.336	−29.022	0.015	0.005	0.006
	(20.679)	(92.352)	(0.099)	(0.009)	(0.013)
Incentive	17.595	95.741	−0.166	−0.019	0.017
	(24.278)	(124.305)	(0.119)	(0.013)	(0.017)
Incen. × *Empow.*	−49.726	−100.926	0.183	−0.010	−0.016
	(36.209)	(157.134)	(0.181)	(0.018)	(0.023)
Control mean	888.225	2,330.848	9.668	0.529	0.376
Observations	11,793	730	11,577	12,727	12,941
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 three standard deviations.

increase in dowry (or decrease in denmeher) associated with marrying later (online Appendix Table OA.14), suggesting that the dowry value of schooling gains from delay are not neutralizing a meaningful dowry penalty for bride age in the incentive arm.

IV. 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 with unschooled mothers, a proxy of low income (online Appendix OA.2.2). 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 (90 percent of girls marry outside the village). In addition, potential grooms come from a wide 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 key results of our field experiment. Namely, an incentive (which is small relative 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 with 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, a dominant bride characteristic (henceforth, "preferred" type) is not fully observable to potential grooms, and second, this characteristic is correlated with the returns to postponing (the nonpreferred 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.[24]

As described in Section I, the dominant bride characteristic in our setting is women's adherence to conservative gender norms, which is highly likely to satisfy both conditions. First, it is 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. 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 (online Appendix OA.2.3).

In this setting, conservative women are likely to have lower (though positive) returns to education than less conservative women, who are 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 less likely to work outside the home or have economic decision-making power inside the household once married (online Appendix Table OA.17), suggesting that less conservative women gain more from education.

[24] 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, 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.

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 nonpreferred types because everyone knows they have relatively higher returns from education and therefore a stronger incentive to stay in school.[25] Thus, all brides enter early, even though everyone would gain if women delayed marriage and attained more schooling.[26] In this environment, an intervention reduces child marriage if and only if it is believed to strengthen the net returns to 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.

A. *The Model*

A marriage market is populated by women of measure $|W| = 1$ and men of measure $|M| > 1$.[27] Women are heterogeneous in preferred type $\Theta \in \{L, H\}$, which is private information, while men are homogeneous.[28] Men desire Θ_H women but cannot learn this type before 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, 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.[29, 30]

[25] We focus on education because there is causal evidence on the impact of early marriage on education, and 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 (Field and Ambrus 2008; Mathur, Greene, and Malhotra 2003; EngenderHealth 2003; Jain and Kurz 2007; Nour 2009; Raj 2010). For instance, Field and Ambrus (2008) show a causal effect of marriage age alone (independent of schooling) on maternal health behavior.

[26] 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.

[27] 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 nonpreferred unobservable type. $|M| > |W|$ is also an empirical regularity in many settings given that men tend to get married more gradually over their lifetime and thus spend a longer fraction of their lives in the state of partner search. In our data, the ratio of unmarried men age 21 to 23 to unmarried women age 15–17 is 1.9 (the average marriage gap is 6 years; see Table 1).

[28] We assume homogeneity of men 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 homogeneous types while allowing women to be heterogeneous in type, we are assuming the most important feature of brides in the marriage market is not perfectly observed, while that of men is relatively observable (e.g., earnings capacity).

[29] This implicitly also assumes incomplete credit markets, as we should not observe high rates of child marriage with complete credit markets: nonpreferred wives would pay higher dowries (i.e., by taking loans), delay marriage, and go to school, while preferred wives would pay lower dowries and marry young.

[30] We do not assume that nonpreferred 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.

Women choose between entering the marriage market early at t_1 or delaying until t_2.[31] 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 $E_H > E_L$.[32] 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: $\omega_M > \omega_W$.[33]

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:

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

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

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.[34] 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.

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

(i) an unmarried woman may declare herself available for marriage and in doing so reveal herself to have low education E_L;

(ii) men may make dowry offers to available women; and

(iii) if a woman receives an offer and accepts it, she pays the dowry, gets married, and exits the market.

