Online Appendix to

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

Contents

OA.1 Review of Literature on Child Marriage

Search Process and Criteria for Inclusion in Literature Review

We searched first for "literature reviews" and "systematic reviews" of the literature on child marriage and early marriage. We confined our search to quantitative studies that attempted to evaluate the impact of programs, policies, or interventions that had child marriage or teenage childbearing as an outcome. We also searched the terms "child marriage" and "early marriage". We used the snowball method, reading the studies rated medium or high-quality in the literature reviews and the studies mentioned in the papers listed in the literature review and found from the original search terms. We searched the database of randomized control trials on the Abdul Latif Jameel Poverty Action Lab website, the International Initiative for Impact Evaluation, the Social Science Registry of randomized trials, and the Impact Evaluation Database at the World Bank for studies that had child marriage or teenage childbearing as an outcome.

We found five reviews of the literature on child marriage of which one was published in a peer reviewed journal (Lee-Rife et al., 2012; Malhotra et al., 2011; Gupta et al., 2008; Mensch and Lloyd, 2005; Parsons and McCleary-Sills, 2005) (Note that we did not specifically search for literature reviews or studies on teenage pregnancy unassociated with early marriage. The contexts where teenage pregnancy mainly occurs outside early marriage are quite different from our context and the drivers of teenage pregnancy outside marriage may be somewhat different from the drivers of child marriage. Our search terms did lead us to read and review some studies where teenage pregnancy was the main outcome. The conclusions we drew from these seem consistent with the wider literature on teenage pregnancy). There was a large overlap in the studies contained in three of the reviews on early/child marriage (Lee-Rife et al., 2012; Malhotra et al., 2011; Gupta et al., 2008), a fourth mainly reviewed (and added to) work on the correlates of child marriage (Mensch and Lloyd, 2005), and a fifth only covered studies funded by the World Bank (Parsons and McCleary-Sills, 2005). Many of the studies covered in these reviews and found in our search process were program evaluation reports which had not gone through peer review and contain little detail on methodology or results. For example, results were often provided in charts with no details on variance, standard errors, confidence intervals, or significance.

Another common issue in many studies was the few number of clusters analyzed. When program assignment is at the cluster level (e.g., community or district), analysis of program impacts must take into account cluster level shocks. This can be done by treating each cluster as one observation or by treating each individual as a separate observation but clustering standard errors at the level of the cluster. Several of the studies summarized in previous literature reviews appear to ignore in the analysis that assignment was at the cluster level. In particular, several studies have one or two treatment units and one or two comparison units. It is not possible to adjust for cluster level shocks with this limited number of clusters.

In our search we found three studies that evaluated a conditional incentive program where families received support if girls avoided child marriage even if

they had dropped out of school. While two of these did not have child marriage as an outcome, and there were some quality concerns with two of the studies, we nevertheless included them in our review given the similarity to the program we evaluated.

Because of the very low quality of even those studies ranked "medium" in previous literature reviews, we did not formally review studies ranked as low-quality in these reviews.[1] The wider search process did, however, reveal several high-quality studies not previously included in other literature reviews on child marriage.

Studies were rated as low-quality if they fit any of the following criteria:

i. there were too few clusters to estimate cluster errors. Several studies had only one or two treatment or comparison areas making it impossible to compare outcomes across treatment and comparison areas;

ii. there was insufficient information on the methodology to judge its quality and/or there was no information on the statistical significance of the results;

iii. there was potential for selection bias between those who participated in the program and those that did not and there was no credible methodology of addressing this selection bias. For example, Krishnan et al. (2014) evaluate the effect of a state-wide program in Haryana by comparing marriage age of girls in Haryana with daughters-in-law living in Haryana but originally from other states. However, girls who marry into another state are likely to be different on many dimensions to those who marry within their state.

Studies were ranked medium-quality if they:

i. provided sufficient information to judge the quality of the study, the risk of selection bias, and level of statistical significance or reported confidence intervals;

ii. used appropriate methodologies to address concerns about selection bias;

iii. were unable to address all concerns about selection bias. Note that most studies ranked medium-quality were explicit that some concerns about selection bias remained.

Studies were ranked high-quality if they:

i. provided sufficient information to judge the quality of the study, the risk of selection bias, and level of statistical significance or reported confidence intervals;

ii. satisfactorily addressed selection bias concerns. Many (though not all) high-quality studies used random assignment with large numbers of units or individuals randomized.

[1] We did, however, review low-quality studies that are very similar to one of our treatment arms.

We did not use publication in peer reviewed journals as a criterion for quality as some of the studies published in peer review journals had serious methodological flaws while there were several studies with sufficient detail to judge quality but that were not published in peer reviewed journals. The latter may reflect the long lag in economics between the completion of full, academic style, working papers and final publication in a journal.

Before summarizing results, it is worth noting that there are cultural differences across regions which are important to keep in mind when comparing results. In particular, teenage childbearing without formal marriage is more common in some parts of Africa than it is in South Asia but usually means that girls cannot stay in school. As child marriage is often associated with teenage childbearing there are important parallels between these two outcomes. Another important relevant difference to keep in mind is that in some countries marriage timing is mainly determined by parents of girls whereas in other countries girls themselves may have more say over cohabitation, marriage, or sexual onset.

Evidence on the Impact of Unconditional Incentives

There is very little evidence on the impact of unconditional incentives on child marriage or teenage childbearing and existing evidence is mostly from Africa. One high-quality study evaluates an RCT in Malawi in which girls and their families received monthly stipends over the course of two school years (Baird et al., 2011). The incentives significantly reduced teenage childbearing and marriage rates. Another low-quality study evaluates an RCT to assess the impact of unconditional cash incentives to orphans in Kenya (Handa et al., 2015). While the program reduced the likelihood of pregnancy, there was no significant impact on early marriage. However, respondents were selected differently within treatment and control arms, leading to systematic differences in characteristics between the two groups. There was substantially higher attrition in the control group than in the treatment group (30% vs. 17%), and the study had overall relatively few clusters (14 in treatment and 14 in control).

