Negotiating a Better Future: How Interpersonal Skills Facilitate Inter-Generational Investment *

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July 22, 2017

Abstract

Two children with the same ability and household financial resources can lead very different lives depending on how much human capital investment they receive. In this paper, we use a randomized control trial that taught girls negotiation skills to explore how intra-household bargaining determines human capital investment. We first study the effects of the training in a lab-in-the-field investment game that simulates parents' educational investment decisions under exogenously varied conditions of communication. Negotiation training leads to better outcomes for daughters when they can communicate with their parents and moves households closer to the efficient frontier. These findings are consistent with an incomplete contracting model of parental investment, in which imperfectly altruistic parents invest less than is efficient in their daughters, and negotiation allows daughters to develop more cooperative relationships with parents. Crucially, we find that negotiation training significantly improves educational outcomes over the next three years, as measured in administrative data. Using machine learning techniques, we find that these positive effects are concentrated in the top 40% of the ability distribution, including those on the margin of enrolling in school.

*We are grateful to Orazio Attanasio, Jiaying Gu, Tatiana Komarova, and Aloysius Siow, as well as seminar participants at UBC, UC San Diego, Zurich, Science Po, Toronto, the IADB, PUC-Chile, and UOttawa for helpful comments and discussions. Clement Bisserbe, Alexia Delfino, Shotaro Nakamura, Tambudzai Matenga, Annika Rigole, Kim Sarnoff, and Nick Swanson provided excellent research assistance.

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1 Introduction

Why do two children with similar financial resources and abilities receive greatly divergent levels of education? To answer this question, we explore the role of intra-household bargaining and parental investments in determining educational outcomes in a particularly vulnerable environment during adolescence, a particularly critical period.[1] We focus on human capital investment for adolescent girls in Zambia, where female secondary school enrollment is low (39%) as in much of Sub-Saharan Africa (31%[2]). While much of the literature has focused on the role of policies that alleviate resource constraints such as scholarships (for a review, see Banerjee et al. (2013)), we instead seek to open the black box of intrahousehold decision-making.

Educational decision-making in the household is not straightforward. Just as a literature on intra-household bargaining shows that spouses have different preferences and may fail to achieve efficient outcomes for the household (Udry, 1996 and Ashraf et al., 2009), children and parents may also have different preferences. In Zambia, where – as in much of Sub-Saharan Africa – secondary schooling is costly, daughters report wanting statistically significantly more schooling than their parents want for them. This difference in preferences can result in inefficiently low educational outcomes. In standard bargaining models, households make efficient investments regardless of the preferences of different household members. However, when there are other incomplete contracting problems within the household, imperfectly altruistic parents may under-invest in their children's education if children cannot commit to repay their investments.

In this paper, we study a three-arm intervention offering girls non-material resources, including a negotiation program, a safe space, and a control group. The negotiation curriculum emphasized identifying one's own and one's partner's deeper interests in a negotiation and using this information to brainstorm solutions that benefited both parties. Thus, in economic terms, the curriculum was more closely aligned with increasing strategic cooperation across many interactions than increasing a girl's bargaining power in one-shot negotiations. Indeed, a theoretical literature in relational contracting (Chassang, 2010 and Watson, 1999) shows that dynamic strategic cooperation can lead to joint outcomes that are closer to the efficient frontier. In contrast, the safe space program did not impart these skills but did provide a safe, physical space with a role model, which is a common female empowerment treatment throughout the world (Baldwin, 2011).

[1] Adolescence is a critical period for girls, both in terms of risk and in the biological sense. Most drop out occurs during this period, pregnancies begin to occur, and girls in Africa undergo initiation rituals. Additionally, a growing psychological literature suggests that this is a critical period for the formation of interpersonal skills (Choudhury et al., 2006).

[2] World Bank (2017).

Following the intervention, we measured the effect of negotiation in two ways. First, several months after the training, we conducted a lab-in-the-field investment game that simulated educational investment decisions[3] to better understand the mechanisms through which negotiation affected girls' educational outcomes. Second, we worked with the Ministry of Education and the National Exam Council to collect administrative data on girls' educational outcomes and pregnancy status up to eleventh grade. These educational outcomes included whether parents paid school fees and attendance rates in 8th and 9th grade, whether girls took a high stakes exam at the end of ninth grade that is required to continue in secondary school, the girls' scores on this exam, and enrollment information through 11th grade.

In the investment game, parents were given tokens that could be converted into cell phone airtime.[4] Tokens sent to their daughters were doubled, daughters received a randomly chosen income shock, and then they decided how many tokens to return to their parents and how many to redeem for prizes. Since the returns to investment are high, if there were no incomplete contracting problems between parents and daughters, parents should have sent all of their tokens. Thus, in the game, the efficient frontier has a precise definition; sending all the tokens maximizes joint gains. When given the opportunity to communicate, negotiation girls' households moved closer to the efficient frontier: parents sent their daughters more tokens. This might occur if negotiation girls committed to return more tokens to their parents, and exploiting variation in the number of tokens received due to the random income shock, we confirm that this is the case. Altogether, these findings suggest that negotiation led parents and girls to cooperate more to increase their joint gains *when they could communicate,* highlighting a pathway through which negotiation could increase educational investment. Evidence from the midline survey showing that negotiation girls asked for more food, were more willing to do chores, and were viewed as more respectful by their parents further supports the idea that negotiation reduces incomplete contracting problems between parents and children by encouraging cooperation and reciprocal behavior.

