

# **Analysis plan for Endline Girls' Subsample Analysis of "Adolescent Girls: Education, Health, and Delayed Marriage"**

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## **Section 1: Introduction**

The purpose of this ex ante analysis plan is to explicitly set out the objectives, hypotheses and outcome variables of the study, in order to avoid concerns about data mining. This document specifies which hypotheses and set of variables will be examined following the wave 3 subsample survey (expected to be completed in March 2017). In particular, it specifies how different variables in the survey will be aggregated to create families for the mean effects analysis, as well as the set of control variables.

## **Section 2: Overview of Interventions**

The objectives of this project, conducted jointly by Abdul Latif Jameel Poverty Action Lab (J-PAL) at MIT and Duke University and implemented by the Bangladesh office of Save the Children USA (hereafter, SCUSA), are to

- 1) Examine and disentangle the separate causal impacts of girls' age of marriage, education and female 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

This evaluation is based on a clustered randomized trial, which J-PAL ran in collaboration with SCUSA between January 2007 and September 2017. The study was carried out in six sub-districts (Daulatkhan, Babuganj, Muladi, Patuakhali Sadar, Bauphal and Bhola Sadar) in south central Bangladesh, where SCUSA was managing a food security program that provided transfers to pregnant and lactating mothers. The conditional incentive program that we evaluate used the distribution infrastructure of this existing program, which operated in all treatment and control communities in our study. To determine which communities were included in the study, we collected census data in all 610 communities in the six sub-districts between January and February 2007. Communities were excluded from our study if they were too remote for distribution or had less than 40 or more than 490 adolescent girls, leaving 460 eligible communities in five subdistricts that were randomized into i) a basic empowerment program, ii) the empowerment program plus a financial literacy program, iii) a conditional incentive to delay marriage, iv) empowerment plus financial literacy plus conditional incentive, or v) the status quo using a stratified randomized design in the ratio 1:1:1:1:2. A random half of communities in the two empowerment plus financial literacy treatment arms were further offered to participate in a savings program. Experimental assignment was carried out by MIT staff without the involvement of Save the Children using Stata. Save the Children staff were informed of the treatment allocation of study communities. Randomization at the community level reduced the risk of inter-household crossovers. We stratified by union, an administrative grouping of roughly 10 communities, and within union by community size.

**Table 1: Clusters per treatment arm**

(1)	Basic	Gender attitudes, literacy education, health information	76
(2)	Livelihoods	Basic plus financial literacy (half, 39, receive savings club)	77
(3)	Full	Livelihoods plus oil (half, 38, receive savings club)	77
(4)	Incentive	Conditional incentive for unmarried girls	77
(5)	Control		153
Total			460
(6)	Savings cross cut	Savings club cross cut with Full and Livelihoods	77

The Bangladesh Development Society (BDS) implemented the empowerment program, called Kishoree Kontha (KK), or “Adolescent Girl's Voice” under the direction of Save the Children. Communities assigned to the empowerment program first underwent a community mobilization phase that informed parents, teachers, and community leaders about the activities and potential benefits of the program, mobilized their support and found locations for “Safe Spaces” - meeting places where girls could meet, socialize, and receive training. Safe Space committees were organized with adult members of the community to help troubleshoot any potential problems, for example if a girl's parents did not want her to attend. All girls aged 10-19 were invited to take part in one of four six-month cycles of the program that ran between December 2007 and August 2010. The empowerment curriculum included education support and social competency training. The education component aimed to enhance the basic literacy, numeracy, and oral communication of both school-attending and illiterate girls. The social competency component trained girls in life skills and nutritional and reproductive health knowledge via a curriculum designed by Save the Children USA. The livelihoods program 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 roll out 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. Overall, the empowerment curriculum was similar in content to many empowerment programs being implemented worldwide, including those designed by BRAC and Unicef.

Each Safe Space had a target of 20 girls, two to four of which were selected to be peer educators. Peer educators were given between 24 and 40 hours of training on the curriculum, which they delivered with the aid of specially designed books that included stories and examples to be read aloud, questions to be discussed, and participatory activities and games to perform. Safe Space groups were designed to meet five or six days a week for two hours each day for six months. Groups could continue to meet once the curriculum was complete but there was no support or new curricula after six months. At the end of the cycle, field staff repeated the mobilization and selection process until the entire community population had been reached. Thus, communities received up to four cycles and 24 safe spaces, depending on the number of girls living there. Monitoring data show Safe Spaces averaged six meetings, or 7.8 hours, per

week, and 45,149 girls, or 84%, of girls in target communities were reached. This makes KK one of the largest adolescent empowerment programs implemented in the developing world.