Evidence on the Impact of Incentives Conditional on Education

Cash and noncash incentives conditional on girls staying in school and/or maintaining good grades appear to be effective in encouraging girls to stay in school but there is mixed evidence on whether this leads to reductions in early marriage, teen pregnancy, or cohabitation during teenage years (note that we did not explicitly search for studies having teen pregnancy or cohabitation as an outcome). In total, seven studies evaluate the impact of such programs and show significant positive effects on schooling (Baird et al., 2011; Duflo et al., 2015; Alam et al., 2011; Hong and Sarr, 2012; Hahn et al., 2015; Angrist et al., 2006; Heath and Mobarak, 2015). Five studies have negative significant effects on marriage, teen pregnancy, or cohabitation, (Duflo et al., 2015; Alam et al., 2011; Hong and Sarr, 2012; Hahn et al., 2015; Angrist et al., 2006), and one has effects that are insignificantly different from zero (Heath and Mobarak, 2015). If we focus on the three high-quality studies, two have significant reductions in pregnancy or cohabitation although neither has formal marriage as an outcome, (Duflo et al., 2015; Angrist et al., 2006)

and one has an effect insignificantly different from zero (Baird et al., 2011). It is worth noting that neither of the high-quality studies linking conditionality on education to marriage are in South Asia or even in countries where parents are the main deciders of marriage age. One is a study of free school uniforms in Kenya (Duflo et al., 2015) and one assesses the impact on teen pregnancy and marriage rates of incentives conditional on school attendance in Malawi (Baird et al., 2011). One evaluates a program in which vouchers to attend private school (which are valuable) are conditional on girls maintaining higher grades in Colombia (Angrist et al., 2006). It is not possible to separate the effect of attending a private school on cohabitation (the outcome in this study) from the effect of the conditionality on grades. The study does, nevertheless, suggest a causal link between education and marriage/cohabitation. The context however, is very different from that in South Asia.

The four low and medium-quality studies examining conditional incentives and their link to child marriage are more closely tied to the context of our study. All are in South Asia, have marriage as an outcome, and evaluate Female Secondary School Stipends (FSSP) in Pakistan or Bangladesh where parents have considerable control over marriage. None, however, completely address concerns of selection bias and they find mixed results. Alam et al. (2011) compare marriage trends for girls in districts in Pakistan where the FSSP was introduced with girls in districts where it was not introduced. They find large effects on education and marriage but these are not very robust to different specifications and results are insignificant in the purest intention-to-treat specification. Program districts were chosen because education of girls was particularly low in these districts. This raises the concern that outcomes for poor performing districts would have risen faster than better performing districts in a process of catchup even without the program. In addition, there is very strong geographic clustering of treatment and control districts with virtually all treatment regions in the South of Punjab and virtually all control districts in the North, meaning that identification relies only on the north south divide, reinforcing concerns that the result could be driven by differential trends. In a robustness check, the authors use a regression discontinuity design to compare districts just above and just below the literacy threshold for receiving the program, yielding results that are significant and similar in size to the ITT estimates. Hong and Sarr (2012), Hahn et al. (2015), and Heath and Mobarak (2015) all evaluate the impact of the same female secondary school stipend (FSSP) in Bangladesh using a difference-in-difference strategy. Hahn et al. (2015) and Hong and Sarr (2012) both exploit the rule that only girls in rural areas were eligible for the stipend and compare education and marriage outcomes for rural vs. urban girls before and after the introduction of the FSSP. The main difference between the two is that Hong and Sarr (2012) also examine the earlier introduction of free secondary education for girls. Both papers find large effects on education and age of marriage from the FSSP (an increase in age of marriage of 0.6 to 2.3 years). It is worth noting two caveats to these results. First, Hahn et al. (2015) find the program has its largest effect on primary school enrollment which is odd for a secondary school program. More importantly, the raw age of marriage data

suggests that the changing gap between rural and urban ages of marriage is driven mainly by a collapse in the age of marriage of urban girls rather than an increase in age of marriage of rural girls. Heath and Mobarak (2015) examine the impact of the FSSP in periurban areas (still classed as rural) by comparing girls who reached secondary school just before the FSSP and just after the FSSP and find no statistical difference. They note that the gender gap on education was closing long before the introduction of the FSSP and that there was no trend break in the steadily increasing secondary school enrollment rates in the country as a whole. A key difference with the other FSSP papers is that they are not exploiting geographic differences in eligibility (which could confound program impact with differential trends in rural vs. urban areas), and compare girls in a much shorter age window around the introduction of the program.

In summary, we conclude that there is limited reliable evidence on the impact on child marriage of cash or noncash incentives that are conditional on education especially in the South Asian context.

Evidence on the Impact of Incentives Conditional on Avoiding Child Marriage

The evidence on the impact on child marriage of cash or noncash incentives that are conditional on marriage is even more limited. Our search revealed four studies on such programs (Krishnan et al., 2014; Nanda et al., 2014; Sinha and Yoong, 2009; Erulkar and Muthengi, 2009), three of which evaluated one of the longest running programs, Apni Beti Apni Dhan (ABAD) in India (Krishnan et al., 2014; Nanda et al., 2014; Sinha and Yoong, 2009), and one evaluated a two-year intervention in Ethiopia (Erulkar and Muthengi, 2009). Only the study on the intervention in Ethiopia was published in a peer reviewed journal. Erulkar and Muthengi (2009) evaluate Berhane Hewane, a program that provided girls with a combination of group formation and study support and offered parents a financial incentive conditional on the girl still being unmarried at the end of the intervention. The authors find a significant effect on marriage; however, with only one control and one treatment community, the quality of the paper is too low for inclusion in our sample. The State of Haryana introduced the ABAD program in 1994 with payments to mothers of girls at the time of their birth. The girl is also given a bond with a guaranteed payout of Rs 25,000 redeemable on her 18th birthday as long as she remains unmarried until that point. Thus the first payments tied to marriage did not take place until 2012. Additional bonuses are linked to the level of education the girl has achieved. Eligibility is limited to the first, second, and third children in a family that is below the poverty line and of specific castes. Because payments tied to marriage were only paid out for the first time in 2012, most studies have only examined the impact of the program on education, not marriage. An exception is Krishnan et al. (2014). However, the methodology used raises serious selection bias concerns. The authors compare ages of marriage for girls born in Haryana with daughters-in-law who have married into Haryana from other states, which did not have the ABAD program. They find that girls from Haryana have a higher age of marriage than daughters-in-law in Haryana who originally came from other states and attribute this difference to

the program. However, girls marrying locally and those marrying into another state are likely to be very different on many dimensions and it is impossible to disentangle the effect of these factors on age of marriage from the impact of the program. Nanda et al. (2014) in contrast compared education outcomes for those eligible and ineligible for the ABAD program. However, they do not state how they determine eligibility. If, for example, eligibility is determined by whether mothers registered their daughter for the ABAD scheme at birth (as is required to claim the marriage contingent payment) then there would be substantial selection bias which would invalidate the comparison.

