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Age at marriage, women's education, and mother and child outcomes in Bangladesh

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Age at marriage, women's education, and mother and child outcomes in Bangladesh

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Summary

This report evaluates the impact of an empowerment training program and a financial incentive program to reduce child marriage in rural Bangladesh. Between January 2007 and September 2015 we ran a clustered randomized trial in 460 villages in collaboration with Save the Children USA to examine alternative strategies to reduce child marriage and increase girls' education. Villages were randomly allocated into four arms to receive a basic empowerment program; a financial incentive to delay marriage; empowerment plus incentive; and the status quo using a stratified randomized design in the ratio 2:1:1:2. In communities randomized to receive the empowerment program, all girls aged 10–19 were eligible to take part in any of four cycles that ran between December 2007 and August 2010. In financial incentive communities, girls aged 15–17 at program launch were eligible to receive the financial incentive every four months from April 2008 to August 2010 until the age of 18 if they remained unmarried. In all study villages, we attempted to resurvey all households with girls aged 15–17 at program launch 1.5 years after program completion (January 2011 to March 2012) and 4.5 years after program completion (May to September 2015).

The main outcomes of interest are child marriage, defined as whether a girl married before age 18, age 17, and age 16, marriage age, whether married and whether in school at midline and endline. Overall, the financial incentive reduced the likelihood of being married under age 18 by 22% (8.7ppts, $p<0.01$). For girls aged 15 at program start, the likelihood of being married under age 16 fell by 23% (4.9ppts, $p<0.05$) in the incentive only group and 29% (6.2ppts, $p<0.01$) in the incentive plus empowerment group. The incentive to delay marriage also has a large positive impact on school enrollment, and we observe a significant dose response to the program: Girls in the incentive only group who were 15 at program launch were 25% (8.5ppts, $p<0.01$) more likely to be in school at midline and 25% (6.8ppts, $p<0.01$) more likely at endline. In the empowerment plus incentive arm, the coefficients are +9.4ppts ($p<0.01$), or 28%, at midline, and +8.1ppts ($p<0.1$), or 30%, at endline. While the empowerment program had no significant effects on marriage outcomes, there is some evidence to suggest that it improves schooling outcomes; girls in the empowerment-only arm who were aged 15 at program launch were 12% more likely to be in school (3.3ppts, $p<0.05$) at endline.

In addition, we evaluate the impact of the financial incentive on various reproductive health outcomes. We find that the financial incentive reduces the likelihood of having given birth at endline by 6% (3.9ppts, $p<0.05$) and increases the age at first birth amongst married girls by 2.6 months (0.23 years, $p<0.05$). The empowerment plus financial incentive decreases the likelihood of having given birth by 5% (3.3ppts, $p<0.05$) and the likelihood of having given birth before age 18 by 24% (2.7ppts, $p<0.01$). There is evidence of stronger reproductive outcomes for younger girls (aged 15 at program start) if they received both empowerment and incentive. We do not observe a separate or additional effect of the empowerment program alone.

We compare the financial incentive with five other interventions that had child marriage or marriage age as an outcome, and find the oil incentive to have a net present value (NPV) per US$1,000 invested that is higher than the NPV of any other program studied. Our outcomes suggest that a relatively small financial incentive is effective in delaying marriage and reducing child marriage in an environment in which minimum age

requirements and dowry prohibitions have proven ineffective in combating child marriage. Given the strong correlations between child marriage and long-term outcomes for girls and their children – such as poor education and health risks – we think this is an important finding. Much of the recent policy focus has been on empowerment programs for adolescents, which we find to be ineffective on marriage outcomes. By contrast, we show that noncash transfers conditional on marriage are not only more effective, but also more cost-efficient than other approaches to delaying child marriages. Our results may thus significantly change the current policy debate on child marriage. The schooling attainment results provide novel evidence of the causal effect of child marriage on schooling attainment, which has not been shown experimentally in previous research. Our results suggest that reducing child marriage would have a substantial impact on girls' schooling attainment. There is also some evidence that empowerment alone can improve schooling attainment.

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Contents

List of figures and tables

Abbreviations and acronyms

2SLS	two-stage least squares
ACPR	Associates for Community and Population Research
ASER	Annual Status of Education Report
BDHS	Bangladesh Demographic and Health Survey
BDS	Bangladesh Development Society
BDT	Bangladeshi *taka*
DPS	deposit pension scheme
ELA	Empowerment and Livelihood for Adolescents Program
FSSAP	Female Secondary School Assistance Project
J-PAL	Abdul Latif Jameel Poverty Action Lab
JOJ	Jibon-O-Jibika program
KK	Kishoree Kontha Adolescent Girls' Empowerment Program
NPV	net present value
OLS	ordinary least squares
PACES	Programa de Ampliación de Cobertura de la Educación Secundaria
SC	Save the Children USA
TBA	trained birth attendant
ToT	treatment on treated
UCT	unconditional cash transfer

1. Background and objectives of study

Adolescent girls in Bangladesh drop out of school at high rates, experience poor health, face restricted mobility and lack the ability to influence key decisions on their marriage and family planning. Rural areas produce worse education and health outcomes than urban areas. According to the 2014 Bangladesh Demographic and Health Survey (BDHS), only 25% of all Bangladeshi girls between the ages of 15 and 19 had completed their secondary education. Rural completion rates were one third of urban rates for all women. Bangladesh is believed to have one of the highest rates of adolescent and child marriage in the world. Despite the fact that the legal age of marriage is 18, the BDHS shows that 59% of women between ages 20–24 were married before 18. About 31% of adolescents aged 15–19 had begun childbearing, and this number is higher in rural areas (32%) than in urban areas (27%). These adolescent mothers face greater health risks associated with lower age of first birth, higher fertility rates and shorter birth spacing. Adolescent girls' access to reproductive health care and services is also poor. Only 51% of married girls aged 15–19 report using any contraception, compared to the average of 62% for all age groups. The need for family planning, including birth spacing and limiting of births, is not met for 17% of women aged 15–19, compared with 12% for all married women.

Table 1: Education and reproductive health outcomes for adolescent girls in Bangladesh

	Age group	BDHS 2007 (%)	BDHS 2011 (%)	BDHS 2014 (%)
Completed secondary education	15–19	10	19	25
Married before age 18	20–24	66	65	59
Begun childbearing	15–19	33	30	31
Used contraception	15–19	42	47	51
Gave birth at home	<20	86	71	64

Source: Bangladesh Demographic and Health Surveys, 2007, 2011 and 2014.

Early marriage, limited education and limited access to resources for women are highly correlated with each other, and with poor health outcomes, for young women and their children. A lack of their own income and financial planning skills has the potential to reduce bargaining power for women within the household as well as investment in their children, affecting future generations. However, there are several challenges in determining the most effective ways of empowering adolescent girls and improving their health. First, without exogenous sources of variation in many closely correlated factors, it is difficult to establish the separate causal role of each in determining the poor outcomes observed in adolescents. Second, it is possible that other factors, such as restrictive cultural norms, are instead, the main drivers observed in cross-sectional correlations. This study was designed to address these questions and help shed light on the most effective ways to empower adolescents and improve maternal and child health with key implications for policy decisions in developing countries.

Researchers at Save the Children US (SC), the Abdul Latif Jameel Poverty Action Lab (J-PAL) at the Massachusetts Institute of Technology, Innovations for Poverty Action and Duke University designed and implemented the evaluation of the Kishoree Kontha (KK) Adolescent Girls' Empowerment Program to:

1. Examine and disentangle the separate causal impacts of girls' education, age of marriage and control over resources, on adolescent, maternal and child health, and health-service utilization.
2. Evaluate the effect of conditional in-kind transfers on female marriage age, marriage outcomes, and health and well-being of girls and their children.
3. Understand which elements or combinations of programs are most effective in empowering adolescent girls.

In this report, we discuss the impact of the different interventions on outcomes that are observable in the short and long-term by analyzing midline as well as endline results. Key outcomes are age of marriage and education. In addition, using midline data we examine intermediate indicators such as girls' negotiation and decision-making skills, awareness and knowledge (including knowledge of contraception), attitudes (including gender attitudes), savings, mobility, income generation activities, along with education and marriage outcomes for girls aged 15–17 at program launch. Analysis uses data from the midline evaluation as well as data from the endline census where available, as endline surveying in a more detailed subsample is still ongoing. Throughout the report, we use the terminology "in-school" and "enrolled" to describe girls who still participate in the education system. All results – if not stated differently – are reported for girls who were still unmarried at baseline and thus eligible for all treatments.

2. Literature review

There is substantial literature showing a correlation between early marriage and women's health, and health-seeking behavior. In general, women who marry early begin childbearing at a young age (Jensen and Thornton 2003), and complications in pregnancy and delivery are a leading cause of death among girls aged 15–19. Maternal mortality in this group is double the rates for women in their twenties. Girls who marry as adolescents face greater health risks associated with lower age of first birth, higher fertility rates and shorter birth spacing related to lower contraceptive use (UNICEF 2001).

Childbearing during adolescence, when physiology is likely to be underdeveloped, is widely believed to result in higher levels of maternal mortality and morbidity – although the degree to which age influences reproductive outcomes is not well established. Girls aged 14 and younger are five times as likely to die from pregnancy complications, and their offspring are also less likely to survive (UNFPA 2004). Young mothers also have higher maternal morbidity rates, including severe complications such as obstructed labor or obstetric fistula, which occur primarily among young women (UNFPA and EngenderHealth 2003; Jarrett 1994).

In addition to the physiological channels, early marriage may also impact health through behavioral channels. First, youth is associated with less active health-seeking behavior and limited health information, which has a negative impact on the health status of married adolescent girls. In Bangladesh, 70% of pregnant girls younger than 20 receive no antenatal care, and 90% deliver their babies at home. Their access to health

information is poor: 20% of adolescent mothers have little knowledge of life-threatening conditions during pregnancy, and the majority (married and unmarried) have no information on sexuality, contraception or sexually transmitted infections or HIV and AIDS (Haider et al. 1997; Nahar and Khan 1999; Barkat et al. 2000; Bruce and Clark 2004).

In addition, younger girls tend to marry significantly older men. Research in Sub-Saharan Africa found that the husbands of girls aged 15–19 are on average 10 years older (UNICEF 2001). Mean spouse age difference is decreasing with women's age at first marriage throughout the world. In West Africa, the mean spouse age difference is 12 years for girls aged 14–15 at first marriage, and 8 years for women married at 24–25 years. The same pattern is found in southern Asia (UNFPA 2004). The presence of a large age gap between spouses can contribute to poor outcomes in a number of ways. First, older husbands tend to be more sexually experienced, which implies greater risk of sexually transmitted infection (Clark 2004; Luke and Kurz 2002). The age gap is also associated with lack of agency in marriage for the adolescent girl, which may contribute to poor health outcomes. Lack of decision-making power may translate into lower reproductive control, or capacity to negotiate sexual relations, contraception and childbearing.

There is qualitative but little rigorous analysis suggesting that isolation, restricted mobility and lack of control over household resources are more common among young married girls (Mensch et al. 1998). Isolation and the increased stress of adult responsibilities may have a direct detrimental impact on psychological health. Lack of mobility is also likely to contribute to low health-care utilization among married adolescent girls. Research in India has documented that married adolescent girls' health-care decisions are mostly controlled by husbands and mothers-in-law (Barua and Kurz 2001). Taken together with restricted mobility, this may limit the ability of adolescent girls to access health services for themselves and their children.

The negative association between early marriage and health extends to the next generation. In Bangladesh, the infant mortality rate is 42 per 1,000 births for infants born to mothers under 20, compared to 34 for mothers aged 20–29. The under-5 mortality rate is 48 per 1,000 for children of mothers under 20, compared to 42 for children born to mothers aged 20–29 (NIPORT, Mitra and Associates and IFC International 2016). How much of this correlation is due to lower utilization of health care (e.g. lower immunization rates) or less knowledge of good health practices by mothers on the part of children is unclear.

There is also substantial evidence of a correlation between education and health-service utilization and health outcomes. Controlling for income, assets, location and community characteristics, women's schooling is positively correlated with lower fertility and lower infant and child mortality (Rosenzweig and Schultz 1982; Berhman 1990; Strauss and Thomas 1995). Moreover, the correlation is generally stronger for mother's (compared with father's) education. Malhotra and others (2003) review a number of studies that show a strong association between women's education and health-seeking behavior while controlling for likely confounding factors such as location (rural versus urban) and socioeconomic status. Educated women are more likely to use antenatal care, to use it early and frequently, and to use trained providers and medical institutions. They are

more likely to have a safe delivery (most often defined by whether or not a delivery was conducted by a trained attendant), and to use postnatal care. Educated women, especially those with higher education, are more likely to seek care for certain reproductive health problems such as acute pelvic inflammatory disease and anemia (LeVine et al. 1991; Obermeyer et al. 1991; Elo 1992; Bhatia and Cleland 1995; Govindasamy 2000; Beegle et al. 2001; Bloom et al. 2001).

Furthermore, in Bangladesh, mothers' education is positively associated with childrens' likelihood of being fully vaccinated; 95% of children of mothers who completed secondary or higher education are fully vaccinated, 88% of mothers who have some secondary education, 76% of mothers who have completed primary school, 75% of mothers who have some primary education and 74% of mothers with no education (NIPORT, Mitra and Associates and IFC International 2016). Mothers' level of education is also inversely related to children's risk of dying. Higher levels of educational attainment are generally associated with lower mortality risks, since education exposes mothers to information about better nutrition, use of contraception to limit and space births, and childhood illnesses and their treatment. The 2014 BDHS shows that under-5 mortality declines sharply with increased level of mothers' education; the rate is almost 50% lower for children whose mothers have completed secondary education, compared with those who have no education. Also, a child's chance of dying in neonatal and postneonatal periods is much lower when the mother has completed secondary education (NIPORT, Mitra and Associates and IFC International 2016).

There is also limited causal evidence indicating that households have fewer children when the wife is more educated. In the 1970s, Indonesia completed a large-scale school construction program, generating variation in the differences in schooling between husbands and wives based only on their region and date of birth. Using this variation, Breierova and Duflo (2004) found that conditional on the household's average education, households have fewer children when the wife is more educated. This suggests that relative education matters for women's health, in this case, through fertility choices.

While these studies suggest that women who marry young have lower education, less access to resources, worse health outcomes and less healthy children, the many and complex interrelations among these different factors mean that it is difficult to disentangle their separate causal effects.

Traditional customs that sanction adolescent marriage are widely blamed for girls' limited schooling achievement, as they are thought to raise the opportunity cost of educating girls: "The pressure for early marriage remains a powerful force that shapes the alternatives girls have and constrains their access to secondary education" (Mahmud 2003). Similarly, early marriage is also likely to limit the earning capacity of women by reducing their education, work experience before marriage and ability to work outside the home while married. If women who marry later contribute more to household income, the improvement in income alone should raise the health outcomes of all family members.

The vast majority of the studies mentioned above are cross-sectional correlation studies and thus unable to rigorously examine the separate causal effects of early marriage, education and own resources on women and their children's health and well-being. Nor can they rule out the possibility that other unobserved factors – such as cultural attitudes,

or girls' self-esteem – may be driving the observed correlation between early marriage, low educational attainment, limited access to resources, and poor maternal and child health. Insofar as these unobserved factors impact health, the correlations cannot be interpreted as causal. If the relationship is not causal, then interventions that increase women's marriage age, education or resources without influencing the unobserved factors would not necessarily have the anticipated effect on health.

Few studies have gone beyond simple correlations to examine causal pathways. Exceptions are a study on the link between women's education and fertility in Indonesia (Breierova and Duflo 2004) and another in Taiwan (Chou et al. 2010). However, these studies are only able to look at a few outcomes related to health. Another exception is a study by Field and Ambrus (2008) in Bangladesh, which shows how postponing marriage by one year between the ages of 11 and 16 increases schooling by 0.3 years and literacy by 6.5%. However, there are no such studies showing the causal effect of early marriage on health and own income on health.

A number of studies have shown that reducing the cost of education or providing positive incentives for students to stay in school are effective in increasing education and can also reduce teenage pregnancy or cohabiting (Angrist and Lavy 2009; Schultz 2004; Baird et al. 2011; Duflo et al. 2015; Alam et al. 2011; Hong and Saar 2012; Hahn et al. 2015; Heath and Mobarak 2015, Angrist et al. 2006). Bandiera and others (2012) also show that girls' clubs in Uganda, which in many ways are similar to the safe spaces evaluated in this study but which focus on older girls and include vocational training, were effective in reducing teenage pregnancy. Another program in Tanzania modeled after the empowerment program in Uganda did not have significant effects on education or marriage outcomes (Buehren et al. 2015). However, there is little evidence of the effectiveness of alternative approaches to reducing child marriage. There is also no existing experimental evidence on the impact of delaying marriage on schooling outcomes.

3. Role of the funding source

The study was funded by the International Initiative for Impact Evaluation (3ie), the US National Institutes of Health, the Nike Foundation, and the International Development Research Center. The funders had no role in study design, data collection, analysis or interpretation of the results.