The conditional incentive program was an in-kind transfer of cooking oil to encourage parents to postpone daughters' marriage until the legal age of consent (18). The value of the incentive was approximately \$16 per year, an amount chosen to offset the estimated financial cost of higher dowry<sup>1</sup>. Cooking oil was chosen as an incentive because it is purchased regularly by every family in Bangladesh and thus has close to cash equivalent value, yet it is less susceptible to theft and graft than cash because of its bulk. It also has a high value to volume ratio, which minimized transport costs. Girls estimated to be age 15-17 at distribution start (based on their reported census age) and confirmed to be unmarried by Community Health Volunteers (CHVs) were issued ration cards to collect the oil. 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 distribution points. Every four months from April 2008 to August 2010 marital status was verified by interviewing family members, neighbors and community leaders and cooking oil was distributed to eligible girls. A total of 5,617 unmarried adolescent girls received the conditional incentive at least once, or 71% of the girls eligible at wave 1. Marriage conditionality was checked before each distribution round by CHVs and independent monitors who asked beneficiaries, neighbors and community leaders about marital status. Those found to be married or who had reached 18 (according to their age at wave 1) had their names removed from the eligibility list and their cards taken away.

### **Section 3: Research Design**

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 to measure whether girls would become more aware of the dangers of child marriage, be valued more by their family, and whether this would translate into higher school participation and delayed marriage.

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 1 and 2 did not experience any increase in age at marriage but experienced an increase in education. We did not observe any differential effects between the two groups in our wave 2 data. If we do not find significant differences between the basic and livelihood groups in the wave 3 subsample, then we will pool both groups to increase power to detect effects.

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 addition of the oil incentive on delaying marriage. If the empowerment program and the financial incentive were 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 control and 4.

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<sup>1</sup> Bruce, J., and J. Sebstad. "Building assets for safe and productive lives: A report on a workshop on adolescent girls' livelihoods." In *Adolescent Girls Livelihoods Meeting*. Population Council, 2004.

The experiment was designed to isolate the effect of delayed marriage from that of increased education, by comparing girls out of school at wave 1 to those in school at wave 1. If delayed marriage benefits girls primarily through increased years of schooling, we should find effects for girls in school at wave 1 but not girls out of school at wave 1, as it is very rare for girls in Bangladesh to return to school after exiting. If delayed marriage benefits girls independent of schooling, we should find insignificantly different effects for girls in and out of school at wave 1.

Similarly, by comparing effects for girls who were eligible for the full two-year of incentive treatment (age 15 to 17 at distribution start), we are able to measure dose response to the conditional incentive.

#### **Section 4: Past Surveys**

A census was carried out in 610 villages in January to February of 2007 in Barisal, 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 ages of 10 and 19 were dropped. The five packages were randomly assigned among the remaining 460 villages. The wave 1 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 (age 10 to 17) in these households were interviewed. Data were collected on 11,350 girls from 9,155 households in total. Topics in the survey 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. Following the completion of the KK program after 2.5 years, both a wave 2 census and the wave 2 survey were conducted from January 2011 to March 2012. The longer time period reflects both issues with survey implementation, discussed below, as well as the fact that many girls had migrated, and girls who had migrated for work to Dhaka and Chittagong were especially hard to track. Surveying for the wave 3 census began in May 2015 and was completed in May 2016 and surveying for the subsample in October 2015.

As the census survey has very few outcomes and is thus less susceptible to data mining, we did not prepare pre-analysis plans for either the wave 2 or wave 3 census surveys. The pre-analysis plan for the more extensive wave 2 subsample survey can be found here: <https://www.socialsciregistry.org/trials/204>.

The subsample survey questions girls about knowledge, access, use, negotiation, attitudes, and decision-making in the following topics: education, nutrition, mobility, reproductive health, economic opportunity, community involvement, religion, and marriage. In addition, it includes anthropometric measurements (height and weight).

