Advancing the Agency of Adolescent Girls

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Abstract

More than 98 million adolescent girls are not in school. Can girls influence their schooling without changes in their family's economic environment? In Rajasthan, India, we examine the impact of a school-based life skills program that seeks to address low aspirations, narrow societal roles for girls and women, restricted networks of social support, and limited decision-making power. We find the intervention causes a 25 percent decline in school dropout that persists from seventh grade through the transition to high school. Improvements in socio-emotional support among girls exposed to the intervention seem especially important in their decision to stay in school.

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1 Introduction

Barriers to gender equity have remained stubbornly persistent in many parts of the world. This paper explores whether adolescent girls can influence decisions about their continued school enrollment in a low-income environment with substantial gender disadvantage. In the setting for this study in Rajasthan, India, over 90 percent of adult women in our respondents' households state that a wife should always obey her husband, and, at an average age of eleven, 17 percent of our subjects are married. In this context of severe limitations on female agency, it might be reasonable to hypothesize that there is very little potential for girls to participate in decisions around their own schooling in early adolescence, and that only interventions that provide material incentives — such as cash (Baird et al., 2013), bicycles (Muralidharan and Prakash, 2017), or uniforms (Duflo et al., 2015)— would be effective in reducing dropout.

To test this hypothesis, this study attempts to experimentally influence girls' motivation for schooling and their ability to advocate for themselves by providing school based life skills classes beginning in sixth grade. Building on two recent papers that have analyzed interventions molding attitudes around gender in the classroom in India (Dhar et al., 2020) and providing negotiating skills to girls in Zambia (Ashraf et al., 2020), we evaluate a broad life skills curriculum designed to engage the wide range of gender-based inequities present in our study context, one of the most gender disadvantaged contexts in India, and seek to understand if this curriculum can reduce dropout among girls still early in their adolescence.[1]

For identification, we utilize a randomized controlled trial including 2,459 adolescent girls

[1]India exhibits substantively lower gender equity than Zambia in the World Economic Forum Index (ranked 112 compared to 45, World Economic Forum, 2020), and our study site of Rajasthan is among the most gender disadvantaged states in India, as measured by the prevalence of crimes against women (Mukherjee et al., 2001) and the skewness of the sex ratio (Oldenburg, 1992). Rajasthan also has the lowest literacy rate for women in the country as of 2018 (National Statistical Office, 2020).

starting at age 11 in 119 schools in the Ajmer district of Rajasthan, India. The intervention of interest is the Girls' Education Program (GEP), a program encompassing twice-monthly life skills classes conducted in school as well as group mentoring sessions for girls.[2] The program is delivered by social mobilizers, women from the area who have completed secondary school and who are managed, trained and deployed by our partner non-governmental organization, Room to Read (RtR). The intervention targets girls beginning in grade six, and was newly rolled out to 60 randomly selected treatment schools in 2016.

The acute gender disadvantage in our study setting motivates the choice to evaluate a multifaceted intervention targeting a range of competencies broadly identified in the literature as life skills or non-cognitive skills. Girls in rural India face a broad set of challenges — low aspirations, narrow views of appropriate societal roles for girls and women, restricted networks of social support, and limited decision-making power — that may be more effectively addressed by a broader curriculum as opposed to a very targeted intervention that may better serve girls in settings where gender disadvantages are more limited. We then examine the impact of GEP using an intent-to-treat empirical strategy fully specified in a pre-analysis plan (Edmonds et al., 2016). 96 percent of girls offered treatment attended at least one life skills class, and 85 percent remained engaged through grade seven. The majority of our analysis is based on surveys conducted with girls and their household members following grade seven, after two full years of program exposure; at that point, we have data on 2,435 girls relative to the original sample of 2,459 randomized into treatment, an attrition rate of less than one percent.

Our empirical analysis demonstrates that random assignment to GEP generates a significant decline in dropout. In the endline survey conducted following seventh grade after

[2]The program's definition of mentoring is not what we expect most readers to have in mind. Mentoring sessions are small group discussions around topics covered in the life skills classes, principally led by students.

two years of the program, we document a 30 percent reduction in dropout (equivalent to a decline of four percentage points) and a parallel increase in grade progression. We also utilize administrative records from schools that extend through the initiation of grade nine and find that the reduction in school dropout continues through the progression into high school, a frequent dropout point, and is consistently around 25 percent in magnitude.

In addition to a decline in dropout, assignment to the GEP treatment is associated with an enhancement in girls' reported life skills six months following their final class in seventh grade.[3] We document improvements in future planning, including reporting concrete goals for the future and effectively articulating plans to achieve those goals. Girls also report more empowerment and a broader sense of agency, with a greater ability to participate in critical life decisions, and enhanced socioemotional support from peers and adults. These experimental effects are corroborated by assessments by parents and enumerators, suggesting that the shifts in life skills were observable to adults interacting with girls exposed to GEP. Moreover, while it is possible that the observed shift in life skills responses could partially reflect treated girls learning the socially desirable answers (social desirability bias), we do not observe larger treatment effects on average in girls prone to give socially desirable answers. There are, however, some areas in which the effects of the intervention seem minimal, and we cannot reject the null of no effect on self-esteem, freedom of movement, perceived stress, locus of control, or life satisfaction.

This finding of the feasibility of teaching some life skills in early adolescence in a setting of gender disadvantage answers an important question raised by Cunha et al. (2006), who note that IQ is largely stable by age 10 and ask whether the same is true of life skills. While a large literature in psychology documents the feasibility of teaching a growth mindset about

[3]We elaborate further on the definition of life skills in Section 2.3; the set of skills we examine is generally similar to competencies often described as non-cognitive skills in the existing literature, though arguably somewhat broader.

learning (Sisk et al., 2018), our findings document a broader range of malleability. These results complement the evidence from McKelway (2020), who provides empowerment training to young, working age women in India and finds that they both learn and use newly taught life skills. Moreover, the documented improvements in life skills are in areas that her subjects are able to immediately put into use, a pattern similar to that reported in Adhvaryu et al. (2018), who analyze a workplace-based non-cognitive skills training program; our clearest treatment effects are also in areas that girls are able to immediately incorporate into their lives. This observation is consistent with the evidence from social psychology experiments in education that utilization is critical for skill take-up (Yeager and Walton, 2011).

In addition, our findings around the malleability of gender attitudes add to the evidence from Dhar et al. (2020) that classroom discussions around gender can change default assumptions of gender roles. Surprisingly, treatment has a negative effect on expectations around marriage. This contrasts with the impact of classes aimed at discouraging child marriage in Bandiera et al. (2019), implemented in Sierra Leone. The difference here appears to be that treatment led to an increase in awareness of the legal age of marriage at 18, effectively eliminating responses that marriage should take place below the legal age, as well as responses of ages above the legal age.[4]

In contrast to these shifts in gender attitudes and marital expectations, we cannot reject the null of no treatment effect on education or employment aspirations. The apparent absence of change in work and schooling aspirations contrast with the evidence presented in Acevedo et al. (2020), who find that life skills training, delivered in the context of a job training program in the Dominican Republic, leads women to become more optimistic

[4]Our results around marriage are driven by attitudinal questions rather than sexual behavior in contrast to Bandiera et al. (2019) where reports of sexual activity before marriage are more common in a significantly older population (their adolescent girls are 18 at baseline compared to 11 here).

about their long-run employment prospects. Again, we hypothesize that context is especially important here: women in the Dominican Republic could immediately exercise these skills in the workplace, an opportunity that was largely absent for girls in this sample.

This study makes three important contributions to the literature on understanding girls' educational decisions in poor countries, in addition to expanding the evidence around strategies to shape education without fundamental shifts in the economic environment. First, it highlights a role for school content in decisions about whether girls stay in school. Several studies have attempted to increase school enrollment through improving how teachers teach with generally little success (Kremer and Holla, 2009), although pedagogical interventions often improve test scores (Evans and Popova, 2016). The objective of increasing enrollment by shaping the in-school experience is also consistent with the increased emphasis on vocational education. An advantage of utilizing life skills education to draw in or retain marginal students at risk of dropout is that life skills are likely complementary inputs to general human capital accumulation, rather than substitutes as vocational training can be. The use of school as a delivery mechanism is also consistent with the fact that the observed impact on dropout is much larger than the impact observed for any of the out of school life skills related interventions previously examined in the literature (Amin et al., 2016; Austrian et al., 2020; Bandiera et al., 2020; Buchmann et al., 2017; Buehren et al., 2017a,b; Prakash et al., 2019). Even compared to Ashraf et al. (2020), our treatment effects on dropout are three times greater in magnitude (30 percent at end of seventh grade versus 8 percent at end of tenth), and apparent at the time of treatment rather than two-plus years later.

Second, in examining potential channels for the observed decline in dropout, we find evidence of a plausible causal mechanism working through enhanced social-emotional support, generated by bonding in the life skills classes. This point was made salient in our qualitative work, and we find statistical evidence consistent with this hypothesis: treatment girls have more friends to rely on, identify more ways in which they rely on these friends, and spend 25 percent more time socializing. While several studies document peer effects increasing schooling of the non-treated in conditional cash transfer evaluations (Bobonis and Finan, 2009; Lalive and Cattaneo, 2009), our mechanism appears to reflect in part treated girls forming tighter groups, augmenting a normal part of adolescent development (Fehr et al., 2013). This resulting desire to stay in school adds to the literature on in-group advantage (Sutter et al., 2019) and highlights an additional channel through which peer effects may promote education (Sacerdote, 2011). Bandiera et al. (2020) identify an analogous mechanism in their exploratory analysis around why their safe spaces intervention prevented teen pregnancies: girls spend time in their safe space because they are closer friends, and in our setting, accessing that analogous space requires continued enrollment in school. In contrast, Ashraf et al. (2020) conclude that the channel of social engagement is generally unimportant in reducing dropout among tenth grade girls in Zambia. It is impossible to identify whether the difference in our findings on mechanisms reflects the relative social isolation of our subjects absent treatment, our subjects' younger ages, or differences in the treatment as our life skills classes are interactive and engage on a broader set of disadvantages.

Third, this paper contributes to the vast literature on intrahousehold decision-making in education. While that literature primarily focuses on spousal conflict over investments in education (Strauss and Thomas, 1995), increasing attention is being placed on the child's agency. For example, Berry (2015) highlights the challenge parents face in engaging children seriously in academic testing, and Bursztyn and Coffman (2012) and Bursztyn et al. (2017) document that parents in Brazil face challenges in monitoring and incentivizing school attendance. The enrollment / dropout question is typically modeled as outside of the child's sphere of control, but our results highlight that girls can influence choices around school enrollment even in early adolescence. In addition, our finding of a reduction in dropout without other measured changes in girls' broader responsibilities in the household is consistent with the hypothesis that there is scope for gains in schooling without needing to fundamentally affect the economic organization of the household.

The paper proceeds as follows. Section 2 describes the setting and Section 3 describes the evaluation design and empirical strategy. Section 4 presents the empirical findings. Section 5 analyzes evidence on causal pathways and Section 6 concludes.

2 Background

2.1 Setting

This evaluation was conducted in Ajmer, Rajasthan in northwest India, a setting characterized by low levels of female educational attainment and very low levels of gender equity. Data from the baseline survey (see Section 3.1) can be used to characterize the sample.[5] Among the mothers of subjects, only 20% reported any post-primary education or engagement in wage employment. A quarter of mothers concurred that boys were naturally better at studying than girls, and 74% stated that dowry was necessary for daughters. While only 11% report that the ideal age for marriage for their daughters is younger than 18, only 7% report an ideal age of marriage above 20, suggesting a strong preference for marriage immediately at the legal age. More than 90% reported that their daughters were not allowed to travel alone to locations other than school or a friend's house.

Among girls in the study sample, 17% were already married when first surveyed, and 84% of girls reported working for pay at that time. More than 70% of girls answered that dowry was always necessary for marriage, and 25% agreed that girls should not be sent for higher education since they have to get married; that boys should be sent to school before girls when money is scarce; and that boys are naturally better at studying than girls. Thus, girls in this setting appear to face a number of obstacles to future educational enrollment and academic achievement.[6]

[5] Table A1 in the Appendix compares sample households to the average household in Rajasthan and India as a whole.

[6] Additional household and child characteristics, including patterns of income-generating activities, are presented in Appendix Table A2 through A4. Appendix tables are available

2.2 Intervention

2.2.1 GEP during evaluation period

The Girls' Education Program (GEP) delivered by Room to Read (RtR) is a seven year program that begins in grade six and continues through secondary school. It has two primary goals: encouraging girls to successfully complete secondary school and developing life skills. Since 2007, more than 95,000 girls in nine countries have been enrolled in GEP.

GEP is delivered by social mobilizers ("SMs"), who provide life skills training and mentoring. GEP life skills training is delivered in twice-a-month sessions conducted by SMs during school hours, utilizing a curriculum developed by RtR. In each treatment school, 16 life skills classes were conducted in both grades six and seven. The curriculum is grade-based and emphasizes ten life skills: self-confidence, expressing and managing emotions, empathy, self-control, critical thinking, decision-making, perseverance, communication, relationship building, and creative problem solving. The intervention also focuses on applying these skills to simulations involving time management, education, physical protection and rights, health, and community involvement. It evolves as girls age and regularly revisits topics, adapting to stay age appropriate and relevant.

To provide an example of the structure of a typical session, the fifth life skills class in grade six focuses on assertive communication. It begins with a review of concepts from the previous life skills class (focused on listening), and continues with a story and an interactive activity that asks participants to consider how they would respond to a potentially conflictual interaction with peers. Girls learn about different communication styles, and then work in small groups to practice assertive communication styles. The session concludes with a self-reflection in which girls write in their journals. The mix of collective, small group and individual activities is typical of the classes.

While attending life skills classes (each of which has duration of approximately an hour),

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on the project website: https://sites.dartmouth.edu/eedmonds/gep/.

girls may miss some lessons in the primary classroom, though this varies by school. In some schools, the boys had recess while the girls attended life skills classes; in some schools, instruction continued. Any class time lost to life skills sessions would not be expected to significantly impact academic performance given the limited number of annual GEP classes, each of only an hour's duration, and existing evidence on the low returns to instructional time in Indian schools (see, for example, Banerjee et al., 2007).

In addition to life skills sessions, the intervention entails twice-a-month small group mentoring sessions proctored by the SM. Mentoring sessions are student-led discussion sections for the life skills lessons taught by the SM. SMs were trained for these mentoring sessions to assist girls in identifying more personal difficulties in their lives related to the life skills lessons and to help them to develop more personalized strategies to cope with these difficulties.[7]

40 SMs were employed full-time as a part of this intervention during the two-year evaluation period, with a maximum of 33 employed at any one time.[8] The typical SM is responsible for two schools (mean of 1.95). GEP aims to have 50 girls per SM. All the SMs (33 years old on average) had completed both secondary and post-secondary education, and all were from Ajmer district; within the district, 58% were from urban areas. Prior to the launch of the intervention, SMs received 14 days of training, and an additional eight days of training are provided at the start of each subsequent school year. Every eight SMs are supervised by a program assistant, and each SM was observed quarterly to assess the quality of her life

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[7]Earlier GEP descriptions presented these mentoring sessions differently. The description here is updated based on the authors' experiences from talking with students and SMs in Ajmer about their actual experiences in the mentoring sessions. Also note that SMs were referred to as "female role models" in previous project documents.

[8]Of the 40 SMs involved in this evaluation, seven left at some point over the two years and were replaced within three months. One was released for poor performance, and others left for personal reasons such as marriage or migration.

skills session and to provide her with support to improve session delivery.

In general, engagement of girls in GEP is high. Figures A2 and A3 contain histograms of the number of life skills classes attended by subjects in grade six and seven, respectively. While there was a growth in children who attended no classes between sixth and seventh grade, overall attendance patterns are similar across both grades with complete attendance the mode in both years.

There are two differences between the intervention as analyzed in this evaluation and GEP as delivered in other contexts. First, the primary data collection for this evaluation was conducted following only two years of program implementation, when the target sampled girls had completed grade seven rather than grade twelve. We refer to 2016–2018 as the evaluation period. Second, the evaluation focuses on a reduced intervention design including only deployment of social mobilizers who deliver life skills classes and mentoring. The full program additionally includes material support and parent and community engagement, but these components were not implemented in Ajmer during the evaluation period.

2.2.2 GEP post-evaluation period

In the original design, our partner only had funding through grade seven, and thus the endline survey would correspond to the conclusion of the program. However, GEP has always been designed as a project continuing from sixth to twelfth grade, and our partner was successful in attaining programmatic funding to continue the GEP in treated schools beyond the period of study. They did not begin treating the control schools and did not extend the program to other, non-study cohorts within treated schools.

However, in 2019 after the evaluation period, RtR added financial support to the GEP in Ajmer. A needs assessment was conducted in fall 2018, and 268 girls began receiving material support in 2019. Material support consisted of in-kind transfers of school supplies, valued at an estimated 500 rupees or approximately \$7. The addition of material support is not relevant for our primary outcome measures collected in the endline survey, as the survey was conducted prior to the rollout of the material support and the delivery of any associated information (students did not anticipate this future source of material support). However, this change is relevant for supplementary analysis conducted using administrative data, as we also analyze data available post-endline, for eighth and ninth grade.

2.3 Hypotheses

As previously noted, our primary objective is to analyze the effectiveness of a non-material intervention — life skills training — in reducing dropout in a setting of acute gender disadvantage. The hypotheses of interest were pre-specified in an analysis plan that also included detailed variable definitions; our two primary hypotheses focus on the treatment's impact on dropout and life skills (Edmonds et al., 2019).

Our first hypothesis is that GEP has no effect on school dropout. We consider the dropout decision to be the result of a negotiation between parent and child where each individual's value function depends on their perceived value of continued school attendance vis-a-vis the opportunity cost of time spent in school. The outcome of the negotiation also reflects the relative weight on the child's and parents' preferences.[9] There are a number of channels through which the life skills treatment could affect dropout: the treatment may affect the weight on girls' preferences relative to parents; may shape girls' or parents' views of the long-term returns to schooling; or may change their perceptions of the value of time in school and the opportunity costs of that time.

More specifically, life skills classes develop girls' skills in advocating for themselves, and thus an effect on dropout could be explained by an increased weight being assigned in the household's problem to the girl's preferences. In addition, perceived returns to education could change through direct messaging provided around future possibilities for the student or by encouraging her engagement with peers and school. The skills developed in class around

[9]In our setting, the modal household reports paying no school fees. Hence, direct costs are less likely to merit separate consideration, and of course the impact of treatment observed here might differ from what would be observed in a setting with large direct costs of schooling.

time management and planning could similarly increase perceived returns to schooling by helping girls to establish and more efficiently achieve educational goals or by reducing the opportunity costs of time in school. (The intervention also emphasizes the legal age of marriage, possibly reinforcing the importance of schooling while reducing the perceived return to an early marriage.) As a third possible channel, additional social support built by the program could directly increase the value of time in school as school provides an opportunity for socializing, or indirectly increase its value by allowing girls to more effectively exercise newly developed skills.

Our second hypothesis is that GEP has no impact on life skills. Yeager and Walton (2011) survey the largely U.S. based literature showing that psychological interventions can improve school performance and emphasize that successful interventions involve the type of active participation and application that GEP provides in the classroom. The intervention explicitly targets future planning, gender norms, aspirations, empowerment / agency, self-esteem, freedom of movement, and socio-emotional support as life skills related outcomes, and we construct normalized indices for these domains.