The results from the lab-in-the-field experiment and midline survey suggest that negotiation skills could play a role in resolving incomplete contracting problems in human capital investment as well. Consistent with this, girls who received the negotiation training had higher human capital in long-run administrative data relative to control girls. On the other hand, there was no significant difference between the control and safe space groups' outcomes. Additionally, we find that negotiation had larger positive effects for a more advantaged subsample of girls. When we estimate the effect of negotiation on the 70% of girls who attended

[3]In fact, outcomes in the game were highly correlated with long-term educational outcomes.

[4]These tokens are fungible and are common as a survey payment in sub-Saharan Africa.

the lab-in-the-field experiment, we discover that it had stronger and more precise effects among this sub-sample. We confirm that this sub-sample is not selected on negotiation status and when we use the methodology of Heckman (1979) to correct for selection bias in the estimated effects, we find as large or larger effects of negotiation on the long-term outcomes. However, the sub-sample is selected in other respects: those who attended the lab-in-the-field experiment are more likely to live with both their biological mothers and fathers and have higher pre-treatment ability.

To better understand this heterogeneity, we use the machine learning methodology of Athey and Imbens (2016) to identify heterogeneous treatment effects. We find that negotiation had particularly strong effects for girls above the 60th percentile of the ability distribution. Since female secondary school enrollment rates in Zambia are 39%, this is exactly the group we would expect to be affected by the treatment in a model where negotiation increases girls' ability to commit to repaying parents for their investments. Since higher ability girls were more likely to appear in the investment game sample, this helps rationalize the difference between the effects of negotiation on the administrative outcomes for the full and investment game-only samples. Additionally, since these girls likely had the highest returns from education among those who were not educated, it appears that negotiation changed educational investments for the group with the highest potential efficiency gains.

This paper contributes to two important literatures. First, it contributes to a growing literature on intra-household decision-making dating back to the important theoretical contributions of Becker (1981) and Chiappori (1992). Empirical work in this literature has typically focused on bargaining and investment problems between spouses (for example, Udry, 1996; Ashraf et al., 2009; Ashraf et al., 2014; Browning et al., 1994; and Browning and Chiappori, 1998). However, a growing theoretical (Banerjee, 2004; Becker et al., 2016; Bernheim et al., 1985) and empirical (Bursztyn and Coffman, 2012; Ashraf et al., 2016; Bau, 2016) literature has begun to consider how intra-household bargaining and incomplete contracting problems can arise between parents and their children, suggesting that households' educational decisions are not on the efficient frontier. In this paper, we provide the first evidence that interpersonal skills can lead to more cooperative routines and resolve incomplete contracting problems within the household, leading to more efficient levels of investment in children's education. More broadly, these results suggest that interpersonal skills could be important for moving the household closer to the efficient frontier when there are incomplete contracting problems between spouses or parents and children.[5]

[5]For example, Udry (1996) shows that household investments in agricultural inputs for farm plots operated by different household members are not efficient. Negotiation skills could potentially facilitate more efficient allocations of inputs across farm plots or other productive activities in the household.

This paper also contributes to a growing literature on non-cognitive skills, which have been shown to affect long-term outcomes like wages as much as cognitive skills (Heckman and Rubinstein, 2001; Heckman et al., 2006; Lindqvist and Vestman, 2011; Carneiro et al., 2007; Adhvaryu et al., 2016). While much of the literature has focused on non-cognitive skills developed during the critical period of early childhood, the development psychology literature increasingly suggests that adolescence is a second critical period. During this period, the brain grows most rapidly in the areas associated with social cognition (Choudhury et al., 2006). Adolescence is also a critical period biologically and socially: the peak periods of female drop out occur, and girls begin to menstruate and to be viewed (and view themselves) as future wives and mothers. Thus, we add to the existing literature by focusing on non-cognitive skills that can be affected during this crucial second period and which can help girls to navigate these challenges. By focusing on adolescence, we also discover unique opportunities to affect non-cognitive skills by intervening within schools, which is likely easier than intervening within households.

This paper is organized as follows. Section 2 describes the negotiation treatment and the experimental design in more detail. Section 3 describes the investment game and reports its results. Section 4 proposes a simple theoretical framework drawing on the results from the investment game. Section 5 provides additional evidence that negotiation led to more cooperative behavior between girls and their parents in the midline survey. Section 6 reports the effects of the negotiation treatment on educational outcomes. Section 7 uses machine-learning techniques to identify heterogeneity in the negotiation effects and shows how our theoretical framework naturally accommodates this heterogeneity, and section 8 tests for spillovers of the negotiation treatment on untreated children. Section 9 concludes.