**Table 2: Timeline**

Activity	Time period
Wave 1 census	January–February 2007
Wave 1 survey	March–October 2007
KK program	December 2007–August 2010
Oil incentive	April 2008 – August 2010
Wave 2 census	January 2011–March 2012
Wave 2 survey	January 2011–March 2012
Wave 3 census	May 2015–May 2016
Wave 3 survey	October 2015–March 2017 (expected)
Wave 4 study	2022 (anticipated)

**Survey Aims and Existing Findings**

Attrition was low and balanced across treatment arms in the wave 2 census (14% excluding Muladi), the wave 2 subsample survey (7% excluding Muladi), and the wave 3 census (13%).

The wave 2 census showed a decrease in marriage and an increase in schooling among girls eligible to receive the financial incentive but no increase in marriage age or education among girls eligible to receive the empowerment program.

The results from the wave 2 survey assessed the outreach and success of the specific interventions in the short term. A combination of process data from the oil monitoring and distribution rounds, Save the Children’s records, as well as survey data, was used to assess the success of the implementation and spillovers into the control groups. Key indicators used were number of safe spaces, number of overall girls reached, enrollment and average attendance in KK meetings, types and numbers of meetings/sessions organized and number girls who received the oil incentive.

The wave 2 survey analysis focused on intermediate indicators such as girls’ awareness, knowledge, attitudes, mobility, income, savings, negotiation, decision-making, school participation, and healthcare utilization. It also included marriage expectations and desired fertility choices. Key indicators used were 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 wave 2 survey also assessed the short-run impact on girls’ awareness, knowledge, attitudes, perceptions, mobility, income, savings, investment, decision-making, desired fertility choices and marriage expectations.

Data from the wave 3 census showed that girls eligible for the incentive were less likely to be married under 18 years of age, less likely to have given birth under 20, and more likely to be in-school. There were no significantly different effects for girls in and out-of-school at wave 1. The empowerment program did not decrease child marriage or teenage childbearing. However, girls eligible for the empowerment program were more likely to be in school and had completed significantly more schooling.

The wave 3 survey analysis will focus on capitalizing on the exogenous variation in marriage age and human capital (education and empowerment) induced by the conditional incentives and the empowerment program. We will assess educational attainments and qualifications, marriage and childbearing, marriage and spousal quality for married girls, including harassment and domestic violence,

as well as long-run impacts on girls' awareness, knowledge, attitudes, and perceptions. In addition, we will assess reproductive, maternal and child health outcomes for girls who have given birth.

## **Section 5: Analysis sample**

Our main analysis sample will not include girls from three groups where data were not collected or where attrition was likely to be high because of exogenous shocks. All three are uncorrelated with treatment. These are villages in Muladi where violence in wave 2 and suspicion of enumerators led to high attrition in wave 3; cyclone damage; and a random sample of girls who were not approached to reduce costs.

While the wave 1 covered all 460 rural communities, there were survey problems during wave 2 and wave 3 in one of the districts (Muladi). In wave 2, rumors that enumerators were abducting or converting girls led to violence and the survey was suspended for several months. In wave 3, the census was completed without incident but attempts to reach girls provoked a strong community reaction and led to a suspension of the survey in the 84 Muladi study villages (leading to the exclusion of 2,061 girls from the survey). In addition, revenues fell due to the depreciation of the Canadian Dollar in 2016, increased costs due to a general countrywide political strike in 2015 (which delayed the survey for months) as well as challenges in tracking migrated girls. Despite raising significant additional funds, there was still a shortfall in funds and it was necessary to drop a random sample of 1,317 girls age 10-14 at wave 1 from the list of girls we attempted to approach.

In addition, 8 villages were completely abolished by cyclone damage. While we attempted to track the households from the washed out villages, attrition in these villages is high. Our main specification will exclude washed-out villages from the analysis. The rationale is that by only including households or girls who have been found, surveyed, and who have responded in those villages, we would be carrying out the analysis on a biased sample. Therefore, the analysis will be carried out both with and without them, though the latter is the more appropriate and preferred approach.

As reported age in the wave 1 census determined eligibility for the incentive program, we will restrict our main analysis to girls age 14-16 in the wave 1 census, and thus age 15-17 at the start of the oil distribution. For all marriage and childbearing outcomes, we will restrict our sample to girls who were unmarried at wave 1 as the programs are unlikely to affect these outcomes for girls already married at program launch. For all education and human capital outcomes, we will restrict our sample to girls who were still in school at wave 1 as girls are unlikely to return to school after exit.