In conceptualizing life skills, our definition is guided by the usage of the implementing partner Room to Read. Life skills are closely related to non-cognitive skills as studied in the educational and labor literatures, and some authors have used these terms (as well as other terms such as soft skills and socio-emotional skills) interchangeably, e.g. Kautz et al. (2014). In addition, a number of the specific skills or constructs we measure have been described and analyzed as non-cognitive skills in other papers.[10] Depending on a reader's

[10]For example, Macours and Laajaj (2019) analyze locus of control, self-esteem and time preference as non-cognitive skills in Kenya. Adhvaryu et al. (2018) analyze locus of control, self-esteem, time preference and mental health in India, as well as measures around extraversion that are related to socio-emotional support. Ashraf et al. (2020) analyze a particular non-cognitive skill, negotiating skills, that is closely related to our measure of empowerment and agency. A large literature spearheaded by Heckman and coauthors uses measures of self-

background in this literature, our definition of life skills may feel broad, including constructs such as gender norms, freedom of movement, and socio-emotional support that are not universally characterized as non-cognitive skills; alternatively, it may feel narrow, excluding characteristics such as health knowledge or behavior and financial literacy that are included in some definitions of life skills. Regardless, our life skills related outcomes were pre-specified in our analysis plan based on Room to Read's long history of engagement in this policy space.

3 Empirical Strategy

3.1 Evaluation Design

This evaluation is a clustered randomized trial with a targeted allocation rate of 1:1, conducted in 119 schools in Ajmer district in Rajasthan between 2015 and 2019. Given that GEP is delivered at the school level to all girls enrolled in the grades of interest, a cluster randomization is appropriate. The selection of schools eligible for inclusion in this evaluation was undertaken between August and November 2015. Randomization was conducted in Stata by the research team, stratifying schools with respect to school quality.[11] In order to identify the target sample of girls, a team of enumerators visited each school between December 2015 and January 2016 to obtain a roster of all girls enrolled in grade five. All female students who were currently enrolled in grade five in these schools as of January 2016 (2,543 female students in total) were eligible for inclusion in the evaluation.

Implementation of GEP was initiated in July 2016 at the beginning of the school year. At the time of design, RtR committed to running GEP in treated schools through the school year ending in the spring of 2018, with the goal of continuing GEP in these schools past that date if possible. Again, for the purposes of this analysis, we define the evaluation period

esteem and locus of control as proxies for non-cognitive skills, as in for example Heckman and Mosso (2014).

[11]Details on stratification are provided in Section A.1 in the Appendix.

as 2016 to 2018, corresponding to grades six and seven. Both baseline and endline surveys involved a household survey administered to the child's caregiver and a direct interview of the girl. Figure A1 in the Appendix summarizes the evaluation timeline.[12]

As noted above, GEP did continue post-2018, and we also report additional results with administrative data from schools, including enrollment and reported grades, collected throughout the evaluation period and post-endline through July 2019. Two points should be noted about the administrative data collection. First, its availability was not foreseen and its analysis was not pre-specified. Second, given the post-endline data collection, administrative data is available beyond the core evaluation period: grades are available through the conclusion of grade eight, and dropout information is available through early in grade nine.[13]

3.1.1 Evaluation Sample

At baseline, the survey team visited every one of the 2,543 girls on the enrollment lists provided by sampled schools; the survey was conducted before students or their families were informed about the life skills education program. Ultimately, any girl on the enrollment lists with either a completed household or child survey is considered to be enrolled into the evaluation. (In some cases, there are multiple sample girls in the same household.) Out of the 2,543 female students on the grade five enrollment lists, a total of 2,459 girls from 2,382

[12]This evaluation draws on a pre-analysis plan registered prior to the baseline survey (Edmonds et al., 2016). A detailed final evaluation report (Edmonds et al., 2019) fully populated the pre-analysis plan without deviations, and is publicly available on-line.The report can be found at *https://sites.dartmouth.edu/eedmonds/gep/*.Details regarding data collection and consent processes are provided in Section A.1 in the Appendix.

[13]In addition, administrative data from RtR report girls' participation in the intervention, including life skills sessions and mentoring. The research team also oversaw qualitative data collection at each phase of the evaluation, including in-depth interviews with girls in a subset of treatment schools as well as their caregivers.

households were enrolled into the evaluation sample, while 84 were excluded due to failure to complete any component of the baseline survey.[14]

However, not every girl who was part of the evaluation sample was interviewed at baseline; there were cases in which only the household survey was completed, as well as a smaller number of cases in which only the girl survey was conducted.[15] There were 2,353 household surveys conducted at baseline, which provide parent-reported data for 2,427 girls, and 2,399 individual girl surveys conducted at baseline. A flow chart summarizing the sample of girls surveyed can be found in Figure A4.

Summary statistics for the child outcomes mapped out in the evaluation design plan are provided in Appendix Tables A3 and A4. 3% of the sampled children had already dropped out of school at baseline. However, conditional on enrollment, 89% attended school in the

[14]Of these 84 cases, 34 were from households that had permanently migrated prior to the date on which the survey team visited the community — a fact reported by neighbors or other community informants — or simply could not be located. 33 were excluded because they did not provide consent. The reasons for non-inclusion for the remaining girls varied but included illness or death of the child (4); parents who were uniformly unavailable during survey hours and thus could not be surveyed or provide consent for the child to be surveyed (3); and cases in which the child was away from home and parents declined to participate in her absence (10).

[15]In addition, 16 girls living in 14 separate households from one primary school were omitted from the baseline in error. A different set of students enrolled in an alternative, adjacent primary school that is outside the evaluation sample were surveyed in their place. Given that these girls were not intended for inclusion in the sample, their data was subsequently dropped, and the correct set of girls were surveyed from the first tracking survey forward. These girls are considered to be enrolled in the evaluation, though they were not surveyed at baseline.

past week. While the modal girl in our study is at an appropriate age for grade 5 (age 10-11), approximately a third of the sampled girls were older.

3.1.2 Attrition

The analysis sample includes all sampled girls represented in the endline survey, conducted between July 2018 and December 2018. At endline, 2,387 child surveys and 2,358 household surveys were conducted. There were 48 girls (in 47 households) for whom a household survey was conducted without a child survey.[16] There was also one girl surveyed whose household did not complete an endline survey. In addition, 24 girls in 23 different households attrited fully at endline with no data collection completed.[17] Attrition patterns are summarized graphically in Figure A5. Within the sample of 2,459 girls randomized, we have some endline information for 2,435 girls, generating an attrition rate below 1 percent.

Attrition does not appear to be a source of bias in our intent to treat analysis. Among the 24 girls who fully attrited, 15 are from the control arm and 9 are from the treatment arm, corresponding to attrition rates of 0.7% and 1.2% in the treatment and control groups, respectively; the probability of full attrition is not significantly correlated with treatment, conditional on strata fixed effects (β=-.004, p=.362). The girls' survey is used in many of

[16]In 14 cases, consent was declined for the girl survey. 19 girls had migrated away from their households permanently; two had migrated temporarily and had not returned by the point at which the survey concluded. Four child surveys were not completed due to the death of the child, and nine child surveys were not completed due to child disability. In these nine cases, the child was similarly not surveyed at baseline, but a household survey was completed at both baseline and endline.

[17]In 10 cases, the household had migrated and could not be reached for follow-up. Consent was declined in 12 cases. In one case, a partial survey was completed but the household declined to continue due to limited time, and in one case, no information was available about the household's whereabouts.

the life skills indexes, and 72 girls did not complete it. 45 are from the control arm and 27 are from the treatment arm, corresponding to attrition rates of 3.6% and 2.2%, respectively; this difference in attrition on the girls' survey is significant at the 10 percent level (β=-.014, p=.087). In the Appendix, we conduct a series of bounding exercises to evaluate the sensitivity of our findings to attrition. Attrition does not substantively affect the empirical results under reasonable assumptions regarding the distribution of missing values.

3.2 Statistical Model

To identify the impacts of assignment to treatment, we estimate the following pre-specified regression. Each outcome Y_{ist} for child i in school s measured at time t is regressed on a dummy for treatment assignment T_s and randomization strata fixed effects μ_s. The specification also includes a vector of age dummies γ_i , a vector of dummies λ_i capturing the most important type of employment in the household at baseline, the lagged (baseline) value of the relevant outcome $Y_{is,t-1}$, and additional controls for baseline variables where imbalance was detected between the treatment and control households, reported in Tables A2 through A4 and discussed in more detail below. These baseline variables are denoted $\xi_{is,t-1}$.[18]

[18]For the family of outcomes corresponding to school dropout and other academic outcomes, age at enrollment and maternal education are additionally included as control variables in equation (1); this methodology was pre-specified in the analysis plan. If the baseline control variable is missing because either the household or child survey was not conducted for a particular girl at baseline, the missing value is coded as zero. Additional dummy variables equal to one for observations with missing values are included for each baseline covariate. For life skills measures added at endline (the Rotter locus of control, the perceived stress index, the Rosenberg self-esteem index), we control for lagged values of overall life skills indices. For the ASER test scores added at endline, we control for baseline school dropout status, attendance, grade progression, time spent studying, hours spent on school and grades

$$Y_{ist} = \beta_1 T_s + \beta_2 Y_{is,t-1} + \beta_3 \xi_{is,t-1} + \mu_s + \gamma_i + \lambda_i + \epsilon_{ist} \tag{1}$$

We also estimate two simpler specifications, similarly pre-specified, to evaluate the robustness of these results: one specification including only the treatment dummy and strata fixed effects, and one specification including only the additional control variables γ_i, λ_i, and $Y_{is,t-1}$. These additional results are reported in the Appendix, but we focus on results from equation (1) in order to maximize precision (we never reject the null hypothesis of no change in treatment effects associated with the added controls).

In all specifications, standard errors are clustered at the school level, yielding 119 clusters. Given that there are a large volume of hypotheses tested regarding life skills, false discoveries (type 1 errors) are probable; accordingly, we present false discovery rate adjusted q-values computed across all life skill outcomes using the same specification (Benjamini and Hochberg, 1995). For completeness, we also present false discovery rate adjusted q-values computed separately for dropout and other academic outcomes as well as for ancillary outcomes.

We also evaluate balance with respect to a number of baseline characteristics. These results are reported in Tables A2 through A4 and discussed in Section A.2 in the Appendix.

4 Findings

4.1 Education

4.1.1 Dropout

The primary purpose of the program evaluated is to advance the education of girls, and following two years of treatment, the endline survey conducted at the end of seventh grade documents that GEP is effectively generating a decline in dropout and an increase in grade progression. This result is captured graphically in Figure 1a, based on estimating equation

in grade five as reported in administrative data. These methodologies for addressing missing baseline values were all pre-specified in the evaluation design plan addendum.

(1), and the corresponding regression results are provided in Table 1.[19] Treatment girls are four percentage points less likely to have dropped out at endline (after seventh grade), corresponding to a 31 percent reduction relative to the control group dropout rate of 13.2 percent. We similarly observe a four percentage point increase in progression to grade seven, suggesting that treatment girls remain with their same cohort as they progress through school (Columns (1) and (2) of Table 1).

The impact of treatment on dropout seems persistent. A year after endline, we compiled school administrative records on dropout and test scores as of the start of grade nine; this analysis was not pre-specified, as we did not anticipate access to this data, but it allows us to identify the effect of the treatment on dropout in grades six through eight and at the transition into high school in grade nine, a frequent point of dropout for girls. The results suggest that the treatment effect on dropout increases from endline through grade eight and grade nine, and remains statistically significant (Columns (3) through (6), Table 1).[20] Because the grade nine records were collected at the start of the school year in late July 2019, they differ from other records based on attendance throughout the school year; data is available only about how many days the child attended school in the last seven days, and how many days the school was open in that period. Hence, defining dropout based on this measure will have measurement error given that there is irregularity in attendance. Accordingly, the findings for grade nine are estimated using a modification of equation (1)

[19]Additional specifications are reported in Appendix Tables A5 through A6 in the Appendix.

[20]In the administrative records, we observe a slightly smaller impact of treatment on dropout in grade seven compared to the household survey. This discrepancy might reflect that a girl is in the administrative records for grade seven if she attended school at all during the grade seven school year, while the endline survey response reflects whether she views herself as a dropout after the conclusion of grade seven.

that also controls directly for the number of days the school was reported open. Without controlling for this form of measurement error from days open, we find a noisy zero effect of treatment on dropout in grade nine (not presented).

In addition, we pool the dropout measures into an index in order to address multiple hypothesis testing concerns; including grade six where there is no treatment effect, we observe a statistically significant decline in dropout overall in the administrative records (t-statistic of 2.6, Column (7) of Table 1). While in percentage terms, the largest effect is observed at the end of grade seven in the endline survey, the magnitude of the treatment effects is consistent across years (around 25 percent). In interpreting these results, it is important to note that grades eight and nine correspond to the post-evaluation period during which some limited material support was rolled out. The data are consistent with our hypothesis that this program change did not meaningfully change the treatment; less than 20% of treatment girls receive any in-kind support, and the average such transfer was valued at $7.

Taken together, these findings provide cause for optimism and suggest that a broad life skills intervention can shift girls' enrollment in a setting characterized by intense gender disadvantage. As such, our findings complement those presented in Ashraf et al. (2020) by indicating that alternative (and more common) life skill curricula that are delivered in school may prove impactful. That these effects are identified even where stark gender disadvantages may limit the weight placed on girls' preferences in household decision-making further highlight the importance of considering the agency that children have in making educational enrollment decisions.

4.1.2 Other Academic Outcomes

In our post-endline qualitative work, one head teacher previewed our results by remarking that the treatment was effective in maintaining girls' enrollment without improving their academic performance. That is exactly what we find: there are no apparent increases in attendance conditional on enrollment, and no positive shifts in test scores or grades. Columns (1) and (2) in Panel A of Table 2 report the treatment's impact on attendance conditional on enrollment, coding attendance as missing for girls who are not enrolled. We cannot reject the null of no effect on the intensive margin of attendance. If we recode attendance as zero for girls who are not enrolled, the extensive margin dominates and we observe a negative and significant coefficient that parallels the observed effect on enrollment. Intensive margin attendance has a mean reported past-week attendance rate for control girls of 92%; accordingly, there is a limited margin for treatment to improve attendance conditional on enrollment. To address potential error in self-reported attendance, we also examine effects on attendance using administrative data reported by the schools, and similarly fail to reject a null effect. We also do not observe any significant shift in girls' reported time spent studying, reported in Columns (3) and (4) in Panel A of Table 2.

In addition, we fail to reject the null of no effect of treatment on test scores. We administered ASER tests in the endline survey (at the conclusion of grade seven) and report estimated treatment effects in Columns (1) through (3), Panel B of Table 2.[21] We similarly observe no positive effect on administratively reported test scores in grades six, seven or eight for enrolled girls, based on records compiled from evaluation schools (Columns (4) through (6) of the same panel).[22] Further exploration of the effects on school test scores,

[21]In Table 2, controls for baseline outcome values cannot be included for cognitive test measures since cognitive tests were not conducted at baseline; specification (1) instead includes controls for baseline school dropout status, attendance, grade progression, time spent studying, hours spent on school, and grades as reported in grade five.

[22]Analysis of school administrative records was not pre-specified, but we follow the same analytical strategy as in the pre-specified analysis. There is heterogeneity within schools across years and across schools in how year-end exams are scored, and thus for comparability purposes, we have computed an average GPA across the three tests based on the letter grades associated with the numerical test scores. One obvious concern might be that negative selection in students who do not drop out is masking the gain in test scores for higher-

including analysis of any potential bias induced by selection into test attendance, is reported in Appendix Tables A18 and A19.[23] While it is certainly possible that the ASER test is an imperfect instrument to measure learning gains for this population, the consistency of the null effects observed across multiple measures of test scores suggest that it is plausible to conclude that there was no effect of the intervention on learning.

These findings are consistent with the hypothesis that treatment is not particularly effective in dimensions such as academic performance where the perceived returns to associated improvements may be realized only in the medium and long run (by expanding future educational opportunities) rather than the short run. In contrast, as we show below and discuss in more detail in Section 5, treatment appears to increase the immediate perceived value of remaining in school for girls by promoting social engagement. This strengthening of social ties likely plays an important role in explaining estimated impacts on dropout, but may also serve as a distraction from academic work and so limit any further academic gains.

4.2 Life Skills

The GEP program aims to promote the agency of girls by teaching them life skills, and even in our setting characterized by extreme gender disadvantage, we document substantial and statistically significant enhancements in life skills for girls assigned to treatment, although not in every measured domain. These findings further indicate that life skills interventions need not be narrowly tailored to be effective; rather, we find that a broad life

achieving students. While dropouts are indeed negatively selected, we similarly do not see positive effects on test scores among students who were higher-achieving at baseline.

[23]A negative and significant effect of treatment on grade six grades is in fact observed in Column (4), Panel B in Table 2. This effect is unlikely to reflect differential selection in school continuity as treatment is only associated with an eight tenths of one percent reduction in dropout in this grade; alternatively, this may be an artifact of the volume of hypothesis tests. The false discovery rate associated with this test is 36 percent.

skills intervention simultaneously shifts girls' life skills across a number of domains.

4.2.1 Direct Responses

At endline, we fielded 259 individual questions related to life skills — many of these corresponding to constructs also labeled non-cognitive skills in the literature — in addition to four demonstration tasks. In our analysis plan, we pre-specified how these questions would be combined into indices of life skills corresponding to our partner's conceptualization. Figure 1b is based on estimating equation (1), and the corresponding regression results are provided in Tables 3 and 4.[24] We also present some illustrative regression results for individual sub-component questions of indices when useful, and these results are reported in Tables A13 through A15 in the Appendix.

Girls assigned to treatment show improvements in future planning, as evident in Column (1) of Panel A in Table 3.[25] The future planning index is constructed around a series of questions that ask girls about concrete goals they have for the future as well as their motivation for thinking about these goals and developing a plan to achieve them; treatment leads girls to express these goals and the steps to achieving them more completely. This finding of the feasibility of teaching planning skills in early adolescents in also documented in classrooms in Turkey by Alan and Ertac (2018).

We also observe that treatment girls express more positive gender norms (Column (2)) supporting the finding in Dhar et al. (2020) that gender norms can be influenced in the

[24]Figure 1b presents primarily results from Table 3, in addition to three selected variables from Table 4. Additional specifications are presented for robustness in Tables A9 through A12 in the Appendix.

[25]For all life skills indices included in the analysis, we normalize each variable based on the mean and standard deviation observed in the control arm, and then average across measures, ensuring that the interpretation is consistent (i.e. higher values of future planning index components all correspond to higher levels of future planning).

classroom. This index consists of a series of opinions offered on statements that reflect differential treatment of boys and girls. Relative to girls in the control group, treatment girls feel that boys and girls should be treated more similarly. Interestingly, while girls show improvements in future planning and articulated gender norms, they do not express greater aspirations about what type of life or career they will have in the future relative to the control sample (Column (3)). This finding is also similar to Dhar et al. (2020) who do not observe an impact of their intervention on the aspirations of treatment girls.

More novel here is that treatment has a negative effect on marriage expectations (Column (4), Panel A of Table 3). The shift in the marital expectations index is driven primarily by a shift downward in the top of the distribution of desired and expected marriage ages. The GEP curriculum strongly emphasizes 18 as the appropriate and legal minimum age of marriage, and treatment girls are then more likely to report 18 as the desired age of marriage relative to both younger and older desired ages. This finding differs from Bandiera et al. (2020), although their marriage related measures focus more on sexual behaviors rather than awareness of marriage laws, an outcome that is more relevant for their 18 year old population than our 11 to 13 year old population.

Girls assigned to treatment articulate a stronger sense of empowerment / agency (Column (5), Panel A of Table 3). The empowerment index consists of questions about who makes decisions in the girl's life over a variety of different domains, and girls assigned to treatment feel that they have more say in what happens in their lives. For example, girls exposed to treatment report a 17 percent increase in the probability they feel they are the sole decision-maker about whether to attend school, a 29 percent increase in the probability they feel they are the sole decision-maker about whether to work, and a 32 percent increase in the probability they feel that they can talk to their parents about marriage.[26] These

[26]These results are reported in Appendix Table A13; the questions are all subcomponents of the empowerment / agency index.

empowerment findings are analogous to Buchmann et al. (2017) who provide empowerment training to 15 year old girls in Bangladesh around avoiding marriage. However, in our case, despite this increase in empowerment, treatment girls do not express more self-esteem or report greater freedom of movement (Column (6), Panel A, and Column (1), Panel B of Table 3).