2 Experimental Design & Data Collection

2.1 Experimental Design and Timeline

The experiment is comprised of several stages. We first collected baseline data from 3,146 girls in 41 primary schools across Lusaka, Zambia. During this baseline survey, we randomly assigned half the girls to receive an information treatment on HIV risk and the returns to education.[6] Appendix A provides a more detailed description of the information treatment. We then randomly allocated 12 schools to be pure control schools, which received no addi-

[6]The decision to provide information on health and returns to education was motivated by Jensen (2010), who shows that providing information on the returns to education increases educational attainment in the Dominican Republic, and Dupas (2011), who shows that providing teenage girls in Kenya with information on HIV risk affects sexual behavior and pregnancy.

tional treatment, and 29 schools to be treatment schools. Within the 29 treatment schools, 8th grade girls were randomly assigned to three groups: (1) control group, (2) safe space group, and (3) negotiation group. Three months later, the girls were invited to attend a lab-in-the-field game with their guardians that simulated the educational investment decision. When they attended the game, the girls and their parents also completed midline surveys. Following this lab-in-the-field experiment, we continued to collect administrative data on the girls' educational and pregnancy outcomes for the next three years. Figure 1 documents the time line of the study. Figure A1 shows the template for the letter sent to parents to invite them to participate in the study. Below, we discuss each stage of the experiment and data collection in more detail.

Baseline Survey. Between May and June 2013, we collected the baseline data. The survey was conducted with the girls during after-school meetings in privacy from their peers. During this baseline survey, we randomly provided an information session to half the girls lasting approximately one hour on two main topics: education and health. In particular, the session addressed the following points: the benefits to girls from staying in school, job opportunities in Zambia, options for families to finance education such as scholarships, and relative risk and prevention of HIV transmission.

Treatment Assignment and Intervention. Following this baseline survey, within each of 29 randomly chosen schools, girls were randomly allocated to one of three different treatments after stratifying on classroom and information status: negotiation, safe space, or a control group, which did not receive a treatment. The girls were told that a computer would randomly select the groups so that families and teachers would have no influence over the experimental assignment. While we initially intended to use the remaining 12 schools as a pure control group, we found that, despite the randomization, the characteristics of treated schools differed substantially from those of untreated schools. For example, treatment schools had statistically significantly more orphans and larger shares of the class receiving scholarship support. They also had significantly fewer classrooms, and were significantly less likely to offer interactive radio instruction. Following term two of 9th grade, the negotiation training was expanded to the girls in the pure control schools (but not the control group in the treated schools).

Therefore, for most of this study, we compare the outcomes of girls *within* the 29 treated schools. Table 1 reports summary statistics for our sample, and table 2 reports the results of balance tests, controlling for school fixed effects. Table 2 suggests that most characteristics are balanced for the negotiation treatment relative to the safe space and control treatments, but there is some evidence that girls who received the negotiation treatment are lower ability. They are 5 percentage points less likely to read or speak Nyanja excellently ($p < 0.05$ and

$p < 0.10$ respectively). However, given that we test balance across 15 outcomes, these may be significant by chance, and if negotiation girls *are* slightly lower ability, this is likely to negatively bias our results.

Appendix table A1 compares our intervention schools to other urban government schools in Zambia that offer 8th grade (columns (1)-(5)), all government schools in Zambia that offer 8th grade (columns (6)-(8)), and all Zambian schools, including private and community schools, that offer 8th grade (columns (9)-(11)). The intervention schools are larger than other urban schools on average,[7] but otherwise resemble other urban schools in terms of the male and female drop out rates and resources. In contrast, the intervention schools are both larger, have lower drop out rates, and have more resources than the full sample of schools, which includes rural schools. Thus, we expect our external validity to be greatest for urban schools.

We summarize the negotiation, safe space, and pure control treatments briefly as follows, with more details on the negotiation treatment in the next sub-section:

1. *Negotiation Group* (801 girls): Girls participated in six after-school sessions over two weeks with female role models covering training on negotiation and interpersonal communication. They received free lunch on session days, a notebook, and pens.

2. *Safe Space Group* (785 girls): Girls participated in six after-school sessions over two weeks, during which they could play games, study or do homework, or just talk with other girls. Trained female Zambian role models supervised these sessions. They received free lunch on session days, a notebook, and pens.

3. *Pure Control Group* (780 girls): Girls assigned to this group did not participate in any after-school program but were told they would be offered the treatment at the end of the study period.

The intervention was implemented at 6-8 schools at a time on a rolling basis. Approximately one to two months after the intervention and again during the first term of grade 9, a one-day booster session was held at each school with the negotiation and safe space groups in order to reinforce the knowledge gained (in the case of the negotiation group) and provide girls with additional support for utilizing the skills in their lives. The same trained coaches from the initial program led these sessions.

Lab-in-the-Field Investment Game and Midline Survey. Between September and October of 2013, all the girls in the sample were invited to attend a lab-in-the-field investment game

[7]This is unsurprising since, to be chosen for the intervention, a school had to be large enough to have sizable negotiation, treatment, and controls groups.

with a parent or guardian, where they participated in a randomly chosen version of the investment game. We describe the games in more detail in section 3. These games were designed to simulate the human capital investment problem in the presence of incomplete contracting by requiring parents to decide how much to invest in daughters – with a high return – and then requiring daughters to decide how much of that investment to return to parents. Thus, the game sheds light on the mechanisms through which negotiation could affect parents' human capital investment decisions. When daughters and their guardians arrived for the investment game, they also participated in a midline survey.