## **Section 6: Analysis and Basic Regression Framework**

The subsample analysis will help examine the long-run effects on girls' education, marriage outcomes, control over resources and women and child health outcomes. We will determine the pathways of change and mechanisms for girls' empowerment and well-being by examining sub-groups and other intermediary influences.

We found in the wave 3 census that 15% of girls in our analysis sample are still unmarried, 23% have not given birth, 47% do not have any children over the age of 5, and 19% are still in school. We will thus use outcomes that minimize the risk of censoring bias. For example, as girls in our subsample are now at least 23 years old and should have long left secondary school, we can test for the uncensored effects of child marriage (under 18), teenage childbearing (under 20) and secondary school completion (grade 12). In addition, we will combine reports from married and unmarried girls when possible, for example whether

a married girl was able to negotiate her marriage with her parents and whether an unmarried girl believes she will be able to do so.

In addition, we will use objective measures (such as measurements of weight and height) whenever possible in order to minimize social desirability bias.

This section outlines and distinguishes impacts that we may expect to see in the long run. A detailed list of variables that will be analysed is given in Section 8.

## 6.1 Analysis

In addition to the outcomes analyzed in the census, the subsample analysis will assess the long-run effects on girls' education, marriage outcomes, control over resources, and women and child health outcomes. Note that where outcomes overlap between the census and the subsample the census will have considerably more statistical power. The complete disaggregated list of outcomes under these broad categories is given in Section 8.

The initial analysis from the wave 3 results will aim to confirm the existence of a first stage that we found in the census, i.e. whether:

- a) The empowerment program led to increased education
- b) The oil program increased age of marriage and education

## 6.2 Basic Regression Framework

Overall, 21% of eligible girls in incentive communities were excluded from the incentive program. They had not been issued eligibility cards as either not enough tracking information was available for them, or they had been married before the beginning of the first oil distribution. Thus, we estimate the impact of the incentive in three different ways: 1) intention-to-treat in the entire sample; 2) intention-to-treat, excluding girls from all treatment arms with insufficient tracking information or who were reported to be married before the first oil distribution; and 3) two-stage least squares (2SLS) regressions, excluding girls from all treatment arms with insufficient tracking information or who were reported to be married before the first oil distribution. While we will show marriage results using all three specifications, our main specification is 2SLS excluding girls with few tracking information or married before oil distribution. In the first stage, program inclusion (card issuance) is predicted by treatment assignment in an ordinary least squares regression (OLS; linear probability). In the second stage, outcomes are regressed against predicted inclusion from the first stage. Under plausible assumptions, this method accounts for errors in program inclusion and produces unbiased and consistent estimates of treatment assignment under full compliance. The most important assumption is that of no cross-household spillovers onto adolescent girls in the same cohort. A comparison of girls in households who did not receive an eligibility card relative to controls suggests that this assumption is valid.

Our first- and second-stage estimations are:

$$\hat{I}_{ti} = \gamma_{0t} + \gamma_{1t}I_i + \gamma_{2t}E_i + \gamma_{3t}I_i \times E_i + \gamma'_{4t}X_{iv} + \mu_{uti} + \varpi_{vti} + v_{ti} \quad (1)$$

$$Y_i = \alpha + \beta_1\hat{I}_{1i} + \beta_2\hat{I}_{2i} + \beta_3E_i + \beta'_4X_{iv} + \mu_{ui} + \varpi_{vi} + \epsilon_i \quad (2)$$

where  $\hat{I}_{1i}$  is predicted program inclusion for person  $i$  to the incentive only program, and  $\hat{I}_{2i}$  is predicted program inclusion for person  $i$  to the empowerment plus incentive program.  $I_i$  is assignment of person  $i$  to either of the incentive treatment arms, and  $E_i$  assignment of person  $i$  to either of the empowerment treatment arms.  $Y_i$  is outcome for person  $i$ .

We regress all outcomes on dummies representing eligibility for the incentive, eligibility for the empowerment program, and an eligibility for both. Both first and second stage regressions include controls measured at wave 1 for strata, household size, an older unmarried sister in the household, school enrollment, mother's level of education, distance from the community center to the closest neighboring community center (a proxy for remoteness), number of schools in the community, and the ratio of adult boys to adult girls in the community (a proxy for marriage market conditions). In addition, we may adjust our list of controls by regressing our main outcomes on our base set of controls as well as a set of alternative controls. We will then select the list of controls that explains the most variation (using adjusted R-squared) for the entire experiment.