Treatment also increases girls' sense of socio-emotional support (Column (2), Panel B of Table 3). This index is composed of questions about friends and the support they offer, and we observe improvements in all the index components. Preferring to be alone (as reported by parents) declines by 14 percent, meeting friends outside of school increases by 7 percent, reporting a place to meet friends increases by 13 percent, and reporting a place to stay if needed increases by 4 percent.[27] In addition, the amount of time reported devoted to socialization (in minutes) increases by 25 percent, reports of phone use double, and total time on the phone increases by more than 400 percent (for phone use, the mean is near zero in the control arm). Even our school travel time measures are consistent with increases in social engagement: conditional on attending school, treatment girls are not more likely to change schools or report a shift in their physical distance from school. However, it seems to take them 24 percent more time to travel to school post treatment.[28]

We measure life satisfaction with Cantril's ladder and common measures of locus of control, perceived stress, and self-esteem. Here, we cannot reject the null of no change in these measures of life satisfaction (Columns (3) through (6), Panel B, Table 3). Confidence

[27] These results are reported in Columns (1) through (4) of Appendix Table A14. The parental-reported variable around the preference to be alone is not part of the socio-emotional support index.

[28] Results for time use are also reported in Appendix Table A14. The analysis of time allocated to socialization was not pre-specified, and is not part of the socio-emotional support index.

intervals are large relative to coefficients on all of these measures, and it is not obvious that a life skills intervention that builds awareness of challenges in adolescent life should improve life satisfaction.

In sum, we find that treatment leads to significant enhancements in a subset of measured life skills, while changes on other domains are statistically insignificant and small in magnitude. All of the life skills we analyzed were selected in order to match those skills that Room to Read seeks to develop in their program. In Section 5, we exploit the pattern of findings described here to narrow down the set of plausible causal pathways that may explain the dropout effects that we measure.

4.2.2 Third Party Observation

We supplement the self-reported responses around life skills with several additional sources of data. We ask parents to assess the life skills of their daughters, and cannot reject the null hypothesis of no treatment effect for parental perceptions of girls' self-efficacy and freedom of movement, as well as parental attitudes around gender norms, schooling and marriage (Columns (2) through (7), Panel A of Table 4). We observe a decline in parents' assessment of girls' strengths, as reported in Column (1) of the same table; this primarily reflects a perception by parents that girls are less considerate and less willing to help.[29] This finding is similar to that reported by Ashraf et al. (2020), who document that mothers view their daughters more negatively following their engagement in a negotiating skills intervention.

We also attempted to assess life skills via four demonstration tasks that we developed, including a scavenger hunt that was designed to demonstrate life skills by having girls negotiate challenges in finding common items. We find no effects of treatment on any of the demonstration tasks (Columns (1) through (4), Panel B of Table 4). Though these demonstration tasks did well in pre-testing, in fielding substantial implementation challenges arose

[29]These results are reported in Columns (1) and (2) of Appendix Table A15.

that complicate the interpretation of these failures to reject.[30]

Enhancements in life skills were also apparent to enumerators who are blind to the child's treatment status. We asked enumerators to directly assess girls based on their focus in the interview, attentiveness, responsiveness, and articulateness. Here, we find a statistically significant improvement in enumerator assessment of the girl's demonstration of these life skills, equal to approximately 0.1 standard deviation (Column (5), Panel B of Table 4).

4.2.3 Parroting and Social-Desirability Bias

Dhar et al. (2020) highlights that measures of life skills are vulnerable to parroting back the lessons taught in life skills classes as well as social desirability bias if the intervention builds awareness in subjects about the "right" answers. For example, future planning activities are a part of the GEP curriculum, and thus we cannot differentiate whether the enhanced future planning expression comes from in-program activities or broader impacts on how girls think about their futures. Similarly, questions about equitable gender norms are closely related to the content of many GEP classes, and thus these responses could reflect a parroting of class lessons (although these surveys are conducted in private at home, where if anything social pressure might run against an expression of the more progressive values implied by class content).

On this parroting concern, our findings that parents and enumerators both report changes in treatment children are relevant. Also, it is important to bear in mind that the timing of

[30] Specifically, we observed that girls' efforts on the mirror drawing task (where they were asked to use a mirror to draw copies of increasingly complex shapes) varied based on the particular environment in which they were surveyed. The within-girl correlation between baseline and endline measures is only 0.06. For the scavenger hunt, variation in the time between the two required surveyor visits (one to introduce the scavenger hunt and one to assess scavenger hunt success), in addition to overall delays in the timing of surveyor re-visits, seems to have limited the signal value of the associated measures.

our survey is such that most subjects are interviewed at endline approximately six months following the final life skills class.[31] In our view, the parroting that we might be capturing after six months reflects a degree of internalization of information that is a goal of the intervention, and is very different than asking girls questions shortly after a class ends. That said, we evaluate parroting / social desirability bias using two robustness checks.[32]

First, to assess parroting, we classify 90 individual life skills questions posed in the endline survey based on whether they are explicitly addressed in the grade six or grade seven curriculum or addressed only indirectly. This distinction is based on whether we could identify an exact curricular match to the question; all of the life skills that we classify as not explicitly addressed may be indirectly addressed in the program. We then construct separate indices characterizing responses to explicitly versus indirectly addressed questions, and estimate the treatment effects for these indices. We find a treatment effect of 0.056 SD for the explicitly addressed questions index (standard error of 0.018) and an effect of 0.016 SD for the indirectly addressed questions index (standard error of 0.019). However, we know that the marital expectations questions show an effect in the opposite direction of that hypothesized, and many marital expectations questions are in the indirect questions index. If we exclude the five questions related to marriage age that are included in the marital expectations index, the treatment effect for the indirectly addressed questions index rises to 0.040 SD (standard error of 0.020), and we cannot reject the hypothesis that the treatment effects for indirect and direct questions are equal in magnitude.[33]

[31]Our surveys are largely collected in July and August; classes ended in February before seventh grade exams and eighth grade life skills classes did not resume until October.

[32]This analysis was not pre-specified.

[33]Complete regression results for every life skills question posed in the survey, categorized as directly or indirectly addressed, are available in Table 13 of the final evaluation report (Edmonds et al., 2019). The report can be found on-line at

Second, to assess social desirability bias, we use the baseline data to gauge the extent to which a child is inclined to provide socially desirable answers. Specifically, in Appendix Tables A23 and A24, we employ a social desirability index that is constructed based on three component responses from the baseline child survey: (1) an indicator for whether the girl reports that she wants to become a "Teacher/School head/Educator" when she grows up, (2) an indicator for whether she reports that she is currently living "the best possible life," and (3) an indicator for whether the surveyor recorded that the girl paid close attention "the whole time" when receiving instructions during the survey. We view these three survey measures as the baseline questions that best capture girls' proclivity to offer responses or to engage in behaviors that would be perceived as socially desirable (and most analogous to how Dhar et al. (2020) execute a similar test). We analyze heterogeneous treatment effects based on whether a child is above the median value of this social desirability index. Above-median index values are highly predictive of more positive expression of life skills in both control and treatment groups. We do not reject the null that treatment effects are the same regardless of whether the child is above or below the median social desirability index.

In sum, six months after the conclusion of the second year of life skills classes, we see enhancements in future planning, attitudes towards gender norms, empowerment / agency, and socio-emotional support. Enumerators similarly document these improvements in girls, and parents, whose attitudes do not seem to change measurably with daughter's engagement in treatment, report treatment girls acting more in their own self interest. We do not see evidence consistent with these findings being driven by parroting or an attempt by treated girls to give more socially desirable answers. In addition, in Appendices A.3 and A.4, we further explore the robustness of the primary results for both educational and life skills outcomes to alternative assumptions about attrition, and find that the primary effects remain consistent in magnitude and significance.

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https://sites.dartmouth.edu/eedmonds/gep/.

5 Discussion

Our principal experimental findings are that school-based life skills classes administered in Rajasthan reduce dropout and promote the expression of some life skills in adolescent girls after two years of programming. As noted above in our hypotheses, the treatment is designed to potentially have effects via multiple channels that could reduce girls' dropout.

The first hypothesized channel was an increase in girls' agency; between our partner's focus on GEP as an empowerment intervention and the existing emphasis in the academic literature on intrahousehold conflict over education, it is logical to hypothesize that improvements in sense of empowerment and agency in life decisions documented in the previous section drive the reduction in dropout. However, the data are not consistent with serious parent-child differences in preferences over education. In the control sample at endline, two-thirds of parents report that they expect their daughters will complete at least senior secondary school, and 71 percent expect their daughters will complete at least five more years of schooling (beyond grade seven). Moreover, 54 percent of parents think that being well-educated is an important characteristic for a potential daughter-in-law, and 74 percent believe that girls need to pursue higher education. As such, parents do not seem less enthusiastic about their daughters' schooling than the girls themselves; while girls do seem to be more empowered, and this may be an important broader channel for intervention effects, they may not be required to deploy this sense of empowerment vis-a-vis their parents in order to remain in school.[34]

The second hypothesized channel was shifts in the perceived returns to education. Here, we cannot reject the null that these perceived returns are unchanged by treatment: we have already documented above that we did not observe a meaningful change in parental attitudes

[34]These patterns are also consistent with Bursztyn and Coffman (2012), who find that parents value their children's attendance at school, and accordingly value conditionality in cash transfers as a strategy to manipulate child school attendance.

toward girls' education, and for girls themselves, we do not see any evidence of greater aspirations, including a shift in the desire to work for wages, a desire to work in an occupation that requires secondary schooling, or how much education girls hope to complete (as reported in Columns (3) through (5), Table A15). For this last outcome, 93 percent of girls in the control arm already report wanting to complete secondary schooling, and the confidence interval on the treatment effect ranges between -2.5 and 2.6 percentage points. Further, if we hypothesized that treatment effects on dropout reflected shifts in the return to education, we might expect to see increases in attendance conditional on enrollment or improvements in academic outcomes; neither are evident in the data. Similarly, the improvements in future planning that we document might be expected to influence the perceived returns to schooling by helping girls to establish and more efficiently achieve educational goals, but we do not observe changes in child time allocation or academic performance that would be consistent with this hypothesis.

The third hypothesized channel was a reduction in the opportunity cost of school. Here, the growth in social-emotional support and social engagement that reduces the cost of school to girls stands out as the most plausible explanation for the reduction in dropout. As discussed in the previous section, treatment enhances social engagement. Girls spend more time socializing and want to be around friends more. Conceptually, we view these shifts as enhancing the value of time in school to the child as they can socialize with these friends, including in the life skills classes which require school enrollment to access. There may also be skills that can be exercised because of these expanded social supports. For example, teasing is a significant issue in our setting and may contribute to dropout. While girls learn in one life skills class to ignore teasing, a collective response to teasing may be more effective than an individual response, and thus strengthened friendship networks can amplify the direct effects of treatment in addition to influencing dropout propensity directly. In this sense, our findings correspond almost inversely to the finding in Ashraf et al. (2020) that a negotiating treatment meaningfully affected girls' ability to exercise agency vis-a-vis their parents, while the social aspect of the treatment was relatively unimportant; we find evidence of the opposite pattern in our context where girls are younger and more isolated absent treatment.

To the extent that enhanced social support plays a key role in reducing dropout, it may also help to explain the null in attendance and academic outcomes. An increase in social support may make it easier to miss school, as friends can assist to make up any missed material, offsetting any increased desire to attend. Similarly, for academic performance, additional social engagement may undo whatever gains might otherwise result from growth in motivation.

Despite the apparent importance of this channel in explaining dropout effects, the enhancements in life skills that we observe are not limited to girls vulnerable to dropping out of school. In Appendix A.5, we examine heterogeneity in treatment effects by dropout propensity (computed based on the correlation between dropout by seventh grade in the control sample and baseline characteristics).[35] The impact of treatment on dropout is concentrated among girls with a high probability of dropping out (Table A25); however, the life skills results described in the previous section do not appear to consistently vary with dropout propensity (Tables A28 through A30). Hence, in an accounting sense, the improvements in life skills cannot explain the reduction in dropout. We interpret this pattern as suggestive evidence that life skills improvements are consistent across girls of varying characteristics at baseline, but that only a subsample of girls are in fact vulnerable to dropout, and the positive effects of treatment on dropout are then evident only for this subsample.

The fact that enhanced agency does not appear to be the primary channel for the increase in dropout in this context, and in fact is associated with increased parental disapproval of

[35]We also examined heterogeneous effects with respect to six pre-specified covariates (school quality, baseline child age, maternal education, and exposure of the household to recent economic shocks, crime shocks, and death/illness shocks), and found no evidence of meaningful heterogeneity; for concision these results are not reported.

girls' behavior, is also relevant for questions of external validity. For the sample households, the direct cost of girls' schooling is zero, and the opportunity cost may also be relatively moderate. In other contexts where the costs of schooling are high and parents exert more control over the required inputs, intrahousehold negotiations around schooling decisions may be more important, and both enhanced girls' agency and the potential parental backlash to this agency may have different implications.

6 Conclusion

In this paper, we analyze evidence around an intervention aimed at improving the life skills of adolescent girls in government schools in Ajmer, Rajasthan, India. Room to Read's Girls' Education Program provided twice-monthly life skills classes during sixth and seventh grade, taught in school by trained social mobilizers, and the modal treatment girl attended all the life skill classes offered. This treatment is associated with both improvements in life skills and a reduction in dropout of around 25 percent that persists through grade nine.

However, while random assignment to life skills classes led to a reduction in dropout and improved expressions in life skills, there are a host of adolescent life outcomes that do not seem to have been impacted by treatment: test scores, child labor, and marriage being among the most important. The finding of a lack of improvement in test scores is consistent with many other studies in the life skills space (e.g. Holmlund and Silva, 2014; Delavallade et al., 2017). In our context of reduced dropout, this naturally raises the question of whether continued school enrollment is valuable in itself. It is possible to have financial returns to education that are not proxied by test scores. Moreover, even if school performance does not change differentially with treatment, an improvement in school progression will itself build human capital as girls continue to advance through school. Continued enrollment may also facilitate delayed fertility, and this may be especially relevant in our setting with pervasive child marriage. The literature on returns to female education further highlights the value of remaining in school for the life skills, experiences, and social relationships that education can help foster. Given that we observe a direct treatment effect on socio-emotional support, treatment can be valuable even it does not promote test-measured learning.

Room to Read's Girls Education Program usually includes outreach and financial support that was not delivered in the study area. While we are able to evaluate the impact of the life skills component of the program in isolation, our study is nonetheless related to recent research evaluating multifaceted interventions that combine life skills training with other social services such as Save the Children's Safe Spaces (Buchmann et al., 2017) and BRAC's Empowerment and Livelihood for Adolescents (ELA) program (Bandiera et al., 2019, 2020). While both Safe Spaces and ELA target older girls than our study, they both document improvements in schooling. Our finding that life skills training (separate from the other components of ELA or Safe Spaces) increases education while building social relationships highlights the potential importance of that specific component of these multifaceted programs. Relatedly, our finding that life skills alone is not sufficient to influence some of the important life decisions that ELA impacts also highlights the additional value of other components of the multifaceted approach, despite the non-experimental evidence in Bandiera et al. (2020) that emphasizes the contribution of life skills training in particular.

Our study delivery method also highlights the tradeoffs inherent in targeting decisions related to life skills programs. Both ELA and Safe Spaces use time and space outside of schools. This allows them to reach more marginalized girls not associated with a school absent the program. However, they face much lower take-up than our school based intervention. While 85 percent of our subjects are still engaged after two years, Safe Spaces only induced 56 percent of girls to attend one class, and ELA take-up in Uganda was below 25 percent. In our context, not only is it easy to reach girls within school, but the girls already have within-school social relationships that can be leveraged. To the extent that the reinforcement and deepening of those social relationships are important in our dropout results, such dynamics might not be present in an intervention targeting out of school girls. Better understanding how social relationships can be leveraged to improve schooling seems a promising area for future research.

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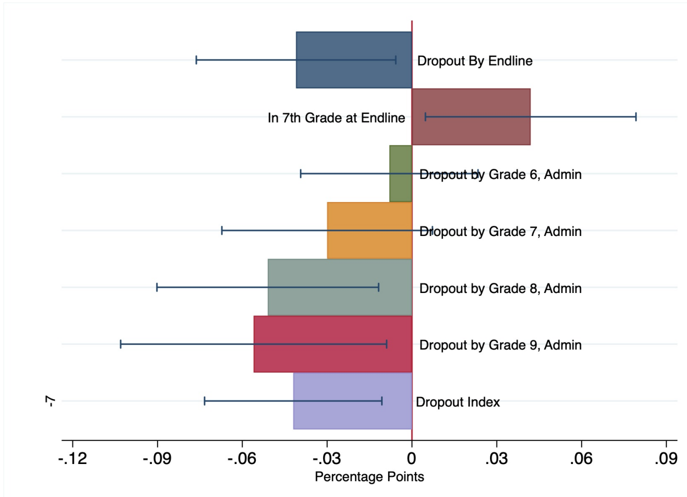
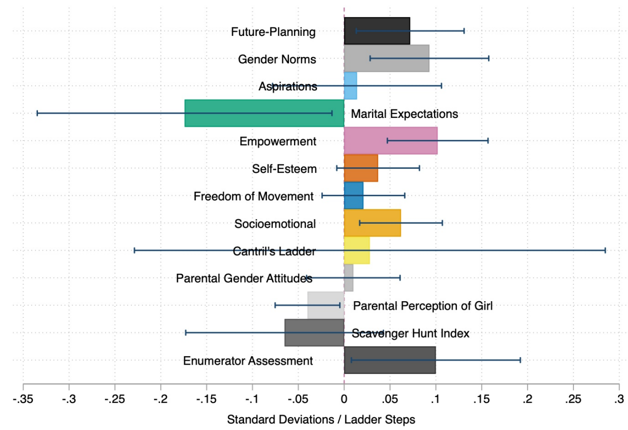


(a) Treatment Effects on Dropout by Grade



(b) Treatment Effects on Life Skills

Figure 1: Primary outcomes

Notes: This figure reports the estimated treatment effects on dropout (and progression to grade 7), controlling for stratum, baseline value, age effects, type of employment in the household, and imbalanced variables. Baseline and endline data are from the respective surveys. The top 2 rows are from the endline survey and were pre-specified. All other specifications are estimated using school administrative records. Grade nine data was collected at the start of the school year and is only based on whether the child attended school in the seven days before surveying; thus all grade nine results also include controls for the number of days the school was open in the seven days prior to survey. Dropout index is constructed by combining all other dropout variables. This figure reports the estimated treatment effects on indexes for life skills, controlling for stratum, baseline value, age effects, type of employment in the household, and imbalanced variables. Baseline and endline data are from the respective surveys. 95 percent confidence intervals are pictured. Standard errors are clustered by school.

Table 1: School Dropout

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Survey data		Administrative data				
	Whether child has dropped out	Whether child progressed to 7th grade	Dropout Grade 6	Dropout Grade 7	Dropout Grade 8	Dropout Grade 9	Dropout Index
Treatment	-.041**	.042**	-.008	-.030	-.051***	-.056**	-.042***
	(.018)	(.019)	(.016)	(.019)	(.020)	(.024)	(.016)
Obs.	2433	2387	2374	2319	2455	2228	2458
R^2	.152	.144	.111	.122	.114	.438	.223
Q-statistic	0.041	0.041	0.600	0.138	0.041	0.041	
Mean Control Group	0.132	0.865	0.075	0.142	0.192	0.290	0.169

Notes: Table contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported), stratification fixed effects, age fixed effects, baseline value of the outcome, a vector of dummies for the most important type of employment in the household at baseline, and controls for variables that appear imbalanced in the balance tables.