4. Kishoree Kontha girls empowerment program

The girls' empowerment program we evaluate was implemented in five sub-districts (Babuganj, Muladi, Patuakhali Sadar, Bauphal and Bhola Sadar), in three coastal districts (Barisal, Patuakhali and Bhola), in the south central region of Bangladesh by SC. It had two main parts: the KK program and the conditional stipend program. The three districts were chosen because SC was already operating a food security program (Jibon-O-Jibika, or JOJ) in the area, which meant KK staff could share office space and logistical support with the existing program. JOJ provided supplemental food for pregnant and lactating women and undertook growth monitoring of young children. The three districts were chosen for the JOJ program because of the high levels of

malnutrition in the area. Other than some logistical support there was relatively little overlap between the JOJ program and the KK program.

4.1 Kishoree Kontha program

SC implemented the KK program, which consisted of two interventions for girls 10–19 years old. The first intervention was made up of three components: community mobilization, education and social competency. Community mobilization aimed to inform parents, teachers and community leaders about the activities and potential benefits of the project and to mobilize their support for the project. A central objective of this component was to induce the community to volunteer locations for “safe spaces”: meeting places where girls, whose mobility is often restricted, can meet, socialize and receive training. Before program implementation, all community members were invited to initial meetings in which the safe spaces were selected. These spaces, which were designed to fit around 20 girls, were typically the front porch of a house, although they were sometimes a classroom in a local school. Providing safe spaces has been advocated as a way of increasing socialization of adolescent girls who may be isolated by restrictions on their mobility and to do so in a way that is acceptable to their parents. The second component, education, aimed to enhance the basic literacy, numeracy and oral communication of adolescent girls. School-attending girls were given study support (e.g. homework help, study skills). Non-literate girls and girls who were no longer in school received basic literacy and numeracy training. The third part, social competency, trained girls on life skills, and nutritional and reproductive knowledge, via a curriculum designed by SC. Girls role-played negotiation techniques and practiced speaking and telling stories in front of others. Groups were encouraged to organize activities in their communities, including disaster preparedness days that went over safety protocols for the community in the event of a cyclone.

A second intervention, financial and livelihood competencies training, was layered on top of the first intervention in some communities and aimed to enhance the ability of adolescent girls to generate their own income, take advantage of microcredit opportunities and manage their own resources effectively. A random sample of these communities was selected to receive an additional intervention designed to promote savings through savings groups. The savings intervention was introduced approximately halfway through the rollout of the program. Promoting savings groups and income generation opportunities was designed to help girls have access to some limited resources that they could tap into for health, education and other needs.

For each safe space, a target of 20 girls and 2 peer educators per component were selected. Each peer educator, typically an older girl, was trained on her component for between 16 and 40 hours. Once training was completed, peer educators would start delivering the curriculum with the aid of specially-designed curriculum books that included stories to be read to the girls, questions to be discussed and participatory activities to perform. Safe space groups were designed to meet five or six days a week for two hours each day, after deciding on a time that was suitable for the girls.

The curriculum was designed to last six months, after which the group would decide whether to continue to meet and how to structure their meetings going forward. At the end of the cycle, the field staff would return to the community to conduct further

community mobilization activities and select new safe spaces. Then, new peer educators were selected and trained and new peer groups were formed. By the end of four six-month cycles, KK aimed to reach 90% of adolescent girls in the target communities.

The implementation of the program was managed by the Education and Reproductive Health Teams in SC's Bangladesh office, in cooperation with two local NGOs, Padakhep and the Bangladesh Development Society (BDS). (Initially, Padakhep and BDS implemented the program in distinct geographic areas; after the first round of the program, BDS took over the implementation in the entire KK area.)

With the exception of community mobilization, all interventions were peer-led. At the time, KK was one of the largest adolescent empowerment programs of its kind ever implemented in the developing world. Similar programs have been and are being implemented by other nongovernmental organizations in Bangladesh and in other countries – including a large BRAC program in Bangladesh, Uganda, Tanzania and Sierra Leone, Freedom from Hunger programs in India and a similar program run by SC in Malawi.

4.2 Conditional financial incentive program

KK staff were influential in designing and helping to administer the conditional stipend program, which used the existing setup of another SC program in the area, JOJ (a food security and livelihoods program implemented in the target areas). The conditional stipend program was designed to test whether such a program might be a complement to empowerment programs like KK, for example by keeping girls unmarried long enough to benefit from the KK program. Conceptually, the proposed incentive builds on the Bangladesh government's then-national Female Secondary School Assistance Project (FSSAP) stipend, albeit with important departures. The criteria for receiving the government stipend changed in 2010, but previously the FSSAP gave a small stipend to girls provided they maintained specific attendance and achievement levels at school and that they remained unmarried until the legal age of marriage (18). FSSAP did not reach out-of-school girls and did not provide any outside help to ensure girls met the conditions for receiving the stipend. In the KK program area, many girls were both out of school and unmarried. Providing these girls and their families with incentives to delay marriage could have a significant impact on the girls' well-being.

The incentive was an in-kind transfer designed to encourage parents not to marry their adolescent daughters before the legal age of consent (18). In Bangladesh, the amount of dowry that the bride's parents transfer to the groom's family increases with the age of the girl. One factor in parents' decisions to marry their daughters young may be to reduce this cost. Demographers have estimated that, for each year that a girl in rural Bangladesh remains unmarried, the dowry cost increases by 1,000 Bangladeshi *taka* (BDT) or US$15 (in 2008 dollars, Population Council 2004). Therefore, the proposed oil incentive included four liters of cooking oil, delivered to girls three times a year for an approximate value of US$16 per year per girl (in 2008 US dollars). Cooking oil was chosen because it is valued by households and, because of its high monetary value per ounce of weight and the therefore low distribution costs.

Girls aged 15, 16 and 17 at the time of oil distribution start and confirmed to be unmarried by hired staff, were issued ration cards to collect the stipend for the first year of the program. Only girls (not their parents) were permitted to collect the oil by presenting their ration card, which was checked against a separate beneficiary list at the distribution point. In the second year of the program, oil monitors visited the house of every girl eligible for oil to check with the family and neighbors to ensure that she was still unmarried. The names of those found to be married were eliminated from the eligibility list.

5. Research design

5.1 Treatment arms

In total, 460 communities in the sample area were allocated randomly to each of the four intervention arms and a control group, as outlined below. Communities were assigned randomly after stratification by union (an administrative unit of roughly 10 communities) and the size of the community to ensure both geographic and population homogeneity across intervention groups. The interventions were composed of the two KK interventions discussed above, and the oil incentive in different combinations.

1) **The basic package:** Included community mobilization, social competency, self-help study support for in-school girls and literacy sessions for illiterate girls; implemented in 76 villages.
2) **The financial literacy package:** Included all components of the basic package with the addition of financial competency (financial planning, budgeting, prioritizing expenditures, savings and small business planning); implemented in 77 villages.
3) **The full package:** Included all components of the financial literacy package, along with a nutrition incentive (cooking oil) for unmarried adolescent girls; implemented in 77 villages.
4) **The conditional stipend/oil incentive package:** Included the nutrition incentive (cooking oil) for unmarried adolescent girls; implemented in 77 villages.
5) **The control group:** Comprised 153 villages that were assigned for comparison.
6) **Savings crosscut:** In addition, 77 villages were chosen randomly from the financial literacy and full packages to receive support in establishing girls' savings clubs.

The packages were structured to reflect different approaches to empowerment of adolescent girls. While it is possible that empowerment requires basic education and life skills training solely (package 1), some argue that practical empowerment derives from transforming adolescent girls to a financial asset rather than a financial burden. Differences between package 1 and package 2 would display the added effect of financial training. It is also possible that girls cannot benefit from the training packages if they do not remain unmarried long enough to participate. By adding a direct incentive to remain unmarried in package 3, we might increase the time that adolescents remain in the program before they are married, thus increasing the potential benefits of the safe-space training while allowing girls to physically mature before they become mothers. The last package (package 4) reflects the notion that increasing a girl's age at marriage, even without empowerment training, may change her bargaining power and status within the

new family while generating health benefits from delaying first pregnancy. By comparing the full package to individual packages, it will be possible to test whether different elements of empowerment programs are effective only when combined into a holistic package.

5.2 Power calculations

For the main outcomes of education and marriage, measured as part of the census, the study was powered to detect a 3.1 ppt reduction in child marriage from a control mean of 96% with 80% power, 0.05 alpha, 15% attrition and an intracluster correlation of 0.021 (Demographic and Health Survey 2004). The two empowerment arms were designed to be combined if necessary to increase power giving a minimum effect size of 1.6 ppts between empowerment and control arms.

5.3 Theory of change

Figure 1: Logical framework

Needs	Input	Output	Intermediate outcome	Impact	Long-term goal
Young girls in rural Bangladesh marry early, drop out of school early and have poor health utilization and outcomes prior, during and post-pregnancy.	KK sets up safe spaces for girls; girls aged 15 to 17 and unmarried are noted and oil purchased	Girls participate in KK sessions; unmarried girls receive oil	Higher awareness, knowledge, negotiation skills, decision-making power and health care utilization	Increased education, improved financial resources, increased age of marriage	Higher income, improved women and child health

The different intervention groups and subgroups were designed to determine channels and disentangle pathways to increased education and marriage age. A comparison of results between group 1 (basic package) and group 5 (control group) was intended to measure whether girls would become more aware of the dangers of child marriage and be valued more by their family, and whether this would translate into higher school participation and delayed marriage. Group 1 did not experience a change in age of marriage or level of education.

The comparison of girls in group 1 (basic) to girls in group 2 (financial literacy) was designed to measure the added impact of financial and livelihoods training, as well as the impact of the ability of income-generation and control of resources on education, marriage and health utilization. Group 2 did not experience any increase in income generation, age at marriage or education compared to the control group.

The difference between the control group and package 4 (financial incentive), and the difference between packages 2 (financial literacy) and 3 (financial literacy plus incentive) was in both cases designed to measure the effect of adding the oil incentive on delaying marriage. If the empowerment program and the financial incentive are complements, the marginal benefit of the financial incentive would be larger when added to the

empowerment program. In other words, the difference between packages 2 and 3 would be larger than between the control group and package 4.

The results from the midline survey assess the outreach and success of the specific interventions in the short term. A combination of process data from the oil monitoring and distribution rounds, SC's records and survey data was used to assess the success of the implementation and spill-overs into the control groups. Key indicators used are number of safe spaces, overall number of girls reached, enrollment and average attendance in KK meetings, types and numbers of meetings or sessions organized and number of girls who received the oil incentive.

The midline survey analysis focused on intermediate indicators such as girls' awareness, knowledge, attitudes, mobility, income, savings, negotiation, decision-making, school participation and health-care utilization. It also included marriage expectations and desired fertility choices. Key indicators used are awareness and participation in the programs, attendance and frequency of participation, and continuation of the group after the end of the formal curriculum. The analysis conducted after the midline survey also assessed the short-term impact on girls' awareness, knowledge, attitudes, perceptions, mobility, income, savings, investment, decision-making, desired fertility choices and marriage expectations.

The second round of analysis after the endline survey examines the long-term effects on girls' education and marriage outcomes. It assesses the long-term effects on girls' education and marriage outcomes as well as control over resources and women and child health outcomes.

5.4 Timeline

Table 2 on the next page outlines the key activities in the KK program and corresponding dates for these activities. A census was carried out in 610 villages between January to February 2007 in Babuganj, Patuakhali, and Bhola districts. Large municipalities, which were expected to have low outreach and coverage, as well as remote villages with poor accessibility and villages with fewer than 40 girls between the age of 10 and 19 were dropped. To maximize power, the largest number of villages were selected to reach the target of 90,000 girls aged between 10 to 19. The remaining sample of 460 villages thus excluded very large as well as very small villages, while the age range included girls eligible for at least one of the treatments.

The five packages were randomly assigned among the 460 villages. The stratification was done at a constructed union level, which sometimes combined smaller unions. Stratifying at this level ensured that each constructed union had villages assigned to each package. The baseline survey began in March 2007 and was completed in October 2007. Twenty households with adolescent girls per village were randomly selected from the census (ten in smaller villages) to be interviewed, and all adolescent girls (aged 10 to 17) in these households were interviewed. Data were collected on 11,350 girls from 9,155 households in total.

Following the completion of the KK program, after 2.5 years, both a midline census and the midline survey were conducted from January 2011 to March 2012. The longer time period reflects issues with survey implementation, discussed below, as well as the fact

that many girls had migrated (and girls who had migrated for work to the cities of Dhaka and Chittagong were especially hard to track). Surveying for the endline census began in May 2015 and surveying for the subsample in October 2015. At the time of this report, 99% of all census households and 70% of all girls in the subsample had been approached (outside Muladi). For reasons described below, surveying in Muladi was discontinued. In addition, spouse surveying was also discontinued because of tracking rates of around 44% of married women. A final round of surveying is planned for 2019.

Table 2: Timeline

Activity	Time period
Baseline census	January–February 2007
Baseline survey	March–October 2007
KK program	December 2007–August 2010
Oil incentive	April 2008–August 2010
Midline census	January 2011–March 2012
Midline survey	January 2011–March 2012
Endline census	May 2015–March 2016
Endline survey	October 2015–August 2016
Follow-up study	2019 (anticipated)

5.5 Analysis sample

Our analysis sample constitutes all 16,234 girls aged 15–17 and unmarried at program launch with endline data not missing. There are three sources of missing endline data, which are described in more detail below. First, during the midline survey, rumors spread in one sub-district (Muladi) that enumerators were abducting or converting girls to Christianity, and several enumerators were attacked. As a result, no endline data were collected in the subdistrict, and all analysis samples exclude Muladi (3,929 girls excluded). Before midline surveying, another 814 observations were mistakenly excluded from our target sample due to random data entry errors unconnected with treatment. Third, several villages were partially or completely washed away due to severe flooding; villages that were completely washed out (>80% flooded) are dropped from the analysis sample (1,007 girls excluded). Finally, of the 18,345 girls we attempted to reach at endline, 2,111 could not be tracked (12% attrition balanced across experimental arms, see Table 5). Our analysis sample thus constitutes 16,234 girls.

5.6 Randomization check

Table 3 and 4 on the next pages show the balance in outcome variables across the treatment groups in the baseline census as well as for several key variables in the baseline survey. For simplification and to increase power, we combine the basic and financial literacy versions of the program into one treatment arm (empowerment). As the tables below show, on average, girls sampled into the different treatment arms did not differ significantly in outcome variables in the census before the program interventions. In the subsample, girls sampled into the incentive treatment had higher average reading scores compared with the control group. However, while statistically significant, the difference in means is too small to be a concern.

Table 3: Census balance check, by treatment

Girls aged 15–17 at program launch (excluding Muladi and washed-out girls)

	Empowerment	Incentive	Empowerment + Incentive	Control	Total
Married and unmarried at baseline					
N	**6,905**	**3,365**	**3,577**	**7,163**	**21,010**
Ever married (%)	8.6	9.7	9.6	8.3	8.8
Std. Dev. (ppt)	(28.0)	(29.5)	(29.4)	(27.7)	(28.4)
N	6,905	3,365	3,577	7,163	21,010
Enrolled (%)	61.9	59.6	59.7	62.0	61.2
Std. Dev. (ppt)	(48.6)	(49.1)	(49.1)	(48.5)	(48.7)
N	6,904	3,364	3,576	7,162	21,006
Highest class passed (0–12)	6.4	6.2	6.2	6.3	6.3
Std. Dev.	(2.6)	(2.6)	(2.7)	(2.6)	(2.6)
N	6,884	3,355	3,569	7,141	20,949
Married at Baseline					
N	**591**	**325**	**342**	**598**	**1,856**
Enrolled (%)	14.2	11.4	10.5	13.6	12.8
Std. Dev. (ppt)	(34.9)	(31.9)	(30.7)	(34.3)	(33.5)
N	591	324	342	597	1,854
Highest class passed (0–12)	4.6	4.6	4.5	4.8	4.7
Std. Dev.	(2.5)	(2.5)	(2.5)	(2.5)	(2.5)
N	691	324	341	596	1,852
Unmarried at baseline					
N	**6,314**	**3,040**	**3,235**	**6,565**	**19,154**
Enrolled (%)	66.4	64.8	64.9	66.4	65.9
Std. Dev. (ppt)	(47.3)	(47.8)	(47.7)	(47.2)	(47.4)
N	6,313	3,040	3,234	6,565	19,152
Highest class passed (0–12)	6.6	6.4	6.4	6.5	6.5
Std. Dev.	(2.5)	(2.6)	(2.6)	(2.6)	(2.6)
N	6,293	3,031	3,228	6,545	19,097
Father's Education (0-17)	4.3	4.0	4.1	4.1	4.1
Std. Dev	(4.4)	(4.1)	(4.2)	(4.2)	(4.3)
N	1,675	866	921	1,879	5,341
Mother's Education (0-17)	3.3	3.1	3.1	3.2	3.2
Std. Dev	(3.3)	(3.3)	(3.1)	(3.3)	(3.3)
N	4,098	1,984	2,139	4,325	12,546
Household size (members)	5.9	6.0	6.1	6.0	6.0
Std. Dev	(1.9)	(2.0)	(2.0)	(2.0)	(2.0)
N	6,314	3,040	3,235	6,565	19,154
Unmarried older sister in household (%)	18.5	17.7	17.6	18.4	18.2
Std. Dev	(38.8)	(38.2)	(38.1)	(38.8)	(38.6)
N	6,314	3,040	3,235	6,565	19,154
Village boy/girl ratio	1.0	1.0	1.0	1.0	1.0
Std. Dev	(0.3)	(0.3)	(0.3)	(0.3)	(0.3)
N	6,314	3,040	3,235	6,565	19,154
Village size (girls age 10-19)	265.2	255.2	260.3	276.0	266.5
Std. Dev	(116.6)	(123.0)	(113.4)	(125.1)	(120.3)
N	6,314	3,040	3,235	6,565	19,154

Balance tests are ordinary least squares (OLS) results with modified Huber-White standard errors clustered at the village level; Group means are listed for each outcome, standard deviations in parentheses. *$P < 0.10$, **$P < 0.05$, ***$P < 0.01$.