We will create summary impact measures by using mean effects analysis such as in Kling et al (2007)<sup>2</sup> on a number of closely related outcomes. The categories and families of outcomes used in the wave 3 analysis are defined in Section 8.

### Robustness Checks

We will check the robustness of our results through excluding controls, including washed-out communities, as well as through using an intention-to-treat specification.

### Subgroups for analysis

Primary heterogeneity analysis:

We have already discussed our two primary analysis subgroup approaches above.

1. In-school vs. out of school girls: Comparing effects for girls in and out of school at baseline will allow us to test whether delayed marriage benefits girls primarily through increased years of schooling or is beneficial in itself.
2. Girls eligible for two years of incentive vs. girls eligible for fewer years of incentive: By comparing the effects for girls eligible for the full period of the incentive, we will be able to estimate dose response to the incentive treatment.

Secondary heterogeneity analysis:

Our secondary subgroup analyses will allow us to explore mechanisms through which our programs may effect outcomes. The first set of these is heterogeneous impacts found in the census sample. We will retest these in the subsample as a way of confirming that the result was not due to chance:

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<sup>2</sup> Kling, Jeffrey R., Jeffrey B. Liebman, and Lawrence F. Katz. "Experimental analysis of neighborhood effects." *Econometrica* 75, no. 1 (2007): 83-119.

3. Girls stunted/underweight vs. Girls not stunted/underweight at wave 1: One concern raised at the proposal stage for this project was that the conditional incentive may operate as a nutritional transfer to poor families. If the incentive only had an effect among malnourished girls, then the cooking oil may have a nutrition-related value for girls. If the effects of the incentive do not differ by wave 1 nourishment status, it is most likely working as a conditional incentive to parents.
4. Girls with vs. Girls without older unmarried sisters: We will test whether the empowerment and incentive programs had differential effects on marriage outcomes of girls with and without unmarried older sisters. If, for example, either of the programs only affected marriage outcomes of girls with unmarried older sisters, then parents may only consider/allow their daughters to remain unmarried if they are not the next in line to get married.
5. Daughters from households of low vs. high socioeconomic status: We observed in the wave 3 census that the empowerment program affected marriage outcomes in households that had high socioeconomic status at wave 1 (measured by parents' education and father's employment status). The incentive, however, affected marriage outcomes across all socioeconomic statuses. This may indicate that a financial incentive could be effective independent of socioeconomic status, while the empowerment program was only effective in families with less financial need and were thus more responsive to daughters' negotiations. However, our measures of economic status in the census are limited so we will redo this analysis with the more detailed data on socioeconomic status in the subsample. Specifically, we will compare impacts for those with higher income or wealth at baseline to those with lower income wealth. Our hypothesis is that rather than simply relieving a liquidity constraint, the incentive can be used to overcome the otherwise negative signal associated with marrying later. We therefore expect an impact of the incentive even in better off households. To test this hypothesis, we will compare various outcomes, including girls' attitudes, between girls from households with low vs. high socioeconomic status. Given challenges of measuring income and wealth, we will examine the correlations between different measures of income and wealth as well as response rates and outliers to see which measures are likely to be the most reliable. Given the unreliability of some of the measures, we will look at the robustness of heterogeneity results with respect to income and wealth using different plausible indicators of liquidity constraints.
6. Conservative vs. non-conservative households/villages: We found in the wave 3 census that the empowerment was effective in delaying marriage in villages that were highly conservative at wave 1 (based on a conservatism index constructed of parents' and teachers' reported attitudes and girls' behavior). We thus hypothesize that the empowerment program may be more effective among girls with low levels of initial empowerment. We will test this hypothesis by comparing outcomes such as girls' attitudes and empowerment between girls from conservative vs. non-conservative villages.