Column (1) uses child and household endline survey data. Column (2) uses child endline survey only (all children enrolled in grade five at baseline). Columns (3) through (6) rely on administrative data. In Columns (3) through (5), dropout is measured based on whether a child attended school at the conclusion of the referenced school year. In Column (6), dropout is measured based on whether a child attended school during the past week (conditional on the school being open). The Column (7) dropout index is constructed as the average of the outcome variables included in Columns (1), (3), (4), (5), and (6). Columns (6) through (7) include a set of fixed effects for the number of days that the school was open in the week before administrative data collection in grade nine.

Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level. Q-statistics are False Discovery Rate corrected q-values based on Benjamini and Hochberg (1995).

Table 2: Attendance, Time Allocation, and Test Scores

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Attendance (Past Week) and Time Allocation (Typical Day in Past Week)						
	Attendance Rate	Any Attendance	Hours studying at home	Hours spent at school		
Treatment	.002	.003	-.043	.164		
	(.009)	(.006)	(.075)	(.188)		
Obs.	2089	2089	2386	2386		
R^2	.027	.019	.052	0.1		
Q-statistic	0.841	0.841	0.841	0.841		
Mean Control Group	0.918	0.982	1.541	7.166		
Panel B: Test Scores						
	ASER Mathematics	ASER Hindi	ASER English	GPA Grade 6	GPA Grade 7	GPA Grade 8
Treatment	-.014	.022	-.068	-.150**	-.145	-.026
	(.070)	(.089)	(.084)	(.074)	(.096)	(.087)
Obs.	2380	2380	2380	2178	1976	1912
R^2	.078	.085	.096	.334	.216	.207
Q-statistic	0.841	0.841	0.841	0.451	0.665	0.841
Mean Control Group	2.353	3.025	2.369	2.259	2.404	2.890

Notes: Table contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported), stratification fixed effects, age fixed effects, baseline value of the outcome, a vector of dummies for the most important type of employment in the household at baseline, and controls for variables that appear imbalanced in the balance tables.

Columns (1) and (2) of Panel A use child endline survey data and are conditional on school being open and child not having dropped out of school. Attendance rate in Column (1) is the fraction of school days attended in the week prior to being surveyed and the Attendance dummy in Column (2) is an indicator for having attended any days in the past week. 298 observations are missing for these measures because of temporary school closures. Time use outcomes in Columns (3) and (4) of Panel A are defined based on time use patterns recorded for “a typical day in the past week.” In Panel B, baseline cognitive test values were not collected, and accordingly Columns (1) through (3) instead include controls for baseline school dropout status, attendance, grade progression, time spent studying, hours spent on school, and grades as reported in grade five.

Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level. Q-statistics are False Discovery Rate corrected q-values based on Benjamini and Hochberg (1995).

Table 3: Life Skills: Child Survey Measures

	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A			
	Future planning index	Gender norms index	Educ. / emp. aspirations index	Marital expectations index	Empowerment / agency index	Self-esteem index
Treatment	.072**	.093***	.016	-.174**	.102***	.037
	(.030)	(.033)	(.039)	(.082)	(.028)	(.023)
Obs.	2380	2380	2380	2380	2380	2380
R^2	.034	.036	.206	.308	.032	.059
Q-statistic	0.103	0.065	0.820	0.122	0.008	0.357
Mean Control Group	-0.016	0.000	0.000	-0.606	-0.002	-0.001
			Panel B			
	Freedom of movement index	Socio-emotional index	Cantril's ladder	Locus of control index	Perceived stress index	Rosenberg self-esteem index
Treatment	.021	.062***	.028	-.026	-.025	.021
	(.023)	(.023)	(.131)	(.046)	(.046)	(.030)
Obs.	2380	2380	2380	2380	2380	2380
R^2	.018	.034	.02	.028	.013	.042
Q-statistic	0.707	0.065	0.867	0.795	0.795	0.755
Mean Control Group	0.000	0.000	4.513	0.000	0.000	0.000

Notes: Table contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported), stratification fixed effects, age fixed effects, baseline value of the outcome, a vector of dummies for the most important type of employment in the household at baseline, and controls for variables that appear imbalanced in the balance tables. For Columns (4) through (6) of Panel B reporting measures added at endline, we control for lagged values of overall life skills indices.

For all included indices, we calculate the normalized difference between treatment and control for each component question, and average over all components, ensuring that the interpretation is consistent (i.e. higher values of empowerment index components all correspond to higher levels of empowerment). Marital expectations index is not mean 0 because married girls are assigned the minimum value calculated for non-married girls. Detailed definitions of all referenced indices can be found in the analysis plan.

Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level. Q-statistics are False Discovery Rate corrected q-values based on Benjamini and Hochberg (1995). These are computed by pooling all specifications included in Tables 3 through 4.

Table 4: Life Skills: Parental Reports, Demonstration Tasks, and Enumerator Assessment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Parental Reports							
	Parental perception of girl's strengths	Parental perception of girl's self-efficacy	Parental perception of freedom of movement	Parent daughter communication	Parental gender attitudes	Parental schooling attitudes	Parental marriage attitudes
Treatment	-.040**	.010	.031	-.009	.010	.043	.022
	(.018)	(.029)	(.028)	(.029)	(.026)	(.037)	(.031)
Obs.	2434	2430	2434	2434	2434	2434	2434
R^2	.021	.03	.017	.027	.045	.127	.037
Q-statistic	0.122	0.820	0.604	0.820	0.820	0.592	0.755
Mean Control Group	0.000	-0.002	0.000	0.000	0.000	0.001	-0.004
Panel B: Demonstration Tasks and Enumerator Assessment							
	Delay discounting	Completed mirror drawings	Mirror drawings (seconds)	Scavenger hunt index	Enumerator assessment index		
Treatment	.003	.072	2.720	-.065	.100**		
	(.032)	(.082)	(4.559)	(.055)	(.047)		
Obs.	2380	2387	2317	2380	2380		
R^2	.021	.027	.022	.064	.052		
Q-statistic	0.914	0.707	0.795	0.592	0.122		
Mean Control Group	0.331	3.269	119.5	0.000	0.000		

Notes: Table contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported), stratification fixed effects, age fixed effects, baseline value of the outcome, a vector of dummies for the most important type of employment in the household at baseline, and controls for variables that appear imbalanced in the balance tables.

For all included indices, we calculate the normalized difference between treatment and control for each component question, and average over all components, ensuring that the interpretation is consistent (i.e. higher values of empowerment index components all correspond to higher levels of empowerment). Detailed definitions of all referenced indices can be found in the analysis plan. Four observations are missing for parental perception of girl's self-efficacy, as the parent answered "Don't know" to all questions. Seven observations are missing from the analysis for both the future discounting and scavenger hunt measures, corresponding to the seven cases in which the respondent responded only to the first section of the child survey. 70 observations are missing for time spent on mirror drawing, corresponding to the 70 respondents who did not attempt it.

Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level. Q-statistics are False Discovery Rate corrected q-values based on Benjamini and Hochberg (1995). These are computed by pooling all specifications included in Tables 3 through 4.

A Appendix

A.1 Data Collection and Validation

A.1.1 School Selection Criteria

In order to select the schools included in the evaluation, a team of enumerators visited all schools in Ajmer that included girls enrolled in the relevant grades (six through eight) and collected information about school facilities, staffing, and enrollment. This information was also linked to administrative records about school facilities and enrollment provided by state educational authorities.

The evaluation team and RtR then jointly identified a set of minimal criteria that would determine whether or not a school was eligible for inclusion in the evaluation. These included the requirements that the schools enrolled girls in grades six through eight, did not have any other non-governmental organizations providing life skills curricula to students, and had a classroom in acceptable condition in which a life skills class could take place. The evaluation team then identified the narrowest possible range of enrollments that would yield a sample of schools enrolling 2500 girls in total; the objective was to have a relatively homogeneous sample of schools in terms of size. This yielded the requirement that the school enrolled between 16 and 32 girls in grade five.

Using information collected in the school survey, the research team created a normalized school quality index, composed of measures of teacher experience, teachers' educational attainment, and classroom and school infrastructure quality. Schools above the median of the index were included in the "high quality" stratum, with the remaining in the "low quality" stratum. School assignment to treatment was conducted separately for the two strata.

Following the initiation of baseline data collection, it was discovered that three of the schools selected to be in the sample did not enroll girls past grade five; for the upper-level grades, these were single-sex schools including only boys. During the sample selection process, these schools were incorrectly designated as including higher-grade girls as well. These three schools (two treatment and one control school) were dropped, and an additional three schools were selected to replace them, constituting an additional third strata. The replacement process for these schools entailed identifying 12 schools that met the eligibility criteria if the enrollment window was slightly lowered to 15. Three schools were randomly chosen to join the sample among the 12, and of these, two were randomly assigned to the treatment group.

A.1.2 Piloting of non-cognitive measures

Prior to conducting the baseline survey, the evaluation team engaged in an extensive period of piloting and questionnaire development. This work focused particularly, though not exclusively, on the development of instruments tailored to measure the life skills that Room to Read seeks to enhance through its programming.

The questionnaire development work had three phases: first, a pilot was conducted with 100 households in Phalodi, a district in which Room to Read was already operating, employing a life skills questionnaire developed under the guidance of Professor Stephanie Jones at the Harvard School of Education. The objective was to explore the relevance and comprehension of life skills questions for girls who had previously been exposed to Room to Read programming.

Second, an intensive period of questionnaire development and continuous piloting was undertaken in Ajmer district under the supervision of Eva Roca, a consultant with extensive experience in measurement of life skills and empowerment among adolescent girls. The objective was to experiment with various questions as well as objective measures of life skills working directly with girls in the sample districts. The key points explored were comprehension, variability, and internal consistency: that is, questions included in the survey should be easily understood by respondents; should generate varied responses; and should be designed such that responses to closely related questions are internally consistent.

Third, the complete child and household questionnaires were piloted with 30 respondents in Ajmer district. In this final phase, the objective was to identify any challenges that had not been addressed and ensure that the questionnaire was of manageable length and flowed smoothly for respondents. Following the completion of this pilot period, the questionnaire was scripted for electronic data collection using tablets.

A.1.3 Consent Process

Prior to the start of each survey round, a training process focused on developing enumerator skills was undertaken. Key points included strategies to locate respondents within the community; the importance of informed consent and how to correctly structure the consent process; establishing a rapport with respondents as well as with other stakeholders in the community; maintaining fidelity to the questionnaire; full comprehension of the questionnaires themselves; and correct use of the tablets. (All data collection was implemented using ODK software on handheld tablets.)

The evaluation team enrolled individual girls and households into the evaluation sample at baseline using a detailed process of consent administered for both household and child surveys. Enumerators were trained to explain the purpose of the study, the benefits of participating, the study's duration, and the frequency of the proposed interviews. Interviews were conducted only after respondents consented to participate and all questions regarding the study were addressed. Separate consents, both verbal and written, were obtained from the members who participated in the household survey. For the child survey, parental consent from the primary caregiver was first obtained before interviewing the child. In case the primary caregiver of the child was not available, consent was obtained from the most senior member of the household. Informed verbal consent was obtained from all children participating in the study. The consent process was then repeated for each subsequent survey.

A.1.4 Quantitative Data Collection

The survey teams deployed to the field using household rosters that were constructed based on the lists of enrolled girls obtained from sampled schools. The information provided by the schools typically included the name of the head of household and the child herself, as well as some identifying information about the location of the household. In general, however, it was also necessary for enumerators and field supervisors to work with community members to locate each household. Field supervisors and field managers would also make courtesy visits to community stakeholders (including the sarpanch or village leader, school headmaster, and teachers) when they first arrived in the community in order to introduce the team and outline the survey's objectives.

Each survey included a minimum of two visits to the household, as the survey administered to the girl herself was divided into two parts. This choice was made in order to maximize attention and avoid fatigue; in addition, the first visit was used to introduce a scavenger hunt task to the girl, so that she could engage in the scavenger hunt prior to the second visit. However, many households required more than two visits total to complete the data collection process, particularly as the household survey included multiple modules to be answered by different individuals. (For example, introductory modules including household rosters were administered to the head of household or the individual most knowledgeable about the household. Modules collecting information about perception of the child's life skills were administered to the individual primarily responsible for the child's care.)

A.1.5 Data Validation

To minimize surveyor error, all survey skip patterns and valid response ranges were pre-programmed onto tablets prior to the start of survey activities. In addition, the survey was designed so that surveyors were required to verify that respondent identifiers and names matched our master file records prior to commencing each round of data collection. To assess data quality in real time, the project research associate was tasked with downloading collected data at the end of each day and running a series of data quality checks in Stata to identify any survey questions generating unexpected response patterns or high rates of missing values. In addition, these data checks identified whether any surveyors were recording missing or "Don't Know" responses with high frequency. When such cases were identified, the field staff worked with the responsible enumerator to correct surveying practices to minimize non-response.

A.1.6 Tracking Surveys

In addition to the baseline and endline surveys, the sample girls were revisited for tracking surveys in December 2016 and December 2017. The tracking surveys included only short modules administered to the girls themselves. Consent was obtained separately for each subsequent survey.

A.1.7 Qualitative Data Collection

Qualitative data collection was conducted at baseline, midline, and endline. This involved research activities in six schools served by Room to Read and in the associated communities. Three schools were selected in which school quality was above average, and two schools were selected in which it was below average; a sixth school was selected because it was an all girls' school. The objective of the qualitative data collection is to understand better the channels through which the GEP changes attitudes, perceptions, and decision-making processes for girls, teachers, parents and other stakeholders. Qualitative data was collected by staff members trained in in-depth interview techniques, and collection included the transcription, translation, and coding of the resulting data. A full overview of the qualitative findings is provided in DeJaeghere and Arur (2019).

A.2 Balance

We pre-specified tests for balance from randomization across a wide variety of variables, and accordingly given 61 individual hypothesis tests, there is ample scope for type 1 errors. The results are reported in Tables A2 through A4. In the column labeled "Difference", we report the coefficients from a simple regression in which the characteristics of interest are regressed on a treatment indicator and strata dummies, clustering standard errors at the school. False discovery rate adjusted q-values using the same specification are reported in the final column (Benjamini and Hochberg, 1995).

Under a true null of no difference between treatment and control, we expect six false rejections at 10 percent, and observe eight such rejections; similarly, we expect three rejections of the null at the five percent level, and observe exactly three. The smallest false discovery rate is 23 percent, and seven of the eight differences that are significant at the ten percent level have false discovery rates above 50 percent. For the 19 household characteristics reported, we also estimate a seemingly unrelated regression (SUR) specification that tests the joint null hypothesis that the treatment coefficient is equal to zero across all characteristics, and fail to reject the null (p = .138). That same SUR test applied to all of the child characteristics rejects the null of no joint difference (p = .003). This rejection is driven by the child work related variables; however, only the indicator for whether child works has a p-value below 0.05 and a false discovery rate below 25 percent (it is 23 percent). Given the high false discovery rates, we do not think baseline imbalance poses a significant risk of bias, but our preferred specification controls for all of the variables with individual differences significant at 10 percent. In practice, these controls add precision, and, as noted, we never reject the null hypothesis of no change in treatment effects with the added controls.

A.3 Selection into Administrative data: Dropout and Grades

In addition to results estimated using survey data, we also present results estimated using administrative data reported on dropout and grades in Tables 1 and 2. In Appendix Tables A17, A18, and A19, we present additional robustness checks analyzing potential bias induced by selection into these administrative data.

In the analysis of school-reported data on dropout, girls are missing if the schools report no data on the girls' whereabouts: i.e., if the girl is no longer enrolled and the school cannot identify whether she has transferred to another school (a process that requires a certificate from the originating school) or definitively dropped out. Attrition from these data is relatively infrequent in grades six through eight, but increases to 11 percent in grade nine as students are more likely to change schools prior to entering high school.

To examine the potential influence of attrition, we estimate lower and upper Lee (2009) treatment effect bounds based on a trimming procedure that assumes treatment status affects selection monotonically. Columns (3) through (5) of Appendix Table A17 report the robustness checks for dropout in grades six, seven, and eight. We do not present corresponding estimates for grade nine given that our associated benchmark specification controls directly for the number of days each school was open in the past week, which is not feasible to incorporate when estimating Lee (2009) bounds given measured rates of cell-specific attrition. As shown, across specifications, the confidence intervals that we estimate when accounting for attrition are nearly identical to those estimated from regressions of each outcome on an indicator for treatment.

For the analysis of school-reported data on test scores, scores are missing for girls who have dropped out of school as well as for other children whose missing exam scores have no singular explanation. (This is an advantage of the in-home ASER tests also conducted; missing data for the ASER scores is minimal, and restricted to those girls who were not observed in the endline survey.)

In order to analyze the potential impact of missing test scores on our findings, we estimate lower and upper Lee (2009) treatment effect bounds as above in Columns (4) through (6) of Panel B of Appendix Table A18. While varying assumptions about the selection into test scores move our estimates of treatment effects, the resulting confidence interval estimates remain consistent with our hypothesis that there is no effect of treatment on in-school test scores.

In Panel A of Appendix Table A19, we examine the relationship between indicators for available test score data and treatment status. We find that treated students are less likely to have missing administrative test score data. In Panels B and C, we assess the degree to which this selection into test data would be expected to bias estimated treatment effects for administrative test score outcomes by interacting treatment status with the baseline (grade 5) administrative test score in Panel B and with the baseline attendance rate in Panel C. Interaction terms are statistically insignificant at conventional levels in all but one specification and are inconsistent in sign, suggesting that differences in missing rates as a function of treatment status are not likely to bias estimates in practice.

A.4 Bounding

Given evidence from Section 3.1.1 that girls in the control group were more likely to attrit from the endline girl survey (though not the endline household survey), we assess the potential importance of missing data in Appendix Tables A17 through A22 for the outcomes included in Tables 1 through 4 (and Appendix Tables A13 through A15). Specifically, we estimate lower and upper Lee (2009) treatment effect bounds based on a trimming procedure that assumes treatment status affects selection monotonically. We present estimates of these bounds as well as a bounds-based 95% confidence interval for the treatment effect that captures both uncertainty due to sample attrition and sampling variation. For comparison, we also present the 95% confidence interval from a specification that regresses the outcome of interest on treatment. While varying assumptions about selection into the endline survey do generate some variation in our estimates of treatment effects, selection-adjusted estimates are generally not statistically distinguishable from the original estimates, and the statistical significance of estimates (relative to a null hypothesis of zero effect) is largely unchanged for included outcomes.

A.5 Heterogeneous Effects

We conduct exploratory analyses related to heterogeneity by predicted dropout propensity. To do so, we first predict control group dropout as measured in the endline survey using four baseline measures: child age (measured in the baseline survey), attendance rate (measured in the baseline survey), time spent socializing (measured in the baseline survey) and fifth grade GPA (measured using administrative records). Since predicted dropout propensity for girls assigned to treatment is determined by the dropout propensity calculated within the subset of control group girls with matching covariate values, a binning procedure is necessary to maximize common support. The precise binning procedure employed is as follows.

For child age, we construct an indicator for whether the girl was older than 11, the median age for girls in our sample, and we include a separate indicator for missing baseline age (32.7% of the sample is above the median age, 65.6% is below the median age, and baseline age is missing for 1.6% of the sample). For child attendance, we construct three bins to categorize attendance among girls who were enrolled in school at the time of the baseline survey: (1) no attendance in the past week, (2) attendance in the past week but with one or more absence, and (3) full attendance during the past week. These bins include 9.2%, 23.3%, and 50.0% of girls, respectively. We also include a separate indicator variable for girls for whom baseline school attendance data are not available (this includes girls who had already dropped out by the time that the baseline survey was conducted and accounts for 17.6% of the study sample). For time spent socializing as reported in the baseline survey, we assign girls to one of three bins: baseline data missing (4.9% of sample), zero reported time spent socializing at baseline (14.4% of sample), and greater than zero reported time spent socializing at baseline (80.7% of sample). Lastly, for baseline (fifth grade) GPA, we assign girls to one of three bins: baseline data missing (4.2% of sample), baseline GPA at or below median value (3) conditional on non-missingness (54.3% of sample), and baseline GPA above median value (3) conditional on non-missingness (41.5% of sample).