Table 4: Survey balance check, by treatment

Girls aged 15–17 at program launch (excluding Muladi and washed-out girls)

	Empowerment	Incentive	Empowerment + Incentive	Control	Total
Married and unmarried at baseline					
N	**772**	**421**	**442**	**794**	**2,429**
Ever married (%)	6.0	6.2	7.0	6.4	6.3
Std. Dev. (ppt)	(23.7)	(24.1)	(25.6)	(24.5)	(24.4)
N	772	421	442	794	2,429
Enrolled (%)	64.0	66.3	65.6	64.9	65.0
Std. Dev. (ppt)	(48.0)	(47.3)	(47.6)	(47.8)	(47.7)
N	742	415	430	764	2,351
Highest class passed (0–12)	6.9	7.2	7.2	7.0	7.0
Std. Dev.	(2.6)	(2.4)	(2.4)	(2.6)	(2.5)
N	742	407	432	761	2,342
Married at Baseline					
N	**46**	**26**	**31**	**51**	**154**
Enrolled (%)	16.3	15.5	25.8	19.1	19.0
Std. Dev. (ppt)	(37.4)	(36.8)	(44.5)	(39.8)	(39.4)
N	43	26	31	47	147
Highest class passed (0-12)	5.7	6.1	7.0	6.1	6.2
Std. Dev.	(2.6)	(2.3)	(1.9)	(2.9)	(2.6)
N	46	26	31	50	153
Unmarried at baseline					
N	**726**	**395**	**411**	**743**	**2,275**
Enrolled at baseline (%)	**67.0**	**69.7**	**68.7**	**67.9**	**68.1**
Std. Dev. (ppt)	**(47.1)**	**(46.0)**	**(46.4)**	**(46.7)**	**(46.6)**
N	**699**	**389**	**399**	**717**	**2,204**
Highest class passed (0–12)	**7.0**	**7.3**	**7.2**	**7.0**	**7.1**
Std. Dev.	**(2.6)**	**(2.3)**	**(2.5)**	**(2.6)**	**(2.5)**
N	**696**	**381**	**401**	**711**	**2,189**
Math score (1–4)	**2.6**	**2.6**	**2.6**	**2.7**	**2.6**
Std. Dev.	**(1.0)**	**(1.0)**	**(1.0)**	**(1.0)**	**(1.0)**
N	**726**	**395**	**411**	**742**	**2,274**
Reading score (1–5)	**4.4**	**4.6***	**4.4**	**4.4**	**4.4**
Std. Dev.	**(1.2)**	**(1.0)**	**(1.2)**	**(1.2)**	**(1.2)**
N	**726**	**395**	**411**	**742**	**2,274**
Total family assets (0–117)	**26.9**	**26.9**	**26.3**	**26.3**	**26.6**
Std. Dev.	**(17.1)**	**(15.8)**	**(15.3)**	**(15.7)**	**(16.1)**
N	**726**	**395**	**411**	**743**	**2,275**
Knowledge of contraception (%)	**63.4**	**64.3**	**69.1**	**67.3**	**65.8**
Std. Dev. (ppt)	**(48.2)**	**(48.0)**	**(46.3)**	**(46.9)**	**(47.4)**
N	**726**	**395**	**411**	**743**	**2,275**

Balance tests are OLS results with modified Huber-White standard errors clustered at the village level; Group means are listed for each outcome, standard deviations in parentheses. *$P < 0.10$, **$P < 0.05$, ***$P < 0.01$.

6. Evaluation

6.1 Instruments

The surveying firm Associates for Community and Population Research (ACPR) was hired following a competitive bidding process to conduct surveying of the census and administer the subsample questionnaire at baseline. After the end of project implementation, Mitra and Associates was hired, following a second competitive bidding process, to conduct all midline surveys, as its bid was lower than ACPR's. Therefore, all previous material was transferred from ACPR to Mitra and Associates. Mitra and Associates won the third competitive bid to conduct the program's endline surveys.

6.1.1 Midline

The midline survey, which lasted from January 2011 until March 2012, included the following questionnaires:

- A census questionnaire, which surveyed members of all households with girls aged 9 to 20, as well as all households that had girls aged 6 to 17 in the baseline census, including their education and marital status.
- An unmarried girl questionnaire, administered to unmarried girls in sample households. Topics included education, nutrition, mobility, health and mental health, economic opportunities, credit and savings, community participation, religion, thoughts on marriage, menstruation, KK and the oil incentive program, a literacy test and contact information.
- A married girl questionnaire that included all of the above as well as more detailed questions on marriage and child outcomes and reproductive health.
- A head or spouse-of-household questionnaire that asked about parental attitudes, perceptions and decision-making processes relevant to the above topics for girls in the household. The questionnaire also asked about assets and household water-use behavior. GPS information was recorded.
- An in-law head-of-household questionnaire that asked the same questions as above to the head of the household into which a married girl had moved.
- A village leader questionnaire, typically administered to the local government official, on availability, access and use of educational, health and economic opportunities.
- A marriage registrar survey, to record marriages in villages over the past three years.
- A school questionnaire for every school in the village, to collect enrollment and attendance data, observe male-to-female attendance ratios, mark available facilities and ask the head teacher about male versus female performance at the school.

6.1.2 Endline

The endline survey, which began in May 2015, included the following questionnaires:

- A census questionnaire, which asked all households surveyed at baseline questions regarding the education, marriage, reproductive history and mobility of girls aged 6 to 17 in the baseline census, a brief history of health issues and deaths, and tracking information.

- A girl questionnaire, which asked a subsample of girls about knowledge, access, use, negotiation, attitudes and decision-making in education, nutrition, mobility, reproductive health, economic opportunity, community involvement, religion, marriage and social networks. The questionnaire also included anthropometric measurements (height and weight) and asked for information on any migration or movement planned for the future.
- A spouse questionnaire for husbands of married girls in the sample, which asked about knowledge, access, use, negotiation, attitudes and decision making relevant to the above topics for girls. It included questions on the spouse's own education, mobility, and economic activities, and the terms of marriage.
- A matchmaker questionnaire for people professionally involved in arranging marriages. The questionnaire included questions regarding educational and spatial facets of matches, the number of in-village marriages and the terms of marriages.

In addition to the quantitative surveys, qualitative tools were developed to conduct in-depth interviews with girls and their husbands, parents or in-laws. The qualitative survey was administered by Innovations for Poverty Action.

6.2 Challenges in survey implementation

Following the baseline census, the pages that contained the personal identifiers of the respondents were provided to a data entry firm to enter this information separately from the rest of the survey. Because the firm lost the sheets with personal identifiers for 20 villages, information on 218 unmarried girls aged 15 to 17 (excluding those from Muladi) was not provided to enumerators at the midline census, resulting in insufficient tracking information. In addition, due to a random error unconnected with treatment, information on 561 households with 596 girls (excluding those from Muladi) was not entered fully or at all. Those girls were not followed up on at midline or endline.

During the last stages of data collection for the midline survey, rumors began to spread in Muladi that enumerators were harming the girls they were interviewing. One version of the rumor suggested that girls who ate from plates given to households following the survey as compensation for their time were being poisoned. Another version said that eating from the plates converted girls to Christianity, and a third suggested that girls were being abducted. In conjunction with SC, local government officials and respected local figures such as teachers and religious leaders, the team reached out to the communities to combat the rumors. However, after enumerators were surrounded by an angry mob, survey work in the area was postponed for a number of months. As a result, no endline survey data were collected in the sub-district. Hence, all 84 Muladi study villages (with 3,929 girls aged 15–17 and unmarried at program launch) were excluded from the evaluation. As randomization was stratified by union, equal numbers of treatment and comparison villages were dropped so all arms were impacted proportionately.

In Bhola district, several villages on river banks were partially or completely washed away due to severe flooding in 2009, as documented by field staff at that time. Households from those villages had to migrate to nearby villages or farther away. We consider a village to be washed out if 80% of it was flooded, as occurred in 11 villages in

our sample. This corresponds to 945 households with a total of 1,007 unmarried girls in the age range of 15 to 17 (excluding Muladi and non-prefilled households).

Endline survey activity was delayed due to political instability in Bangladesh as a result of a dispute over Parliament elections in 2014. Across our survey areas, several political incidents were reported, ranging from mild (nonviolent protests) to severe (involving loss of life and property), rendering surveying potentially unsafe until April 2015.

Furthermore, by the time of the endline census, two additional villages were washed out. Flooding at endline further reduced the tracking rates.

Prior to the endline census and subsample surveying in Muladi, Innovations for Poverty Action staff met with local government officials in Muladi to gain their approval and support for the upcoming survey activities. However, surveying had to be discontinued due to low tracking rates in the rumor-affected regions. Soon after surveying began in Muladi, rumors started to spread again that girls would be converted to Christianity or abducted when talking to enumerators. Belief in the later rumor was reinforced by a recent abduction in one village, which was imputed to the enumerators. On several occasions, family members beat girls who participated in interviews, providing another reason for discontinuing endline surveying.

Similarly, the spouse survey was suspended across all sub-districts because of very low tracking rates, which can be attributed in part to high male labor force participation. In addition, exchange rate fluctuations between the Canadian and US dollars significantly decreased funds for surveying, making follow-up at the spouses' workplaces too costly.

At both midline and endline, tracking girls who have migrated has been difficult. The main reasons for migration have been marriage, work and study. Finding or talking to girls who had migrated to large cities such as Dhaka and Chittagong proved particularly difficult. Reasons for this include the fact that the girls, who work primarily in the garment sector, tend to be busy, change employers or residences frequently and in general are more cautious about meeting with enumerators due to the higher incidence of crime in the cities. Another implication of this migration is that our sample is now much more scattered geographically, making field work difficult to execute efficiently.

6.3 Attrition

Survey teams completed the midline surveying in rumor-affected areas in the sub-district of Muladi between February and March 2012. As shown in Table 5 below, attrition in the midline census was 21% for unmarried girls aged 15 to 17 at program launch. Excluding washed-out and non-prefilled households, attrition decreased to 15% overall, indicating that attrition due to households that were not found and refused responses was generally low. Attrition was significantly lower in the incentive arm ($p<0.1$) for the analysis sample (excluding non-prefilled and washed-out observations).

Attrition in the midline survey was 15% overall. In the entire sample of unmarried girls aged 15 to 17 at program launch, attrition was significantly lower in the incentive arm when excluding households influenced by rumors, washed away and non-prefilled.

Of the 18,345 girls we attempted to reach at endline, 2,111 could not be tracked (12% attrition balanced across experimental arms, see Table 5). Our analysis sample thus constitutes 16,234 girls.

Table 5: Attrition and attrition bias, by treatment

Unmarried girls aged 15–17 at program launch

	Empowerment	Incentive	Empowerment + Incentive	Control	Total
Midline census					
Total	21.2	23.0	19.5	19.3	20.6
	(40.9)	(42.1)	(39.6)	(39.5)	(40.4)
--, excluding data entry errors	18.3	15.8	14.5	16.8	16.8
	(38.7)	(36.4)	(35.2)	(37.4)	(37.4)
--, excluding washed-out villages	15.5	12.6*	13.8	15.2	14.7
	(36.2)	(33.1)	(34.5)	(35.9)	(35.4)
Midline survey					
Total	15.4	12.8*	14.0	17.2	15.3
	(36.1)	(33.4)	(34.7)	(37.8)	(36.0)
--, excluding Muladi	10.5	8.2*	9.5	11.3	10.2
	(30.6)	(27.4)	(29.3)	(31.7)	(30.2)
--, excluding washed-out villages	10.2	7.5**	9.1	11.0	9.8
	(30.2)	(26.4)	(28.8)	(31.3)	(29.7)
Endline census					
Total	17.9	21.9	15.1	17.4	17.9
	(38.3)	(41.4)	(35.8)	(37.9)	(38.3)
--, excluding data entry errors	15.3	15.6	10.7	14.9	14.4
	(36.0)	(36.3)	(30.9)	(35.6)	(35.2)
--, excluding washed-out villages	10.9	11.9	10.3	12.6	11.5
	(31.1)	(32.4)	(30.3)	(33.1)	(31.9)

Balance tests are OLS results with modified Huber-White standard errors clustered at the village level; Group means are listed for each outcome, standard deviations in parentheses.

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

6.4 Noncompliance

As part of the midline survey (one year after program completion), girls were asked if they had ever attended an organized girls' club. If so, they were asked which organization had established the club, the name of the program and how regularly they had attended. The results confirmed that few or no similar activities were occurring in the study area. A large number of girls in communities that were not randomized to receive the program nevertheless attended some session or were members of the KK program.

Table 6 below reports whether a girl who was unmarried and between the age of 15 and 17 and thus eligible for all treatments attended at least one session of KK, and whether a girl reported being a member of KK. The table lists rates for any KK treatment village (comprising empowerment, empowerment plus incentive villages), any oil treatment village (comprising incentive and empowerment and incentive villages), as well as the rates broken down by treatment type.

The results highlight that KK attendance and membership rates were higher in full treatment villages (empowerment plus incentive) compared to basic empowerment villages. Even the percentage of spillovers, as defined by girls who attended KK sessions or were KK members in any of the non-KK villages, was higher in villages that received the oil incentive: About 38% of girls in incentive villages reported they were KK members, compared to 14% in control villages. This could be due to three reasons: 1) girls in villages in which cooking oil was also distributed could have been much more favorable toward the project and thus also sought KK participation, 2) girls in the oil incentive villages may have remained unmarried for a longer time and thus be allowed to participate and 3) girls who remembered having received oil may also assume that they participated in other programs administered in their village and thus overreported KK membership. Noncompliance was very low in non-oil villages, in which less than 1% of non-eligible girls reported receiving oil.

Table 6: Self-reported KK attendance and membership for unmarried girls aged 15–17 in the baseline survey

Treatment group	Attended at least 1 KK session (%)		Member of KK (%)		Oil take-up (%)	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Empowerment	49.9	50.1	56.3	61.2	0.6	7.8
Incentive only	32.9	47.1	37.8	57.9	67.5	46.9
Empowerment + Incentive	70.2	45.8	77.6	56.8	73.9	44.0
Control	11.0	31.3	13.7	41.4	0.8	9.0
Any KK	57.6	49.4	64.4	60.4	.	.
Any Oil	.	.	.	.	70.8	45.5

7. Monitoring program implementation

Using the maximum number of monthly enrollments of all safe spaces in the monitoring data suggests that 33,523 adolescent girls were reached outside Muladi between December 2007 and August 2010 through peer education sessions in the four learning cycles. Average attendance at all safe spaces fluctuated around 80%. Using the total number of girls in the age range of 10 to 19 in the baseline census, we thus reached an enrollment rate of about 89%. This is higher than the self-reported enrollment rate of about 63% in the age group of 10–19 years, as girls may have forgotten that they participated in the program and were counted in the enrollment rate even if they were members for a very short time.

Monitoring data also shows that girls spent an average of 6 2/3 hours in KK sessions every week, or 173 hours in total over a program cycle.

In the midline survey, we did not observe a difference in time spent studying per week across all treatment groups, indicating that the empowerment program did not substitute for study time.

Additionally, 4,830 adolescent girls in our analysis sample received the oil incentive at least once, or 74% of the 6,556 girls eligible at baseline – again, higher than the self-reported rate of about 71%. We discuss program outreach more in Appendix B.