### Predicting empowerment participation

Using existing data, we have found correlations between participation in the empowerment sessions and regional proximity to a safe space. In addition, using the wave 3 census, we observed that the empowerment program had the largest effect on education in communities with many safe spaces. We thus inferred that empowerment take-up is a function of regional proximity to a safe space. This also explains high crossovers at the border of empowerment program villages. Using gps data of households and safe spaces, we will thus construct binary variables that describe whether a girl lived within 100, 200, or 500 meters within a safe space during the empowerment program implementation. We will then instrument our binary measures through treatment assignment, and regress our outcomes on the part of safe space proximity that is predicted by treatment assignment. This will give us the approximate effect

of the empowerment program among girls with higher likelihood of program participation. We will also test whether participation (as reported in the wave 2 survey) can be predicted through the semi-random distance between a respondent's household and the closest empowerment village center (as distance to the closest safe space may not be random because safe spaces were usually located in wealthier neighborhoods).

## **Section 7: Measurement verifications**

Estimates on marriage, education, childbearing and reproductive health are collected both in the census and the subsample survey. This allows us to verify our main outcomes using parents' reports and rule out systematic misreporting. In addition, we are also collecting our main outcomes using various questions to cross check answers within the survey. For example, age at first birth will be calculated using the girl's reports of her age at first birth, the reported age of her oldest living or dead child, and the birth year of her oldest living or dead child. Finally, we are collecting marriage dates from marriage certificates were available. In case of discrepancies between parents' and girls' reports, we will use the more conservative outcome (e.g. lowest marriage age, age at first birth, and last class passed).

## **Section 8: List of indicators for analysis**

With a large number of outcome variables, it is important to specify which variables will be used during the analysis. We will use mean effects analysis similar to Kling et al (2007)<sup>3</sup> when there are several variables for a given outcome. All outcomes groups are listed below. One outcome may consist of various variables, which are all individually listed in the Excel document, which accompanies this analysis plan.

We will first check whether we are able to replicate our findings from the census in the smaller subsample, to test whether we have a first-stage for the effect of delayed marriage and increased education on our subsample outcomes. We differentiate between primary outcomes, which include outcomes that allow us to measure impacts on health, empowerment, income-generating activities, and mobility, as well as secondary outcomes that look at mechanisms. In addition we will check whether misreporting is balanced by treatment status. We highlight important indicators, which we will analyze separately. Finally, we will perform several validity checks to confirm our hypothesized channels of impact.

We have listed the optimal groups of indicators here. We will adjust these groups only if we find patterns that indicate systematic errors in surveying or nonresponse rates above 10% of our analysis sample.

\*\*Important indicators

### **Outcomes testing for a first-stage in the subsample:**

#### *1. Marriage outcomes*

- Whether girl is married or engaged (current marital status)
- Girl's age of marriage
- Married under age 18

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<sup>3</sup> Kling, Jeffrey R., Jeffrey B. Liebman, and Lawrence F. Katz. "Experimental analysis of neighborhood effects." *Econometrica* 75, no. 1 (2007): 83-119.

- Married under age 16
- 2. *Education outcomes*
  - Current enrollment status
  - Secondary school completion
  - Years of schooling completed/highest class passed
- 3. *Childbearing Outcomes*
  - Child birth under age 20
  - Child birth under age 18
  - Child birth under age 16
  - Age at first birth
  - Number of children by age 20

**Primary Outcomes:**

1. *Health*

- Girls' health:
  - Physical health:
    - BMI\*\*
    - Girl's mortality (number of deceased girls per 1,000)
    - Incidence of illness/injury in last 30 days
    - Severe injuries (affects activities)
  - Mental health:
    - K10 composite score
    - Reported satisfaction with life
    - Frequency of positive feelings
    - Frequency of negative feelings
  - Channels of influence: i) Early marriage is highly correlated with poor health outcomes for women. Young girls are more vulnerable to maternal morbidity and mortality and may have lower status within the household, potentially reducing their healthcare decision-making power. There is causal evidence that early marriage alone reduces use of health services, presumably by reducing decision-making power<sup>4</sup>. In addition, if marrying later improves a woman's power in the household, she may have better access to nutrition, and thus higher weight. Finally, young married women in Bangladesh have very little say over key dimensions of life, including where they go, what to buy, and what tasks to complete. Thus, girls in the incentive arm may have potentially better physical and mental health. ii) Girls who have more knowledge and/or are more empowered and have better negotiation skills may be more likely to negotiate factors important to their health with their husbands. In addition, lack of control over everyday aspects of life is an important determinant of stress. Increases in negotiation power could thus translate into reductions in stress. We could thus see improvements in physical and mental health among girls in the empowerment arm. iv) There is evidence that educated women are more likely to visit trained providers and medical institutions. If this is the main driver of physical and/or mental health,

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<sup>4</sup> Field E., Ambrus A. (2008). "Early marriage, age of menarche, and female schooling attainment in Bangladesh." *Journal of Political Economy* 116(5):881–930.

we should see improvements among girls in the incentive and empowerment arms proportional to the relative increases in education.