After constructing these binned measures, within the sample of girls assigned to the control group, we regress an indicator for endline dropout on the full set of interactions between the indicator variables described in the prior paragraph. We use the regression results to construct predicted dropout propensity for girls in the control group; we are able to construct a predicted dropout measure for 1,222 out of 1,245 girls in the control group (predicted dropout is missing for girls with missing endline dropout data and for singletons). We then assign predicted dropout values to girls in the treatment group based on the value assigned to girls in the control group with the same binned measures of baseline child age, attendance rate, time spent socializing, and fifth grade GPA (we are able to construct a predicted dropout measure for 1,178 out of 1,214 girls in the treatment group).

We proceed to conduct heterogeneity analyses by regressing outcome variables of interest on an indicator for treatment, an indicator for whether a girl is at or above the median predicted dropout propensity (0.143), and the interaction of the two measures (as well as all of the additional controls included in our benchmark model). The results are reported in Tables A25 through A30 and are discussed in Section 4.

Appendix Figures

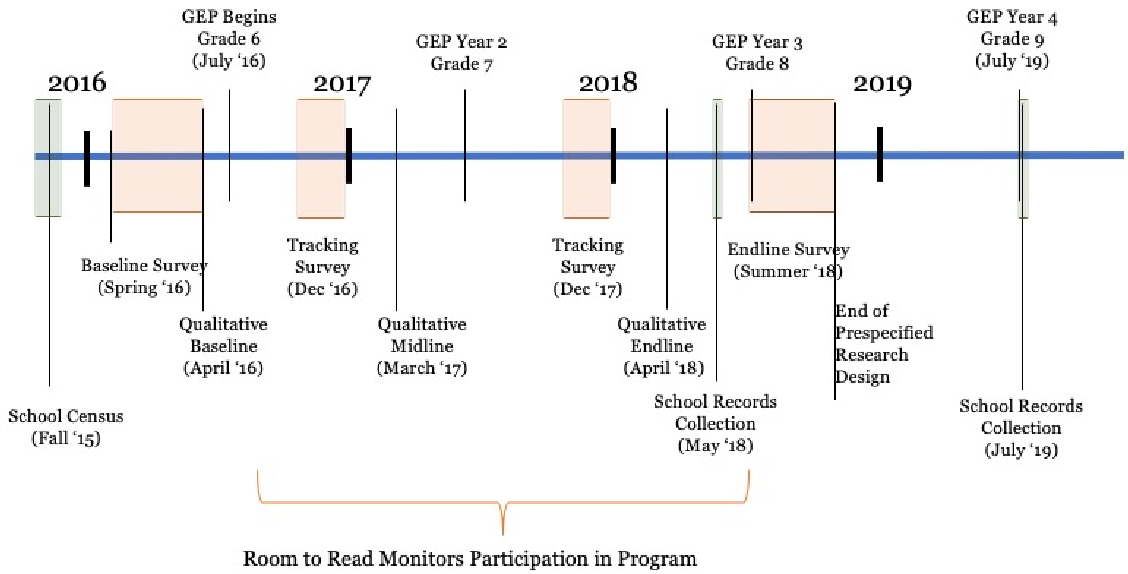


Figure A1: Intervention and Data Collection Timeline

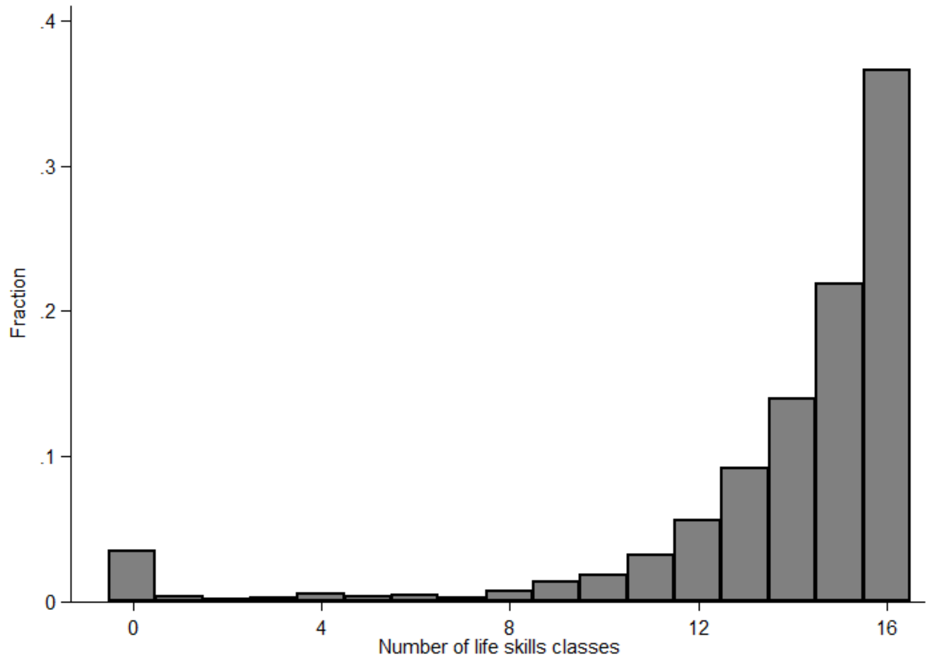


Figure A2: Number of Life Skills Classes Attended by Treatment Group Subjects in Grade 6 (out of 16 Classes)

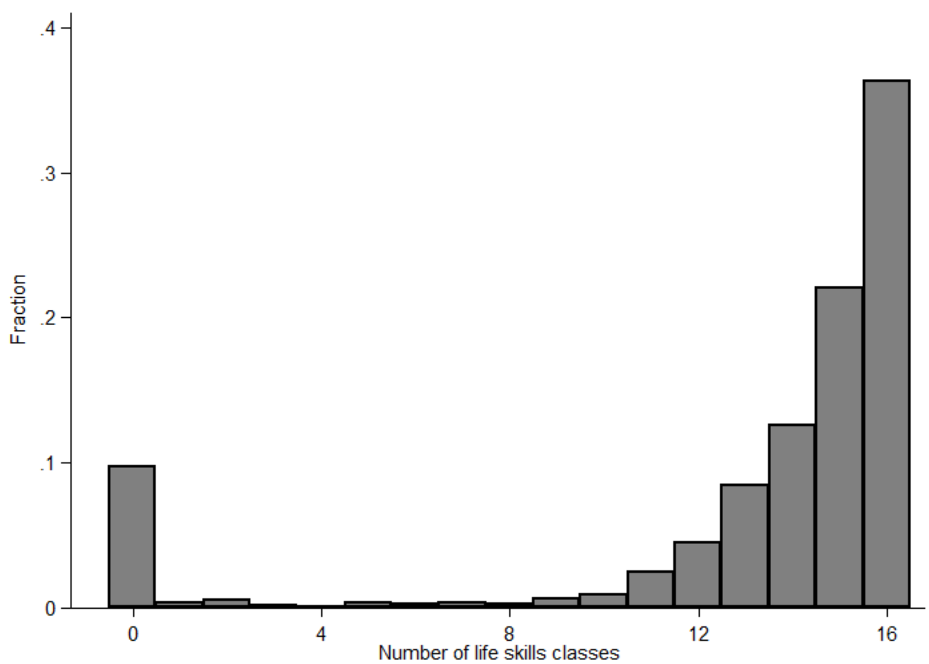


Figure A3: Number of Life Skills Classes Attended by Treatment Group Subjects in Grade 7 (out of 16 Classes)

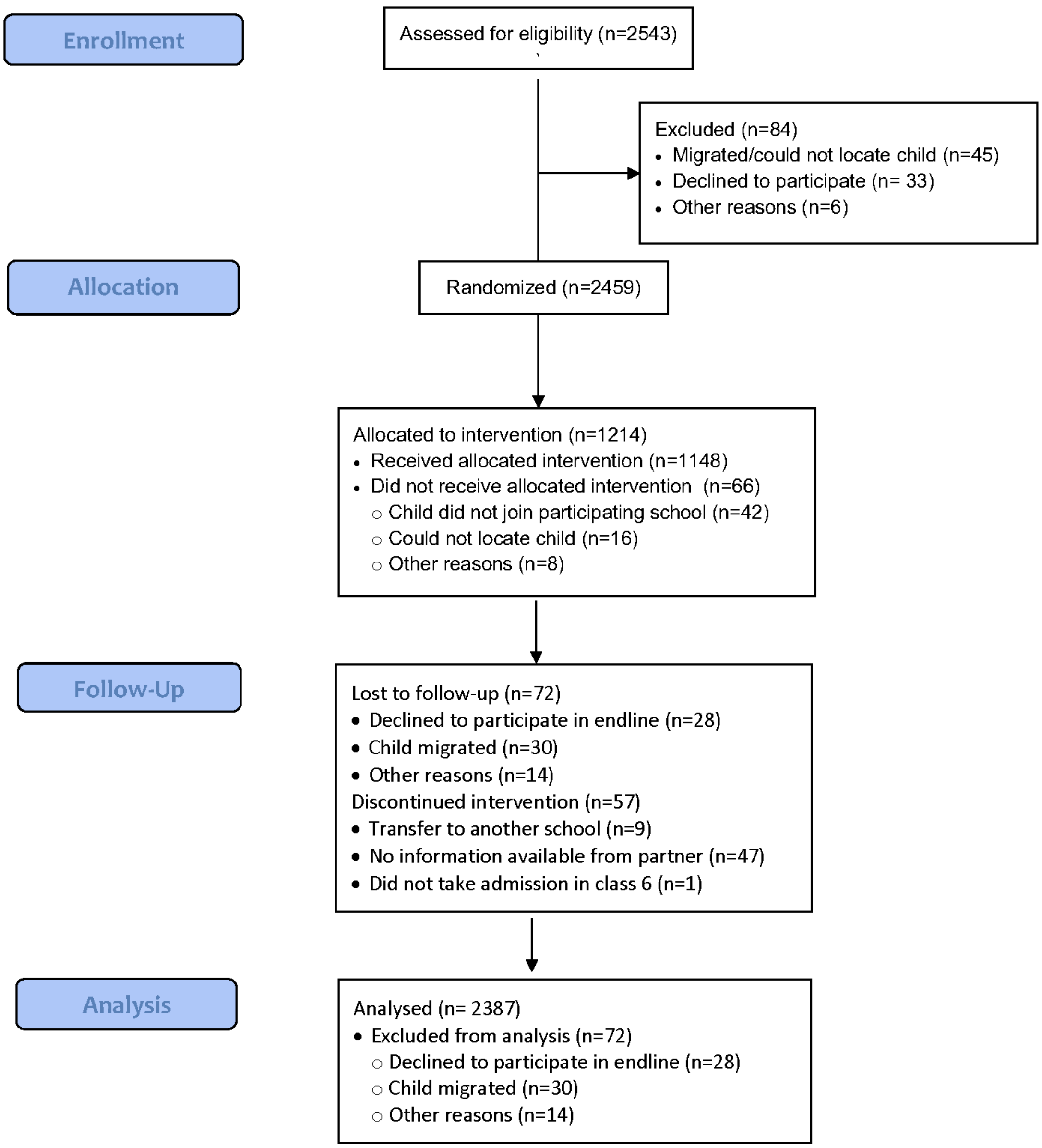


Figure A4: Flow Chart of Participants

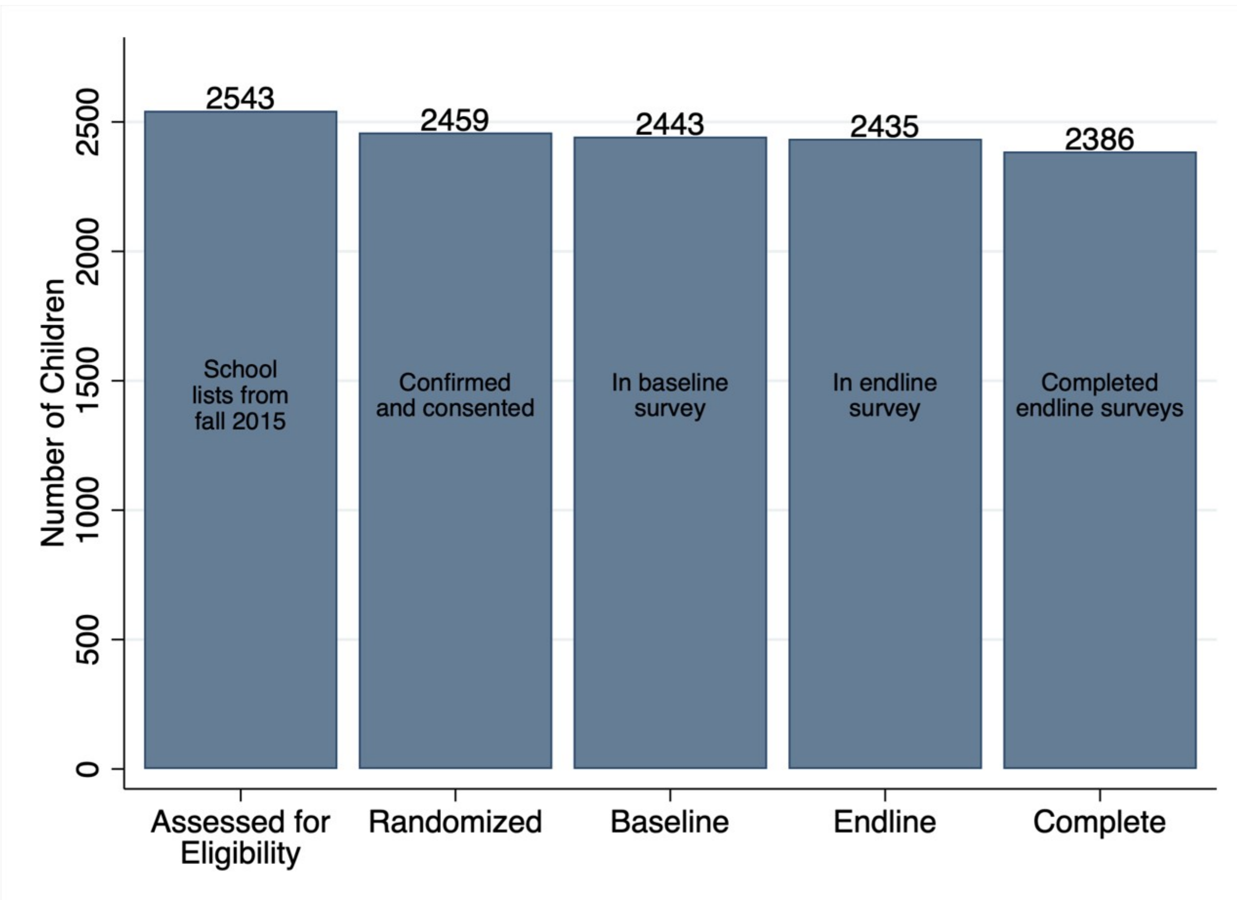


Figure A5: Attrition by Data Collection Round and Survey Type

Notes: Completed endline surveys refers to the completion of both the child and household surveys.

Appendix Tables

Table A1: Summary Statistics for Sampled Households

	(1) Sample Mean	(2) Rajasthan Mean	(3) India Mean
Number of household members	6.838	5.091	4.692
Number of boys in household (under 18)	1.379	1.005	0.834
Number of girls in household (under 18)	2.438	0.898	0.775
Enrollment: girls 10-11	97.5	92.8	95.5
Enrollment: boys 10-11	97.7	95.9	95.6
Enrollment: girls 12-14	92.0	84.0	90.2
Enrollment: boys 12-14	92.4	92.8	91.4
Marriage rate: girls 13-14	0.1095	0.0166	0.0162
Muslim	0.214	0.080	0.125
Other Backward Class	0.674	0.459	0.442
Scheduled Caste/Scheduled Tribe	0.250	0.337	0.312
Land owned (bighas)	6.283	23.924	10.664

Notes: Column (1) presents mean values averaged over all households in the study sample. Households with multiple study subjects occur as multiple observations. 16 study subjects completed a baseline child survey but no baseline household survey and thus are not represented in these summary statistics. Columns (2) and (3) present household-level mean values for respondents to the 2015-2016 Indian Demographic and Health Survey. Enrollment measures take on values from 0 to 100. Marriage rate takes on values from 0 to 1, and Muslim, Other Backward Class, and Scheduled Caste/Scheduled Tribe are all indicator variables.

Table A2: Balance Tests for Household Variables

	Control		Treatment		Difference		
	Mean	Std. dev.	Mean	Std. dev.	Coef.	Std. error	Q-stat
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Number of sampled girls in household	1.057	0.241	1.067	0.251	0.010	(0.015)	0.834
Number of household members	6.893	2.763	6.781	2.860	-0.106	(0.140)	0.829
Number of boys in household (under 18)	1.358	1.047	1.402	1.003	0.046	(0.050)	0.745
Number of girls in household (under 18)	2.456	1.340	2.419	1.380	-0.037	(0.063)	0.834
Other backward castes household	0.631	0.483	0.720	0.449	0.088**	(0.038)	0.579
Primary source of employment = wage / salary earning	0.536	0.499	0.527	0.499	-0.012	(0.031)	0.910
Primary source of employment = Self-employment (ag.)	0.210	0.407	0.220	0.415	0.011	(0.032)	0.930
Primary source of employment = Self-employment (non-ag.)	0.072	0.258	0.089	0.285	0.018	(0.015)	0.655
Primary source of employment = Casual labor (ag).	0.015	0.120	0.012	0.108	-0.003	(0.005)	0.834
Primary source of employment = Casual labor (non-ag.)	0.162	0.369	0.151	0.358	-0.010	(0.018)	0.834
Non-food expenditures in Rupees (last 30 days)	1.0e+04	5.4e+04	9453.617	1.8e+04	-879.716	(1678.969)	0.834
Food expenditures in Rupees (last 30 days)	2.2e+04	2.9e+05	1.0e+04	1.1e+04	-1.2e+04	(7903.035)	0.655
Durables expenditures in Rupees (last year)	1.1e+05	5.5e+05	1.4e+05	1.3e+06	3.3e+04	(4.0e+04)	0.784
Land owned (bighas)	5.653	11.828	6.901	19.153	1.246	(1.104)	0.655
Land cultivated (bighas)	2.069	8.285	2.540	15.738	0.455	(0.743)	0.834
Household holds NREGA card	0.712	0.453	0.802	0.399	0.090	(0.063)	0.655
Economic shock	0.593	0.491	0.620	0.486	0.027	(0.024)	0.655
Crime shock	0.126	0.332	0.138	0.345	0.011	(0.017)	0.834
Death / illness shock	0.396	0.489	0.417	0.493	0.023	(0.021)	0.655

Notes: Households with multiple study subjects occur as multiple observations. 16 study subjects completed a baseline child survey but no baseline household survey and thus are not represented in these baseline summary statistics. Primary household source of employment measures are indicator variables. 182 households report that they do not own land but access collectively owned land. 315 households cannot estimate the amount of land owned. 206 households do not report land cultivated because it is cultivated collectively, and 588 households cannot estimate the amount of land cultivated. Economic shock is an indicator for loss of employment or lowered income of any household member or bankruptcy of family business in last 12 months. Crime shock is an indicator for having experienced robbery, assault, physical aggression, a land dispute, or a family dispute in last 12 months. Death/illness shock is an indicator for death, serious illness, or accident of a household member in last 12 months. The columns under the header “Difference” report the result of the regression of the row variable on an indicator for treatment and stratification fixed effects. Standard errors are clustered by school. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level. Q-statistics are False Discovery Rate corrected q-values based on Benjamini and Hochberg (1995). These are computed by pooling all specifications included in Tables A2 through A4.