8. Results

8.1 Descriptive statistics

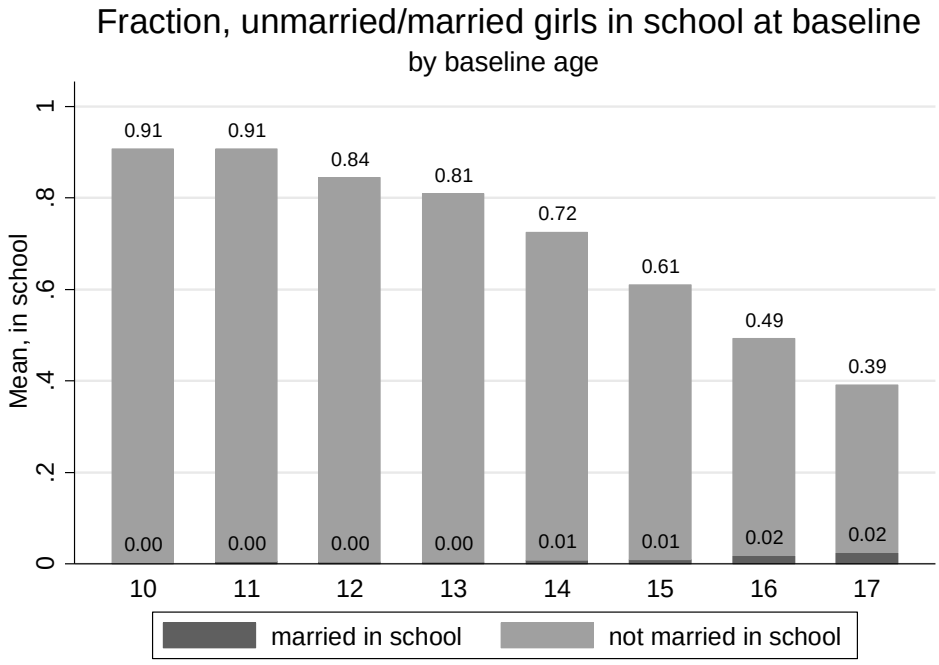
Below we highlight some descriptive statistics for our main outcomes at baseline, midline and endline – education and age of marriage. For descriptive statistics of other variables, see Appendix C.

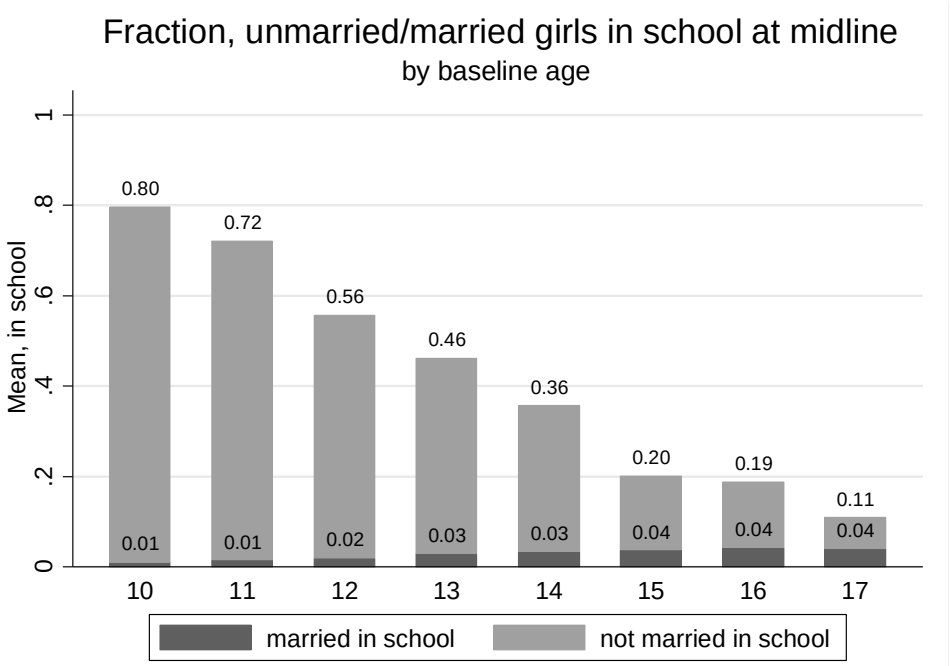
8.1.1 Education

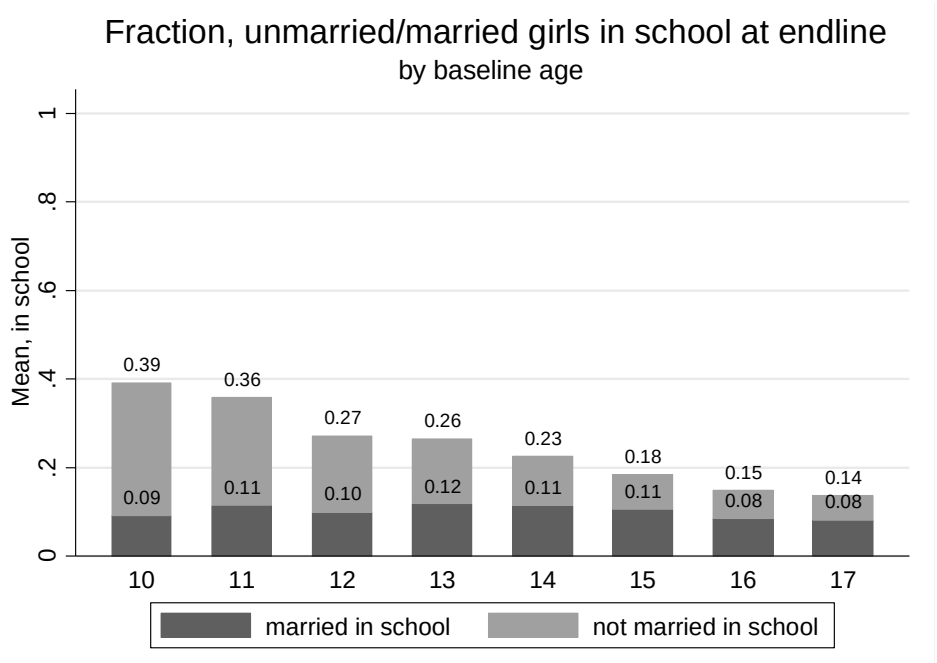
Overall, about 42% of girls were still in school at midline, down by almost half from the baseline level of 74%. Figure 2 below shows the fraction of girls, by age, who were still in school at baseline, midline and endline, and the fraction of these girls who were married. While 80% of girls aged 10 at baseline were still in school at midline, this number drops to about 36% for girls aged 14 at baseline and as low as 19% and 11%, respectively, for girls aged 16 and 17 at baseline. Overall education enrollment dropped to about 26% at endline, and only 39% of girls aged 10 at baseline were still in school — almost a quarter of whom were married. More than half of the girls in school who were age 17 at baseline were married at the time of the endline census. Slightly more than a quarter of girls (28%) aged 14 to 16 at baseline were still in school at midline, and 20% of girls aged 14 to 16 at baseline were still in school at endline. It may be premature to know the true impact of the program on highest class passed, an outcome we will further evaluate at the time of next survey round.

Figure 2: Fraction of unmarried and married girls in school, by age

At baseline, top; midline, middle; and endline, bottom (the first label is the fraction of married girls and the second is the total, so the unmarried fraction is the difference)

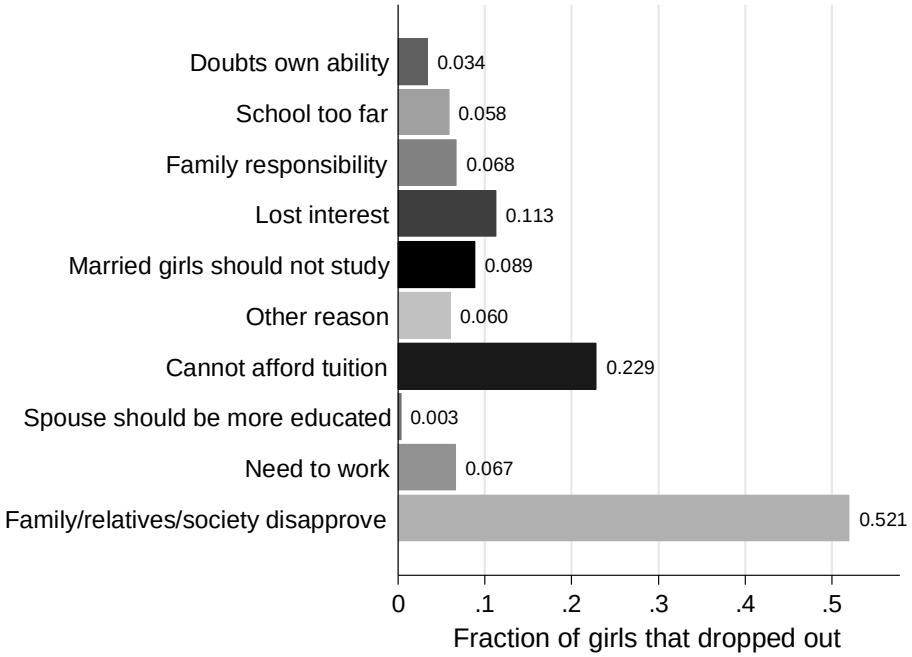






In the midline census, the mean class passed of girls who had attended school at some point in their lives is 7.3. Approximately 91% of all girls had completed primary school (grade 5), and 45% of girls had completed secondary school (grade 10). About 37% of girls had completed higher secondary school (grade 12). In the midline survey, for girls who were still in school, the average class where they expected to face the most obstacles in continuing their education was higher secondary, or grade 12. Reasons for dropping out of school are shown in Figure 3 below. The most frequently reported reason was disapproval of the girl's family, including parents, in-laws, husband and other relatives. The other most commonly reported reason was inability to afford school tuition, which we discuss below in the context of the girls' stipend program. Note that the sum of fractions adds up to greater than 1 since this was a multiple-response question.

Figure 3: Reasons reported for dropping out of education in the Midline survey



Girls aged 15 to 17 and unmarried at baseline (excluding Muladi and washed-out villages) had a mean reading score of 4.4 at midline and an average math score of 3.6. Figures 4 and 5 below show the results by treatment group. When controlling for stratification and baseline characteristics, math scores and reading scores do not differ by treatment. We do not observe an effect of the empowerment program alone on reading or math ability.

Figure 4: Math score at midline

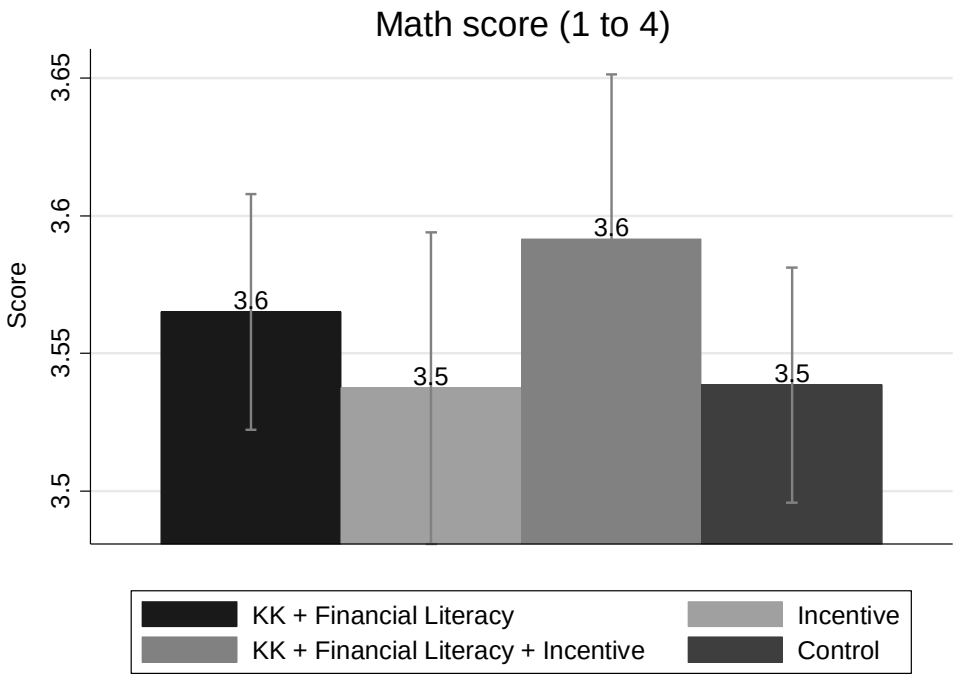
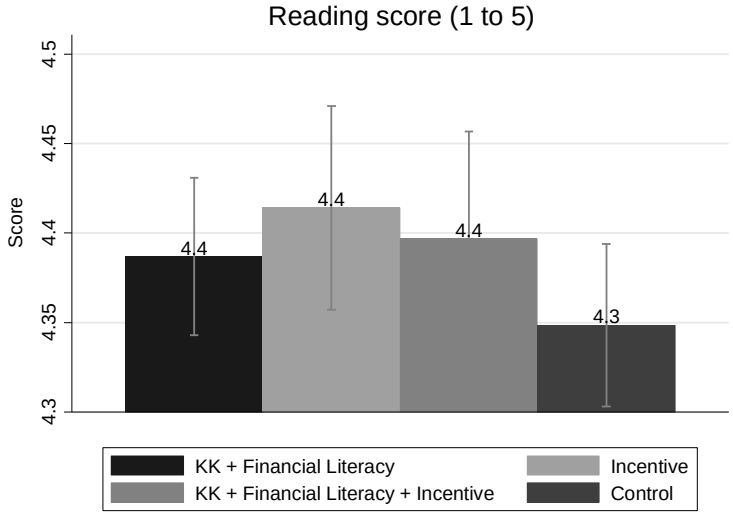


Figure 5: Reading score at midline

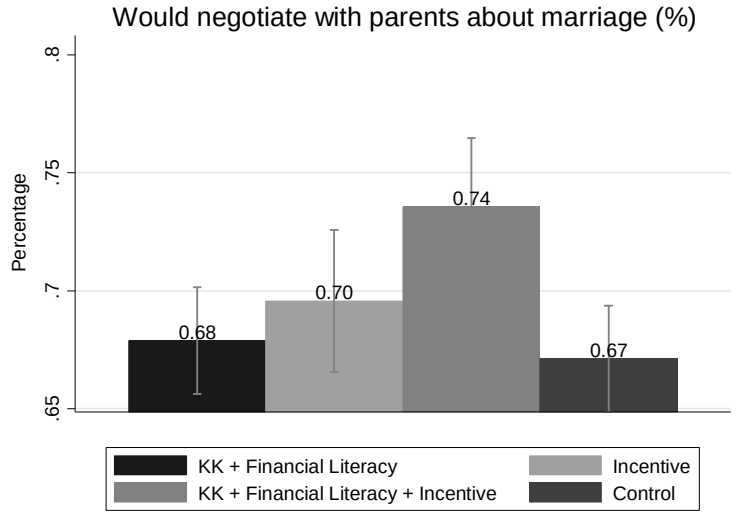


8.1.2 Negotiation and decision-making

Girls in the subsample were asked in the survey whether they would negotiate with their parents if they were to receive a marriage proposal. Figure 6 shows that 74% of girls in the full treatment group would do so, as compared to 62% in the control group and 61% in the basic empowerment treatment arm. When adjusting for baseline characteristics and stratification this difference is significantly different ($p<0.05$).

Figure 6: Fraction of girls who would negotiate marriage with parents, by treatment

Girls unmarried at midline.



8.1.3 Awareness and knowledge

We do not find any significant differences across treatment arms for 1) the percentage of girls who were aware of special health risks to a young pregnant girl and her child, 2) the percentage of girls who had heard of HIV and AIDS, 3) the percentage of girls who had heard of contraceptives, 4) the share of girls who had used contraceptives. Girls in the full treatment arm (empowerment plus incentive) correctly identified significantly more iron-rich foods ($p<0.1$).

8.1.4 Attitudes

Our findings for attitudes are inconclusive.

8.1.5 Savings and spending practices

We do not observe a significant difference in investment patterns across treatment arms at midline. However, the empowerment-only treatment arm increased the amount saved per month by 100 Bangladeshi Taka (BDT) on average ($p<0.1$) and the amount spent on schooling by 92 BDT ($p<0.01$), indicating that empowerment may influence girls' spending decisions.

8.1.6 Mobility

In a comprehensive list of mobility outcomes, there were no significant differences in mobility across treatment arms.

8.1.7 Income generation activity2

At midline, 38% of girls had ever worked for income, and 27% had worked in the past year, including 27% of girls aged 14 to 17 at baseline. Of those girls, 34% were married and 34% had migrated. Most were involved in one income-generating activity. Figure 7 below shows the activities that girls were engaged in. Tutoring was the most common activity. The next most common activity was a salaried job, which would include garment work. Note that the proportions add up to greater than 1 as girls could be engaged in multiple occupations. Average earnings in the previous month for girls who had worked in the past year was 1,242 BDT (US$15.39), with no significant differences across treatment groups.

Figure 7: Fraction of income-generating activities that girls are engaged in

8.2 Participation in KK

What type of girls did the KK program attract? In what ways were they different from girls who did not participate? Table 7 on the following page displays the means for KK members and nonmembers on a number of observable baseline characteristics. KK members had a significantly higher reading score, but were equally as likely to be in school as nonmembers. There was also no significant difference in knowledge of contraception and AIDS.