The highest-quality study of ABAD is Sinha and Yoong (2009). The authors utilize details of the eligibility criteria of the program to predict who is eligible. They use detailed asset information from surveys to predict whether a family is below the poverty line along with information on birth order and caste to determine if a girl is eligible for the program. The authors find no evidence that ABAD increased the likelihood of girls attending school although conditional on first attending, they were more likely to continue. Again there is a concern about differential trends as those eligible girls have lower initial schooling outcomes and thus might have experienced faster growth than ineligible girls.

Empowerment, Life Skills, and Livelihood Programs and Age of Marriage

There were four high-quality studies of the effect of empowerment or livelihood programs on age of marriage or cohabitation or teenage pregnancy (Bandiera et al., 2020; Buehren et al., 2015; Edmonds et al., 2019; Ashraf et al., 2020). One was a large clustered randomized control trial of BRAC's ELA centers in Uganda (Bandiera et al., 2020). Girls in communities with ELA centers were more likely to practice safe sex and were less likely to have had a pregnancy than those in control areas. The BRAC program was developed at the same time and along similar lines to the Kishoree Kontha program evaluated in Bangladesh. One key difference between the studies is context: Bandiera et al. (2020) studies a program in Uganda where marriage and sexual onset are less determined by parents and are more in the control of girls than is the case in Bangladesh. Other differences are that: BRACs ELA centers are permanent while the KK program only lasts for six months; BRAC centers are run by adults while KK was run by peer educators; and BRAC centers provide vocational training while KK provided more general life skills training. Another empowerment program in Tanzania and modeled after the BRAC empowerment program in Uganda did not have significant effects on education or marriage outcomes (Buehren et al., 2015). Additionally, the Girls' Education Programme in Rajastan provided life skills trainings and group mentoring to girls in grade six. The classes were led by social mobilizers, local women who had completed secondary school and trained by the NGO Room to Read. The intervention was primarily meant to increase secondary school completion and life skills. A clustered RCT found significant reductions in school drop outs, but no significant impacts on marriage decisions from participation in the program (Edmonds et al., 2019). A negotiation skills training for 9th grade girls in Zambia increased educational attainment for three years following the program, but had

no significant impacts on teenage pregnancy (Ashraf et al., 2020).

OA.2 Empirical Support for Theoretical Assumptions

OA.2.1 Observability of Social Conservatism

Table OA.1: All variables considered: Share of women (unmarried age 10-17 at program start) for which answers to responses to social conservatism questions differ from those of parents and sisters

	Response differs from	
Variable (girls', parents' means):	parents (%) (N=5100)	sister (%) (N=1404)
Wives should be less educated than men (63%, 63%)	39.12	35.61
Women should be allowed to study as far as they want (96%, 96%)	7.86	7.76
Desired education $\geq$ secondary school (96%, 92%)	8.09	7.62
Max. acceptable marriage age $>$ 20 (69%, 66%)	37.24	34.62
Desired marriage age $>$ 20 (35%, 29%)	34.12	32.98
Women should be allowed to wear make-up (67%, .)	.	31.34
Women should be allowed to wear what they want (49%, .)	.	37.96
Comfortable to discuss education with parents (87%, .)	.	13.96
Comfortable to discuss puberty with parents (64%, .)	.	37.54
Comfortable to discuss marriage timing with parents (40%, .)	.	31.20
Comfortable to discuss marriage choice with parents (35%, .)	.	29.06
Comfortable to discuss dowry with parents (32%, .)	.	26.92
Comfortable to discuss harassment with parents (37%, .)	.	26.35
Ever discussed harassment with parents (18%, .)	.	20.23
Women shouldn't work outside the home (4%, .)	.	4.84
Women should only work in case of emergency (8%, .)	.	10.11
Stops activities during menstruation (11%, .)	.	13.96
Would tell someone about rape (71%, .)	.	33.40

Notes: The table shows the share of women who report differently than either their parents or their closest sister in age in the baseline subsample or baseline household survey.

OA.2.2 Empirical Support for Model Assumptions

Table OA.2: Marriage preferences in our data

	Reprod. health (1)	Social Conservatism (3)
Highest class passed	0.038	
	(0.008)	
Husband desires education		-0.144
		(0.049)
Mean if X=0	0.26744	0.51123
Observations	656	519
FE	Union	Union

Notes: The table shows results from OLS regressions among women in control communities who were unmarried at program start and followed-up at endline, adjusted for baseline characteristics and stratification (see notes to table 1 in the main paper), with Huber-White robust SEs clustered at the community level. Column (1) presents results from a survey of a subsample of women (aged 10 - 17 at baseline) at endline, and column (2) presents results from a survey of a subsample of husbands of the subsample of surveyed women age 10 - 17 at baseline. "Education" is the wife's education at endline, and "Husband desires education" is an indicator that is 1 if the husband reported "Education" as one of three desired wife characteristic. "Reproductive health" is a Kling Mean effects index including whether the woman's first birth was not born at home, no miscarriage or still born, and had no birth complication. "Social Conservatism" is an indicator that is 1 if the husband chose either "Nature", "Character", "Reputation", or "Tradition and Religion" as a desired wife characteristics. Regression (1) also controls for age at first birth and regression (2) also controls for husband's age, education and income.

OA.2.3 Formation of Index

The social conservatism index is the mean across four indices: 1) ever married index (entering positively), 2) married under 18 index (entering positively), 3) still in school index (entering negatively), 4) education index (entering negatively).

To construct each index, we first standardize all variables listed in Table OA.1. We then predict each outcome (ever married, married under 18, still in school, education) by all standardized variables and union fixed effects in the control arm. The individual indices are then:

$$\frac{1}{N}\sum_{i=1}^{N}\frac{\beta_i}{SE_i}var_i, \tag{1}$$

where var_i is the standardized variable, β_i is the coefficient of var_i and SE_i is the clustered standard error of β_i. This gives larger weights to variables that predict the outcomes well and smaller weights to variables that predict the outcome with

noise.

The three variables with the highest weights (and their sign) for each index are:

- ever married: can she comfortably discuss harassment with someone (-), ever discussed harassment at school with parents (+), marriage age desired (-)
- marriage under 18: marriage age desired (-), comfortable to discuss education goals with parents (-)
- still in school: highest education desired (+), comfortable to discuss education with parents (+), desired marriage age (+)
- education: highest education desired (+), desired marriage age (+), comfortable to discuss education with parents (+)

OA.2.4 Knowledge about the Incentive Program in Non-Incentive Communities

Table OA.3: Share of girls who have heard about the incentive

	Treated girl in incentive village	Untreated girl in incentive village	Untreated girl in non-incentive village	
Distance to closest incentive village:			<= 500 meters	> 500 meters
Girl heard about incentive (%)	97.05	79.30	34.52	24.19
SD	16.93	40.55	47.69	42.83
N	509	657	168	1910

Notes: The table shows the mean of girls age 10-17 and unmarried at program start in the women's survey who were followed up at endline and reported at midline having heard about the incentive program. We show means for treated girls in incentive villages (received an incentive pick-up card), untreated girls in incentive villages (did not receive an incentive pick-up card) and untreated girls in non-incentive villages, split up by whether or not the village center was within 500 meters of an incentive village center at baseline.