Table A3: Balance Tests for Child Variables

	Control		Treatment		Difference		
	Mean	Std. dev.	Mean	Std. dev.	Coef.	Std. error	Q-stat
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Stratification (Baseline school characteristics):							
Below median school quality	0.515	0.500	0.502	0.500	0.000	(—)	
Above median school quality	0.485	0.500	0.498	0.500	0.000	(—)	
Subject characteristics:							
Age	10.960	1.411	11.019	1.440	0.058	(0.081)	0.829
Maternal education (1=completed primary or above)	0.187	0.390	0.156	0.363	-0.030	(0.023)	0.655
Girl's marital status (1=married)	0.141	0.348	0.194	0.396	0.053*	(0.028)	0.579
Child has dropped out of school	0.024	0.154	0.026	0.159	0.001	(0.010)	0.967
Child is in grade five	0.975	0.155	0.974	0.158	-0.000	(0.010)	0.981
Any attendance in last week (conditional on not dropping out)	0.870	0.336	0.908	0.290	0.040	(0.025)	0.655
Attendance rate in last week (conditional on attendance)	0.768	0.353	0.808	0.319	0.044	(0.028)	0.655
Delay discounting	0.171	0.376	0.186	0.390	0.013	(0.026)	0.834
Completed mirror drawings	2.489	1.228	2.331	1.242	-0.149	(0.117)	0.655
Mirror drawings (seconds)	69.452	65.875	67.318	74.578	-1.833	(6.043)	0.937
Scavenger hunt index	-0.000	0.965	-0.048	0.973	-0.045	(0.076)	0.834
Socio-emotional index	-0.000	0.480	0.035	0.447	0.034	(0.030)	0.655
Freedom of movement index	0.000	0.577	-0.002	0.628	-0.004	(0.046)	0.972
Empowerment index	-0.000	0.406	-0.008	0.426	-0.007	(0.029)	0.955
Self-esteem index	0.000	0.503	0.027	0.472	0.027	(0.030)	0.746
Future planning index	0.020	0.610	0.084	0.589	0.062*	(0.033)	0.579
Marital expectations index	-0.401	1.348	-0.595	1.514	-0.192*	(0.104)	0.579
Education / employment aspirations index	-0.002	0.788	-0.033	0.801	-0.029	(0.053)	0.834
Gender norms index	-0.000	0.498	-0.005	0.521	-0.005	(0.034)	0.967
Cantril's ladder	8.029	2.395	7.877	2.440	-0.154	(0.152)	0.685
Enumerator assessment index	-0.000	0.859	-0.056	0.922	-0.055	(0.052)	0.655

Notes: One household did not complete a roster and thus is not represented in these baseline summary statistics. Maternal education is measured at endline and is missing if child is not present in endline survey. Any attendance in last week is missing if child has dropped out or her school was not open in past week. Attendance rate in last week is missing if child has dropped out, her school was not open in past week, or she did not attend school in past week. Details regarding the remaining variables and indices can be found in the analysis plan posted on-line. The columns under the header "Difference" report the result of the regression of the row variable on an indicator for treatment and stratification fixed effects. Standard errors are clustered by school. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level. Q-statistics are False Discovery Rate corrected q-values based on Benjamini and Hochberg (1995). These are computed by pooling all specifications included in Tables A2 through A4.

Table A4: Balance Tests for Child Variables, cont.

	Control		Treatment		Difference		
	Mean	Std. dev.	Mean	Std. dev.	Coef.	Std. error	Q-stat
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Parental perception of girl's strengths	-0.000	0.374	0.007	0.357	0.008	(0.033)	0.955
Parental perception of girl's self-efficacy	0.000	0.611	0.048	0.636	0.047	(0.038)	0.655
Parental perception freedom of movement	-0.000	0.532	-0.043	0.643	-0.044	(0.035)	0.655
Parent-daughter communication	0.001	0.415	0.002	0.429	0.002	(0.028)	0.972
Parental gender attitudes	-0.000	0.424	0.003	0.439	0.003	(0.025)	0.967
Parental schooling attitudes	0.003	0.682	0.012	0.709	0.010	(0.050)	0.955
Parental marriage attitudes	-0.005	0.503	-0.005	0.530	0.000	(0.033)	0.999
Child works	0.884	0.320	0.945	0.227	0.060***	(0.020)	0.234
Child works for pay	0.829	0.376	0.859	0.349	0.029	(0.026)	0.655
Child works outside of family activity	0.674	0.469	0.721	0.449	0.046	(0.035)	0.655
Child labor	0.855	0.352	0.893	0.310	0.035	(0.023)	0.655
Hazardous child labor	0.620	0.486	0.665	0.472	0.042	(0.037)	0.655
Other worst forms of child labor	0.219	0.414	0.231	0.422	0.010	(0.026)	0.910
Hours economically active in a day	0.945	1.636	1.164	1.741	0.217*	(0.125)	0.632
Hours in unpaid household services in a day	1.415	1.454	1.480	1.441	0.065	(0.068)	0.721
Total hours active	2.360	2.243	2.644	2.367	0.282*	(0.152)	0.579
Hours active outside house	0.719	1.387	0.935	1.513	0.215**	(0.098)	0.579
Hours studying at home	0.713	0.966	0.694	0.944	-0.013	(0.061)	0.955
Total hours spent on school	6.014	2.845	6.199	2.799	0.192	(0.270)	0.829

Notes: One household did not complete a roster and thus is not represented in these baseline summary statistics. Details regarding the included variables and indices can be found in the analysis plan posted on-line. The columns under the header "Difference" report the result of the regression of the row variable on an indicator for treatment and stratification fixed effects. Standard errors are clustered by school. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level. Q-statistics are False Discovery Rate corrected q-values based on Benjamini and Hochberg (1995). These are computed by pooling all specifications included in Tables A2 through A4.

Table A5: School Dropout (Stratification Controls)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Survey data		Administrative data				
	Whether child has dropped out	Whether child progressed to 7th grade	Dropout Grade 6	Dropout Grade 7	Dropout Grade 8	Dropout Grade 9	Dropout Index
Treatment	-.033*	.037*	-.007	-.025	-.043**	-.051*	-.035**
	(.020)	(.020)	(.017)	(.020)	(.021)	(.026)	(.017)
Obs.	2433	2387	2374	2319	2455	2228	2458
R^2	.003	.004	.007	.011	.005	.399	.112
Mean Control Group	0.132	0.865	0.075	0.142	0.192	0.290	0.169

Notes: Table contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported) and stratification fixed effects. Column (1) uses child and household endline survey data. Column (2) uses child endline survey only. Columns (3) through (6) rely on administrative data. In Columns (3) through (5), dropout is measured based on whether a child attended school at the conclusion of the referenced school year. In Column (6), dropout is measured based on whether a child attended school during the past week (conditional on the school being open). The Column (7) dropout index is constructed as the average of the outcome variables included in Columns (1), (3), (4), (5), and (6). Columns (6) through (7) include a set of fixed effects for the number of days that the school was open in the week before administrative data collection in grade nine. Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A6: School Dropout (Stratification + Age, Baseline Outcomes, and Economic Status Controls)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Survey data		Administrative data				
	Whether child has dropped out	Whether child progressed to 7th grade	Dropout Grade 6	Dropout Grade 7	Dropout Grade 8	Dropout Grade 9	Dropout Index
Treatment	-.035*	.038**	-.009	-.025	-.044**	-.053**	-.036**
	(.018)	(.018)	(.016)	(.019)	(.020)	(.024)	(.016)
Obs.	2433	2387	2374	2319	2455	2228	2458
R^2	.129	.128	.095	.101	.096	.433	.205
Mean Control Group	0.132	0.865	0.075	0.142	0.192	0.290	0.169

Notes: Table contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported), stratification fixed effects, age fixed effects, baseline value of the outcome, and a vector of dummies for the most important type of employment in the household at baseline. Column (1) uses child and household endline survey data. Column (2) uses child endline survey only. Columns (3) through (6) rely on administrative data. In Columns (3) through (5), dropout is measured based on whether a child attended school at the conclusion of the referenced school year. In Column (6), dropout is measured based on whether a child attended school during the past week (conditional on the school being open). The Column (7) dropout index is constructed as the average of the outcome variables included in Columns (1), (3), (4), (5), and (6). Columns (6) through (7) include a set of fixed effects for the number of days that the school was open in the week before administrative data collection in grade nine. Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A7: Attendance, Time Allocation, and Test Scores (Stratification Controls)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Attendance (Past Week) and Time Allocation (Typical Day in Past Week)						
	Attendance rate	Attendance dummy	Hours studying at home	Hours spent at school		
Treatment	.006	.003	-.062	.183		
	(.010)	(.005)	(.077)	(.189)		
Obs.	2089	2089	2386	2386		
R^2	.002	.002	.004	.003		
Mean Control Group	0.918	0.982	1.541	7.166		
Panel B: Test Scores						
	ASER Mathematics	ASER Hindi	ASER English	GPA Grade 6	GPA Grade 7	GPA Grade 8
Treatment	-.021	.032	-.074	-.119	-.121	-.033
	(.077)	(.093)	(.090)	(.074)	(.092)	(.083)
Obs.	2380	2380	2380	2178	1976	1912
R^2	.004	.004	.002	.013	.006	.004
Mean Control Group	2.353	3.025	2.369	2.259	2.404	2.890

Notes: Table contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported) and stratification fixed effects. Columns (1) and (2) of Panel A use child endline survey data and are conditional on school being open and child not having dropped out of school. Attendance rate in Column (1) is the fraction of school days attended in the week prior to being surveyed and the Attendance dummy in Column (2) is an indicator for having attended any days in the past week. 298 observations are missing for these measures because of temporary school closures. Time use outcomes in Columns (3) and (4) of Panel A are defined based on time use patterns recorded for "a typical day in the past week." Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A8: Attendance, Time Allocation and Test Scores (Stratification + Age, Baseline Outcomes, and Economic Status Controls)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Attendance (Past Week) and Time Allocation (Typical Day in Past Week)						
	Attendance rate	Attendance dummy	Hours studying at home	Hours spent at school		
Treatment	.004	.003	-.067	.132		
	(.009)	(.006)	(.076)	(.187)		
Obs.	2089	2089	2386	2386		
R^2	.022	.014	.044	.092		
Mean Control Group	0.918	0.982	1.541	7.166		
Panel B: Test Scores						
	ASER Mathematics	ASER Hindi	ASER English	GPA Grade 6	GPA Grade 7	GPA Grade 8
Treatment	-.032	.008	-.089	-.159**	-.145	-.028
	(.070)	(.089)	(.084)	(.073)	(.095)	(.086)
Obs.	2380	2380	2380	2178	1976	1912
R^2	.073	.083	.091	.33	.215	.206
Mean Control Group	2.353	3.025	2.369	2.259	2.404	2.890

Notes: Table contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported), stratification fixed effects, age fixed effects, baseline value of the outcome, and a vector of dummies for the most important type of employment in the household at baseline. Columns (1) and (2) of Panel A use child endline survey data and are conditional on school being open and child not having dropped out of school. Attendance rate in Column (1) is the fraction of school days attended in the week prior to being surveyed and the Attendance dummy in Column (2) is an indicator for having attended any days in the past week. 298 observations are missing for these measures because of temporary school closures. Time use outcomes in Columns (3) and (4) of Panel A are defined based on time use patterns recorded for "a typical day in the past week." In Panel B, baseline cognitive test values were not collected, and accordingly Columns (1) through (3) instead include controls for baseline school dropout status, attendance, grade progression, time spent studying, hours spent on school, and grades as reported in grade five. Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A9: Life Skills: Child Survey Measures (Stratification Controls)

Panel A

	Future planning index (1)	Gender norms index (2)	Educ. / emp. aspirations index (3)	Marital expectations index (4)	Empowerment / agency index (5)	Self-esteem index (6)
Treatment	.070**	.088***	-.016	-.321***	.095***	.042*
	(.031)	(.034)	(.051)	(.115)	(.027)	(.024)
Obs.	2380	2380	2380	2380	2380	2380
R^2	.014	.019	.055	.057	.019	.012
Mean Control Group	-0.016	0.000	0.000	-0.606	-0.002	-0.001

Panel B

	Freedom of movement index (1)	Socio-emotional index (2)	Cantril's ladder (3)	Locus of control index (4)	Perceived stress index (5)	Rosenberg self-esteem index (6)
Treatment	.021	.067***	-.012	-.015	-.030	.019
	(.023)	(.024)	(.132)	(.046)	(.046)	(.030)
Obs.	2380	2380	2380	2380	2380	2380
R^2	.014	.014	.012	.006	.006	.013
Mean Control Group	0.000	0.000	4.513	0.000	0.000	0.000

Notes: Table contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported) and stratification fixed effects. For all included indices, we calculate the normalized difference between treatment and control for each component question, and average over all components, ensuring that the interpretation is consistent (i.e. higher values of empowerment index components all correspond to higher levels of empowerment). Detailed definitions of all referenced indices can be found in the analysis plan. Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A10: Life Skills: Child Survey (Stratification + Baseline Controls)

	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A			
	Future planning index	Gender norms index	Educ. / emp. aspirations index	Marital expectations index	Empowerment / agency index	Self-esteem index
Treatment	.065**	.088***	-.0002	-.198**	.097***	.037
	(.031)	(.034)	(.039)	(.081)	(.027)	(.023)
Obs.	2380	2380	2380	2380	2380	2380
R^2	.029	.029	.197	.304	.027	.055
Mean Control Group	-0.016	0.000	0.000	-0.606	-0.002	-0.001
			Panel B			
	Freedom of movement index	Socio-emotional index	Cantril's ladder	Locus of control index	Perceived stress index	Rosenberg self-esteem index
Treatment	.022	.063***	.0007	-.020	-.024	.024
	(.023)	(.023)	(.131)	(.046)	(.047)	(.030)
Obs.	2380	2380	2380	2380	2380	2380
R^2	.015	.032	.016	.026	.011	.036
Mean Control Group	0.000	0.000	4.513	0.000	0.000	0.000

Notes: Table contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported), stratification fixed effects, age fixed effects, the baseline value of the outcome, and a vector of dummies for the most important type of employment in the household at baseline. For Columns (4) through (6) of Panel B reporting measures added at endline, we control for lagged values of overall life skills indices. For all included indices, we calculate the normalized difference between treatment and control for each component question, and average over all components, ensuring that the interpretation is consistent (i.e. higher values of empowerment index components all correspond to higher levels of empowerment). Detailed definitions of all referenced indices can be found in the analysis plan. Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A11: Life Skills: Parental Reports, Demonstration Tasks, and Enumerator Assessment (Stratification Controls)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Parental Reports						
	Parental perception of girl's strengths	Parental perception of girl's self-efficacy	Parental perception of freedom of movement	Parent daughter communication	Parental gender attitudes	Parental schooling attitudes	Parental marriage attitudes
Treatment	-.042**	.004	.021	-.014	.0004	.032	.022
	(.018)	(.029)	(.029)	(.029)	(.026)	(.042)	(.031)
Obs.	2434	2430	2434	2434	2434	2434	2434
R^2	.004	.0001	.003	.002	.011	.003	.003
Mean Control Group	0.000	-0.002	0.000	0.000	0.000	0.001	-0.004
	Panel B: Demonstration Tasks and Enumerator Assessment						
	Delay discounting	Completed mirror drawings	Mirror drawings (seconds)	Scavenger hunt index	Enumerator assessment index		
Treatment	-.0004	.056	2.172	-.079	.073		
	(.032)	(.085)	(4.472)	(.057)	(.050)		
Obs.	2380	2387	2317	2380	2380		
R^2	.005	.003	.001	.004	.002		
Mean Control Group	0.331	3.269	119.5	0.000	0.000		

Table contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported) and stratification fixed effects. For all included indices, we first take the difference between each component survey response value and the mean within the control group and then divide by the control group standard deviation. We then average over all index components, ensuring that values for each component are constructed so that the index interpretation is consistent (i.e. higher values of empowerment index components all correspond to higher levels of empowerment). Detailed definitions of all referenced indices can be found in the analysis plan posted on-line. Four observations are missing for parental perception of girl's self-efficacy, as the parent answered "Don't know" to all the relevant questions. In total, seven observations are missing from the analysis for both the future discounting and scavenger hunt measures, corresponding to the seven cases in which the respondent elected only to respond to the first section of the child survey. 70 observations are missing for time spent on mirror drawing measure, corresponding to the 70 respondents who did not attempt any mirror drawings. Delay discounting is an indicator for whether the respondent would prefer 60 Rs. in one week over 30 Rs. now (respondents were informed that they would have a chance to receive a gift valued correspondingly). Completed mirror drawings takes on values from zero to four and Mirror drawings (seconds) measures the total number of seconds spent on mirror drawings, conditional on having attempted at least one mirror drawing. Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A12: Life Skills: Other (Stratification + Age, Baseline Outcomes, and Economic Status Controls)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Parental Reports						
	Parental perception of girl's strengths	Parental perception of girl's self-efficacy	Parental perception of freedom of movement	Parent daughter communication	Parental gender attitudes	Parental schooling attitudes	Parental marriage attitudes
Treatment	-.043**	-.0007	.025	-.009	.003	.027	.023
	(.018)	(.030)	(.028)	(.028)	(.026)	(.038)	(.031)
Obs.	2434	2430	2434	2434	2434	2434	2434
R^2	.019	.022	.015	.025	.037	.113	.033
Mean Control Group	0.000	-0.002	0.000	0.000	0.000	0.001	-0.004
	Panel B: Demonstration Tasks and Enumerator Assessment						
	Delay discounting	Completed mirror drawings	Mirror drawings (seconds)	Scavenger hunt index	Enumerator assessment index		
Treatment	-.003	.070	2.610	-.072	.092*		
	(.032)	(.085)	(4.535)	(.055)	(.047)		
Obs.	2380	2387	2317	2380	2380		
R^2	.016	.02	.014	.06	.05		
Mean Control Group	0.331	3.269	119.5	0.000	0.000		

Notes: Table contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported), stratification fixed effects, age fixed effects, baseline value of the outcome, and a vector of dummies for the most important type of employment in the household at baseline. For all included indices, we calculate the normalized difference between treatment and control for each component question, and average over all components, ensuring that the interpretation is consistent (i.e. higher values of empowerment index components all correspond to higher levels of empowerment). Detailed definitions of all referenced indices can be found in the analysis plan. Four observations are missing for parental perception of girl's self-efficacy, as the parent answered "Don't know" to all the relevant questions. In total, seven observations are missing from the analysis for both the future discounting and scavenger hunt measures, corresponding to the seven cases in which the respondent elected only to respond to the first section of the child survey. 70 observations are missing for time spent on mirror drawing measure, corresponding to the 70 respondents who did not attempt any mirror drawings. Delay discounting is an indicator for whether the respondent would prefer 60 Rs. in one week over 30 Rs. now (respondents were informed that they would have a chance to receive a gift valued correspondingly). Completed mirror drawings takes on values from zero to four and Mirror drawings (seconds) measures the total number of seconds spent on mirror drawings, conditional on having attempted at least one mirror drawing. Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A13: Understanding Channels: Child Empowerment/Agency Index Subcomponents

	(1) Sole decision: Attend school	(2) Sole decision: Continue schooling	(3) Talks to parents about marriage
	Panel A: Benchmark Specification		
Treatment	.072***	.107***	.087***
	(.024)	(.026)	(.023)
Obs.	2380	2380	1976
	Panel B: Stratification Controls Only		
Treatment	.068***	.099***	.080***
	(.024)	(.025)	(.023)
Obs.	2380	2380	1976
	Panel C: Stratification Controls + Age, Economic Status, and Baseline Values		
Treatment	.070***	.105***	.080***
	(.025)	(.025)	(.023)
Mean Control Group	.413	.375	.270
Obs.	2380	2380	1976

Notes: Panel A contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported), stratification fixed effects, age fixed effects, baseline value of the outcome, a vector of dummies for the most important type of employment in the household at baseline, and controls for variables that appear imbalanced in the balance tables. Panel B contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported) and stratification fixed effects. Panel C adds age fixed effects, baseline value of the outcome, and a vector of dummies for the most important type of employment in the household at baseline to the Panel B specification.
Sole decision-maker: Attend school and Sole decision-maker: Continue schooling are indicators for whether the girl responds "I do/I will" when asked who mostly makes decisions about whether or not the girl will go to school and whether or not the girl will continue in school past eighth grade, respectively. Talks to parents about marriage is an indicator for whether the girl responds that she can talk to her parents about her preferences regarding who she will marry. This measure is missing for girls who are already married. Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A14: Understanding Channels: Child Socio-emotional Support Index Subcomponents and Additional Measures

	(1) Prefers to be alone	(2) Meets friends outside	(3) Has place to meet friends	(4) Has place to stay if needed	(5) Total social time	(6) Reports time on mobile	(7) Time on mobile	(8) Time traveling to school
	Panel A: Benchmark Specification							
Treatment	-.055**	.035	.060**	.029	9.610***	.006**	.379*	8.734**
	(.024)	(.022)	(.029)	(.018)	(3.695)	(.003)	(.229)	(3.669)
Obs.	2434	2380	2380	2380	2387	2387	2387	2387
	Panel B: Stratification Controls Only							
Treatment	-.053**	.042*	.065**	.034*	9.956***	.006**	.346	9.066**
	(.023)	(.023)	(.028)	(.017)	(3.659)	(.003)	(.217)	(3.675)
Obs.	2434	2380	2380	2380	2387	2387	2387	2387
	Panel C: Stratification Controls + Age, Economic Status, and Baseline Values							
Treatment	-.057**	.039*	.063**	.029	10.126***	.006*	.372	9.188**
	(.023)	(.022)	(.028)	(.018)	(3.720)	(.003)	(.241)	(3.717)
Mean Control Group	.391	.635	.47	.759	39.077	.003	.092	37.146
Obs.	2434	2380	2380	2380	2387	2387	2387	2387

Panel A contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported), stratification fixed effects, age fixed effects, baseline value of the outcome, a vector of dummies for the most important type of employment in the household at baseline, and controls for variables that appear imbalanced in the balance tables. Panel B contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported) and stratification fixed effects. Panel C adds age fixed effects, baseline value of the outcome, and a vector of dummies for the most important type of employment in the household at baseline to the Panel B specification.