KK members also appeared to come from families that were better off, as they lived closer to the safe space centers, which villagers often selected from houses that were large enough to fit about 20 girls on their veranda or porch. Prior to the start of the program, there was a concern that girls from conservative families would not be allowed to participate. However, there appeared to be no difference in the religiosity of the parents of members and nonmembers. There also appeared to be no difference in the initial health of members and nonmembers, as proxied by body mass index. These differences suggest that there is room for improvement on most fronts (barring reading score), thus we don't expect KK estimates to be biased in any direction.

Table 7: Means of baseline observables of KK members and nonmembers, units in brackets, girls unmarried and aged 15–17 at baseline, excluding Muladi and washed-out

	KK members (mean)	Non-members (mean)	Difference of means	p-value	t-stat
Education					
Girl in school (%)	73.7	66.0	7.7	0.18	1.33
Math score (1–4, Annual Status of Education Report or ASER)	2.7	2.6	0.1	0.30	1.03
Reading score (1–5, ASER)	4.6	4.4	0.2 ***	<0.01	2.76
Knowledge					
Contraception knowledge (%)	67.2	65.0	2.2	0.33	0.98
AIDS knowledge (%)	76.8	73.1	3.7	0.75	0.32
Gender attitudes					
Better to be a man (%)	48.3	47.1	1.2	0.92	0.10
Boys more education (%)	35.0	34.7	0.3	0.24	1.18
Religiosity					
Girl religiosity (1–4)	2.2	2.2	0.0	0.94	-0.07
Father religiosity (1–4)	2.4	2.4	0.0	0.77	-0.29
Mother religiosity (1–4)	2.5	2.5	0.0	0.20	-1.29
Girl health					
Body mass index (WHO tables)	18.7	18.7	0.0	0.35	-0.93
Proximity to safe space					
Distance to nearest safe space (kilometers)	0.38	0.63	-0.25 ***	0.00	-3.64

* p<0.1, ** p<0.05, *** p<0.01, two-sample t-test with equal variances.

8.3 Regression results

Because 21% of eligible girls in incentive villages were excluded from the incentive due to name-matching errors, our main specification is a 2SLS treatment-on-treated estimate using the Wald estimator for the estimate of β, the difference in outcomes between girls in treatment and control villages:

$$Y_{iv} = \beta_0 + \beta_1 Emp_v + \beta_2 Incentive_v + \beta_3 KKIncentive_v + \gamma X_{iv} + \mu_v + \varepsilon_{iv}$$

Where Y_{iv} s are girl i's outcomes in village v; Emp is a dummy variable for whether the girl was in a community assigned to either of the empowerment packages, Incentive is a dummy for girls in communities assigned to the Financial Incentive only treatments; and

KK Incentive indicates assignment to both Incentive and KK. A set of control variables in a matrix X includes union dummies and village size tier that approximate the original stratification. The standard errors are clustered at the village level, which was the unit of randomization. First-stage results are presented in Table 8 below. Note that the first-stage in a 2SLS model regresses endogenous covariates in the equation of interest (i.e. eligibility for incentive treatments) on all exogenous variables in the model and the excluded instruments (i.e. randomized treatment status of the village). The predicted values of eligibility obtained from the first stage are then used in the equation of interest, replacing the endogenous eligibility variable.

Both at midline and at endline, many girls had not yet finished their education, making it impossible to know the final class that these girls passed. Therefore, we can only estimate the impact of the program on 'still in school' for which we have outcomes for the entire sample (and thus do not face censuring bias). In addition, we analyze fertility by age 22, as the whole sample had reached age 22 at the time of endline surveying.

We restrict our estimation of the program to girls who were eligible for all treatment arms (those aged 15–17 and unmarried at program launch) to decrease the noise by girls who were not eligible to receive the incentive treatment.

8.3.1 First Stage

Table 8: Girls aged 15–17 and unmarried at program launch

	Eligibility for Incentive	Eligibility for Emp. + Incentive	Eligibility for Incentive	Eligibility for Emp. + Incentive
Age at program launch	15–17	15–17	15	15
	(1)	(2)	(3)	(4)
Incentive	0.759***	0.002867	0.799***	0.000466
	(0.0199)	(0.00411)	(0.0217)	(0.00428)
Emp. + Incentive	-0.00135	0.812***	-0.00110	0.840***
	(0.00535)	(0.0142)	(0.00546)	(0.0153)
Empowerment	-0.00113	0.005385	0.000122	0.00464
	(0.00422)	(0.00343)	(0.00409)	(0.00349)
Control mean	0%	0%	0%	0%
N	16,234	16,234	5,840	5,840

Standard errors in parentheses, * p<.1, ** p<.05, *** p<.01

8.3.2 Marriage outcomes

Table 9 shows that the financial incentive reduced the likelihood of being married by 6% at midline (3.4ppts, p<0.05), and the empowerment plus incentive reduced the likelihood by 5% (2.9ppts, p<0.1). At endline, the likelihood of marrying under 18 fell by 22% (8.7ppts, p<0.01) with the incentive only and 16% (6.5ppts, p<0.01) in the incentive plus empowerment group. The likelihood of marrying under age 17 decreased by 19% (5.1ppts, p<0.01) in the incentive only treatment arm. The coefficient on the incentive plus empowerment program indicates a similar reduction of 16% (4.3ppts, p<0.01). Among girls who were 15 years old at program launch and thus eligible for the incentive for at least two years (Table 10), the likelihood of being married at midline fell by 13% (6.3ppts, p<0.01) in the incentive only arm and 12% (5.8ppts, p<0.01) in the incentive

plus empowerment arm. At endline, the estimated impact on underage marriage of the financial incentive is 22% (10.0ppts, p<0.01) in the incentive only and 19% (8.7ppts, p<0.01) in the incentive plus empowerment group. The likelihood of being married under age 17 fell by 23% (7.3ppts, p<0.01) in the incentive only group and 21% (6.6ppts, p<0.01) in the incentive plus empowerment group; the likelihood of being married under age 16 fell by 23% (4.9ppts, p<0.05) in the incentive only group and by 29% (6.2ppts, p<0.01) in the incentive plus empowerment group.

The regression on marriage age indicates that the financial incentive increased marriage age an average of 5.2 months (0.44 years, p<0.01) when given alone and 4.5 months when given alongside the empowerment program (0.38 years, p<0.01). For girls who were eligible for at least two years of the incentive, marriage age increased by 5.3 months (0.44 years, p<0.01) on average in the incentive only group and by 5.6 months (0.47 years, p<0.01) in the incentive plus empowerment group. If all girls who were unmarried and age 15 at distribution start were persuaded to wait until age 18, average marriage age would have increased 5.6 months, a measure of potential program effect under maximum take-up. Thus, our estimated treatment effect of 5.3 months for the incentive only group and 5.6 months for the incentive plus empowerment group is the equivalent of almost every family at risk responding to the incentive for the duration of the program. The coefficients on child marriage and marriage age did not differ significantly between the two incentive arms, and the coefficients on the empowerment arm were consistently insignificantly different from zero in all specifications.

Table 9: Marriage outcomes for girls aged 15–17 and unmarried at program launch

	Whether married	Whether married	Marriage age	Whether married <18	Whether married <17
Survey wave	Midline	Endline	Endline	Endline	Endline
	(1)	(2)	(3)	(4)	(5)
Incentive	-0.0336**	-0.0274**	0.436***	-0.0873***	-0.0513***
	(0.0150)	(0.0137)	(0.0971)	(0.0156)	(0.0139)
Emp. + Incentive	-0.0294*	-0.0185	0.379***	-0.0653***	-0.0432***
	(0.0158)	(0.0125)	(0.0848)	(0.0148)	(0.0132)
Empowerment	0.00935	0.00134	-0.0149	-0.00258	0.00262
	(0.0108)	(0.00740)	(0.0534)	(0.00913)	(0.00856)
Control mean	56.4%	85.7%	18.2	40.1%	27.0%
N	15,419	16,223	13,823	16,211	16,211

Standard errors in parentheses, * p<.1, ** p<.05, *** p<.01

Table 10: Marriage outcomes for girls aged 15 and unmarried at program launch

	Whether married	Whether married	Marriage age	Whether married <18	Whether married <17	Whether married <16
Survey wave	Midline	Endline	Endline	Endline	Endline	Endline
	(1)	(2)	(3)	(4)	(5)	(6)
Incentive	-0.0626***	-0.0300	0.438***	-0.100***	-0.0729***	-0.0491**
	(0.0211)	(0.0187)	(0.120)	(0.0214)	(0.0202)	(0.0191)
Emp. + Incentive	-0.0584***	-0.0250	0.469***	-0.0874***	-0.0660***	-0.0619***
	(0.0223)	(0.0190)	(0.126)	(0.0227)	(0.0189)	(0.0180)
Empowerment	0.0237	-0.00177	-0.0817	0.00819	0.00328	0.0127
	(0.0163)	(0.0116)	(0.0782)	(0.0144)	(0.0133)	(0.0120)
Control mean	50.0%	83.0%	17.6	45.2%	31.6%	21.5%
N	5,523	5,834	4,811	5,831	5,831	5,831

Standard errors in parentheses, * p<.1, ** p<.05, *** p<.01

8.3.3 Education outcomes

The incentive to delay marriage also had a large positive impact on school enrollment (Table 11). At midline, girls aged 15–17 at program launch were 10% (3.0ppts, p<0.05) more likely to be in school in the incentive only arm and 14% (4.0ppts, p<0.01) more likely in the incentive plus empowerment arm. Girls in the incentive plus empowerment arm were also 22% (6.0ppts, p<0.01) more likely to be in school at endline. Again we observed a significant dose response to the program: Girls who were 15 at program launch were 25% (8.5ppts, p<0.01) more likely to be in school at midline in the incentive only arm and 28% (9.4ppts, p<0.01) more likely in the incentive plus empowerment arm. At endline, the coefficients were +6.8ppts (p<0.01), or 25%, in the incentive only arm and +8.1ppts (p<0.01), or 30%, in the incentive plus empowerment arm. Once again, the difference between the incentive and empowerment plus incentive arms was not significant at the 95% confidence level. In addition, girls who were 15 at program launch in the empowerment arm were 12% more likely to be in school at endline (3.3ppts,p<0.05).

Table 11: In-school girls aged 15–17 and unmarried at program launch

Survey wave	Midline	Endline	Midline	Endline
Baseline age	15-17	15-17	15	15
	(1)	(2)	(3)	(4)
Incentive	0.0297**	0.0386*	0.0852***	0.0675***
	(0.0139)	(0.0198)	(0.0228)	(0.0247)
Emp. + Incentive	0.0395***	0.0602***	0.935***	0.0811***
	(0.0149)	(0.0211)	(0.0232)	(0.0277)
Empowerment	0.00346	0.0172	0.00172	0.0327**
	(0.00850)	(0.0119)	(0.0156)	(0.0164)
Control mean	28.9%	27.2%	33.6%	26.9%
N	15,189	10,801	5,458	4,323

Standard errors in parentheses, * p<.1, ** p<.05, *** p<.01

8.3.4 Reproductive health outcomes

Table 12 shows that the financial incentive reduced the likelihood of having given birth at endline by 6% (3.9ppts, p<0.05) and that empowerment plus the incentive reduced the likelihood by 5% (3.3ppts, p<0.05). Among married girls, age at first birth increased by 2.6 months in the incentive arm (0.22 years, p<0.05) and by 2.8 months in the empowerment plus incentive arm (0.23 years, p<0.01). The likelihood of having given birth under age 18 fell by 24% (2.7ppts, p<0.01) in the empowerment plus incentive group. The financial incentive decreased the number of children born by age 22 by 0.05 (p<0.1) and the empowerment plus incentive by 0.06 (p<0.05). We did not observe any significant differences in children's mortality rate across treatment arms.

The coefficients on reproductive health outcomes, among girls who were 15 at program launch and thus eligible for the incentive for at least two years (Table 13), did not differ significantly from the coefficients in the overall sample. In addition, the coefficients on all reproductive health outcomes did not differ significantly between the two incentive arms.

Table 12: Reproductive health outcomes for girls aged 15–17 and unmarried at program launch

	Given Birth	Age at first birth	Given birth by age 18	Fertility	Mortality rate
	(1)	(2)	(3)	(4)	(5)
Incentive	-0.0385**	0.217**	-0.0130	-0.0450*	0.00165
	(0.0166)	(0.101)	(0.0109)	(0.0241)	(0.00600)
Emp. + Incentive	-0.0325**	0.230***	-0.0272***	-0.0550**	-0.00663
	(0.0165)	(0.0758)	(0.00923)	(0.0257)	(0.00513)
Empowerment	0.00953	-0.0107	0.00560	0.0104	0.00427
	(0.0101)	(0.0509)	(0.00630)	(0.0151)	(0.00336)
Control mean	66.7%	20.0	11.3%	0.9	2.6%
N	16,189	10,692	16,156	16,189	9,245

Table 13: Reproductive health outcomes for girls aged 15 and unmarried at program launch

	Given birth	Age at first birth	Given birth by age 18	Fertility	Mortality rate
	(1)	(2)	(3)	(4)	(5)
Incentive	-0.0337	-0.00717	0.00388	-0.0109	0.00121
	(0.0219)	(0.138)	(0.0176)	(0.0345)	(0.0116)
Emp. + Incentive	-0.0375	0.223**	-0.0292*	-0.0766**	-0.0132
	(0.0228)	(0.105)	(0.0158)	(0.0342)	(0.00911)
Empowerment	0.0288*	-0.0845	0.0147	0.0135	-0.00862
	(0.0152)	(0.0756)	(0.0114)	(0.0227)	(0.00623)
Control mean	61.5%	19.3	15.1%	0.8	3.2%
N	5,822	3,594	5,815	5,822	3,007

8.4 Robustness checks

We checked the robustness of our results to excluding controls, and running regressions in the subsample only. None of these changed the results significantly. Standard errors in parentheses * p<.1, ** p<.05, *** p<.01.

8.4.1 Excluding controls

Table 14: Marriage outcomes for girls aged 15–17 and unmarried at program launch

	Whether married	Whether married	Marriage age	Whether married <18	Whether married <17
Survey wave	Midline	Endline	Endline	Endline	Endline
	(1)	(2)	(3)	(4)	(5)
Incentive	-0.0284*	-0.0221	0.400***	-0.0798***	-0.0460**
	(0.0164)	(0.0137)	(0.104)	(0.0170)	(0.0151)
Emp. + Incentive	-0.0331**	-0.0185	0.397***	-0.0666***	-0.0451***
	(0.0168)	(0.0128)	(0.0906)	(0.0156)	(0.0138)
Empowerment	0.00857	0.00233	-0.0208	-0.00152	0.00308
	(0.0116)	(0.00761)	(0.0572)	(0.00980)	(0.00914)
Control mean	56.4%	85.7%	18.2	40.1%	27.0%
N	15,419	16,223	13,823	16,211	16,211

Table 15: Marriage outcomes for girls aged 15 and unmarried at program launch

	Whether Married	Whether married	Marriage age	Whether married <18	Whether married <17	Whether married <16
Survey wave	Midline	Endline	Endline	Endline	Endline	Endline
	(1)	(2)	(3)	(4)	(5)	(6)
Incentive	-0.0588***	-0.0229	0.422***	-0.0927***	-0.0699***	-0.0462**
	(0.0219)	(0.0194)	(0.131)	(0.0228)	(0.0213)	(0.0199)
Emp. + Incentive	-0.0661***	-0.0247	0.525***	-0.0926***	-0.0739***	-0.0666***
	(0.0236)	(0.0198)	(0.132)	(0.0247)	(0.0198)	(0.0177)
Empowerment	0.0288*	0.00509	-0.113	0.0169	0.00885	0.0181
	(0.0169)	(0.0120)	(0.0827)	(0.0150)	(0.0138)	(0.0120)
Control mean	50.0%	83.0%	17.6	45.2%	31.6%	21.5%
N	5,523	5,834	4,811	5,831	5,831	5,831

Table 16: In-school girls aged 15–17 and unmarried at program launch

Survey wave	Midline	Endline	Midline	Endline
Baseline age	15–17	15–17	15	15
	(1)	(2)	(3)	(4)
Incentive	0.0228 (0.0165)	0.0251 (0.0212)	0.0893*** (0.0252)	0.0586** (0.0278)
Emp. + Incentive	0.0447** (0.0178)	0.0533** (0.0216)	0.107*** (0.0268)	0.0765*** (0.0287)
Empowerment	0.00350 (0.0104)	0.0130 (0.0120)	-0.00231 (0.0177)	0.0265 (0.0170)
Control mean	28.9%	27.2%	33.6%	26.9%
N	15,189	10,801	5,458	4,323

Table 17: Reproductive health outcomes for girls aged 15–17 and unmarried at program launch

	Given Birth	Age at first birth	Given birth by age 18	Fertility	Mortality rate
	(1)	(2)	(3)	(4)	(5)
Incentive	-0.0315* (0.0174)	0.169 (0.103)	-0.0110 (0.0109)	-0.0344 (0.0267)	0.0000552 (0.00606)
Emp. + Incentive	-0.0343* (0.0175)	0.227*** (0.0754)	-0.0296*** (0.00883)	-0.0614** (0.0276)	-0.00809 (0.00511)
Empowerment	0.0101 (0.0106)	-0.0220 (0.0519)	0.00500 (0.00623)	0.0107 (0.0164)	0.00342 (0.00338)
Control mean	66.7%	20.0	11.3%	0.9	2.6%
N	16,189	10,692	16,156	16,189	9,245