OA.3 Theory Extensions

OA.3.1 Husband Quality

In the benchmark, we study homogeneous men, to focus on the unobservable heterogeneity of women, as that is what is fundamental to the signaling story. However, in practice, men may also vary in quality. In particular, women who marry later may marry husbands of lower quality. Quality could be embodied by many different characteristics, including employment and probability of divorce. Different measures of quality will have vastly different implications for equilibrium matching.

This adds a substantial layer of complexity to the marriage matching problem. Intuitively, the equilibrium will depend both on the complementarities or substitutability of husband quality in marriage utility, as well as how observable

husband quality is. Without putting quite a bit of additional structure on the problem, it is ambiguous whether lower or higher quality husbands will have a stronger preference for marrying women in t_1 versus t_2. If husband quality is observable, and high-quality husbands are relatively scarce, and husband's and wife's education are substitutes in marriage utility (e.g., in impacts on children), then it's possible that non-preferred type women marry lower quality husbands on average in treatment areas.

OA.3.2 Proof of Corollary 1: There Are No Mixed-Strategy Equilibria in Control Communities

Note that if the same dowry is charged regardless of when a woman enters, then all women prefer to enter later, because they get educational benefit.

So, any mixed-strategy equilibrium would have to involve entering earlier and paying some lower dowry, or entering later and paying some higher dowry (an agent mixes between two strategies if and only if she is indifferent between them).

Because the non-preferred type has a higher marginal gain from getting educated, for any dowries where the preferred type is indifferent between entering at t_1 and t_2, the non-preferred type will strictly prefer the dowry offered for t_2 entrants. So, IF mixed strategies are played, they must only be played by preferred types, with non-preferred types entering at t_2.

But then that means M must believe that t_1 entrants are preferred type for sure.

Suppose Θ_H plays "enter at t_1 with probability $(1-\lambda)$, enter at t_2 with probability λ". Θ_L enters at t_2 for sure. Then beliefs are: $\mu(\Theta_H|t_1) = 1$, $\mu(\Theta_H|t_2) = \frac{\lambda f}{\lambda f+(1-f)}$ (all states are reached with positive probability).

Then D_1, D_2 must satisfy: $\mu(\Theta_H, E_L) - D_1 = \mu(\Theta_H, E_H) - D_2 \Rightarrow D_1 = D_2 - (\mu(\Theta_H, E_H) - \mu(\Theta_H, E_L))$

1. Suppose M competes for women in t_2. What's D_2?

   M's beliefs are $\Pr(\Theta_H|t_2) = \frac{\lambda f}{\lambda f+(1-f)}$, so M's period 2 utility is $\frac{\lambda f}{\lambda f+(1-f)}\mu(\Theta_H, E_H) + \frac{(1-f)}{\lambda f+(1-f)}\mu(\Theta_L, E_H) + D_2$

   Since M compete for women, $D_2 = \omega_M - \left(\frac{\lambda f}{\lambda f+(1-f)}\mu(\Theta_H, E_H) + \frac{(1-f)}{\lambda f+(1-f)}\mu(\Theta_L, E_H)\right)$

   Then $D_1 = \omega_M - \left(\frac{\lambda f}{\lambda f+(1-f)}\mu(\Theta_H, E_H) + \frac{(1-f)}{\lambda f+(1-f)}\mu(\Theta_L, E_H)\right) - (\mu(\Theta_H, E_H) - \mu(\Theta_H, E_L))$

2. Do these dowries satisfy M's participation constraint?

   What is M's utility in t_1 at this D_1, given that $\mu(\Theta_H|t_1) = 1$?

   $$\mu(\Theta_H, E_L) + \omega_M - \left(\frac{\lambda f}{\lambda f + (1-f)}\mu(\Theta_H, E_H) + \frac{(1-f)}{\lambda f + (1-f)}\mu(\Theta_L, E_H)\right) - (\mu(\Theta_H, E_H) - \mu(\Theta_H, E_L))$$

This is smaller than his outside option ω_M iff:

$$\begin{aligned}
&\mu(\Theta_H, E_L) - \left(\frac{\lambda f}{\lambda f + (1-f)}\mu(\Theta_H, E_H) + \frac{(1-f)}{\lambda f + (1-f)}\mu(\Theta_L, E_H)\right) \\
&- (\mu(\Theta_H, E_H) - \mu(\Theta_H, E_L)) < 0 \\
\Leftrightarrow &- \frac{(1-f)}{\lambda f + (1-f)}\mu(\Theta_L, E_H) < (\mu(\Theta_H, E_H) - \mu(\Theta_H, E_L)) \\
&+ \frac{\lambda f}{\lambda f + (1-f)}\mu(\Theta_H, E_H) - \mu(\Theta_H, E_L)
\end{aligned}$$

Sufficient:

$$0 < (\mu(\Theta_H, E_H) - \mu(\Theta_H, E_L)) + \frac{\lambda f}{\lambda f + (1-f)}\mu(\Theta_H, E_H) - \mu(\Theta_H, E_L)$$

This clearly holds since $\frac{\lambda f}{\lambda f+(1-f)} \leq 1 \ \forall \lambda$, so that:

$$\begin{aligned}
&\mu(\Theta_H, E_H) - \mu(\Theta_H, E_L) > 0 \ \& \\
&\mu(\Theta_H, E_H) - \mu(\Theta_H, E_L) > \left|\frac{\lambda f}{\lambda f + (1-f)}\mu(\Theta_H, E_H) - \mu(\Theta_H, E_L)\right|
\end{aligned}$$

But that means that M's utility from this in t_1 is less than his outside option ω_M, which violates his participation constraint. As this is true for all λ, there are no mixed-strategy equilibria because either M has to give a t_1 discount he can't afford, or charge an expensive t_2 dowry that women can't afford.