If a woman receives an offer and rejects it, or does not receive an offer, she remains unmarried and may reenter in t_2 (but it will be known that she entered in t_1 and remained unmarried).

[31] We assume 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.

[32] In our setting, marriage and education co-move closely for young women who would be considered child brides. However, 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.

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

[34] For tractability, we abstract from denmeher in our model without loss of generality, as dowry value can be thought of as a vector of contract elements including denmeher.

At the beginning of t_2,

(i) unmarried women may declare availability for marriage. They will be known to have education E_H;

(ii) unmarried men may make dowry offers to available women;

(iii) if a woman receives an offer and accepts it, she pays the dowry, gets married, and exits the market;

(iv) 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.[35] 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) $\sigma(\Theta)t_1 + \left[1 - \sigma(\Theta)\right]t_2$: 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 (i)–(iii) are as follows:

(i) Beliefs $\Pr(\Theta = H | t_1)$ and $\Pr(\Theta = H | t_2)$ must respect Bayes' rule.

(ii) 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.

(iii) 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.

B. *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 (i) an inefficiently large number of women marrying young and (ii) scope for small financial incentives to generate large changes in marriage timing since they have the potential to influence behavior of all types. Hence, we begin by characterizing conditions under which a pooling equilibrium would arise and therefore justify greater policy intervention—i.e., 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 (separating equilibrium).

[35] 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).

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

See Appendix AA for a formal proof. The intuition is the following. 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 $D \mid t_2$, then Θ_L strictly prefers to do so. Thus, if a separating equilibrium exists, it must be that preferred types enter early and nonpreferred 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 $D \mid t_2$ that exceeds the preferred type's marginal gain from increasing her education but does not exceed the nonpreferred type's.

The liquidity constraint condition in Result 1 ensures that no woman can afford the minimum $D \mid t_2$ that both induces separation and satisfies the man's participation constraint.[36] Examining the condition, we see that a separating equilibrium is harder to achieve the smaller the gap between preferred and nonpreferred types' marginal returns to delaying marriage and harder to achieve the more men value bride type relative to bride education. Hence, these are the conditions under which we are more likely to observe marriage markets pooling on early marriage due to signaling considerations.

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.*

We provide intuition in Appendix AB and formal proof in online Appendix OA.3.1. 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

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

RESULT 2: *Suppose that*

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

[36] If nonpreferred 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 percent of average expected dowry at baseline).

Then the unique equilibrium is as follows:

(*i*) *All women enter the marriage market at* t_1.

(*ii*) *Men propose dowries:*

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

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

(*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 AC 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 nonpreferred type has an even stronger preference to delay. A nonpreferred type could convince men she is nonpreferred type, but men do not like marrying nonpreferred types, so the dowry a man would charge a woman entering at t_2 whom he believes is nonpreferred with certainty exceeds her budget constraint.

Thus, child marriage is 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.

C. *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 nonpreferred. 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 considering the policy potential of small transfers that operate through their effect on signaling rather than through income effects that alter the intrinsic structure of the marriage market.[37] Thus, we consider the case in which the dowry a man requires to be willing to marry an educated woman who is known to be nonpreferred type still exceeds her budget constraint, even if she receives the conditional transfer C.[38]

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 nonpreferred types, and $(1-\tau)(1-f)$ are untreated nonpreferred types. Since neither type nor treatment status is observable, men continue to condition dowry only on marriage timing.

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 online Appendix OA.3.2 for a formal proof. The key insight is that, because the treatment is random and therefore independent of type, preferred and nonpreferred types are equally likely to have the eased liquidity constraint in t_2. Crucially, the conditional incentive does not increase the difference between the nonpreferred and preferred type's marginal gains from education or the relative ability of the nonpreferred 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 semiseparating 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 $\bar{\bar{\mu}}(E_H)$ 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*

$$C > \mu(\Theta_H, E_L) - \bar{\bar{\mu}}(E_H) - \left[\mu(\Theta_H, E_H) - \mu(\Theta_H, E_L)\right].$$

[37] In our experiment, the annual value of the transfer is approximately 1 percent of average expected dowry and 7 percent of average annual household income at baseline.