Table 18: Reproductive health outcomes for girls aged 15 and unmarried at program launch

	Given birth	Age at first birth	Given birth by age 18	Fertility	Mortality rate
	(1)	(2)	(3)	(4)	(5)
Incentive	-0.0259 (0.0231)	-0.00284 (0.145)	0.00416 (0.0181)	-0.00138 (0.0363)	0.000739 (0.0116)
Emp. + Incentive	-0.0420* (0.0243)	0.274*** (0.104)	-0.0352** (0.0151)	-0.0864** (0.0369)	-0.0148 (0.00907)
Empowerment	0.0363 (0.0161)	-0.105 (0.0790)	0.0174 (0.0114)	0.0256 (0.0241)	-0.00868 (0.00604)
Control mean	61.5%	19.3	15.1%	0.8	3.2%
N	5,822	3,594	5,815	5,822	3,007

8.4.2 Girls in subsample households

Table 19: Marriage outcomes for girls aged 15–17 and unmarried at program launch

	Whether married	Whether married	Marriage age	Whether married <18	Whether married <17
Survey wave	Midline	Endline	Endline	Endline	Endline
	(1)	(2)	(3)	(4)	(5)
Incentive	-0.0183 (0.0318)	-0.0312 (0.0254)	0.0193 (0.213)	-0.0312 (0.0349)	-0.00550 (0.0292)
Emp. + Incentive	-0.0203 (0.0315)	-0.0182 (0.0248)	0.242 (0.164)	-0.0592** (0.0286)	-0.0269 (0.0272)
Empowerment	0.0305 (0.0233)	0.00583 (0.0163)	-0.211 (0.129)	0.0258 (0.0216)	0.0483** (0.0195)
Control mean	53.5%	84.9%	18.5	36.2%	23.1%
N	2,807	2,810	2,364	2,809	2,809

Table 20: Marriage outcomes for girls aged 15 and unmarried at program launch

	Whether Married	Whether married	Marriage age	Whether married <18	Whether married <17	Whether married <16
Survey wave	Midline	Endline	Endline	Endline	Endline	Endline
	(1)	(2)	(3)	(4)	(5)	(6)
Incentive	-0.0748 (0.0560)	-0.0890** (0.0440)	0.145 (0.311)	-0.0894* (0.0498)	-0.0476 (0.0463)	-0.0537 (0.0403)
Emp. + Incentive	-0.0818* (0.0481)	-0.0111 (0.0378)	0.532** (0.270)	-0.124*** (0.0456)	-0.0607 (0.0414)	-0.0347 (0.0362)
Empowerment	0.0553 (0.0345)	0.00841 (0.0270)	-0.351** (0.175)	0.0442 (0.0329)	0.0477 (0.0297)	0.0392 (0.0279)
Control mean	47.1%	82.4%	18.0	41.5%	25.9%	17.6%
N	1,064	1,077	876	1,076	1,076	1,076

Table 21: In-school girls aged 15–17 and unmarried at program launch

Survey wave	Midline	Endline	Midline	Endline
Baseline age	15–17	15–17	15	15
	(1)	(2)	(3)	(4)
Incentive	0.0116 (0.0308)	0.0218 (0.0379)	0.127*** (0.0489)	0.116** (0.0533)
Emp. + Incentive	0.0386 (0.0292)	0.0661 (0.0414)	0.137*** (0.0461)	0.0614 (0.0518)
Empowerment	0.0248 (0.0213)	0.00943 (0.0264)	0.0549* (0.0315)	0.0480 (0.0356)
Control mean	30.7%	27.2%	34.4%	25.4%
N	2,752	1,974	1,047	841

Table 22: Reproductive health outcomes for girls aged 15–17 and unmarried at program launch

	Given Birth	Age at first birth	Given birth by age 18	Fertility	Mortality rate
	(1)	(2)	(3)	(4)	(5)
Incentive	-0.0285 (0.0322)	0.133 (0.199)	-0.0174 (0.0200)	-0.0693 (0.0530)	-0.0257* (0.0136)
Emp. + Incentive	-0.0123 (0.0336)	0.254 (0.158)	-0.0356** (0.0177)	-0.0693 (0.0514)	-0.00830 (0.0149)
Empowerment	0.0268 (0.0223)	-0.0942 (0.123)	0.0151 (0.0132)	0.0102 (0.0368)	-0.00805 (0.00938)
Control mean	64.7%	20.1	10.2%	0.9	4.2%
N	2,804	1,788	2,787	2,804	1,547

Table 23: Reproductive health outcomes for girls aged 15 and unmarried at program launch

	Given birth	Age at first birth	Given birth by age 18	Fertility	Mortality rate
	(1)	(2)	(3)	(4)	(5)
Incentive	-0.0919* (0.0541)	-0.0477 (0.306)	0.0166 (0.0363)	-0.0985 (0.0866)	-0.0152 (0.0277)
Emp. + Incentive	-0.0832* (0.0499)	0.346 (0.266)	-0.0269 (0.0311)	-0.184** (0.0714)	-0.0109 (0.0334)
Empowerment	0.0712** (0.0354)	-0.128 (0.180)	0.0268 (0.0256)	0.0954* (0.0548)	-0.0286* (0.0171)
Control mean	58.6%	19.4	13.9%	0.8	6.0%
N	1,073	621	1,070	1,073	522

8.5 Qualitative analysis

To gain a deeper understanding of project activities and the girls' and their parents' perceptions of those activities, SC conducted a qualitative assessment through focus group discussions and key informant interviews in May 2010. Villages were selected randomly for participation from all KK treatment villages and by location (remote and close to an urban center) to ensure the sample selected included a variety in each category. One control village was included for comparison. However, as there was no way to identify the girls who would have attended safe spaces in the control villages, there were likely systematic differences between the girls interviewed in treatment and control villages. The quantitative study shows that girls who participated in KK tended to be more educated and come from better-off households than the average in a given village.

An important caveat is the danger of social desirability bias whereby respondents report what they think the evaluator will want to hear. Nevertheless, the qualitative work is

useful, particularly in providing details on participants' reaction to the program, what they remember as being important and engaging and what they hoped the program would achieve. The following is heavily quoted and partly excerpted from the 2010 KK project qualitative assessment (Titus 2010).

Bhola district was not included in the selection due to weather and time constraints. The assessment included 14 focus group discussions consisting of:

- One group of mothers of participating girls
- One group of fathers of participating girls
- One group of out-of-school participating girls
- Ten groups of participating girls
- One group of control girls

A total of 19 key informant interviews were conducted:

- Eleven interviews with participant girls
- One interview with a control group girl
- One interview with a mother of a participating girl
- Seven interviews with project staff at all levels of responsibility, from the program manager down to a field trainer

Discussion focused on three outcome-related themes: cultural norms regarding gender and marriage; spending and savings practices; and learning and behavior change.

8.5.1 Cultural norms regarding gender and marriage

The majority of the respondents indicated that girls are treated very differently from boys in their communities. The girls explained that sons are seen as longer-term assets to the family, while it is assumed that a daughter will marry and move to her husband's home and so will become the in-laws' asset. As a result, the males in the family receive more investment. The girls provided specific examples of the higher status of boys — for example, boys receive more food, have cell phones and enjoy the privilege of spending time with friends.

Respondents in both the mothers' and fathers' focus group discussions described treating boy and girl children differently. One father said that, prior to the project, he favored his sons. He claimed that, post-project, he started viewing his daughters as assets and explained that people understand that girls continue to take care of their parents longer (beyond marriage). Mothers made similar statements in their mothers' group discussions. It is possible that some of these comments were made in part because the parents assumed the evaluators wished to hear them. Girls and parents – independently of each other– talked about new perspectives on the treatment of girls, signaling awareness of the issues.

The project participants' comments on the "normal" age for marriage for girls varied slightly, although the most common response was 18. However, all were also aware that the legal age of marriage in Bangladesh is 18; therefore, it is likely that the respondents were providing what they knew to be the "correct" answer. When drawn into further – more relaxed – conversation about marriage, the respondents often talked about friends or other girls in the village who were married at ages ranging from 13 to 16. Probing further, girls quickly explained that in these cases, the parents were "unaware,"

"uneducated" or "illiterate." They also noted that some families were under particular financial strain and so, while they might be aware of would be considered early marriage, these families must marry off their daughters because they could afford to support them. This finding points to participants' awareness of the power structure within their society. Similarly, conversations in the control village noted that 16 is considered "early" for marriage and is undesirable; however, participants remarked that there is nothing that can be done if the parents insist on the marriage despite a daughter's objections. The typical age for boys was more consistent: Participants was most often reported that marriage takes place between age 21 and 25, and there were very few reports of "early marriage" for boys. There was no difference in the quantitative evaluations between KK and non-KK communities regarding the appropriate age for marriage.

The assessment showed mixed findings around the topic of dowries. In several focus group discussions, participants commented that dowries were not paid in their communities and were illegal. However, this was inconsistent with information revealed later in the discussion or in other interviews. Girls explained that dowries were necessary because, as noted above, communities typically viewed daughters as a "burden" to the family and therefore must provide an incentive to the groom's family to take on the extra liability.

8.5.2 Spending and savings practices

In the qualitative discussions, KK participants suggested that they had moved from a wants-based to more needs-based spending approach and were more aware of the benefits of saving and of moving away from unsafe savings practices such as placing money under a mattress or in a clay pot. The control group indicated they carried what money they had in their schoolbags. Girls said they learned how to budget so as to save and wanted to cut down on non-essential purchases. Some girls reported using their small savings to purchase seeds or small plants or animals to undertake small income-generating activities. A few girls described using savings to support mini-handicraft and tailoring businesses. It is worth noting that the quantitative analysis found no robust difference in savings rates between KK and non-KK communities, on average.

The research found that one of the project groups that had not been introduced to group savings had nonetheless begun saving jointly. At the time of the qualitative assessment, the group had already completed a six-month savings cycle and planned to conduct a year-long cycle. Many of the girls in the group were using their money to conduct small-scale rearing of hens and goats. They hoped that, after a year, they could collectively buy a cow with the savings so as to earn more money. The girls in this particular group had dropped out of school and were especially enthusiastic about these income-generating activities, which they saw as another negotiating point against early marriage. They explained that the activities helped demonstrate that they could contribute to the household income and were not financial burdens on their families. The specific origins of this group's savings practices or which model they were using for their savings activities were unclear.

8.5.3 Learning and behavior change

Girls shared some of the information they had learned in the project sessions and provided examples, such as learning about nutrition, how to combine food groups to meet energy needs and the importance of safe drinking water. They also discussed

learning “about themselves,” facilitated by the project’s social competency module and practiced in ongoing discussions with one another, their families and community members. Girls’ comments regarding feeling able to negotiate situations with parents and boys and better understand their own wants and goals were indicative of an increase in self-awareness and self-esteem. Many of the young girls mentioned learning critical thinking and problem-solving skills. An example of problem-solving cited in more than one focus group was confronting harassment by boys. Girls stated that, prior to the project, they would simply accept harassment and that, in some severe instances, it may have affected their mobility. They suggested that they now felt they possessed the tools and courage to confront the boys, explaining that their behavior was “harming the community.”

Across all focus group discussions and key informant interviews, the project girls stressed foremost their learning around early marriage. Program participants frankly discussed the dangers that early marriage poses to a girl’s health and well-being. In several focus group discussions, girls added that adolescence was a time for studying and playing, not for taking on family responsibilities. Respondents provided several examples of instances in which group members – and in one case an entire KK group – had spoken to parents and persuaded them to not move ahead with an early marriage.

While the girls mentioned poverty as a risk factor for early marriage, their comments and stories underscore other pressures such as finding “good grooms” and preserving a girl’s virginity. Because offers of marriage come only intermittently, parents may feel pressure to accept a “good groom,” worrying that if they turn down the opportunity, a daughter may not receive an equally good proposal in the future (see Appendix D). Parents also worry when young girls begin to interact with boys and often consider marriage before a daughter becomes spoiled. Perceived safety, economic status and uncertainty about future prospects were all mentioned as important motivations for early marriage.

Another area of learning that girls mentioned was personal hygiene and menstrual care. In focus group discussions and key informant interviews, project participants spoke of learning about proper use menstrual hygiene products to avoid infections. They also noted the need for extra nutrients during menstruation. The girls said they had shared this information with their mothers with the objective of receiving more food during their periods. Mothers also mentioned discussions of menstruation practices and nutrition in focus groups. However, the quantitative research found no difference in knowledge of iron-rich foods between girls in KK and non-KK communities.

One objective of the KK program was to increase KK participants’ self-confidence. During the conversations, girls frequently commented that they now felt able to approach adults in the community, to speak up and share information at home and to negotiate resolutions to problems. They described how having access to information helped them assume roles as “informant” on certain topics (nutrition, health and hygiene, safe drinking water and others). In many communities, SC explicitly used KK girls to describe and organize disaster-preparedness sessions to ensure readiness for possible cyclones. Project staff remarked that girls appeared to emerge from their shells during the project and that communities seemed to take pride in their newly “empowered” girls. Some fathers commented on the girls’ ability to organize events, contribute to budgets, prepare

the logistics and content of celebrations of special days, prepare the household for natural disasters and provide information about nutrition.

Ongoing qualitative work funded by another grant focuses on strategies that girls and women use to overcome barriers to continue education and generate income. Appendix D presents two case studies from this ongoing work.

9. Cost-effectiveness and cost-benefit analysis

We conducted cost-effectiveness and cost-benefit analyses comparing various approaches to reducing the incidence of early marriage in developing countries. We included only interventions for which there are medium- to high-quality evaluations that tracked child marriage or marriage age as an outcome. We thus consider five programs from South Asia, Sub-Saharan Africa and Bangladesh that demonstrate impacts on child marriage rates or girls' age of marriage.

We find the financial incentive to translate into 1.62 years of delayed marriage and 1.00 years of schooling for every US$1,000 invested – the highest impact among the high-quality papers included in our cost-efficacy analysis (Table 29). Likewise, with a NPV of US$1,986 per US$1,000 invested, the financial incentive conditional on marriage has a relative NPV that is higher than the NPV of any other study with a significant impact on marriage age or child marriage when converting (Figure 9).

9.1 Interventions included in cost-benefit analysis

9.1.1 Intervention 1: The Female Secondary School Stipend Program in Bangladesh

Bangladesh introduced the FSSAP, a large-scale education promotion program, in 1994 to make secondary education free for girls in rural areas. The program aimed to address the gender gap in secondary education by encouraging more girls to complete secondary education. In addition to free schooling, the FSSAP paid a small stipend to eligible girls conditional on their enrollment, a minimum 75% attendance rate in school, a minimum 45% average on annual exams and remaining unmarried. The level of the stipend varied by grade and by year. The program covered more than 2 million girls each year and was the Bangladesh government's most prominent education program through the 1990s and 2000s (Hahn et al. 2015). The program was later changed to include stipends for poor boys in addition to girls. We only consider the first iteration of the FSSAP in our analysis.

Hong and Sarr (2012), Hahn and others (2015) and Heath and Mobarak (2015) all evaluate the impact of the FSSAP in Bangladesh using a difference-in-difference strategy. Hahn and others and Hong and Sarr both exploit the fact that only girls in rural areas were eligible for the stipend and compare education and marriage outcomes for rural versus urban girls before and after the introduction of the FSSAP. The main difference between the two is that Hong and Sarr also examine the earlier introduction of free secondary education for girls. Both papers find large effects on education and age of marriage from the FSSAP (an increase in age of marriage of 0.6 to 2.3 years). It is worth noting a caveat to these results: The raw age of marriage data suggest 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 the age of marriage of rural girls. Heath and Mobarak examine the impact of the FSSAP in peri-urban areas (still classed as rural) by comparing girls who reached secondary school just before and just after the FSSAP, and find no statistical difference. They note that the gender gap in education was closing long before the introduction of the FSSAP 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 FSSAP papers, is that the authors do not exploit geographic differences in eligibility (which could confound program impact with differential trends in rural versus urban areas), and they compare girls in a much shorter age window around the introduction of the program.

9.1.2 Intervention 2: Vouchers for private education in Colombia

In 1991, the Colombian government established a voucher program for low-income students to attend private schools as a way to rapidly expand secondary school access despite limited public secondary schools. The Programa de Ampliación de Cobertura de la Educación Secundaria (PACES) was one of the largest voucher programs to date, providing over 125,000 students from poor urban neighborhoods with vouchers that cover more than half the cost of private secondary school in Colombia. PACES vouchers covered the average tuition of low-to-middle-cost private schools in Colombia's largest cities. The vouchers were available for both boys and girls and were distributed by random lottery within the pool of eligible applicants. The random allocation of the vouchers allowed for a series of evaluations to establish rigorous evidence of the impacts of the program (Angrist et al. 2002, Angrist et al. 2006, Bettinger et al. 2010). The vouchers increased test scores for girls and led to a decrease in cohabitation.