Prefers to be alone is an indicator for whether the caregiver responds that it is "Certainly true" that the girl would rather be alone than with other youth. Meets friends outside school is an indicator for whether the girl responds that she has met with her friends outside of school in the last week. Has place to meet friends is an indicator for whether the girl responds that she has a place to meet her female friends at least once a week. Has place to stay if needed is an indicator for whether the girl responds that she has someone in the community who would take her in for the night if her parents were out of town and she needed a place to stay. Time allocation measures are constructed based on girls' responses regarding time spent in minutes on particular activities during a typical day in the last week. In Column (6), Reports time on mobile is an indicator for whether the girl reports spending any time using a mobile phone during a typical day in the last week.

Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A15: Understanding Channels: Parental Perceptions of Girls' Strengths Index and Child Educational/Employment Aspirations Index Subcomponents

	(1) Willing to help	(2) Considerate	(3) Wants educated job	(4) Wants work for pay	(5) Wants to complete secondary
			Panel A: Benchmark Specification		
Treatment	-.037*	-.051**	.037	.025	.003
	(.021)	(.022)	(.022)	(.021)	(.011)
Obs.	2434	2434	2387	2380	2380
			Panel B: Stratification Controls Only		
Treatment	-.040**	-.053**	.032	.013	.0008
	(.020)	(.021)	(.023)	(.023)	(.013)
Obs.	2434	2434	2387	2380	2380
			Panel C: Stratification Controls + Age, Economic Status, and Baseline Values		
Treatment	-.042**	-.054**	.031	.016	-.0008
	(.021)	(.022)	(.022)	(.021)	(.011)
Mean Control Group	.813	.692	.698	.759	.932
Obs.	2434	2434	2387	2380	2380

Notes: Panel A contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported), stratification fixed effects, age fixed effects, baseline value of the outcome, a vector of dummies for the most important type of employment in the household at baseline, and controls for variables that appear imbalanced in the balance tables. Panel B contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported) and stratification fixed effects. Panel C adds age fixed effects, baseline value of the outcome, and a vector of dummies for the most important type of employment in the household at baseline to the Panel B specification. Willing to help is an indicator for whether the caregiver responds that it is "Certainly true" that the girl often offers to help others. Considerate is an indicator for whether the caregiver responds that it is "Certainly true" that the girl is considerate of other people's feelings. Wants educated job is an indicator for whether the girl responds that when she grows up she would like to work in a profession that requires completed higher secondary schooling. Wants work for pay is an indicator for whether the girl responds that she hopes to work for pay in the future. Wants to complete secondary is an indicator for whether the girl responds that she wants to complete at least secondary schooling. Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A16: Ancillary Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Married	Child works (Economically active)	Child works for pay	Child works outside of family	Child labor	Hazardous child labor	Other worst forms of child labor	Hours worked in a day	Hours worked unpaid work	Hours active (Paid + (unpaid)	Hour active outside house
Panel A: Benchmark Specification											
Treatment	.004	.031	.012	-.008	-.012	-.006	.012	-.024	-.009	-.036	-.038
	(.018)	(.035)	(.025)	(.030)	(.032)	(.031)	(.020)	(.124)	(.069)	(.152)	(.083)
Obs.	2435	2386	2386	2387	2386	2386	2387	2386	2386	2386	2386
R^2	.338	.08	.035	.018	.065	.064	.025	.132	.097	.175	.069
Q-statistic	0.892	0.892	0.892	0.892	0.892	0.892	0.892	0.892	0.892	0.892	0.892
Panel B: Stratification controls Only											
Treatment	.042	.049	.021	-.011	.004	.009	.021	.060	.026	.086	.00004
	(.029)	(.040)	(.025)	(.030)	(.037)	(.036)	(.021)	(.138)	(.074)	(.171)	(.086)
Obs.	2435	2386	2386	2387	2386	2386	2387	2386	2386	2386	2386
R^2	.005	.005	.009	.005	.004	.003	.004	.005	.003	.007	.004
Panel C: Stratification controls + Age, Economic Status, and Baseline Values											
Treatment	.011	.044	.023	-.008	.006	.012	.021	.001	.006	-.005	-.023
	(.018)	(.037)	(.025)	(.029)	(.034)	(.033)	(.020)	(.120)	(.069)	(.148)	(.080)
Obs.	2435	2386	2386	2387	2386	2386	2387	2386	2386	2386	2386
R^2	.332	.045	.022	.015	.035	.037	.016	.129	.092	.173	.067
Mean Control Group	0.191	0.651	0.228	0.186	0.583	0.458	0.180	1.157	1.642	2.800	0.602

Notes: Panel A contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported), stratification fixed effects, age fixed effects, baseline value of the outcome, a vector of dummies for the most important type of employment in the household at baseline, and controls for variables that appear imbalanced in the balance tables. Panel B contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported) and stratification fixed effects. Panel C adds age fixed effects, baseline value of the outcome, and a vector of dummies for the most important type of employment in the household at baseline to the Panel B specification. Married is an indicator variable for whether girl is married or committed (engaged). The set of survey questions used to construct each of the indicator variable outcomes in Columns (2) through (7) can be found in the analysis plan posted on-line. Time use outcomes in Columns (8) through (11) are defined based on time use patterns recorded for "a typical day in the past week." One observation is missing for the majority of the child labor and time use outcomes reported in the table, corresponding to one respondent who did not answer question 311 in the child survey. Married is constructed using household survey data and so the number of observations exceeds that in subsequent columns which use only child survey data. Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A17: School Dropout (Lee Bounds)

	(1)	(2)	(3)	(4)	(5)
	Survey data		Administrative data		
	Whether child has dropped out	Whether child progressed to 7th grade	Dropout Grade 6	Dropout Grade 7	Dropout Grade 8
Treatment (lower bound)	-.037* (.020)	.035* (.020)	-.018 (.019)	-.046* (.024)	-.044* (.023)
Treatment (upper bound)	-.033* (.020)	.049** (.023)	-.006 (.017)	-.023 (.022)	-.042* (.023)
Bounds-Based 95% CI	[-0.074,0.004]	[-0.025,0.090]	[-0.051,0.024]	[-0.086,0.014]	[-0.088,0.0020]
Benchmark 95% CI	[-0.072,0.006]	[-0.004,0.077]	[-0.041,0.028]	[-0.068,0.017]	[-0.085,0.0003]
Obs.	2433	2387	2374	2319	2455
Mean Control Group	0.132	0.865	0.075	0.142	0.192

Notes: Table presents lower and upper Lee (2009) treatment effect bounds based on a trimming procedure that assumes treatment status affects selection monotonically. In addition, we present a bounds-based 95% confidence interval for the treatment effect that captures both uncertainty due to sample attrition and sampling variation, and we present the 95% confidence interval from a specification that regresses the outcome of interest on treatment (for comparison). Column (1) uses child and household endline survey data. Column (2) uses child endline survey only. Columns (3) through (5) rely on administrative data. In Columns (3) through (5), dropout is measured based on whether a child attended school at the conclusion of the referenced school year. Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A18: Attendance, Time Allocation, and Test Scores (Lee Bounds)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Attendance (Past Week) and Time Allocation (Typical Day in Past Week)						
	Attendance Rate	Any Attendance	Hours studying at home	Hours spent at school		
Treatment (lower bound)	.002 (.010)	.003 (.006)	-.135* (.081)	.091 (.190)		
Treatment (upper bound)	.039*** (.014)	.018*** (.005)	-.045 (.076)	.284 (.194)		
Bounds-Based 95% CI	[-0.015,0.062]	[-0.0071,0.027]	[-0.270,0.083]	[-0.228,0.611]		
Benchmark 95% CI	[-0.014,0.025]	[-0.008,0.014]	[-0.220,0.086]	[-0.204,0.549]		
Obs.	2089	2089	2386	2386		
Mean Control Group	0.918	0.982	1.541	7.166		
Panel B: Test Scores						
	ASER Mathematics	ASER Hindi	ASER English	GPA Grade 6	GPA Grade 7	GPA Grade 8
Treatment (lower bound)	-.041 (.077)	.022 (.094)	-.095 (.091)	-.209** (.087)	-.286** (.114)	-.137 (.137)
Treatment (upper bound)	.006 (.081)	.070 (.094)	-.047 (.092)	-.063 (.081)	.002 (.094)	.135 (.095)
Bounds-Based 95% CI	[-0.176,0.148]	[-0.143,0.235]	[-0.258,0.120]	[-0.353,0.081]	[-0.473,0.158]	[-0.363,0.290]
Benchmark 95% CI	[-0.175,0.132]	[-0.155,0.222]	[-0.255,0.106]	[-0.268,0.029]	[-0.303,0.061]	[-0.196,0.133]
Obs.	2380	2380	2380	2178	1976	1912
Mean Control Group	2.353	3.025	2.369	2.259	2.404	2.890

Notes: Table presents lower and upper Lee (2009) treatment effect bounds based on a trimming procedure that assumes treatment status affects selection monotonically. In addition, we present a bounds-based 95% confidence interval for the treatment effect that captures both uncertainty due to sample attrition and sampling variation, and we present the 95% confidence interval from a specification that regresses the outcome of interest on treatment (for comparison). Columns (1) and (2) of Panel A use child endline survey data and are conditional on school being open and child not having dropped out of school. Attendance rate in Column (1) is the fraction of school days attended in the week prior to being surveyed and the Attendance dummy in Column (2) is an indicator for having attended any days in the past week. 298 observations are missing for these measures because of temporary school closures. Time use outcomes in Columns (3) and (4) of Panel A are defined based on time use patterns recorded for “a typical day in the past week.” Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A19: Selection into Survey-Based and Administrative Test Data

	(1) ASER Score Available	(2) GPA Grade 6 Available	(3) GPA Grade 7 Available	(4) GPA Grade 8 Available
Panel A: Selection into Test Data (Stratification controls)				
Treatment	0.011	0.032	0.053**	0.069***
	(0.008)	(0.022)	(0.024)	(0.026)
Obs.	2459	2459	2459	2459
R^2	.0018	.0051	.0064	.0085
Panel B: Selection into Test Data by Grade 5 GPA (Stratification controls)				
Treatment	0.048	-0.143*	-0.087	0.068
	(0.032)	(0.073)	(0.098)	(0.111)
Treatment * Grade 5 GPA	-0.011	0.053**	0.042	-0.003
	(0.010)	(0.023)	(0.031)	(0.033)
Grade 5 GPA	0.011	-0.007	0.036	0.086***
	(0.008)	(0.017)	(0.023)	(0.025)
Obs.	2356	2356	2356	2356
R^2	.0033	.0089	.0170	.0287
Panel C: Selection into Test Data by Grade 5 Attendance (Stratification controls)				
Treatment	0.036	0.053	0.054	0.148**
	(0.022)	(0.056)	(0.063)	(0.070)
Treatment * Grade 5 Attendance	-0.031	-0.020	0.010	-0.099
	(0.025)	(0.060)	(0.072)	(0.077)
Grade 5 Attendance	0.036*	0.134***	0.168***	0.266***
	(0.019)	(0.048)	(0.054)	(0.058)
Obs.	2026	2026	2026	2026
R^2	.0053	.0252	.0319	.0470
Mean Control Group	.962	.870	.778	.744

Notes: Panel A contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported) and stratification fixed effects. Panels B and C contain results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported), the interaction of treatment with the specified characteristics (reported), the characteristics (reported), and stratification fixed effects. Missing observations in Panels B and C correspond to missing baseline values of the specified characteristic. The dependent variable in Column 1 is an indicator for whether survey-administered ASER test score data is available. The dependent variables in Columns 2-4 are indicators for whether administrative test score data from grades 6-8 is available. Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A20: Life Skills: Child Survey Measures (Lee Bounds)

	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A			
	Future planning index	Gender norms index	Educ. / emp. aspirations index	Marital expectations index	Empowerment / agency index	Self-esteem index
Treatment (lower bound)	.061*	.082**	-.024	-.345**	.082***	.034
	(.033)	(.036)	(.057)	(.135)	(.028)	(.024)
Treatment (upper bound)	.094***	.107***	-.013	-.277**	.106***	.061**
	(.030)	(.036)	(.056)	(.133)	(.026)	(.026)
Bounds-Based 95% CI	[0.004,0.145]	[0.020,0.170]	[-0.131,0.092]	[-0.583,-0.042]	[0.035,0.150]	[-0.006,0.105]
Benchmark 95% CI	[0.011,0.131]	[0.019,0.156]	[-0.121,0.095]	[-0.561,-0.072]	[0.040,0.146]	[-0.007,0.087]
Obs.	2380	2380	2380	2380	2380	2380
Mean Control Group	-0.016	0.000	0.000	-0.606	-0.002	-0.001
			Panel B			
	Freedom of movement index	Socio-emotional index	Cantril's ladder	Locus of control index	Perceived stress index	Rosenberg self-esteem index
Treatment (lower bound)	.018	.060***	-.094	-.101*	-.053	-.008
	(.027)	(.023)	(.122)	(.056)	(.054)	(.032)
Treatment (upper bound)	.059*	.087***	.013	.013	.025	.028
	(.031)	(.025)	(.126)	(.065)	(.058)	(.029)
Bounds-Based 95% CI	[-0.027,0.110]	[0.022,0.128]	[-0.302,0.227]	[-0.193,0.121]	[-0.143,0.122]	[-0.061,0.076]
Benchmark 95% CI	[-0.025,0.063]	[0.024,0.117]	[-0.293,0.235]	[-0.106,0.078]	[-0.116,0.069]	[-0.044,0.074]
Obs.	2380	2380	2380	2380	2380	2380
Mean Control Group	0.000	0.000	4.513	0.000	0.000	0.000

Notes: Table presents lower and upper Lee (2009) treatment effect bounds based on a trimming procedure that assumes treatment status affects selection monotonically. In addition, we present a bounds-based 95% confidence interval for the treatment effect that captures both uncertainty due to sample attrition and sampling variation, and we present the 95% confidence interval from a specification that regresses the outcome of interest on treatment (for comparison). For all included indices, we calculate the normalized difference between treatment and control for each component question, and average over all components, ensuring that the interpretation is consistent (i.e. higher values of empowerment index components all correspond to higher levels of empowerment). Marital expectations index is not mean 0 because married girls are assigned the minimum value calculated for non-married girls. Detailed definitions of all referenced indices can be found in the analysis plan. Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A21: Life Skills: Parental Reports, Demonstration Tasks, and Enumerator Assessment (Lee Bounds)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Parental Reports							
	Parental perception of girl's strengths	Parental perception of girl's self-efficacy	Parental perception of freedom of movement	Parent daughter communication	Parental gender attitudes	Parental schooling attitudes	Parental marriage attitudes
Treatment (lower bound)	-.047** (.020)	.004 (.031)	.019 (.027)	-.022 (.030)	-.006 (.028)	.027 (.069)	.013 (.032)
Treatment (upper bound)	-.036* (.019)	.013 (.030)	.039 (.029)	-.008 (.029)	.007 (.027)	.078* (.043)	.030 (.034)
Bounds-Based 95% CI	[-0.082,-0.0025]	[-0.052,0.069]	[-0.028,0.089]	[-0.076,0.044]	[-0.056,0.056]	[-0.093,0.151]	[-0.043,0.089]
Benchmark 95% CI	[-0.079,-0.0058]	[-0.054,0.062]	[-0.037,0.077]	[-0.072,0.042]	[-0.056,0.054]	[-0.053,0.115]	[-0.040,0.085]
Obs.	2434	2430	2434	2434	2434	2434	2434
Mean Control Group	0.000	-0.002	0.000	0.000	0.000	0.001	-0.004
Panel B: Demonstration Tasks and Enumerator Assessment							
	Delay discounting	Completed mirror drawings	Mirror drawings (seconds)	Scavenger hunt index	Enumerator assessment index		
Treatment (lower bound)	-.006 (.031)	.049 (.083)	-4.006 (5.501)	-.177*** (.060)	.066 (.052)		
Treatment (upper bound)	.006 (.032)	.107 (.090)	4.283 (5.358)	-.081 (.062)	.107* (.058)		
Bounds-Based 95% CI	[-0.061,0.063]	[-0.095,0.262]	[-13.098,13.140]	[-0.276,0.021]	[-0.024,0.207]		
Benchmark 95% CI	[-0.062,0.065]	[-0.111,0.228]	[-6.527,11.231]	[-0.195,0.033]	[-0.027,0.172]		
Obs.	2380	2387	2317	2380	2380		
Mean Control Group	0.331	3.269	119.5	0.000	0.000		