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

Then a semiseparating 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 nonpreferred types, treated preferred types, and treated nonpreferred types marry at* t_2.

(*ii*) *Men propose dowries:*

$$D\,|\,t_1 \;=\; \omega_M \;-\; \mu\big(\Theta_H, E_L\big),$$

$$D\,|\,t_2 \;=\; \omega_M \;-\; \bar{\bar{\mu}}\big(E_H\big).$$

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

See online Appendix OA.3.3 for a formal proof. Note that as treatment coverage τ increases, the size of the transfer C needed decreases. We discuss this further in online Appendix OA.3.1.

As delaying marriage is no longer a certain signal of nonpreferred type, untreated nonpreferred 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 nonpreferred type, or an untreated nonpreferred type and receiving a higher dowry $D\,|\,t_2$. The equilibrium dowries are determined by this competition.

Thus, this semiseparating 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 semiseparating 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 (nonpreferred 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.[39]

Men are indifferent because competition for women drives them to receive their outside options, which are unaffected by small conditional incentives. However, since the total pie has increased through increased education, it is easy to see that

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

slightly relaxing perfect competition for women would result in a strict improvement for every individual, including men.

This set of results shows why random conditional incentives might effectively reduce signaling-driven child marriage and lead to a Pareto improvement. In online Appendix OA.3.4, we consider the optimal way to structure this type of program to maximize impact (including positive spillovers) given the transfer size. If child marriage persists due to signaling motives, the most cost-effective random conditional incentive is one with lower coverage and larger transfers if nonpreferred types are believed to be prevalent (to maximize spillovers), while greater coverage and smaller transfers are preferred if nonpreferred types are believed to be rare.

D. *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 straightforward: as the likelihood grows that a girl in a given marriage market is a nonpreferred type, there is greater incentive for preferred types to deviate from marriage delay.[40] We formalize this in the following corollary.

COROLLARY 2: *The smaller f is—that is, the larger the share of nonpreferred types—the more likely it is that women will pool on early marriage and low education.*

See online Appendix OA.3.5 for formal proof. The key insight is that, given a reasonable level of uncertainty about type distribution in the population, a decrease in f will cause men to face a higher risk of marrying a nonpreferred type and thus will strengthen women's desire to signal they are preferred by entering the marriage market early and forgoing education, which everyone knows yields differentially higher returns to nonpreferred women.

E. *Testable Predictions*

In the previous subsections, we formally described the conditions 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.

Our model generates the following three predictions, which align with our main empirical findings.

[40] Note that we do not anticipate any direct effects of the empowerment program on adolescent outcomes precisely because adolescents have little to no agency over marriage timing and school dropout in this setting, as is reflected in our modeling choices. Hence, the only way in which the empowerment program can influence adolescent outcomes in our model is indirectly via the marriage market by way of changing perspective grooms' expectations of a girl's type.

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 nonpreferred 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 our 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 nonpreferred 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 nonpreferred type women.

V. 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 nonpreferred types should also delay marriage when treatment is not perfectly observed. 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 nonpreferred, or less socially conservative, women. Because treatment status was not randomized, we make use of spatial variation in the observability of treatment status based on proximity to village boundaries.

First, we show that treatment status was not perfectly observed by community members. At wave 2, 8 percent 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. It is reasonable to anticipate that verification of the bride's village of residence, and also her treatment status, is likely to be difficult when she lives within 500 yards of the border because marriages are arranged through third-party matchmakers, parents-in-law often live far away, and high population density makes borders between close neighboring villages frequently unclear.[41] Anthropological studies also provide evidence that

[41] Given the importance of maintaining the integrity to the randomized design, both program implementers and the evaluation team carefully monitored that implementation followed these not always obvious borders of villages. Indeed, our data indicate high compliance, with only 0.9 percent of control girls reporting having received the incentive.

families mislead potential in-laws in marriage negotiations. For example, in her book, *Arguing with the Crocodile*, Sarah White (quoted in Wahhaj 2018) says "Mismatches occur not only through lack of information but also through deliberate deception" (White 1992, pp. 99). At the same time, knowledge about the incentive program spread to untreated households: 79 percent of untreated women in incentive communities and 25 percent of untreated women in nonincentive communities had heard about the incentive program at midline (online Appendix OA.1.8).