9.1.3 Intervention 3: Free school uniforms in Kenya

While the Kenyan government eliminated school fees in 2003, students are generally still required to purchase and wear uniforms to attend school. The Child Sponsorship Program, an ICS Africa project, implemented a non-cash transfer intervention in Busia, Kenya, that provided school uniforms at no cost to primary school students. The uniforms were meant to decrease financial barriers to schooling and increase attendance for both boys and girls. Schools were randomly assigned to participate in the program, which allowed for causal inferences from a randomized evaluation. In total, 83 schools received the standalone school uniform subsidy, with an average of 29.3 eligible girls per school. (Duflo et al. 2005). The program increased schooling and decreased marriage rates.

9.1.4 Intervention 4: Empowering adolescent girls in Uganda

From 2008 to 2010, a large cluster randomized trial evaluated BRAC's Empowerment and Livelihood for Adolescents Program (ELA) in Uganda.The program worked with BRAC's permanent centers in villages to provide life skills and vocational training for girls aged 14–20 through adolescent development clubs and sessions led by young female mentors. The clubs were open five afternoons per week after school and covered issues of sexual and reproductive health, menstruation, pregnancy, sexually transmitted infections and HIV and AIDS awareness, family planning and rape. The trainings also provided information on conflict resolution and legal standards regarding bride price, child marriage and domestic violence. Vocational skills training focused on small business development, including courses on tailoring, computing and dancing. The clubs served as recreation centers for the girls and provided safe spaces where they could

meet and discuss their problems privately. ELA centers led to decreases in underage marriage and cohabitation and significantly reduced childbearing. Additionally, girls in villages with ELA centers were more likely to practice safe sex and were less likely to have had a pregnancy (Bandiera et al. 2015).

9.1.5 Intervention 5: Unconditional cash transfers for girls in Malawi

Between 2008 and 2009, researchers performed a randomized evaluation of an unconditional cash transfer (UCT) program for girls in Malawi. The study took place in southern Malawi's Zomba district in both the large urban center, Zomba city, and many surrounding rural and semi-rural villages. Girls and their families received monthly stipends – ranged from US$4 to US$10 plus the cost of school fees– over the course of two school years. Girls receiving the cash stipends were 8 percentage points less likely to be married and 7 percentage points less likely to be pregnant than girls in the comparison group. A parallel study of a cash transfer conditional on 80% school attendance found no significant effects on marriage or pregnancy outcomes but did find improvements in school enrollment, attendance and test scores. The UCT program may have been more effective in delaying marriage and childbearing because it enabled girls who dropped out of school to support themselves without relying on a husband (Baird et al. 2011, 2016).

9.2 Methodology

Benefits of delayed marriage are calculated based on the cumulative education wage premium for girls eligible for each program. We assume girls start working at age 17.5, the median age of marriage for girls in the control group of the oil incentives evaluation (Buchmann et al. 2017), and continue working until age 60. 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 also assume that extra education delays girls' entries into the workforce, and that they begin working immediately after finishing their studies, provided they are older than the median age of marriage. To consider all interventions in the same time frame, we consider all interventions as if they had started in 2008.

To estimate the wage premium benefits of the program, we use the estimated wage premium from Montenegro et al. (2014) to determine the estimated income for girls in each year of each program. They use a modified Mincer equation to estimate girls' expected wages in each year of their working life as a function of education and experience, which we modify to calculate a girl's age in each year:

$$income_t = \prod_{k=1}^{t}(1+\rho_k) \quad * e^{(\alpha + \beta_1(years\ of\ schooling) + \beta_2(experience_t) + \beta_3(experience_t{}^2) + \varepsilon}$$

Where α is the log of mean income 2005–2014 for women in Bangladesh with no education or experience (Montenegro et al. 2014). To account for growth in wage levels over time, ρ is the GDP per capita growth in Bangladesh for years until 2014 and the mean growth rate from 2005–2014 for 2015 and after. *t* is the difference between the year of the analysis and the base year. All interventions are considered as beginning in 2008, the first year of the oil incentive program, which we use as the base year for our calculations. Girls enter the workforce in 2010, defined as the year they reach the

median age of marriage, adjusted for additional education induced by each intervention. Experience is calculated as the number of years since a woman entered the workforce. β_1 is the private return to an additional year of secondary schooling in Bangladesh, 6.9% in 2005 (Montenegro et al. 2014), the latest year for which data are available. The terms β_2 and $\beta_3$are estimates of the returns to experience and experience squared for women in Bangladesh.

To calculate the benefits for each year of an intervention, we first calculate estimated wages for a girl receiving the intervention and the estimated wages for a girl not receiving the intervention.

For girls receiving the intervention, income in each year is estimated as:

$$income_t = \prod_{k=1}^{t}(1+\rho_k) \quad * e^{(\alpha + \beta_1(years\ of\ schooling) + \beta_2(experience_t) + \beta_3(experience_t{}^2) + \varepsilon}$$

Where λ is the point estimate for the education benefit of a program and ω is a girl's age in year t. For comparibility, we use OLS for all interventions. For girls not receiving the program, income in each year is estimated as:

$$income_t = \prod_{k=1}^{t}(1+\rho_k) \quad * e^{(\alpha + \beta_1(years\ of\ schooling) + \beta_2(experience_t) + \beta_3(experience_t{}^2) + \varepsilon}$$

For the girls receiving the program, this takes into account the educational income premium from the additional years of schooling induced by the program as well as the loss of work experience from staying in school. As mentioned above, experience begins to accrue for all girls not in school after the median age at first marriage.

The present value of both costs and benefits of each program are then defined as follows:

$$Present\ Value\ = \sum_{t=0}^{T} \frac{(annual\ amount_t)}{(1+\theta)^t}$$

Where T is the number of years between the beginning of the intervention and the end of a woman's working life and θ is the social discount rate. We report results using a social discount rate of 5% and additionally provide sensitivity analyses with discount rates of 3% and 10%.

For all calculations, we first take the present value of the program cost and benefit streams in 2008 BDT as described above. We then reflate the streams to 2014 BDT using year-specific inflation rates. Finally, we convert the streams from 2014 BDT to 2014 USD. For implementation cost calculations, we first convert from local currency to BDT in the year of the intervention. We then inflate the costs to 2008 BDT using inflation rates from Bangladesh and proceed using the same order of operations as for the other streams.

9.3 Benefits calculations

We define the benefits each year as the estimated income for a girl receiving the intervention minus the estimated income a girl in the program would have received without the education benefit of the program. This is the annual education benefit that we expect girls to receive from having been induced to study longer by each intervention. As detailed above, the estimated income for a girl receiving a program is:

$$\prod_{k=1}^{t}(1+\rho_k) * e^{(\alpha+\beta_1(median\ schooling+\lambda)+\beta_2(\omega_t-median\ marriage\ age-\lambda)+\beta_3((\omega_t-median\ marriage\ age-\lambda)^2)}$$

From this we subtract the counterfactual wages for a girl who participated in the program but did not receive the education benefit:

$$\prod_{k=1}^{t}(1+\rho_k) * e^{(\alpha+\beta_1(median\ schooling)+\beta_2(\omega_t-median\ marriage\ age-\lambda)+\beta_3((\omega_t-median\ marriage\ age-\lambda)^2)}$$

This gives us the benefit per eligible girl in any given year. The annual benefit is defined as the above term multiplied by the total number of girls eligible.

For the cost-benefit analyses, we also consider the value of stipends or transfers as benefits, discounted to 2008 and reflated to 2014 USD as described above.

Note that wage premiums are based on the wages of those in the labor force with monetized wages. The assumption behind Mincer equations and our estimates is that education increases productivity as much for women not earning a wage (including those working in the household) as it does for women working for a wage outside the household. Our results are likely to be sensitive to this assumption, but the assumption has the same impact on all programs equally. Cost-effectiveness calculations using only costs to implementers, however, do not require reliance on this assumption and simply express how many additional years of education can be gained from spending on a given program.

9.4 Estimates of additional years of schooling from delayed marriage

For each program for which we have data on impacts on education, we calculate the educational benefits to delayed marriage using two different methods to convert impacts on age of marriage to additional years of schooling. First, we estimate overall additional years of schooling using the education coefficient from each respective analysis and multiplying it by the number of girls eligible for the program. This is the observed impact on education in the particular context of the evaluation. Second, we estimate the additional years of schooling per year of delayed marriage for the financial incentives to delay marriage in Bangladesh. We then apply this conversion factor to the other interventions to estimate the implied additional years of schooling had the program taken place in southern Bangladesh. For studies in which we have age at first marriage as an outcome, this results in a conversion factor which is 0.86 additional years of schooling for every additional year unmarried. Where we have only child marriage rates, we use a

conversion factor of 0.04 additional years of schooling for every percentage point reduction in child marriage. We use only the converted impact for the evaluations that did not have a statistically significant impact on education outcomes. Where possible, we present estimates of the benefit-cost ratio for programs using both methods to calculate additional years of schooling.

We take this approach for a number of reasons. First, the quality of the evaluations varies, and the estimated educational returns to the program may not be equally reliable. Standardizing the assumed educational returns to delayed marriage from a recent rigorous evaluation helps normalize the quality of the estimates. Second, not all the interventions took place in Bangladesh. We might expect the educational returns to delayed marriage to be quite different in Kenya and Colombia than in Bangladesh. Estimating the educational benefits using the educational returns from Bangladesh allows us to estimate the benefit-cost ratio of the other programs had they been implemented in Bangladesh.

As discussed above, years of schooling and age of marriage are causally linked in both directions; delaying marriage leads to more schooling, and more schooling leads girls to delay marriage. Many of the programs we consider in the cost-benefit analysis are principally intended to increase girls' educational attainment. In this cost-benefit analysis, we are primarily interested in programs that reduce child marriage and seek to quantify the benefits of that reduction in child marriage through its impact on education. By applying a conversion factor derived from a program that primarily targeted age of marriage (and likely influenced age of marriage even for out-of-school girls), we may be disadvantaging the oilincentive program at the expense of those programs that are more focused on education.

9.5 Cost calculations

For all programs, we consider the costs to the beneficiary as well as the costs to the implementer. For the cost to the implementer, we consider actual program costs, where available, and estimate program costs elsewhere. Where monitoring costs for programs with conditional eligibility were not available, we consider the monitoring costs per girl per year of the oil incentive program multiplied by the number of eligible girls and years in the relevant intervention.

We also consider the opportunity cost of girls' foregone income over their entire working life from having fewer years of work experience. This cost is the difference in lifetime income for a girl due to having less experience because she was induced to stay in school. For each year, we calculate this cost by first estimating the income of a girl with median education and experience:

$$\prod_{k=1}^{t}(1+\rho_k) * e^{(\alpha+\beta_1(median\ schooling)+\beta_2(\omega_t-median\ marriage\ age)+\beta_3((\omega_t-median\ marriage\ age)^2)}$$

From this we subtract the counterfactual income of a girl with median schooling and fewer years of experience equal to the education effect of the relevant intervention:

$$\prod_{k=1}^{t}(1+\rho_k)$$
$$* e^{(\alpha+\beta_1(median\ schooling)+\beta_2(\omega_t-median\ marriage\ age-\lambda)+\beta_3((\omega_t-median\ marriage\ age-\lambda)^2)}$$

This term is the same term subtracted from the wage of a girl in the treatment arm to calculate the benefits of the program, and results in the foregone wages due to fewer years of workforce experience. The total costs of each program include the program-specific costs to the implementer plus the income opportunity cost of education over a participant's working life. We calculate the NPV of the cost stream using the method described above.

9.6 Additional assumptions for the cost and benefit estimates for specific interventions

In addition to these general assumptions, intervention-specific assumptions are described in more detail below.

9.6.1 Intervention 1: The female school stipend program in Bangladesh

Because we did not have access to administrative cost data, we estimate program costs as the costs of secondary school stipends plus the monitoring costs per girl from the oil incentive program. For the transfer cost of the stipends, we apply the transfer rate from a similar program in Pakistan, approximately 40% of the cost of the stipend (Chaudhury and Parajuli 2006). The stipend costs are estimated by using the number of total stipends per yearand the amount of the average stipend per grade, and assume equal distribution of stipends across grade years and that the amount of the stipend remained constant for girls while they participated in the program. We use estimates of cohort size from several sources to estimate the number of girls eligible for the stipend each year (Raynor 2016, Hahn et al. 2015, Hong et al. 2012). We only consider girls who were eligible for a full five years of the stipend and assume equal distribution of girls per grade. This results in estimates of approximately 11,752,000 eligible girls.

9.6.2 Intervention 2: Vouchers for private education in Colombia

To estimate the cost of the voucher program, we use average annual secondary education costs to the government per female scholarship winner (Bettinger et al. 2010). The costs include the annual value of a PACES scholarship and the expenditure for scholarship costs for students who would have enrolled in private school adjusted for expenditures resulting from transfers from public to private schools and cost savings from reduced grade repetition.

Because researchers did not directly measure age at first marriage, we use the reported change in teen pregnancy as a proxy for changes in child marriage rates. Additionally, since the results come from a non-published intermediate paper, both the costs and benefit results may still change.

To establish the number of eligible girls, we use the number of vouchers distributed (90,000), divided by the take-up rate (90%) for a full sample of 100,000 girls (Angrist et al. 2002). For each subsequent year, we use a take-up rate of 77%, the average re-enrollment rate for scholarship recipients (Angrist et al. 2002).

Since the scholarship program moved some students from public to private schools, there may have been a number of costs and benefits to the government that we do not include. For example, we do not count changes in tertiary education costs, loan subsidies, forgone tax revenue from VAT tax, changes in government revenue or forgone net government transfers through payroll taxes. Many of these costs are specific to the Colombian government context, and these costs may not be relevant for replications of the program in Bangladesh. Additionally, we exclude those costs for better comparability to the other programs considered.

9.6.3 Intervention 3: Free school uniforms in Kenya

For the free school uniforms program in Kenya, we use reported program costs including the cost of girls' school uniforms and NGO worker wages and travel costs. These costs are all detailed in the J-PAL website's cost-effectiveness section.

9.6.4 Intervention 4: Empowering adolescent girls in Uganda

To estimate the costs and benefits of the BRAC Uganda program, we use extensive program cost data from Bandiera and others (2015). Reported costs for the 3,964 girls in the treatment group include office space and equipment, program management and staff compensation, training and refresher course costs for adolescent leaders, club materials and rent, and the direct costs of financial literacy and livelihood trainings for girls. We exclude the country and branch office overheads reported in the paper for consistency with cost calculations across programs.

9.6.5 Intervention 5: Unconditional cash transfers in Malawi

For the UCT program in Malawi, we estimate the number of girls eligible for the program by multiplying the average number of girls per enumeration area, the unit of randomization, by the total number of enumeration areas. Costs include the fixed and variable costs to distribute the transfers, the cost of a census to establish the number of eligible girls and the value of the transfers themselves.

9.7 Conditional financial incentives in Bangladesh

We have the most complete cost data for the cooking oil incentives in Bangladesh. Cost estimates include the costs of oil, monitors to confirm girls' marital status, transportation costs to deliver the cooking oil and staff salaries of district point people, field officers, volunteers, and distribution workers.

The oil transfers were delivered through a food security program (JOJ) in the area, that provided food transfers to pregnant and lactating mothers. JOJ's existing infrastructure led to cost savings for the oil transfer program. We estimated the program costs both including and excluding the benefit from working with an existing distribution partner.

We consider costs and benefits only for girls who were 15 at the start of oil distribution because they received the full program, as would be the case if the program were scaled up. To estimate costs, we assume the costs for 15-year-old girls are proportional to the percentage of 15-year-old girls eligible for the program each year. We calculate all the costs (for ages 15 to 17) and then discount them to reflect the portion of girls who were 15 at the time of the oil distribution. This portion increases for each year as a result of girls aging out of the program.

9.8 Net present value per US$1,000 investment

We calculate the NPV as the difference between the discounted benefits and the discounted costs of each program. Since the programs vary widely in scale, the NPVs themselves are not directly comparable. The larger programs we consider have considerably larger NPVs, but only because they reached many more girls at scale. To aid in comparing the programs, we divide the NPV by the total amount invested (costs to implementer and beneficiaries). We then present the NPV for each program in terms of NPV per $1,000 invested.

9.9 Benefit-cost ratio

We present a benefit-cost ratio for each of the programs. For these calculations, we divide the total NPV of the benefits of a program by the NPV of its costs. All discounting and conversions are calculated as described above.

9.10 Cost-effectiveness analysis

In addition to the cost-benefit analysis, we provide estimates of the cost-effectiveness of each program in terms of a variety of outcomes including child marriages averted, additional years of schooling and years of delayed marriage. These estimates are meant to give a sense of the relative efficiency of the programs at meeting particular outcomes without requiring the full set of assumptions of a cost-benefit analysis. To calculate cost-effectiveness, we estimate the amount of a given outcome that would be achieved for a given investment.