3. Alternatively, M competes for women in t_1, so that $D_1 = \omega_M - \mu(\Theta_H, E_L)$. To maintain H's indifference, it must be that $D_2 = \omega_M - \mu(\Theta_H, E_L) + (\mu(\Theta_H, E_H) - \mu(\Theta_H, E_L))$. But liquidity constraints mean that D_2 is not feasible (this is the condition for separating equilibrium not being feasible).

OA.3.3 Optimal Policy - Coverage and Transfer Size

Here we characterize the minimum coverage $\tau_{\min}$ necessary (assuming random assignment and given the size of the transfer, C) needed to cause the treated preferred types (and therefore the treated non-preferred types and the untreated non-preferred types) to delay marriage. From the assumption of result 4:

$$\tilde{f}(\tau_{\min}) = \frac{2\mu(\Theta_H, \Theta_L) - [\mu(\Theta_H, \Theta_H) + \mu(\Theta_L, \Theta_H)] - C}{[\mu(\Theta_H, \Theta_H) - \mu(\Theta_L, \Theta_H)]}$$

where $\tilde{f}(\tau) = \frac{\tau f}{\tau f + (1-f)}$ are the beliefs about the probability a woman who delays marriage is a preferred type, given that treated preferred and all non-preferred types delay marriage.

Solving for $\tau_{\min}(C)$, we get:

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

This is decreasing and concave in C. Thus, when transfers are larger, a smaller coverage is needed. (Of course, less coverage implies that the group of untreated preferred types, who continue to marry early, is larger than it would be under more coverage.) Similarly, the more coverage there is, the smaller the transfer that is needed.

Suppose we say there is a social benefit B_H to preferred-type women delaying marriage, and B_L to non-preferred-type women delaying marriage (and 0 if women of either type get married as a child). Then total welfare generated by (coverage, transfer size) is

$$\begin{aligned} & \tau_{\min}(C) f B_H + (1-f) B_L - \tau_{\min}(C) C \\ \frac{\partial}{\partial C} \quad : \quad & \tau'_{\min}(C)[f B_H + C] - \tau_{\min}(C) = 0 \\ \frac{\partial^2}{\partial C^2} \quad : \quad & \tau''_{\min}(C)[f B_H + C] < 0 \end{aligned}$$

So, there is an interior optimal coverage and transfer size—full coverage is not what maximizes total welfare.

If we had estimates for the match utilities depending on the different unobservable desirability and education types ($\mu(\Theta, E)$), as well as the fraction of preferred types f, we could obtain a number for C^*, $\tau(C^*)$.

OA.3.4 Optimal Policy - Non-random and Observable Treatment

1. First observation: if treatment coverage is complete, then observability and randomness are moot. So, suppose treatment coverage is partial.

2. Suppose $\tau < 1$ and treatment is random but fully observable.

   Then, all treated women can delay marriage (depending on size of conditional transfer), but we'd lose the spillovers. The untreated who tried to delay marriage would be known to be non-preferred type (because they have the higher marginal returns to education, under the signaling story and given our observation of mostly pooling on child marriage in the control). We think the parameters are such that husbands do not want to marry known non-preferred types, even if they have education (this must be true if the signaling story underlies pooling on child marriage in the status quo).

   Thus, it could be that our randomized treatment was partially observed in practice, attenuating our spillovers—if it had been totally unobserved, we may have seen even bigger spillovers.

3. If treatment is not random

   (a) Not observable, treated more likely to be Θ_H ($\tau_H > \tau_L$): this strengthens the treatment, in the sense that all treated types delay, but untreated types (both preferred and non-preferred) now have stronger incentive to also delay.

(b) Not observable, treated more likely to be Θ_L: this dampens all women's incentive to delay.

(c) Observable, treated more likely to be Θ_H: strengthens treated women's incentives to delay, but dampens untreated women's incentives to delay (spillovers), as treated being more likely to be Θ_H implies that untreated is more likely to be Θ_L, and treatment status is observable.

(d) Observable, treated more likely to be Θ_L: different contracts for treated and untreated at t_2. Treated at t_2 face more expensive dowries, but still delay because they receive the conditional transfer–treatment status is observable no matter when you enter, so you might as well delay and get $+C$ and education. Untreated at t_2 are offered lower dowries, and this may be low enough to cause even untreated preferred types to prefer delay. The key intuition is that men's beliefs about your unobservable type are now more about your treatment status than when you enter. This makes it possible for potentially all women to delay, even though not all women are treated.

This analysis suggests an interesting policy insight, which is that the "best" policy (if it were possible) could be to observably give more non-preferred than preferred type women the conditional transfer (but not only give it to non-preferred type–there has to be some probability treated are preferred type).

We do not think this is feasible, because we do not think researchers have a special ability to observe this unobservable type.

However, if the concern is that people can sometimes observe treatment status and think that treated women are more likely to be non-preferred type, that actually strengthens the treatment effect. Partial observability and non-randomness (as long as observed treatment is not thought to be only given to non-preferred type) could be most effective.

OA.4 Program Information

OA.4.1 Community Summary Statistics

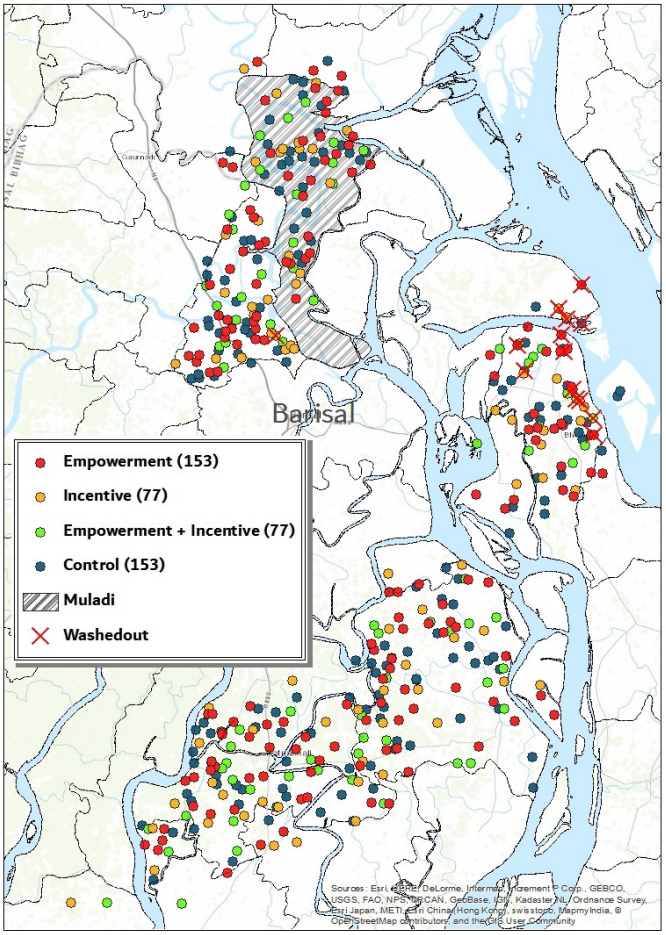
Table OA.4: Community Characteristics (N=460)