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 percent more likely to have heard about the incentive program (35 percent of women living within 500 meters versus 24 percent of women living farther than 500 meters from the closest incentive community center; online Appendix OA.1.8). Hence, using communities' geo-locations, we test for spillovers among untreated women who live close to treated communities. Our regression estimates of program spillovers in columns 1 and 2 in Table 5 thus include all households in nonincentive 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 or participation in the empowerment program, we control for distance of the community center to the geographic center of the closest community and to the closest safe space.

As shown in column 1, women age 15–17 at program start who lived close to incentive communities are 10 percent (-2.9 ppts, $p < 0.10$) 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 age 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 nontreatment 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 who 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., nonpreferred type) untreated women should delay marriage. As women's social conservatism is not fully observable from household conservatism (online Appendix Table OA.16), we test these predictions among the subsample of program participants who were tracked and surveyed directly in the young women's survey. We control for distances to the closest community center and safe space from both the community center and the household as well as for several baseline income proxies to ensure that social conservatism is not simply proxying for socioeconomic status: (i) the girl's BMI, (ii) an indicator for whether the girl was stunted, and (iii) the household income.[42] Consistent with our theoretical prediction, we observe spillovers only on less socially conservative women (columns 3 and 4, Table 5): less socially conservative women who lived close to incentive communities are 41 percent (-14.7 ppts,

[42] Results are robust to excluding all additional controls from the young women's survey and to running weighted regressions (see Buchmann et al. 2018).

Table 5—Market Spillovers on Child Marriage (Married <18) in Nonincentive Communities, by Girl's Social Conservatism (SC)

	Parents' survey married <18		Young women's survey married <18 (age 15–17)	
	Age 15–17 (1)	Age 7–14 (2)	Low SC (3)	High SC (4)
Close to incentive village	−0.029	−0.009	−0.147	0.093
	(0.016)	(0.015)	(0.059)	(0.080)
Control mean	0.288	0.390	0.360	0.479
Observations	10,544	40,136	682	417
FE	Union	Union	Union	Union

Notes: The table shows results from OLS regressions, adjusted for stratification and baseline characteristics (see notes to Table 2). The regressions in the young women's survey also control for several baseline income proxies collected in the detailed subsample only to ensure that social conservatism is not simply proxying for socioeconomic status: (i) the girl's BMI, (ii) an indicator for whether the girl was stunted, and (iii) the household income. 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 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 young women's survey in which we collected household geo-locations).

$p < 0.05$) 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. Meanwhile, consistent with our theory, we find no heterogeneity in social conservatism among eligible women (see online Appendix Table OA.18).

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, 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. The presence of cross-community spillovers implies that the direct impacts of the program we measure in Tables 2 and 3 are likely to underestimate the full program impact on marriage and schooling.

General equilibrium effects of our intervention at the level of the marriage market will be small because the marriage market is large and diffuse. While many brides marry within a union (roughly 10 villages), 38 percent marry further afield. Moreover, spillover effects within the union would lower our ability to detect differences across treatment and control villages within the union. Note that while the impact of the incentive on girls living close to but ineligible for the program is

important for testing the theory, it does not affect many girls and thus is unlikely to imply a substantial general equilibrium effect: only 22 out of 306 nonincentive communities in the study are within 500 meters of an incentive community.

VI. 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 US$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.[43] This analysis suggests that the conditional incentive generated US$1,010 in Net Present Value for every US$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.

VII. 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

[43] 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.

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.

Appendix A. Theory Appendix

A. *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 nonpreferred 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 nonpreferred 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 $\Delta \in \big(\mu(\Theta_H, E_H) - \mu(\Theta_H, E_L), \mu(\Theta_L, E_H) - \mu(\Theta_L, E_L)\big)$. 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). ∎

B. *Intuition for 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 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.