For example, for child marriages averted, we first multiply the point estimate of reduction in underage marriage by the number of girls eligible for the program. This gives us the total number of child marriages averted by the program. We then divide the present value of the costs of the program by the number of child marriages averted to determine the cost per child marriage averted. We then divide 1,000 by the cost per child marriage averted to express the figure in terms of returns on an investment of US$1,000.

For the oil incentive program, we present results both including and excluding foregone income as a cost. This allows us to show the cost efficiency both from the perspective of the implementer alone and for the implementer and beneficiary. For all cross-comparisons with other interventions, we include both foregone income and implementation costs.

9.11 Limitations of the analysis

Our estimates of the returns to education are based on a standard Mincer equation that compares earnings for those women in the workforce with different levels of education. Two key assumptions are necessary for this to reflect the gains to the economy of increases in education. First, it assumes the high wages of women who are more educated are the result only of their education and not due to unobservables (such as motivation) that may be correlated with higher than average education. Second, it assumes that women who are not in the workforce but have had more education have an equal increase in productivity in the work they do at home as those who are in the workforce. If there is selection of more able or more motivated women into education, or

if education raises productivity less for those not in the labor force, then our estimates will overestimate the NPV of all the programs discussed here.

Causal evidence shows the effects of child marriage on educational attainment. However, as discussed above, corollary links form the bulk of the evidence on other effects of child marriage. We may imagine a range of long-term benefits for adolescent girls who delay marriage in terms of improved health outcomes, higher household decision-making power or intergenerational well-being. The lack of causal evidence on these channels, along with the difficulty of monetizing their benefits, severely limits what can be included in a cost-benefit analysis. As a result, when monetizing benefits, we consider only the benefits of delayed marriage through the wage premium from increased years of schooling. We are therefore likely undervaluing the total benefits of each intervention.

Similarly, on the cost side, only the direct costs of each program and foregone wages are considered. We do not include costs associated with changes in the size or timing of dowries, for instance, or other less tangible benefits and costs that would require a large number of additional assumptions for which evidence is limited.

The availability of cost data varies across interventions. For the financial incentives in Bangladesh, school uniforms in Kenya, and vouchers in Colombia, we have extensive information about the actual costs of delivering the interventions. However, for the FSSAP programs in Bangladesh, we have estimates only for the cost of the stipends themselves and thus have to estimate other costs. For the BRAC Uganda intervention, we have extensive records of the program costs but only a rough estimate of the number of eligible girls. This makes true comparisons of the efficiencies of the programs difficult and may lead us to overestimate the cost-effectiveness of the programs for which we have limited cost data. In particular, the high involvement of researchers in programs evaluated by randomized controlled trials may mean that cost data are collected more comprehensively, making those programs appear costlier. In addition, programs tested at small scale may have higher costs than they would if they were scaled up. We attempt to address this in at least one way by using the converted monitoring costs for programs for which we do not have monitoring cost estimates.

We are unable to take into account general equilibrium effects. Most importantly, if increases in education lead to a decline in the marginal return to education, this would depress the benefits of all the interventions discussed here. Working in the other direction, there may be social benefits to education that are not captured in Mincer regressions, and even complementarities in the returns to additional education, which would suggest our benefits are underestimates.

The dynamics of marriage are very different in South Asia than in Africa and Latin America, the locations of several of our comparative cases. Whereas parents in Bangladesh exercise significant control over their daughters' marriage age decisions, girls in Malawi and Colombia have more influence over their own decisions. As a result, much of the evidence from regions outside South Asia focuses on teenage sexual activity or cohabitation rather than marriage. These differences make cross-comparisons difficult, and we may expect interventions to have very different results given the dynamics of each cultural context.

9.12 Results

9.12.1 Cost-benefit analyses

We compare the net benefits of six programs with significant effects on child marriage in terms of their cost-effectiveness in increasing years unmarried, child marriages averted and additional years of schooling using point estimates of the effects of each intervention. We then present benefit-cost ratios for each program, first using the estimated education impacts from the individual evaluations and second assuming the educational returns to delayed marriage from the financial incentives to delay marriage in Bangladesh. For interventions that took place outside Bangladesh, we present only the latter results. Sensitivity analyses and more details on each intervention are included in the appendixes.

In Figures 8 and 9, we compare all of the inventions using the education conversion coefficient from the oil incentives program in Bangladesh to simulate the expected return to the program if they were implemented there. We present the results both as NPV per US$1,000 investment and as the benefit-cost ratio of each program to control for the size of each program. Figures 10 and 11 compare the outcomes of years unmarried and child marriages averted per US$1,000. Not surprisingly, we observe a high correlation between the cost-effectiveness in marriage outcomes and cost-benefit.

Figure 8: NPV per US$1,000 (cost to beneficiary and implementer), 5% discount rate

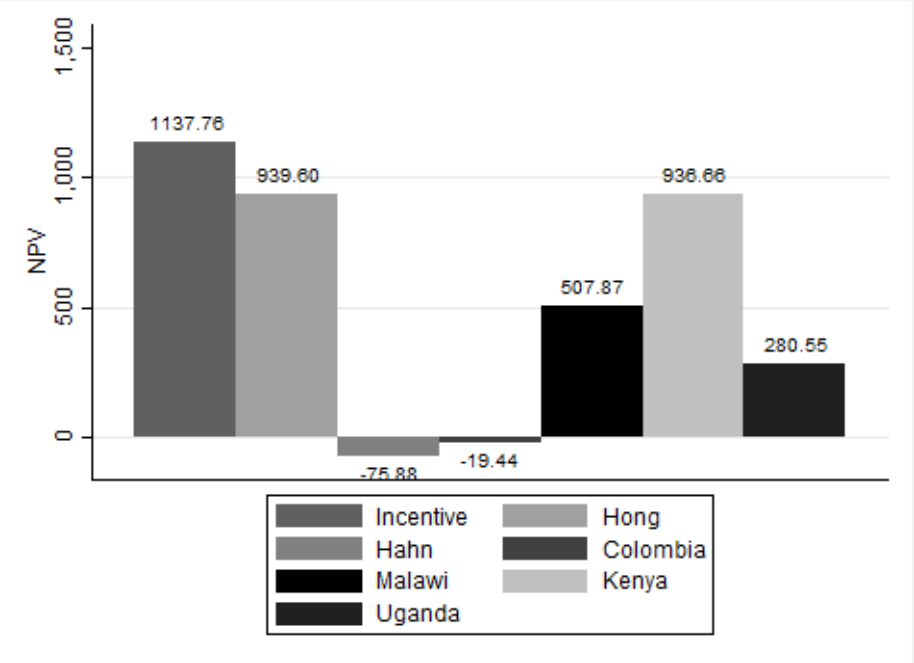


Figure 9: Benefit-cost ratio (cost to beneficiary and implementer), 5% discount rate

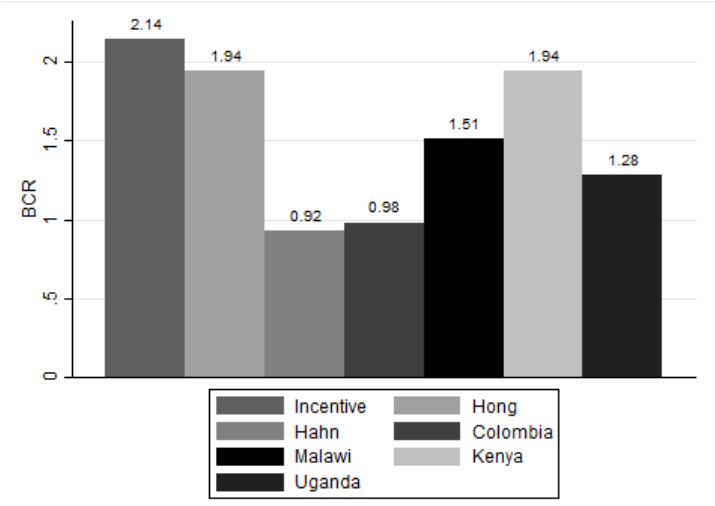


Figure 10: Years unmarried per US$1,000 (cost to beneficiary and implementer), 5% discount rate

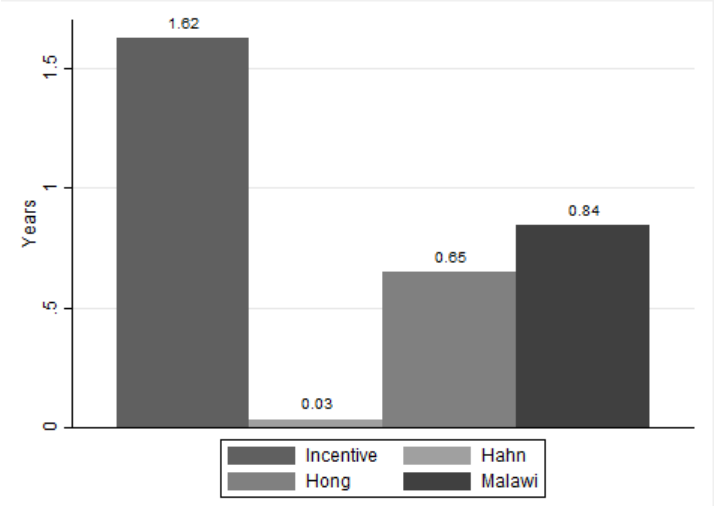


Figure 11: Child marriages averted per US$1,000 (cost to beneficiary and implementer), 5% discount rate

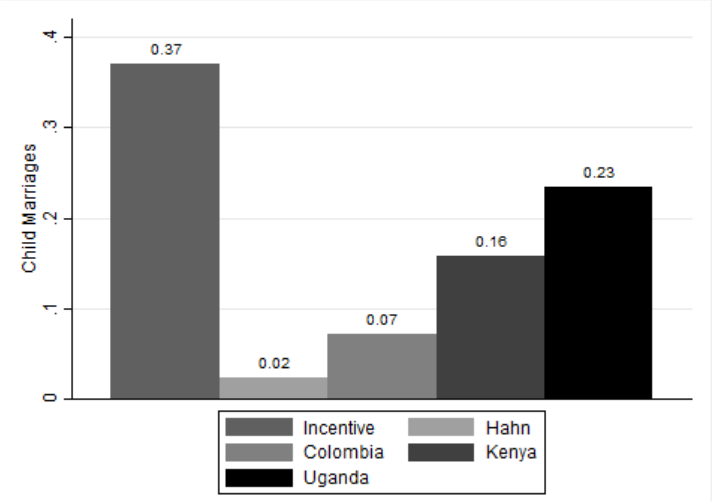


Table 24: Summary of cost-effectiveness and cost-benefit analysis, 5% discount rate

Intervention type	Intervention	Location	Marriage impact	Outcome measure	Outcome per US$1,000
Financial incentive to delay marriage	Oil incentive	Bangladesh	Yes	Additional years unmarried	1.62
				Child marriages averted	0.37
				Additional years of schooling	1.00
Incentives to increase girls' education	FSSAP • Hahn et al.	Bangladesh	Yes	Additional years unmarried	0.03
				Child marriages averted	0.02
				Additional years of schooling	0.13
	FSSAP • Hong and Sarr			Additional years unmarried	0.65
				Additional years of schooling	0.82
	Vouchers for private schools	Colombia	Yes	Child marriages averted	0.07
				Additional years of schooling	0.19
	Free school uniforms	Kenya	Yes	Child marriages averted	0.16
				Additional years of schooling	0.88
	Conditional cash transfer	Malawi	No	Additional years unmarried	0.00
Unconditional cash transfer	UCT	Malawi	Yes	Additional years unmarried	0.84
				Additional years of schooling	0.52
Girls' empowerment	Empowerment and Livelihood for Adolescents Program (BRAC)	Uganda	Yes	Child marriages averted	0.23
				Additional years of schooling	0.64
	Empowerment and Livelihood for Adolescents Program (BRAC)	Tanzania	No	Additional years unmarried	0.00

Table 25: Summary of cost-effectiveness and cost-benefit analysis for interventions with estimated marriage impact, 5% discount rate

Intervention type	Intervention	Location	Benefit-cost ratio	NPV per US$1,000
Financial incentive to delay marriage	Oil incentive	Bangladesh	2.14	1,137.76
Incentives to increase girls' education	FSSAP • Hahn et al.	Bangladesh	0.92	-75.88
	FSSAP • Hong and Sarr		1.94	939.60
	Vouchers for private schools	Colombia	0.98	-19.44
	Free school uniforms	Kenya	1.94	936.66
Unconditional cash transfer	UCT	Malawi	1.51	507.87
Girls' empowerment	Empowerment and Livelihood for Adolescents Program (BRAC)	Uganda	1.28	280.55

10. Conclusion

We provide novel evidence that a relatively inexpensive conditional stipend program targeted to the families of adolescent girls in a setting with a high rate of adolescent marriage is effective in delaying the marriage of participants. We also find concrete evidence that delaying adolescent marriage leads directly to large increases in school enrollment in this setting. Finally, our results indicate that a well-crafted and quite intensive empowerment program targeting adolescent girls in a disadvantaged setting was ineffective in reducing rates of early marriage. There is, however, some evidence that the program has an effect on education enrollment. It is also possible that there are benefits of adolescent empowerment that do not translate into changes in marriage age but that will translate into gains in reproductive health outcomes or marital bargaining power at a later stage.

The evidence also suggests that both the incentive-only and full treatment (incentive plus empowerment) arms significantly increase age at first birth and reduce childbearing. However, it appears that the effect on childbearing outcomes is stronger for the full treatment on younger girls. Potentially, younger girls that received the incentive stayed unmarried for a longer time and were thus allowed to attend the empowerment sessions as opposed to some of their married peers. This also explains higher self-reported and monitored take-up in the full treatment arm as opposed to the empowerment-only arm.

In addition to evaluating a particular program, a key objective of this study was to create exogenous variation in school, age of marriage and financial independence to be able to examine the causal impact of those factors on women's health, health-care utilization,

decision-making power and income-generating ability. Both the midline and endline results suggest the incentive program was successful in generating this exogenous variation in increased education and delayed age of marriage. Overall, the incentive program increased mean marriage age and lowered the likelihood of child marriage. We observe strongest effects among the girls who were eligible for the full two years of the financial incentive.

Marriage rates 4.5 years following program completion were within 1 ppt of each other across incentive arms, suggesting that the incentive delayed but did not prevent marriage, which is to be expected given that eventual marriage in this setting is considered desirable by the majority of individuals. The distribution of marriage ages suggests fewer marriages took place in the incentive arms between ages 14 and 17.5 and more took place between ages 18 and 20 compared to the control. That some marriages were delayed past 18, the age cutoff for the incentive, could be explained by the presence of search frictions in the marriage market. Qualitative interviews suggested parents receive proposals over many months and even years before choosing a match. The impact of the program on enrollment in school is even more persistent. A possible mechanism, supported by qualitative interviews, is that it is more socially acceptable to continue higher education while married than to continue secondary school. Thus, if marriage can be delayed until secondary school is completed, girls may be able to continue in higher education. An older and more educated girl may also be better able to negotiate continuation of her education or further delay her marriage.

One possible concern with the validity of our estimates is the loss of observations from Muladi, washed-out villages, and data entry errors that meant 5,750 girls in the survey list were dropped from the analysis. However, the problem in neither case was related to treatment and thus is unlikely to bias our results. Among the households in which enumerators attempted to find every girl at endline, and excluding washed-out villages, attrition was just 12%, which is low given the long duration of this study (nine years), and it was balanced across arms.

In terms of the cost-efficacy of the program, we find the financial incentive translates into 1.62 years of delayed marriage and 1.00 years of schooling for every US$1,000 invested – the highest impact among the high-quality papers included in our cost-efficacy analysis. Likewise, with an NPV of US$1,138 for every US$1,000 invested, the financial incentive conditional on marriage has a relative NPV that is higher than the NPV of any other study with a significant impact on marriage age or child marriage.

While we find no impact of a well-crafted and implemented empowerment program on child marriage, we do find a positive impact of empowerment program on school enrollment and it is possible that there are greater or additional benefits of adolescent empowerment programs that do not translate into changes in marriage age but that will translate into gains in reproductive health outcomes or marital bargaining power at a later stage in a girl's life. Our evidence on intermediate empowerment outcomes is mixed, but we will re-evaluate once girls' subsample endline surveying has been completed.

Online appendixes

Note to the reader: These online appendixes are published as they have been received from the authors. These have not been copy-edited or formatted by 3ie.

Appendix A: Monitoring program implementation

This appendix is only available online and can be accessed from http://www.3ieimpact.org/sites/default/files/2019-01/ow112-appendix-a.pdf

Appendix B: Take-up

This appendix is only available online and can be accessed from

http://www.3ieimpact.org/sites/default/files/2019-01/ow112-appendix-b.pdf

Appendix C: Descriptive statistics from the midline survey

This appendix is only available online and can be accessed from http://www.3ieimpact.org/sites/default/files/2019-01/ow112-appendix-c.pdf

Appendix D: Qualitative case studies

This appendix is only available online and can be accessed from

http://www.3ieimpact.org/sites/default/files/2019-01/ow112-appendix-d.pdf

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