	Mean	S.D.
Number of households in community	350.9	205.4
Closest motorable road >1hr away (%)	16.2	36.9
Primary school in community (%)	50.9	50.0
Secondary school in community (%)	24.6	43.1
Community has at least 1 matchmaker (%)	94.6	22.6
3 or more matchmakers in community (%)	70.2	45.8

Notes: The data come from the following sources: Panel A: "Number of households in community" from the baseline parents' survey; "Closest motoroble road>1hr away", "Primary school in community" and "Secondary school in community" from the baseline village leader survey; "Community has at least 1 local matchmaker" and "3 or more matchmakers in community" from the endline matchmaker survey.

OA.4.2 Treatment Region

Figure OA.1: Communities by Treatment Status



Notes: The figure presents the geolocations of all communities included in our study as well as their treatment status. We highlight communities in Muladi, in which rumors spread at midline, as well as washedout communities, in which some households were completely washedout and thus excluded from this study.

OA.4.3 Randomization

Prior to randomization, the list of communities was organized in two steps:

1. Communities were organized by the number of girls age 10 to 19 at baseline.
2. Size tiers of communities were determined and communities then ordered by unionIDs and size tiers, whereby the order of communities within union and size tiers was random.

First, the number of multiples of 6 was determined per union as this was the number of randomizations to be performed per union (each treatment status was

related to one number with the empowerment and control arms being assigned two numbers).

Then, in each union, the treatment status of the first community was randomized. The treatment status of the following communities was assigned in the sequence of 1-6 (e.g., if the first community was randomly assigned treatment 3, the subsequent communities were assigned treatments 4,5,6,1 etc.).

Lastly, all remainder communities in excess of the multiples of 6 were ordered by unions and tiers and the treatment status of the first community assigned by randomization and of all subsequent remainder communities by filling the sequence 1-6.

OA.4.4 Take-Up

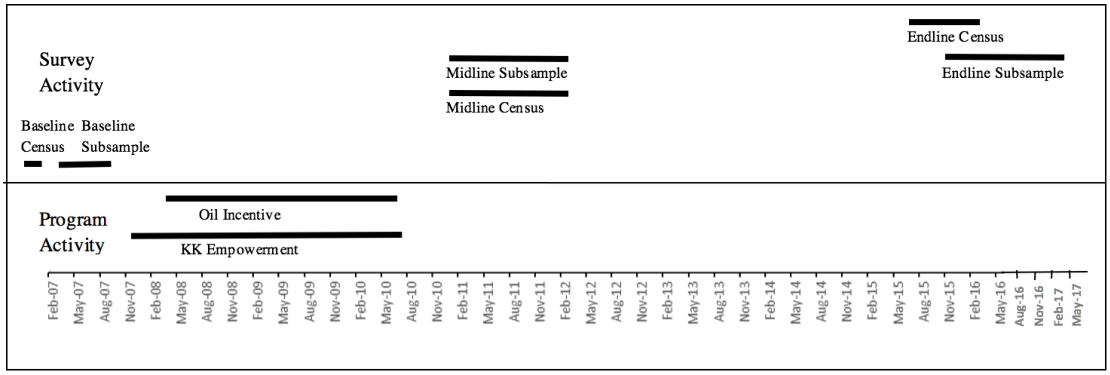
Table OA.5: Take-Up, calculated from monitoring data and self-reported data

Treatment Group	**Empowerment Enrollment** (%)		**Incentive Take-Up Unconditional** (%)		**Incentive Take-Up Cardholders** (%)	
	Admin.	Self-Reported	Admin.	Self-Reported	Admin.	Self-Reported
Empowerment	90.6	49.1	.	0.7	.	.
Incentive	.	21.8	69.5	67.2	90.3	78.2
Empowerment+Incentive	96.7	68.1	75.6	73.5	93.1	84.6
Control	.	12.2	.	0.9	.	.
Any Empowerment	92.7	56.0	.	28.7	.	84.6
Any Incentive	.	45.5	72.7	70.6	91.8	81.7

Notes: To calculate the empowerment take-up from monitoring data, we divide the number of distinct girls on the KK enrollment sheets in each community by the number of eligible girls in each community (age 10-19 at baseline). To calculate the unconditional incentive take-up from monitoring data, we divide the number of distinct girls on the incentive pick-up sheets in each community by the number of eligible girls in each community (age 15-17 and unmarried at program start). The incentive take-up among cardholders is the share of girls who were handed an incentive pick-up card who were listed on the incentive pick-up sheets. The self-reported take-up is the mean of unmarried and eligible girls (age 10-19 at baseline for the empowerment program and age 15-17 at program start for the incentive program) who reported at midline (2011) that they had attended at least 1 KK session, or received the incentive at least once. Reported incentive take-up among cardholders is the reported mean among girls who were handed an incentive pick-up card.

OA.4.5 Program Timeline

Figure OA.2: Program timeline



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

OA.5 Verification of Marriage Age

Table OA.6: Marriage age checks, comparison of marriage age using verified reports and marriage certificates. Women age 15-17 and unmarried at program start

	Empowerment (%)			**Incentive** (%)			**Empow.+Incen.** (%)			**Control** (%)		**Total** (%)	
	Mean	S.D.	Diff.	Mean	S.D.	Diff.	Mean	S.D.	Diff.	Mean	S.D.	Mean	S.D.
Parents' - girls':	2.1	14.8	0.6	1.5	16.7	0.1	2.0	14.4	0.5	1.5	14.3	1.8	14.9
Parents' - certificates:	-2.8	18.9	-5.1	2.4	5.4	0.0	-1.1	6.7	-3.4	2.4	8.9	0.3	12.6
Girls' - certificates:	-7.3	22.7	-2.0	-6.5	28.1	-1.2	-0.6	6.8	4.7	-5.3	19.9	-5.5	21.1

Notes: The table shows the difference in months between different ways to calculate age at first marriage, by treatment arm. For each treatment arm, the differences between surveys are compared to the difference between surveys in the control arm. We collected marriage certificates in a subsample of households in the subsample interviews of young women. We thus have three measures of marriage age: Marriage age as calculated using the date reported on the marriage certificate, marriage age as reported by the parents, and marriage age as reported by the women themselves in a random subsample. No differences from OLS regressions with Huber-White robust SEs clustered at the community level differ significantly by treatment arm.