Administrative Data Collection. Additionally, from the start of the treatment to December 2014, we collected data on school fee payment, national exam attendance, attendance rates, and national exam scores from the treatment schools, and until term 1 of grade 11, we continued to collect enrollment data and data on whether girls became pregnant.[8] Appendix B discusses the collection of the administrative data in more detail.

2.2 Negotiation Treatment

The program was comprised of six, two hour training sessions, including activities like role-play, group discussion, storytelling, and games. Attendance rates for these sessions were high and were not statistically significantly different between the negotiation and safe space treatments.[9] Some activities directly mimicked real situations that girls might face, such as negotiating with siblings and parents to split housework so that they could spend more time on schoolwork, refusing unwanted sexual advances from older men, and asking their parents to pay for school fees. Other types of exercises were more abstract and intended to highlight the gains from cooperation. These included games, such as a prisoner's dilemma-type game called "Lion-Zebra" that illustrated the impact of the other party's decisions on one own's outcomes and how different strategies affected long-term cooperation.

The curriculum was structured around four key principles: me, you, together, and build (see figure 2). Below, we describe each of these principles in more detail. We also use stories from the girls' diaries (collected at the booster session in term 2 of grade 8) and stories relayed to us by the negotiation coaches to illustrate how the girls practiced these principles at home.

Me. This principle taught girls to understand their own interests – that is, to concentrate

[8]Following the expansion of the treatment to pure control schools, pregnancy data was only collected from the treated schools.

[9]The average girl in the negotiation treatment attended 4.8 out of the 6 days of training, and the average girl in the safe space treatment attended 5 days. Consequently, ITT and TOT regressions that instrument for number of classes attended with whether a girl was assigned to the negotiation or safe space treatment deliver similar results.

on their deeper needs and values rather than the proximate cause of a dispute. Knowing one's own interests is a necessary step for identifying potential gains from trade, since a girl can then identify other ways a negotiation partner can make her better off beyond conceding on a disputed issue. Additionally, girls were taught to know their outside option so that they knew their "walk away" point (the point they would not compromise on) and entered the negotiation with a back up plan.[10]

Girls were also taught to focus on regulating their emotions, "taking five" when they were angry. Fabiansson and Denson (2012) show that such emotional regulation is important since anger hinders bargaining. Emotional regulation may have also played an important role in contributing to a better relationship between parents and daughters. Indeed, in section 5, we will show that the parents of negotiation girls consider their daughters more respectful.

You. This principle emphasizes the importance of understanding the other party. Girls learned to "step to the side" of the other party, taking their perspective. Perspective-taking entails asking open questions to determine the other party's interests. Just as identifying the girl's own deeper interests is a necessary step to identify potential gains from trade, so is identifying the interests of the other party. Indeed, Galinsky et al. (2008) show that individuals who can take their partner's perspective uncover underlying interests and generate more efficient solutions, enlarging joint gains, in cases where a deal did not seem to be possible.

Perspective-taking includes choosing a way to approach the other party respectfully that emphasizes shared values and is consistent with cultural expectations. This was particularly important in the Zambian context, where girls are expected to be obedient and not to talk back to their parents. For example, asking direct questions is considered to be rude, so girls were taught to use indirect questions to identify their parents' interests.[11]

Together. This principle focused on bringing the two sides together. First, girls were taught to find common ground with the other person by emphasizing their shared values rather than differences of opinion. For example, a girl might say, "We both care about education" rather than, "I want you to pay my school fees." Second, they were taught to

[10] One diary entry describes how a girl utilized knowing her outside option in a negotiation with a boyfriend who wanted to have sex with her:

> *I told him that I am sorry, I can't take it, and I asked him what was his other option, but unfortunately, he had no other option. Then, I told him that... I have other options. It's either we end this relationship or stop telling me about this nonsense.*

[11] One girl describes the importance of choosing the right approach in her diary:

> *One day I wanted to ask for money for school shoes from my mother. Then I went to ask my mother, I just went without greeting her, didn't kneel down and I asked indirect questions, and she did not answer me because I did not kneel down. Then I went again and I asked her, first I greeted her, knelt down and asked indirect questions, she didn't refuse, she gave me because I respected and knelt down.*

recognize that the other party's "no" may come from an external roadblock or problem that could be solved together if they could identify ways to remove that roadblock.