Nonpreferred 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 nonpreferred 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 nonpreferred 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.3.1 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 semiseparating equilibrium. ∎

C. *Proof of Result 2: The Unique Equilibrium in Control Communities Is Pooling on t_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.3.1), 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

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

Since entry at t_1 is a probability 0 event, Bayes' rule does not impose any restriction on the off-equilibrium beliefs $\Pr(\Theta = H \mid t_1) = \gamma \in [0,1]$. 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:

$$\begin{aligned}\mu(\Theta_H, E_H) - \left\{\omega_M - \left[f\mu(\Theta_H, E_H) + (1-f)\mu(\Theta_L, E_H)\right]\right\} \\ > \mu(\Theta_H, E_L) - \left[\omega_M - \mu(\Theta_L, E_L)\right].\end{aligned}$$

By single-crossing, the nonpreferred type also prefers to enter at t_2. So, we ask whether this equilibrium survives the intuitive criterion.

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:

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

Then Θ_H prefers deviating and entering at t_1 over entering at t_2 if and only if

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

On the other hand, deviating to t_1 is equilibrium-dominated for Θ_L if and only if

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

Thus, "pooling on t_2" fails the intuitive criterion if and only if

$$\begin{aligned}\mu(\Theta_H, E_H) - \mu(\Theta_H, E_L) &< \mu(\Theta_H, E_L) - \left[f\mu(\Theta_H, E_H) + (1-f)\mu(\Theta_L, E_H)\right] \\ &< \mu(\Theta_L, E_H) - \mu(\Theta_L, E_L).\end{aligned}$$

This condition reflects that (i) "type" is first-order in marriage, (ii) education has more marriage value-added for a nonpreferred 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 intuitive criterion because the preferred type should be able to deviate to entering at t_1 and convince men that she is preferred type because a nonpreferred 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.

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

$$D \mid t_1 = \omega_M - \left[f\mu(\Theta_H, E_L) + (1-f)\mu(\Theta_L, E_L)\right].$$

If $\Pr(\Theta = H \mid t_2) = 0$, then $D \mid t_2 = \omega_M - \mu(\Theta_L, E_H)$. Θ_H prefers entering at t_1 if and only if

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

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:

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

Clearly, $D \mid t_2 < D \mid t_1$ 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.