- Reproductive and child health:
  - Reproductive health:
    - Home birth
    - Proportion of miscarriages/abortions to pregnancies
    - Proportion of stillborns to pregnancies
    - Birth complications
  - Infant health:
    - Infant mortality (under-1)
  - Contraception:
    - Recent contraception use (last month)
    - Use of modern contraception method
  - Channels of influence: i) Older girls may have better reproductive health and healthier children<sup>5</sup>. Physiologically, girls who marry and give birth early have less-developed pelvises, which put them at risk for complications in childbirth and obstetric fistula<sup>6</sup>. If physiological reasons are the main determinants of reproductive and child health, we would expect to see improvements mainly among girls in the incentive arms. ii) Mothers who are more empowered or have better negotiation skills should also be better positioned to advocate for their children's needs and be sufficiently independent to take them to a clinic as needed. Similarly, girls with knowledge about reproductive and child health may also use antenatal and postnatal care more frequently, and seek care for reproductive or child health problems. If knowledge and negotiation are the main drivers of reproductive and child health, we should see improvements mainly among girls in the empowerment arms. iii) There is evidence that educated women are more likely to have lower fertility, use antenatal and postnatal care early and frequently, visit trained providers and medical institutions, have safe deliveries, and seek care for reproductive health problems. Thus, if we observe improvements in reproductive and child health that are proportional to the relative increases in education in the incentive and empowerment arms, reproductive and child health may be improving through education.

## 2. Empowerment

- Gender attitudes:
  - Girl believes domestic violence is justified in certain situations (if she argues, burns the food, goes out w/o telling him, neglects children, refuses to have sex)
  - Girl believes it's better to be a boy
  - Girl believes a husband should be more educated than his wife
  - Girl believes boys should be given more education resources/opportunities
  - Girl believes boys should be fed first
  - Son preference (desired boy-girl ratio among own children above 1)

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<sup>5</sup> Jensen R., Thornton R. (2003). "Early female marriage in the developing world." *Gender & Development* 11(2):9–19.

<sup>6</sup> Nour, N.M. (2009). Child marriage: a silent health and human rights issue. *Reviews in obstetrics and gynecology*, 2(1), p.51.

- Latest age a girl should marry
- Girl values personality traits of spouse (nature, romantic, responsible)
- Girl believes a girl should be allowed to wear whatever she wants without being harassed
- Channels of influence: i) Girls who marry later also move in later with their husbands and thus have more time to develop and grow. Their personalities may be less malleable than those of younger girls. This could lead to improvements in gender attitudes outcomes among girls in the incentive arms. ii) If directly discussing gender issues positively affects attitudes, we should see increases in outcomes among girls eligible for the empowerment program. iii) There is evidence of positive correlations between education and girls' empowerment and gender attitudes. If education is the main driver of gender attitudes, we should see improvements in gender attitudes that are proportional to the increases in education in the empowerment and incentive programs respectively.
- Decision-making:
  - Health:
    - Girl involved in healthcare decisions for herself/her family
    - Girl wouldn't seek treatment for sick child independently
    - Girl would purchase medicine herself
  - Reproductive health:
    - Girl involved in decision on contraception use\*\*
    - Girl has discussed family planning with husband
    - Girl has discussed contraception with husband
    - Difference between desired and actual age at first delivery
  - Marriage relations:
    - Girl had ability to refuse marriage proposals
    - Girl got to meet husband prior to wedding
    - Girl involved in decisions about visiting her parents' home
  - Freedom of dress:
    - Husband's/in-laws' control over clothing choice
    - Girl allowed to wear modern fashions
  - Economics:
    - Decision-making on purchases
    - Girl has power over decision to take loan
    - Girl herself keeps ornaments/wedding gifts safe
    - Girl can access cash for immediate purchases
    - Girl involved in decision on how to use any money received/earned
  - Channels of influence: As the incentive increased marriage age and education, and the empowerment changed education but not marriage age, we can disentangle the effect of marriage age and education on girls' decision-making. i) If age is one of the main drivers of decision-making (for example through more developed personalities), then girls in the incentive should have more decision-making power in the household. ii) If increased marriage age and increased education are important to giving girls more decision-making power, then we should see improvements in both incentive arms. iii) If increased education is the main characteristic that allows girls to attain more decision-making power, we should see improvements among both the incentive and empowerment arms. iv) Improved negotiation skills and attitudes,