Notes:Table presents lower and upper Lee (2009) treatment effect bounds based on a trimming procedure that assumes treatment status affects selection monotonically. In addition, we present a bounds-based 95% confidence interval for the treatment effect that captures both uncertainty due to sample attrition and sampling variation, and we present the 95% confidence interval from a specification that regresses the outcome of interest on treatment (for comparison). For all included indices, we calculate the normalized difference between treatment and control for each component question, and average over all components, ensuring that the interpretation is consistent (i.e. higher values of empowerment index components all correspond to higher levels of empowerment). Detailed definitions of all referenced indices can be found in the analysis plan. Delay discounting is an indicator for whether the respondent would prefer 60 Rs. in one week over 30 Rs. now. Completed mirror drawings takes on values from zero to four and Mirror drawings (seconds) measures the total number of seconds spent on mirror drawings, conditional on having attempted at least one drawing. Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A22: Understanding Channels (Lee Bounds)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Child Agency								
	Sole decision: Attend school	Sole decision: Continue schooling	Talks to parents about marriage					
Treatment (lower bound)	.061** (.024)	.094*** (.028)	.066*** (.025)					
Treatment (upper bound)	.073*** (.025)	.105*** (.027)	.119*** (.037)					
Bounds-Based 95% CI	[0,.018,0.117]	[0.044,0.152]	[0.025,0.180]					
Benchmark 95% CI	[0.019,0.116]	[0.050,0.150]	[0.034,0.127]					
Obs.	2380	2380	1976					
Panel B: Socio-emotional Support								
	Prefers to be alone	Meets friends outside	Has place to meet friends	Has place to stay if needed	Total social time	Reports time on mobile	Time on mobile	Time traveling to school
Treatment (lower bound)	-.057** (.023)	.038* (.023)	.059** (.029)	.032** (.016)	4.511 (4.457)	-.0025 (.0030)	-.092 (.122)	3.624 (4.573)
Treatment (upper bound)	-.052** (.024)	.050** (.024)	.071** (.029)	.044** (.017)	10.572*** (3.944)	.0060* (.0034)	.348 (.217)	9.647** (3.911)
Bounds-Based 95% CI	[-0.100,-0.0086]	[-0.0026,0.092]	[0.0064,0.124]	[0.0044,0.074]	[-2.873,17.106]	[-0.0074,0.012]	[-0.293,0.706]	[-3.961,16.134]
Benchmark 95% CI	[-0.100,-0.0075]	[-0.0036,0.087]	[0.0080,0.1214]	[-0.0002,0.070]	[2.603,17.129]	[-0.0000,0.012]	[-0.0086,0.770]	[1.706,16.257]
Obs.	2434	2380	2380	2380	2387	2387	2387	2387
Panel C: Parental Perceptions and Expectations								
	Willing to help	Considerate	Wants educated job	Wants work for pay	Wants to complete secondary			
Treatment (lower bound)	-.041** (.020)	-.056** (.022)	.027 (.023)	.010 (.024)	-.000 (.014)			
Treatment (upper bound)	-.035* (.019)	-.051** (.022)	.042* (.024)	.022 (.025)	.012 (.016)			
Bounds-Based 95% CI	[-0.077,0.0004]	[-0.097,-0.011]	[-0.013,0.084]	[-0.033,0.065]	[-0.024,0.040]			
Benchmark 95% CI	[-0.079,0.0005]	[-0.097,-0.011]	[-0.014,0.077]	[-0.034,0.059]	[-0.026,0.027]			
Obs.	2434	2434	2387	2380	2380			

Notes: Table presents lower and upper Lee (2009) treatment effect bounds based on a trimming procedure that assumes treatment status affects selection monotonically. In addition, we present a bounds-based 95% confidence interval for the treatment effect that captures both uncertainty due to sample attrition and sampling variation, and we present the 95% confidence interval from a specification that regresses the outcome of interest on treatment (for comparison). Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A23: Life Skills (Child Survey): Heterogeneous Effects by Social Desirability

	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A			
	Future planning index	Gender norms index	Educ. / emp. aspirations index	Marital expectations index	Empowerment / agency index	Self-esteem index
Treatment	.076	.094**	.021	-.138	.059	.025
	(.047)	(.044)	(.061)	(.118)	(.037)	(.044)
Treatment·Above Median	-.011	-.003	.0002	-.070	.061	.014
	(.053)	(.042)	(.065)	(.123)	(.038)	(.046)
Above Median	.166***	.105***	.192***	.325***	.020	.080***
	(.040)	(.032)	(.049)	(.075)	(.029)	(.029)
Obs.	2333	2333	2333	2333	2333	2333
			Panel B			
	Freedom of movement index	Socio-emotional index	Cantril's ladder	Locus of control index	Perceived stress index	Rosenberg self-esteem index
Treatment	-.024	.080***	-.012	-.020	-.011	.032
	(.039)	(.030)	(.174)	(.071)	(.070)	(.037)
Treatment·Above Median	.070*	-.023	.055	.001	-.017	-.016
	(.039)	(.037)	(.187)	(.083)	(.085)	(.039)
Above Median	.007	.099***	.107	.010	-.055	.013
	(.026)	(.024)	(.127)	(.066)	(.075)	(.032)
Obs.	2333	2333	2333	2333	2333	2333

Notes: Table contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported), the interaction of treatment with an indicator for at or above-median social desirability index responses (reported), an indicator for at or above-median social desirability index responses (reported), stratification fixed effects, age fixed effects, baseline value of the outcome, a vector of dummies for the most important type of employment in the household at baseline, and controls for variables that appear imbalanced in the balance tables. For Columns (4) through (6) of Panel B reporting measures added at endline, we control for lagged values of overall life skills indices. The social desirability index is constructed from three baseline variables: (1) an indicator for whether the girl reports that she wants to become a "Teacher/School head/Educator" when she grows up, (2) an indicator for whether she reports that she is currently living "the best possible life", and (3) an indicator for whether the surveyor recorded that the girl paid close attention "the whole time" when receiving instructions. For all included indices, we calculate the normalized difference between treatment and control for each component question, and average over all components, ensuring that the interpretation is consistent (i.e. higher values of empowerment index components all correspond to higher levels of empowerment). Detailed definitions of all referenced indices can be found in the analysis plan. Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A24: Life Skills (Other): Heterogeneous Effects by Social Desirability

Panel A: Parental Reports

	Parental perception of girl's strengths (1)	Parental perception of girl's self-efficacy (2)	Parental perception of freedom of movement (3)	Parent daughter communication (4)	Parental gender attitudes (5)	Parental schooling attitudes (6)	Parental marriage attitudes (7)
Treatment	-.063**	-.023	.002	.010	.040	.041	.038
	(.030)	(.045)	(.047)	(.042)	(.031)	(.054)	(.045)
Treatment·Above Median	.036	.045	.043	-.028	-.047	-.0005	-.025
	(.033)	(.047)	(.044)	(.042)	(.033)	(.057)	(.049)
Above Median	.003	.044	.058*	.067**	.071***	.124***	.065**
	(.022)	(.037)	(.030)	(.033)	(.023)	(.039)	(.029)
Obs.	2376	2374	2376	2376	2376	2376	2376

Panel B: Demonstration Tasks and Enumerator Assessment

	Delay discounting (1)	Completed mirror drawings (2)	Mirror drawings (seconds) (3)	Scavenger hunt index (4)	Enumerator assessment index (5)
Treatment	-.040	.018	-.488	-.092	.094
	(.036)	(.109)	(6.298)	(.080)	(.081)
Treatment·Above Median	.064*	.071	5.012	.031	.000
	(.038)	(.119)	(7.196)	(.083)	(.084)
Above Median	.004	.082	8.055*	.103*	.098
	(.024)	(.088)	(4.741)	(.058)	(.067)
Obs.	2333	2340	2273	2333	2333

Notes: Table contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported), the interaction of treatment with an indicator for at or above-median social desirability index responses (reported), an indicator for at or above-median social desirability index responses (reported), stratification fixed effects, age fixed effects, baseline value of the outcome, a vector of dummies for the most important type of employment in the household at baseline, and controls for variables that appear imbalanced in the balance tables. The social desirability index is constructed from three baseline variables: (1) an indicator for whether the girl reports that she wants to become a "Teacher/School head/Educator" when she grows up, (2) an indicator for whether she reports that she is currently living "the best possible life", and (3) an indicator for whether the surveyor recorded that the girl paid close attention "the whole time" when receiving instructions. For all included indices, we first take the difference between each component survey response value and the mean within the control group and then divide by the control group standard deviation. We then average over all index components, ensuring that values for each component are constructed so that the index interpretation is consistent. Detailed definitions of all referenced indices can be found in the analysis plan posted on-line. Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A25: School Dropout: Heterogeneous Effects by Dropout Propensity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Survey data		**Administrative data**				
	Whether child has dropped out	Whether child progressed to 7th grade	Dropout Grade 6	Dropout Grade 7	Dropout Grade 8	Dropout Grade 9	Dropout Index
Treatment	.004	-.003	.001	-.016	-.020	-.027	-.015
	(.012)	(.013)	(.013)	(.016)	(.017)	(.025)	(.012)
Treatment·Above Median	-.086***	.082***	-.016	-.021	-.053*	-.044	-.044**
	(.028)	(.030)	(.022)	(.028)	(.028)	(.035)	(.022)
Above Median	.134***	-.143***	.044**	.102***	.137***	.120***	.112***
	(.027)	(.029)	(.019)	(.028)	(.027)	(.030)	(.022)
Obs.	2391	2359	2318	2262	2398	2174	2399

Notes: Table contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported), the interaction of treatment with an indicator for at or above-median dropout propensity (reported), an indicator for at or above-median dropout propensity (reported), stratification fixed effects, age fixed effects, baseline value of the outcome, a vector of dummies for the most important type of employment in the household at baseline, and controls for variables that appear imbalanced in the balance tables. Column (1) uses child and household endline survey data. These data were collected at the start of eighth grade for girls who progressed one grade level each year. Columns (2) uses child endline survey only. Columns (3) through (6) rely on administrative data. In Columns (3) through (5), dropout is measured based on whether a child attended school at the conclusion of the referenced school year. In Column (6), dropout is measured based on whether a child attended school during the past week (conditional on the school being open). The Column (7) dropout index is constructed as the average of the outcome variables included in Columns (1), (3), (4), (5), and (6). Columns (6) through (7) include a set of fixed effects for the number of days that the school was open in the week before administrative data collection in grade nine. Dropout propensity is constructed by predicting endline survey dropout in the control group as a flexible function of baseline age, baseline (fifth grade) GPA, baseline attendance rate, and baseline time spent socializing. Predicted dropout propensity for girls assigned to treatment is determined by the dropout propensity calculated within the subset of control group girls with matching covariate values. Additional details are provided in Appendix Section A.5.
Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A26: Attendance (Last Week) and Time Allocation: Heterogeneous Effects by Dropout Propensity

	(1) Attendance rate	(2) Attendance dummy	(3) Hours studying at home	(4) Total hours spent at school
Treatment	.0005	-.0005	-.056	-.096
	(.010)	(.006)	(.106)	(.199)
Treatment·Above Median	.002	.005	.015	.485*
	(.016)	(.012)	(.113)	(.291)
Above Median	.0002	-.007	-.207**	-.984***
	(.013)	(.009)	(.104)	(.270)
Obs.	2062	2062	2358	2358

Notes: Table contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported), the interaction of treatment with an indicator for at or above-median dropout propensity (reported), an indicator for at or above-median dropout propensity (reported), stratification fixed effects, age fixed effects, baseline value of the outcome, a vector of dummies for the most important type of employment in the household at baseline, and controls for variables that appear imbalanced in the balance tables. Time use outcomes in Columns (3) and (4) are defined based on time use patterns recorded for "a typical day in the past week." Dropout propensity is constructed by predicting endline survey dropout in the control group as a flexible function of baseline age, baseline (fifth grade) GPA, baseline attendance rate, and baseline time spent socializing. Predicted dropout propensity for girls assigned to treatment is determined by the dropout propensity calculated within the subset of control group girls with matching covariate values. Additional details are provided in Appendix Section A.5. Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A27: Cognitive Skills: Heterogeneous Effects by Dropout Propensity

	(1)	(2)	(3)	(4)	(5)	(6)
	Survey data			**Administrative data**		
	ASER Mathematics	ASER Hindi	ASER English	GPA Grade 6	GPA Grade 7	GPA Grade 8
Treatment	-.067	-.014	-.079	-.152*	-.138	-.002
	(.087)	(.094)	(.097)	(.083)	(.122)	(.102)
Treatment·Above Median	.114	.086	.039	-.010	-.026	-.056
	(.098)	(.111)	(.109)	(.063)	(.081)	(.075)
Above Median	-.513***	-.699***	-.631***	-.071	.004	-.097
	(.080)	(.107)	(.102)	(.053)	(.057)	(.065)
Obs.	2352	2352	2352	2134	1941	1879

Notes: Table contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported), the interaction of treatment with an indicator for at or above-median dropout propensity (reported), an indicator for at or above-median dropout propensity (reported), stratification fixed effects, age fixed effects, baseline value of the outcome, a vector of dummies for the most important type of employment in the household at baseline, and controls for variables that appear imbalanced in the balance tables. In Columns 1-3, controls for baseline outcome values cannot be included since cognitive tests were not conducted at baseline; we instead include controls for baseline school dropout status, attendance, grade progression, time spent studying, hours spent on school, and grades as reported in grade five. ASER test score outcomes in Columns (1) through (3) and GPA outcomes in Columns (4) through (6) take on values between zero and four. Dropout propensity is constructed by predicting endline survey dropout in the control group as a flexible function of baseline age, baseline (fifth grade) GPA, baseline attendance rate, and baseline time spent socializing. Predicted dropout propensity for girls assigned to treatment is determined by the dropout propensity calculated within the subset of control group girls with matching covariate values. Additional details are provided in Appendix Section A.5. Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A28: Life Skills (Child Survey): Heterogeneous Effects by Dropout Propensity

	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A			
	Future planning index	Gender norms index	Educ. / emp. aspirations index	Marital expectations index	Empowerment / agency index	Self-esteem index
Treatment	.060*	.073*	-.047	-.269***	.114***	-.018
	(.036)	(.041)	(.048)	(.097)	(.034)	(.028)
Treatment·Above Median	.016	.039	.116	.172	-.025	.102***
	(.052)	(.047)	(.074)	(.114)	(.038)	(.037)
Above Median	-.172***	-.125***	-.264***	-.219**	.001	-.132***
	(.045)	(.040)	(.057)	(.092)	(.031)	(.033)
Obs.	2352	2352	2352	2352	2352	2352
			Panel B			
	Freedom of movement index	Socio-emotional index	Cantril's ladder	Locus of control index	Perceived stress index	Rosenberg self-esteem index
Treatment	.017	.056*	.062	.042	-.012	.006
	(.023)	(.032)	(.171)	(.064)	(.066)	(.037)
Treatment·Above Median	.006	.011	-.074	-.119	-.037	.028
	(.039)	(.038)	(.173)	(.075)	(.084)	(.042)
Above Median	-.034	-.044	-.078	.181***	-.034	-.047
	(.031)	(.029)	(.148)	(.063)	(.068)	(.036)
Obs.	2352	2352	2352	2352	2352	2352

Notes: Table contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported), the interaction of treatment with an indicator for at or above-median dropout propensity (reported), an indicator for at or above-median dropout propensity (reported), stratification fixed effects, age fixed effects, baseline value of the outcome, a vector of dummies for the most important type of employment in the household at baseline, and controls for variables that appear imbalanced in the balance tables. For Columns (4) through (6) of Panel B reporting measures added at endline, we control for lagged values of overall life skills indices. Dropout propensity is constructed by predicting endline survey dropout in the control group as a flexible function of baseline age, baseline (fifth grade) GPA, baseline attendance rate, and baseline time spent socializing. Predicted dropout propensity for girls assigned to treatment is determined by the dropout propensity calculated within the subset of control group girls with matching covariate values. Additional details are provided in Appendix Section A.5. For all included indices, we calculate the normalized difference between treatment and control for each component question, and average over all components, ensuring that the interpretation is consistent (i.e. higher values of empowerment index components all correspond to higher levels of empowerment). Detailed definitions of all referenced indices can be found in the analysis plan. Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A29: Life Skills (Other): Heterogeneous Effects by Dropout Propensity

Panel A: Parental Reports

	Parental perception of girl's strengths (1)	Parental perception of girl's self-efficacy (2)	Parental perception of freedom of movement (3)	Parent daughter communication (4)	Parental gender attitudes (5)	Parental schooling attitudes (6)	Parental marriage attitudes (7)
Treatment	-.056**	.011	.066*	-.022	-.003	-.022	-.001
	(.026)	(.038)	(.038)	(.035)	(.034)	(.043)	(.037)
Treatment·Above Median	.030	-.001	-.060	.022	.024	.121**	.044
	(.028)	(.044)	(.042)	(.038)	(.037)	(.060)	(.042)
Above Median	-.043*	-.041	.014	-.030	-.059**	-.264***	-.072**
	(.022)	(.038)	(.031)	(.031)	(.028)	(.048)	(.036)
Obs.	2391	2387	2391	2391	2391	2391	2391

Panel B: Demonstration Tasks and Enumerator Assessment

	Delay discounting (1)	Completed mirror drawings (2)	Mirror drawings (seconds) (3)	Scavenger hunt index (4)	Enumerator assessment index (5)
Treatment	-.004	.058	1.171	-.134**	.041
	(.041)	(.117)	(6.893)	(.067)	(.064)
Treatment·Above Median	.015	.021	2.119	.123	.113
	(.045)	(.125)	(7.808)	(.080)	(.073)
Above Median	-.057	-.094	-11.055**	-.245***	-.229***
	(.036)	(.106)	(4.637)	(.067)	(.057)
Obs.	2352	2359	2289	2352	2352

Notes: Table contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported), the interaction of treatment with an indicator for at or above-median dropout propensity (reported), an indicator for at or above-median dropout propensity (reported), stratification fixed effects, age fixed effects, baseline value of the outcome, a vector of dummies for the most important type of employment in the household at baseline, and controls for variables that appear imbalanced in the balance tables. Dropout propensity is constructed by predicting endline survey dropout in the control group as a flexible function of baseline age, baseline (fifth grade) GPA, baseline attendance rate, and baseline time spent socializing. Predicted dropout propensity for girls assigned to treatment is determined by the dropout propensity calculated within the subset of control group girls with matching covariate values. Additional details are provided in Appendix Section A.5. For all included indices, we first take the difference between each component survey response value and the mean within the control group and then divide by the control group standard deviation. We then average over all index components, ensuring that values for each component are constructed so that the index interpretation is consistent. Detailed definitions of all referenced indices can be found in the analysis plan posted on-line. Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table A30: Understanding Channels: Heterogeneous Effects by Dropout Propensity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Child Agency								
	Sole decision: Attend school	Sole decision: Continue schooling	Talks to parents about marriage					
Treatment	.086***	.075**	.085**					
	(.032)	(.034)	(.033)					
Treatment·Above Median	-.027	.063	.002					
	(.043)	(.041)	(.040)					
Above Median	.011	-.059*	-.049*					
	(.034)	(.032)	(.027)					
Obs.	2352	2352	1951					
Panel B: Socio-emotional Support								
	Prefers to be alone	Meets friends outside	Has place to meet friends	Has place to stay if needed	Total social time	Reports time on mobile	Time on mobile	Time traveling to school
Treatment	-.034	.069**	.067*	.030	11.678**	.008	.636	10.806**
	(.032)	(.028)	(.035)	(.024)	(5.025)	(.005)	(.464)	(4.937)
Treatment·Above Median	-.040	-.069*	-.013	-.006	-4.241	-.002	-.487	-4.217
	(.041)	(.036)	(.037)	(.035)	(4.747)	(.007)	(.499)	(4.657)
Above Median	-.022	.052*	.055*	-.039	-11.746***	.003	.172	-11.749***
	(.035)	(.030)	(.029)	(.031)	(3.400)	(.004)	(.172)	(3.255)
Obs.	2391	2352	2352	2352	2359	2359	2359	2359
Panel C: Parental Perceptions and Expectations								
	Willing to help	Considerate	Wants educated job	Wants work for pay	Wants to complete secondary			
Treatment	-.081***	-.053*	.030	-.003	-.002			
	(.026)	(.031)	(.027)	(.026)	(.011)			
Treatment·Above Median	.083***	.006	.012	.045	.015			
	(.030)	(.038)	(.033)	(.039)	(.024)			
Above Median	-.054**	-.001	-.083***	-.127***	-.067***			
	(.026)	(.036)	(.024)	(.028)	(.022)			
Obs.	2391	2391	2359	2352	2352			

Notes: Table contains results from regressing the outcome variable indicated by the column header on an indicator for treatment (reported), the interaction of treatment with an indicator for at or above-median dropout propensity (reported), an indicator for at or above-median dropout propensity (reported), stratification fixed effects, age fixed effects, baseline value of the outcome, a vector of dummies for the most important type of employment in the household at baseline, and controls for variables that appear imbalanced in the balance tables. Standard errors, clustered by school, in parenthesis. * significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.