Appendix B. Empirical Appendix

Table B1—Baseline Characteristics, Women Age 15–17 at Program Start

	Empowerment		Incentive		Empowerment + Incentive		Control	Total
	Mean (SD)	β_E (p-value)	Mean (SD)	β_I (p-value)	Mean (SD)	β_{E+I} (p-value)	Mean (SD)	Mean (SD)
Panel A. Parents: Married and unmarried at baseline								
Observations	8,739		4,176		4,503		8,990	26,408
Ever married (percent)	8.55	−0.11	9.36	0.71	8.82	0.16	8.65	8.76
	(27.96)	(0.90)	(29.13)	(0.49)	(28.36)	(0.85)	(28.12)	(28.27)
Age	14.97	−0.01	14.98	0.01	14.96	−0.01	14.97	14.97
	(0.79)	(0.71)	(0.80)	(0.63)	(0.81)	(0.66)	(0.80)	(0.80)
Still in school (percent)	60.24	−0.93	59.21	−1.97	60.24	−0.93	61.17	60.40
	(48.94)	(0.61)	(49.15)	(0.35)	(48.95)	(0.67)	(48.74)	(48.91)
p-value from joint significance test		0.90		0.76		0.93		
Panel B. Parents: Unmarried at baseline								
Observations	7,992		3,785		4,106		8,212	24,095
Age	14.91	−0.01	14.92	0.00	14.92	0.00	14.92	14.92
	(0.78)	(0.73)	(0.80)	(0.99)	(0.80)	(0.80)	(0.79)	(0.79)
Still in school (percent)	64.62	−1.22	64.10	−1.75	65.07	−0.78	65.84	65.03
	(47.82)	(0.46)	(47.98)	(0.39)	(47.68)	(0.70)	(47.43)	(47.69)
Unmarried older sister in HH (percent)	18.87	0.34	18.04	−0.49	18.00	−0.54	18.53	18.48
	(39.13)	(0.64)	(38.46)	(0.54)	(38.42)	(0.54)	(38.86)	(38.81)
Mother education (0–17)	3.19	0.13	3.01	−0.06	3.03	−0.03	3.07	3.09
	(3.30)	(0.46)	(3.30)	(0.79)	(3.14)	(0.86)	(3.32)	(3.28)
HH size (members)	6.04	0.05	6.05	0.06	6.02	0.03	5.99	6.02
	(1.95)	(0.46)	(1.97)	(0.47)	(1.99)	(0.74)	(2.04)	(1.99)
Community is connected to public transport (percent)	37.42	4.33	37.36	4.26	34.71	1.61	33.10	35.48
	(48.40)	(0.50)	(48.38)	(0.59)	(47.61)	(0.84)	(47.06)	(47.85)
p-value from joint significance test		0.84		0.93		1.00		
Panel C. Young women: Unmarried at baseline								
Observations	874		475		531		911	2,791
Age	14.85	−0.03	14.86	−0.02	14.88	−0.01	14.88	14.87
	(0.77)	(0.36)	(0.79)	(0.65)	(0.80)	(0.84)	(0.80)	(0.79)
Still in school (percent)	69.07	−2.31	71.03	−0.35	70.13	−1.24	71.38	70.36
	(46.25)	(0.35)	(45.41)	(0.91)	(45.81)	(0.67)	(45.22)	(45.68)
Unmarried older sister in HH (percent)	18.54	1.52	16.00	−1.01	18.64	1.63	17.01	17.63
	(38.88)	(0.41)	(36.70)	(0.61)	(38.98)	(0.44)	(37.60)	(38.11)
Mother education (0–17)	3.30	0.07	3.14	−0.09	3.41	0.19	3.23	3.27
	(3.29)	(0.77)	(3.10)	(0.74)	(3.16)	(0.53)	(3.31)	(3.24)
HH size (members)	5.76	0.07	5.59	−0.10	5.74	0.05	5.69	5.71
	(1.93)	(0.56)	(1.63)	(0.42)	(1.90)	(0.69)	(2.03)	(1.91)
Community is connected to public transport (percent)	36.16	−2.37	39.37	0.84	33.52	−5.01	38.53	36.98
	(48.07)	(0.72)	(48.91)	(0.92)	(47.25)	(0.52)	(48.69)	(48.28)
BMI	20.84	2.16	18.51	−0.16	18.54	−0.14	18.68	19.31
	(61.19)	(0.34)	(2.49)	(0.49)	(2.49)	(0.57)	(6.18)	(34.72)
Stunted (percent)	31.82	2.57	31.66	2.40	32.86	3.61	29.25	31.13
	(46.61)	(0.27)	(46.57)	(0.40)	(47.02)	(0.21)	(45.52)	(46.31)
Monthly HH income (US$)	19.97	−0.32	19.80	−0.50	19.45	−0.85	20.30	19.96
	(18.65)	(0.76)	(15.49)	(0.66)	(15.55)	(0.47)	(22.55)	(19.07)
p-value from joint significance test		0.82		0.57		0.05		

Notes: The table shows baseline characteristics by treatment arm of women age 15–17 at program start. We show means and standard deviations within treatment arms as well as for the overall sample. In addition, we show coefficients and p-values from OLS regressions regressing each variable on the treatment indicators with modified Huber-White SEs clustered at the community level. We also present p-values from joint significance tests of all variables included in the balance tests. In addition, none of the pairwise differences across treatment arms are significant in OLS regressions that exclude the control group and vary the excluded treatment group.