OA.6 Robustness Checks

OA.6.1 Marriage Outcomes

Table OA.7: Marriage outcomes, unmarried women age 15-17 at program start, excluding controls

	Married<18		Married<16	Ever married at midline		Ever married at endline		Marriage age		Birth<20	
	Age 15-17	Age 15	Age 15	Age 15-17	Age 15	Age 15-17	Age 15	Age 15-17	Age 15	Age 15-17	Age 15
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Empowerment	-0.009	-0.004	0.007	0.012	0.015	0.005	0.001	0.022	-0.007	0.005	0.006
	(0.008)	(0.016)	(0.009)	(0.012)	(0.017)	(0.008)	(0.012)	(0.043)	(0.068)	(0.008)	(0.013)
Incentive	-0.047	-0.072	-0.019	-0.018	-0.049	-0.005	-0.016	0.214	0.316	-0.014	-0.037
	(0.011)	(0.020)	(0.012)	(0.013)	(0.019)	(0.011)	(0.016)	(0.060)	(0.082)	(0.010)	(0.017)
Incen.*Empow.	0.020	0.018	-0.007	-0.019	-0.010	-0.005	0.000	-0.062	-0.054	-0.001	0.004
	(0.016)	(0.028)	(0.017)	(0.020)	(0.028)	(0.014)	(0.022)	(0.085)	(0.123)	(0.016)	(0.025)
Control Mean	0.29324	0.38495	0.11262	0.45829	0.41492	0.83701	0.81990	18.96879	18.29337	0.24092	0.32554
Observations	15549	5861	5861	14891	5604	15562	5864	12993	4773	15494	5847
FE	Union	Union	Union	Union	Union	Union	Union	Union	Union	Union	Union

Notes: The table shows results from OLS regressions with Huber-White robust SEs clustered at the community level. Columns (1)-(3) and columns (6)-(11) present results from the endline parents' survey and columns (4)-(5) show results from the midline parents' survey. The sample includes all women age 15-17 and unmarried at program start. The sample excludes washedout households as well as households with insufficient tracking data. "Empowerment" is an indicator that is 1 if the woman lived in any of the empowerment communities (empowerment only or empowerment plus incentive) and "Incentive" is an indicator that is 1 if the woman lived in any of the incentive communities (incentive only or empowerment plus incentive). The regressions control for strata (whether the community is in the first, second, or third tercile in terms of number of houses in the community and union fixed-effects).

Table OA.8: Marriage outcomes, women age 15-17 at program start and unmarried at baseline, including women married before program start

	Married<18		Married<16	Ever married at midline		Ever married at endline		Marriage age		Birth<20	
	Age 15-17	Age 15	Age 15	Age 15-17	Age 15	Age 15-17	Age 15	Age 15-17	Age 15	Age 15-17	Age 15
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Empowerment	-0.009	-0.005	0.009	0.009	0.010	0.003	-0.002	0.023	-0.005	0.001	0.007
	(0.008)	(0.014)	(0.011)	(0.010)	(0.015)	(0.007)	(0.010)	(0.046)	(0.075)	(0.008)	(0.012)
Incentive	-0.064	-0.080	-0.031	-0.030	-0.058	-0.020	-0.027	0.312	0.370	-0.032	-0.041
	(0.011)	(0.018)	(0.015)	(0.011)	(0.016)	(0.010)	(0.014)	(0.065)	(0.098)	(0.010)	(0.016)
Incen.*Empow.	0.010	0.006	-0.029	-0.007	-0.001	-0.003	0.005	0.031	0.093	-0.005	-0.015
	(0.016)	(0.025)	(0.020)	(0.016)	(0.023)	(0.013)	(0.020)	(0.090)	(0.140)	(0.014)	(0.023)
Control Mean	0.40523	0.45346	0.21349	0.56008	0.49913	0.86251	0.84031	18.15230	17.63943	0.33023	0.38706
Observations	18668	6698	6698	18176	6491	18681	6701	15995	5565	18604	6679
FE	Union	Union	Union	Union	Union	Union	Union	Union	Union	Union	Union

Notes: The table shows results from OLS regressions, adjusted for baseline characteristics and stratification (see notes to table 2 in the main paper), with Huber-White robust SEs clustered at the community level. Columns (1)-(3) and columns (6)-(11) present results from the endline parents' survey and columns (4)-(5) show results from the midline parents' survey. The sample includes all women age 15-17 at program start and unmarried at baseline (we do not drop women married before the program start). The sample excludes washedout households as well as households with insufficient tracking data. "Empowerment" is an indicator that is 1 if the woman lived in any of the empowerment communities (empowerment only or empowerment plus incentive) and "Incentive" is an indicator that is 1 if the woman lived in any of the incentive communities (incentive only or empowerment plus incentive).

Table OA.9: Marriage outcomes, unmarried women age 15-17 at program start, correcting for assignment errors

	Married<18		Married<16	Ever married at midline		Ever married at endline		Marriage age		Birth<20	
	Age 15-17	Age 15	Age 15	Age 15-17	Age 15	Age 15-17	Age 15	Age 15-17	Age 15	Age 15-17	Age 15
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Empowerment	-0.007	-0.007	0.006	0.011	0.009	0.006	0.002	0.013	0.012	0.005	0.004
	(0.008)	(0.015)	(0.009)	(0.011)	(0.017)	(0.008)	(0.012)	(0.040)	(0.064)	(0.007)	(0.012)
Incentive	-0.060	-0.090	-0.024	-0.030	-0.064	-0.012	-0.022	0.261	0.394	-0.020	-0.048
	(0.012)	(0.023)	(0.014)	(0.015)	(0.023)	(0.013)	(0.019)	(0.063)	(0.096)	(0.011)	(0.019)
Incen.*Empow.	0.025	0.037	-0.001	-0.012	0.011	-0.002	0.007	-0.070	-0.122	-0.002	0.016
	(0.017)	(0.031)	(0.019)	(0.022)	(0.030)	(0.017)	(0.025)	(0.089)	(0.139)	(0.017)	(0.027)
Control Mean	0.29324	0.38495	0.11262	0.45829	0.41492	0.83701	0.81990	18.96879	18.29337	0.24092	0.32554
Observations	15549	5861	5861	14891	5604	15562	5864	12993	4773	15494	5847
FE	Union	Union	Union	Union	Union	Union	Union	Union	Union	Union	Union

Notes: The table shows results from 2SLS regressions (instrumenting being listed on an incentive list by program assignment), adjusted for baseline characteristics and stratification (see notes to table 2 in the main paper), with Huber-White robust SEs clustered at the community level. Columns (1)-(3) and columns (6)-(11) present results from the endline parents survey and columns (4)-(5) show results from the midline parents survey. The sample includes all women age 15-17 and unmarried at program start. The sample excludes washedout households as well as households with insufficient tracking data. "Empowerment" is an indicator that is 1 if the woman lived in any of the empowerment communities (empowerment only or empowerment plus incentive) and "Incentive" is an indicator that is 1 if the woman was handed an incentive pick-up card, it is instrumented by assignment to the incentive treatment. "Empow.*Incen." is the interaction between assignment to the empowerment treatment and being handed an incentive pick-up card, it is instrumented by the interaction of assignment to the empowerment treatment and assignment to the incentive treatment.