Build. Finally, girls were taught to look for "win-win" agreements that met the needs of both negotiating partners. This step builds crucially on the previous steps, which allowed girls to identify both their own and their partners' true interests and determine what roadblocks were leading to conflict. To remove roadblocks, girls were taught to brainstorm ways to overcome external barriers and find productive trades with their partners. Girls were taught that "building an agreement is like building a house you can both live in," and therefore, that an agreement should give both parties something they want. In effect, this skill taught girls to use what they had learned in previous steps in the negotiation to look for solutions closer to the efficient frontier. A story relayed to us by one of the negotiation coaches illustrates a girl successfully using all the steps together to convince her parents to pay her school fees

> *I asked my parents if they could talk with me. I put on my chitenge [traditional material skirt], and knelt before them. I chose to approach with respect and so they asked me to stand and sit in the chair near them and tell them what I wanted to say. I said that I really wanted to be able to go back to school but wasn't able to because the school fees weren't paid. They said I knew that the family had no more money so it wasn't possible. I said I know that mom sells chickens out of the house. I see that some people sell them in the marketplace near by. If I can sell some chickens in the market over the school holiday, could I use the money for my school fees? They agreed and that is how I got to go back to school.*

Altogether, the curriculum was designed to allow girls to understand the other party's interests and identify gains from trade that were not immediately obvious. By choosing the right approach, regulating their emotions, and finding common ground with their partners, girls were taught to initiate cooperation and sustain positive relationships with their negotiation partners over the long term. Thus, the approach laid out by the curriculum more closely maps onto repeated, cooperative interactions rather than a single interaction where a girl tried to extract the entire surplus. A theoretical literature in relational contracting demonstrates how the establishment of dynamic cooperation can lead to more efficient outcomes (Chassang, 2010; Kranton, 1996; Ghosh and Ray, 1996).

To test whether girls in fact learned the negotiation curriculum and could apply what they learned to a new situation two months later, girls were presented with a scenario and asked to imagine themselves in that situation during the midline survey. Each girl was given a scenario where she needed to study for a test and had asked her sister to take care

of her younger brother, but the sister refused, saying that she wanted to go visit a friend. After reading the description of the scenario, the girls were asked three open-ended questions about what they would do if they were in this situation. We blindly coded girls' answers to these questions on a scale of 1-7, with "7" indicating the best answer according to the negotiation curriculum. Appendix table A2 regresses girls' scores on each of these questions and their average score across the questions on an indicator variable for whether girls received the negotiation treatment. As the table shows, girls who were taught negotiation scored substantially better on all three questions. Given that the midline occurred several months after the negotiation classes, this provides evidence that the classes had long-term effects on girls' knowledge of negotiation. Additionally, it shows that the safe space and control girls who did not receive the training were not able to fully learn the negotiation skills from their classmates.

3 Lab-in-the-Field Investment Experiment

3.1 Experimental Design

The lab-in-the-field investment experiment was intended to simulate parents' educational investment decision-making and provide us with a controlled environment in which to trace out the different mechanisms through which the negotiation treatment could affect outcomes. Two months after the negotiation training took place, girls were asked to bring a parent or guardian to participate in the experiment. 70% of the girls attended this experiment with a parent or guardian, but there was no significant selective attrition by negotiation or safe space status (see rows 1 and 2 of appendix table A3 for the associations between appearing in the sample and the negotiation and safe space treatments). In the majority of cases (57%), girls attended the game with their biological mothers.

After arriving for the experiment, girls were assigned to one of several variations of the investment experiment, each of which was designed to isolate a different mechanism through which negotiation could affect human capital investment. Regardless of what version daughters were assigned to, at the start of the experiment, parents received ten tokens that could be redeemed for cell phone air time. In the simplest version of the game, the dictator game, parents then decided how many tokens to send to their daughters, the tokens sent to daughters were doubled, and daughters received a randomly determined "income shock" of 2 or 4 tokens. Importantly, these tokens were sent to daughters along with the tokens sent by the parent, and daughters were not aware of the size of their income shock. This aspect of the experiment was intended to avoid painful cases where daughters discovered that parents

had sent them zero tokens. Unlike in typical dictator games, which are played by strangers, the results of a game between daughters and parents could easily be undone after the game if daughters could redeem the tokens for a fungible good. To solve this problem, daughters redeemed their tokens for prizes at a "Chuck E. Cheese" store selling girl-specific items, including consumption items (like hair bands), school supplies, and household items (like socks). Figure 3 shows the prizes.

In the simple dictator game, in which 333 girls took part, only altruism effects how many tokens parents send daughters. While there are high returns to investment (100%), daughters have no way of compensating parents for these investments. Therefore, if negotiation affects educational investment by affecting parental altruism, negotiation girls should receive more tokens in this version of the game.

The remaining girls were assigned to an investment game that more closely simulated the incomplete contracting problem in educational investment. In the investment game, after parents sent girls the tokens, girls still received an income shock, but they could now decide how many tokens to return to their parents and how many to spend at the "Chuck E. Cheese" store. Since the returns to investment are high in the basic version of the investment game (100%), if girls and their parents are able to achieve the efficient outcome, a parent should send all her tokens to her daughter. In fact, on average, a parent sent only 5 tokens in the investment game and only 2.4% sent all their tokens, indicating that incomplete contracting problems were large.

In the basic version of this investment game, there are now two standard mechanisms that affect parental investment in daughters: altruism and expected reciprocity, which we can think of as daughters' ability to commit to return tokens. These two mechanisms map onto the mechanisms in educational investment decisions where the returns to educating a child may be large, but it is difficult for the child to commit to repay a parent for the educational investment. In such cases, if parents are imperfectly altruistic, they will invest less than is efficient in the child's education (Banerjee, 2004).