independent of age, may incentivize and enable girls to negotiate with their parents and/or in-laws for more decision-making power. This could lead to increases in decision-making power in the empowerment but not incentive arms. By comparing the coefficients of each treatment arm, we will be able to estimate the added impacts of each program and the relative importance of marriage age and education in determining girls' decision-making power.

- Mobility:
  - Girl requires permission to visit friends
  - Number of days visiting friends last month
  - How often she visits parents
  - Channels of influence: i) Younger wives as young girls are believed to be more malleable and less independent. Similarly, older girls may be considered to be at smaller risk when leaving the house by themselves. In addition, qualitative surveys conducted as part of our wave 3 surveys showed that many old and experienced girls use indirect tactics to achieve their marriage goals. Mobility could thus increase among older girls, and thus in the incentive arm. ii) If education makes girls abler to negotiate mobility, then mobility should increase in both the empowerment and incentive arms. iii) Similarly, girls in the empowerment program may have obtained negotiation skills that allow them to gain more mobility even at a younger age, leading to an increase in mobility among girls eligible for the empowerment program, regardless of age.

### 3. *Income-generating activities*

- Ever worked\*\*
- Currently performing IGA
- Number of months spent on activity in past 12 months
- Time spent on IGA per week/day
- Monthly income from current IGA
- Monthly income from past IGA
- Total overall income last month
- Channels of influence: i) Girls who marry later, may have already started employment by the time they get married and may continue to pursue their income-generating activity after marriage. They may also be more confident in negotiating a continuation of their work with their parents, and future husband and in-laws. This would lead to an increase in IGAs and income among girls in the incentive arms. ii) The increased education wage premium increases girls' opportunity cost of not working. This may incentivize girls to work after leaving school and induce husbands and in-laws to be more receptive to girls' participation in the labor market. If the education channel is the main channel affecting labor participation and income, we should see proportional improvements to the increases in education in the empowerment and incentive arms respectively. iii) Girls may be able to participate in the labor market if they are better able to negotiate with their husbands. This could be one mechanism through which we would observe improvements in IGA outcomes in the empowerment arm only.

## Secondary Outcomes:

1. *Denmeher*
2. *Attitudes on childbearing:*
  - Ideal age for female to have first child
  - Whether couple should have children within one year of marriage
  - Desired number of children
  - Desired gap between marriage and first child
3. *Knowledge and awareness of health-related issues:*
  - Number of contraceptive methods identified
  - Number of correctly identified iron-rich foods
  - Number of early pregnancy risks identified
  - Girl has heard about HIV
  - Number of correctly identified methods to avoid HIV
4. *Time spent on self/others:*
  - Time spent on household work
  - Time spent working for others (caring for own children/ill family members/other children, time spent on helping family IGA)
5. *Savings and credit:*
  - Savings:
    - Girl has ever saved\*\*
    - Girl currently saves
    - Savings in last month
    - Total savings balance
    - Has specific plan/goal for savings
    - Clarity of goal
  - Credit and investment:
    - Girl has ever taken credit
    - Taken credit in last 24 months
    - Allocation of income to investment last month

## Validity Checks:

1. Spousal/ marriage quality:
  - Spousal quality:
    - Husband's (fiancé's) age
    - Husband's (fiancé's) education
    - Husband (fiancé) has completed secondary school
    - Husband (fiancé) formally employed
    - Husband's income
    - Proximity of parents' bari to husband's residence
  - Harassment and domestic violence:
    - Frequency of husband being controlling/possessive
    - Frequency of husband psychologically harming girl
    - Frequency of husband inflicting sexual violence on girl
    - Frequency of husband physically harming girl
    - Frequency of husband showing anger

- Marriage quality:
  - Whether separated/divorced
  - Husband comes home late often
- 2. Migration:
  - Migrated for studies
  - Migrated for work