Table B2—Social Conservatism Index, Unmarried Girls Age 10–17 at Program Start

	Age 10–17 (1)	Age 15–17 (2)
Empowerment	−0.035	−0.013
	(0.017)	(0.029)
Incentive	0.018	0.056
	(0.023)	(0.036)
Incen. × *Empow.*	0.018	−0.027
	(0.031)	(0.048)
Control mean	0.000	0.000
Observations	5,209	1,729
FE	Union	Union

Notes: The table shows results from OLS regressions, adjusted for baseline characteristics and stratification (see notes to Table 5). We also adjust for the distance of the household to the closest incentive community (to control for signaling concerns in nonincentive households). Huber-White robust SEs clustered at the community level in parentheses. The "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 B3—Marriage and Childbearing Outcomes, Unmarried Girls Age 15–17 at Program Start in the Young Women's Subsample Survey

	Married <18		Married <16	Married at midline		Marriage age		Birth <20	
	Age 15–17 (1)	Age 15 (2)	Age 15 (3)	Age 15–17 (4)	Age 15 (5)	Age 15–17 (6)	Age 15 (7)	Age 15–17 (8)	Age 15 (9)
Empowerment	0.054	0.068	0.032	0.046	0.081	−0.322	−0.468	0.037	0.045
	(0.029)	(0.046)	(0.033)	(0.030)	(0.044)	(0.121)	(0.182)	(0.025)	(0.039)
Incentive	−0.030	−0.103	−0.066	−0.013	−0.118	−0.065	0.137	−0.041	−0.112
	(0.035)	(0.055)	(0.036)	(0.034)	(0.053)	(0.133)	(0.214)	(0.027)	(0.044)
Incen. × *Empow.*	0.030	0.024	−0.010	−0.026	−0.006	0.168	0.435	0.011	0.020
	(0.051)	(0.081)	(0.051)	(0.049)	(0.072)	(0.205)	(0.319)	(0.040)	(0.065)
Control mean	0.379	0.478	0.155	0.361	0.357	18.175	17.620	0.215	0.295
Observations	1,737	755	755	1,649	728	1,464	629	1,742	757
FE	Union	Union	Union	Union	Union	Union	Union	Union	Union
Age 15–17 versus 15:									
Empowerment		0.637			0.257		0.238		0.756
Incentive		0.043			0.003		0.193		0.018
Incen. × *Empow.*		0.909			0.683		0.214		0.834

Notes: The table shows results from OLS regressions, adjusted for baseline characteristics and stratification (see notes to Table 2). We also control for the girl's social conservatism index at baseline. Huber-White robust SEs clustered at the community level. The bottom three rows present *p*-values from cross-equation equality tests of the coefficients for girls age 15–17 and girls age 15 at program start for each of the interventions.

Table B4—Married at Endline, Women Unmarried at Program Start

	Parents' survey		Young women's survey	
	Age 15–17 (1)	Age 15 (2)	Age 15–17 (3)	Age 15 (4)
Empowerment	0.006	0.002	0.008	0.004
	(0.008)	(0.012)	(0.021)	(0.031)
Incentive	−0.009	−0.018	0.004	−0.015
	(0.010)	(0.016)	(0.026)	(0.040)
Incen. × *Empow.*	−0.002	0.005	−0.009	0.037
	(0.014)	(0.022)	(0.034)	(0.057)
Control mean	0.837	0.820	0.847	0.834
Observations	15,562	5,864	1,742	757
FE	Union	Union	Union	Union
Age 15–17 versus 15				
Empowerment		0.622		0.876
Incentive		0.439		0.449
Incen. × *Empow.*		0.619		0.224

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 fixed effects, household size, the presence of an older unmarried sister in the household, school enrollment, mother's level of education, and whether the community is accessible via public transport (a proxy for remoteness). Columns 1 and 2 present results from the endline parents' survey, and columns 3 and 4 present results from the endline young women's survey. The sample includes all women age 15–17 and unmarried at program start. The sample excludes washed out 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 outcome variable, "Married at endline," is equal to one if ever married at endline. The bottom three rows present *p*-values from cross-equation equality tests of the coefficients for girls age 15–17 and girls age 15 at program start for each of the interventions.

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