We further introduce two additional variations to the investment game: (1) a random subset of girls and their parents were allowed to communicate before parents decided how many tokens to send, and (2) for a random subset of girls, the tokens sent to girls were only doubled if they successfully completed a word search. The first of these variations gave girls an opportunity to use their negotiation skills to affect parents' investment decisions. The second variation was intended to allow the returns on parents' investments in their daughters to vary based on daughters' ability. This maps onto the possibility that parents are uncertain about the returns to education or their child's ability, leading them to under-invest in the child. Therefore, this variation tests for another mechanism through which

negotiation could increase parents' educational investments in their children – allowing the child to communicate her ability to the parent or increase parents' perceptions of her ability.

These variations were cross-randomized so that a total of 318 girls participated in the normal investment game (without either variation), 331 participated in the word search version without communication, 318 participated in the communication version without the word search, and 330 participated in a game that featured both the word search and communication.

3.2 Results

This section reports the results from the investment games. Our key regressions take the form

$$y_i = \beta_0 + \beta_1 negotiation_i + \beta_2 safe_space_i + \beta_3 communication_i + \beta_4 negotiation_i \times communication_i + \beta_5 safespace_i \times communication_i + \mathbf{\Gamma X_i} + \epsilon_i, \quad (1)$$

where y_i is the outcome of interest (tokens sent to the girl, tokens the girl ended the game with, number of tokens a girl spent on a type of item), $negotiation_i$ is an indicator variable for the negotiation treatment, $safe_space_i$ is an indicator variable for the safe space treatment, $communication_i$ is an indicator variable for the variant of the game where girls can communicate with parents, num_tokens_i is the number of tokens the daughter has received (including the income shock), and $\mathbf{X_i}$ is a set of controls.

Since randomization took place within schools, all the regressions control for school fixed effects. Additionally, to maximize precision, we control for ethnicity fixed effects and a rich set of controls for socioeconomic status, consisting of controls for receiving the information treatment, both parents alive, lives with the biological father, lives with the biological mother, lives with a mother and a father, parents paying school fees in the pre-treatment period, reading Nyanja excellently, speaking Nyanja excellently, reading English excellently, speaking English excellently, reading Nyanja well, speaking Nyanja well, reading English well, and speaking English well. We also report estimates from variations on equation (1) that restrict $\beta_4 = 0$ and and $\beta_5 = 0$.

Tokens Sent to Daughter. Table 3 reports the effect of negotiation on the number of tokens parents sent to their daughters. Column (1) reports the results from the dictator game alone. We find that neither negotiation nor safe space had a statistically significant effect on the number of tokens that parents sent to their daughters. Column (2) estimates the effects of negotiation and safe space for the investment game sample. Again, negotiation and safe space have no significant effects on the number of tokens sent. These columns

appear to confirm that negotiation did not generally increase or decrease parents' altruism toward their daughters.

In column (3), we allow the effect of communication to interact with negotiation and safe space. Column (3) shows that communication and negotiation has a strong, positive interaction. When girls with negotiation skills can communicate, they receive 0.785 more tokens than when they cannot. The interaction between safe space and communication (0.316) is half the size and only marginally significant. On the other hand, in the non-communication version of the game, girls in the negotiation and safe space treatments received 0.469 and 0.397 *fewer* tokens. When we combine these coefficients, we find that girls in the negotiation treatment who can communicate receive 0.316 ($p < 0.10$) more tokens than control girls who can communicate, while girls in the safe space treatment who can communicate receive almost exactly the same number of tokens as the control girls. Given that column (1) shows parents are *not* less altruistic toward negotiation and safe space daughters, our findings in column (3) suggest that parents believe negotiation daughters are less reciprocal than control girls when they can't communicate but more reciprocal when they can. This could be because the female empowerment elements of negotiation and safe space lead girls to display relatively less altruism toward their parents and more "me first" behavior in the absence of communication, exacerbating incomplete contracting problems between children and parents.[12] However, when negotiation girls can communicate, they are able to use their negotiation skills to increase parents' expectations of reciprocity, leading them to receive more tokens. To further explore whether this is the case, in our next table, we will examine how girls spent the tokens in the different versions of the game.

Column (4) reports the average effect of the word game, and shows that the word game treatment had a zero effect. In appendix table A4, we further test whether the word game interacts with safe space, communication, or negotiation in meaningful ways. In all three cases, we see little evidence that the word game interacted with the treatments to affect investment decisions. In general, we find that the word game had zero effect across our outcomes. As a result, in most of our regressions, we pool word game with the other treatments.