OA.6.2 Education Outcomes

Table OA.10: Education outcomes, unmarried women age 15-17 at program start and in school at baseline, excluding controls

	In school at midline		In school at endline		Last class passed		Secondary complete	
	Age 15-17	Age 15	Age 15-17	Age 15	Age 15-17	Age 15	Age 15-17	Age 15
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Empowerment	0.012	0.016	0.019	0.028	0.226	0.222	0.021	0.022
	(0.013)	(0.020)	(0.012)	(0.017)	(0.099)	(0.134)	(0.013)	(0.019)
Incentive	0.023	0.084	0.016	0.048	0.082	0.244	0.013	0.038
	(0.015)	(0.024)	(0.017)	(0.023)	(0.133)	(0.208)	(0.018)	(0.026)
Incen.*Empow.	0.022	-0.004	0.011	-0.017	-0.080	-0.183	-0.005	-0.019
	(0.023)	(0.032)	(0.023)	(0.031)	(0.198)	(0.264)	(0.026)	(0.036)
Control Mean	0.46585	0.48183	0.27990	0.27760	11.33658	10.83291	0.44360	0.40641
Observations	10226	4272	10930	4545	10857	4518	10857	4518
FE	Union	Union	Union	Union	Union	Union	Union	Union

Notes: The table shows results from OLS regressions with Huber-White robust SEs clustered at the community level. Columns (1)-(2) present results from the midline parents survey and columns (3)-(8) show results from the endline parents survey. The sample includes all women age 15-17 and unmarried at program start and in school at baseline. The sample excludes washedout households as well as households with insufficient tracking data. "Empowerment" is an indicator that is 1 if the woman lived in any of the empowerment communities (empowerment only or empowerment plus incentive) and "Incentive" is an indicator that is 1 if the woman lived in any of the incentive communities (incentive only or empowerment plus incentive). The regressions control for strata (whether the community is in the first, second, or third tercile in terms of number of houses in the community and union fixed-effects).

Table OA.11: Education outcomes, women age 15-17 at program start and and unmarried and in school at baseline, including women married before program start

	In school at midline		In school at endline		Last class passed		Secondary complete	
	Age 15-17	Age 15	Age 15-17	Age 15	Age 15-17	Age 15	Age 15-17	Age 15
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Empowerment	0.008	0.008	0.017	0.028	0.181	0.213	0.016	0.021
	(0.011)	(0.018)	(0.011)	(0.015)	(0.090)	(0.124)	(0.012)	(0.017)
Incentive	0.033	0.089	0.028	0.046	0.097	0.168	0.018	0.031
	(0.014)	(0.021)	(0.015)	(0.019)	(0.121)	(0.184)	(0.015)	(0.022)
Incen.*Empow.	0.010	0.001	0.007	-0.014	-0.019	-0.038	-0.001	-0.004
	(0.020)	(0.029)	(0.021)	(0.028)	(0.180)	(0.242)	(0.024)	(0.032)
Control Mean	0.39866	0.42934	0.25398	0.25858	11.06249	10.57974	0.40790	0.37766
Observations	11895	4779	12406	4998	12330	4971	12330	4971
FE	Union	Union	Union	Union	Union	Union	Union	Union

Notes: The table shows results from OLS regressions, adjusted for baseline characteristics and stratification (see notes to table 2 in the main paper), with Huber-White robust SEs clustered at the community level. Columns (1)-(2) present results from the midline parents survey and columns (3-(8) show results from the endline parents survey. The sample includes all women age 15-17 at program start and unmarried and in school at baseline (we do not drop women married before the program start). The sample excludes 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).

Table OA.12: Education outcomes, unmarried women age 15-17 at program start and in school at baseline, correcting for assignment errors

	In school at midline		In school at endline		Last class passed		Secondary complete	
	Age 15-17	Age 15	Age 15-17	Age 15	Age 15-17	Age 15	Age 15-17	Age 15
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Empowerment	0.012	0.014	0.014	0.023	0.179	0.174	0.016	0.018
	(0.013)	(0.019)	(0.011)	(0.016)	(0.092)	(0.123)	(0.012)	(0.017)
Incentive	0.036	0.102	0.028	0.060	0.156	0.298	0.024	0.047
	(0.018)	(0.025)	(0.018)	(0.025)	(0.141)	(0.219)	(0.019)	(0.028)
Incen.*Empow.	0.015	-0.011	0.007	-0.024	-0.089	-0.206	-0.008	-0.022
	(0.026)	(0.034)	(0.025)	(0.034)	(0.209)	(0.278)	(0.028)	(0.038)
Control Mean	0.46585	0.48183	0.27990	0.27760	11.33658	10.83291	0.44360	0.40641
Observations	10226	4272	10930	4545	10857	4518	10857	4518
FE	Union	Union	Union	Union	Union	Union	Union	Union

Notes: The table shows results from 2SLS regressions (instrumenting being listed on an incentive list by program assignment), adjusted for baseline characteristics and stratification (see notes to table 2 in the main paper), with Huber-White robust SEs clustered at the community level. Columns (1)-(2) present results from the midline parents survey and columns (3)-(8) show results from the endline parents survey. The sample includes all women age 15-17 and unmarried at program start and in school at baseline. The sample excludes washedout households as well as households with insufficient tracking data. "Empowerment" is an indicator that is 1 if the woman lived in any of the empowerment communities (empowerment only or empowerment plus incentive) and "Incentive" is an indicator that is 1 if the woman was handed an incentive pick-up card, it is instrumented by assignment to the incentive treatment. "Empow.*Incen." is the interaction between assignment to the empowerment treatment and being handed an incentive pick-up card, it is instrumented by the interaction of assignment to the empowerment treatment and assignment to the incentive treatment.

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