Next, in table 4, we estimate the effects of the different treatments and their interactions with the communication treatment on how girls spent the tokens. We aggregate total spending into three broad categories: school supplies (colored pens, math books, notebooks, pencils, erasers, rulers, and pencil sharpeners); in-between spending, which consists of non-educational but useful spending (socks and sanitary pads); and pure consumption (hair ties, scarves, bracelets, lip balm, snacks, and snakes and ladders games). Consistent with the

[12]This hypothesis is consistent with Angelucci and Garlick (2016), who show that more traditional households exhibit more efficient decision-making than less traditional households in Mexico.

idea that safe space and negotiation girls exhibit more "me first" behavior in the absence of the communication treatment, they spend more (and often significantly more) on pure consumption relative to control girls in both the investment and dictator games (columns 3 and 9). However, when negotiation girls (and to a lesser extent, safe space girls) can communicate with their parents, they spend less on pure consumption (column 9) and more on in-between goods (column 8). These results provide further evidence that there is greater incomplete contracting between parents and girls in the negotiation and safe space treatments *in the absence of communication.* However, communication appears to more than undo this incomplete contracting problem for the negotiation girls.

Finally, we verify that the negotiation skills themselves play an important role in increasing the number of tokens daughters are sent when daughters can communicate. In appendix table A5, we include the girl's total score on the knowledge of negotiation test and its interaction with the communication treatment in the regressions. We find that knowledge has no effect when girls cannot communicate, but when girls can communicate, they are also sent significantly more tokens, even controlling for the interaction of the negotiation treatment with communication. This provides further evidence that girls used their negotiation skills specifically to convince parents to send them more tokens in the communication treatment.

Tokens Returned to the Parent. Our findings in table 3 suggest that negotiation girls were able to increase parents expectations of reciprocity (by committing to send them back more tokens) when they could communicate, leading parents to send them more tokens. If this is the case and parents have rational expectations, girls in the negotiation $\times$ communication cell should be more likely to send parents back a marginal token. We now explicitly test whether this is the case. Since the number of tokens a girl receives is endogenous, we cannot simply regress the number of tokens a daughter sends back to her parent on the number of tokens that she receives to estimate the pass-through of the marginal token sent to a daughter to her parent. Instead, we take advantage of the fact that daughters received a random income shock of two or four tokens before they decided how many tokens to send to their parents. This allows us to use the random income shock to instrument for the daughter's total number of tokens and estimate the marginal return to a parent of giving a

daughter one additional token. Therefore, the regression of interest is

$$\begin{aligned} tokens_returned_i = \beta_0 &+ \beta_1 negotiation_i + \beta_2 safe_space_i + \beta_3 communication_i \\ &+ \beta_4 negotiation_i \times communication_i + \beta_5 safe_space_i \times communication_i \\ &+ \beta_6 num_tokens_i + \beta_7 num_tokens_i \times negotiation_i \\ &+ \beta_8 num_tokens_i \times communication_i \\ &+ \beta_9 num_tokens_i \times communication_i \times negotiation_i \\ &+ \mathbf{\Gamma X_i} + \epsilon_i, \end{aligned}$$

where i denotes a daughter, $tokens_returned_i$ is the number of tokens a daughter sends her parent, num_tokens_i is the number of tokens the daughter has received (including the income shock), and $negotiation_i$, $safe_space_i$, $communication_i$, and $\mathbf{X_i}$ are defined in the same way as before. Then β_9, the relative increase in the pass-through of the marginal token given to the daughter to the parent is the coefficient of interest. Since num_tokens_i is endogenous to the parent's expectation of repayment, we instrument for it using $high_token_i$, an indicator variable for the girl being selected to receive 4 tokens instead of 2. Therefore, there are 4 endogenous variables in this regression (num_tokens_i and each of its interactions) and 4 instruments ($high_token_i$ and each of its interactions).

Column (1) of table 5 reports the results of this regression. We estimate that β_9 is 0.653 ($p < 0.05$), consistent with the idea that girls who received the negotiation treatment and communicated with their parents were more likely to transfer an additional token back to their parents. In column (2), we include an analogous set of interactions between $communication_i$, num_tokens_i, and $safe_space_i$. We find that the coefficient of $communication_i \times safe_space_i \times num_tokens_i$ is less than one-half the size of β_9 and statistically insignificant. This suggests that it is the negotiation aspect of the treatment itself and not other aspects of the treatment (such as female empowerment) that affect girls' and their parents' behavior.

Using the coefficients from table 5, we calculate what fraction of an additional token parents should expect to receive when a girl in the negotiation, safe space, or control treatment who can communicate receives an additional token. Control and safe space girls pass-through about one-fifth of a token, although in the case of safe space girls, the pass-through rate is not significantly different from zero. In contrast, negotiation girls pass through one-half of an additional token ($p < 0.01$), consistent with the idea that they credibly commit to send their parents more tokens. This further supports the idea that negotiation (in the presence of communication) increases parental investment by increasing the expectation of reciprocity.

Interestingly, in the non-communication treatment, both negotiation and safe space girls have a marginally significant one-third of a token lower pass-through rate, consistent with the idea that, in the absence of communication, these more empowered girls internalize their parents' preferences less and demonstrate more "me-first" behavior.

Daughter's Welfare. Finally, in table 6, we investigate whether negotiation made daughters better off in the investment game. The regressions in table 6 duplicate the specifications in columns (2)-(4) of table 3, but the outcome variable is now the final number of tokens the daughter received in the investment game.[13] Table 6 reports the results of these regressions. The results in table 6 echo the results in table 3. Column (1) suggests that girls in the negotiation and safe space treatments end the game with no more tokens on average (and perhaps fewer in the case of safe space), but column (2) shows that this masks important heterogeneity. Girls who received the safe space and negotiation treatments and were not allowed to communicate with their parents end the game with marginally significantly fewer tokens, consistent with column (5) of table 3. Negotiation girls who have been taught to negotiate and can communicate with their parents end the game with 0.954 ($p < 0.05$) more tokens than girls who could not communicate. The coefficients indicate that negotiation girls who can communicate receive 0.417 more tokens than control girls who communicate, while safe space girls receive 0.285 *fewer* tokens. While neither of these effects are statistically different from 0, they are statistically different from one another at the 10% level. These findings suggest that negotiation – with communication – not only increased the total size of the surplus (by increasing the number of tokens sent) but also provides some evidence that it increased the girl's welfare. As in table 3, the word game has no significant effect on the number of tokens daughters received (see column (3)).

Taken together, the results from the investment game suggest that negotiation is most likely to affect parents' educational decisions through expectations about reciprocity from the girl rather than through greater parental altruism or changes to a parent's beliefs about a daughter's ability.

4 Theoretical Framework

The results from the lab-in-the-field investment game motivate a simple model of parental investment in children in which parents are imperfectly altruistic. In this model, both altruism and a child's ability to commit to repaying a parent for investments affect a parent's decision to invest in a child. In the model, parents of daughter i choose $E \in \{0, 1\}$. If $E = 1$,

[13] The final number of tokens a daughter received is given by $daughter\ final\ tokens = 2 \times tokens\ guardian\ sent + income\ shock - tokens\ daughter\ returned$.

parents educate the daughter i at cost a f_s, and in period 2, she receives a return to education R_i, whose distribution is given by F. Daughters can commit to return τ to parents if they are educated. τ includes both repayment in terms of labor (like doing chores or helping with the family business) and repayment in the future (for example, supporting parents in their old age). Then, the parents' utility from educating their daughter is given by

$$U_i^p = -f_s + \tau + \delta(R_i - \tau),$$

where $\delta < 1$ is the parents' altruism toward the daughter. Then parents will educate a daughter if

$$\delta(R_i - \tau) + \tau \geq f_s.$$

This framework suggests that if $\delta \neq 1$, parents' and daughters' incentives are misaligned: a daughter wants more education for herself than her parents want for her. Empirically, this is indeed the case. During our survey, daughters told us they wanted to complete 1.37 years more of education than their parents or guardians told us they would complete given the obstacles they faced, and they wanted to complete 0.11 more years of education than their parents or guardians wanted them to complete in the absence of obstacles. In both cases, the difference was highly statistically significant.

From this simple framework, there are two possible reasons negotiation could make parents more likely to educate their daughters. Parents' utility from educating daughters will increase if their altruism toward their daughters increases (δ increases) or if daughters' ability to commit to repaying them increases (τ increases). In our view, the results of the lab-in-the-field experiment are more consistent with the latter mechanism. Parents do not invest more in the daughters who receive the negotiation treatment in the absence of communication, inconsistent with greater altruism of parents toward their children, and the daughters treated with negotiation, who can communicate with their parents, *do* send more of the returns on investment to their parents. This leads us to the following prediction.

Prediction 1. Increasing τ will increase the share of girls who are educated.

Proof. See appendix C. □

If the negotiation treatment raises τ, this prediction indicates that the negotiation treatment should increase parents' human capital investments in their daughters. In section 6, we verify whether this is the case by estimating the effect of negotiation on daughters' longer-term outcomes in the administrative data.

In our theoretical framework, we do not take a strong stance on *how* negotiation allows daughters to commit to repaying parents for their investment. However, we note that in reality, human capital investment decisions are unlikely to be a one-shot game and are more likely to resemble the cooperative routines described by Chassang (2010). Indeed, parents make investment decisions repeatedly over time, and daughters have the opportunity to repay parents (for example, through chores or good behavior) between these investments. In this case, the negotiation treatment can intuitively be thought of as facilitating daughters' movement from a "low investment" to a "high investment" equilibrium in a repeated game rather than convincing their parents that they will repay them for their investments much later in life.

5 Evidence on the Effects of Negotiation Within the Household

In this section, we use the midline survey to provide suggestive evidence on the different mechanisms through which negotiation could affect educational outcomes. In table 7, we estimate the effect of negotiation on a variety of outcomes. In particular, in line with the model developed in section 4, we test whether girls changed their behavior toward their parents (by asking for more food) and whether there is evidence that girls also "repay parents" (by doing chores). Additionally, to test whether negotiation could have affected educational outcomes through other mechanisms, we test whether the negotiation treatment negatively or positively affected parents' views of their daughters and whether the treatment led parents to have different views of or to be more informed about their daughters' abilities.

In the first column, we regress an indicator variable for whether girls report asking about getting enough food on the negotiation treatment. Girls who have been taught negotiation are significantly more likely to ask about getting enough food. In the second column, we regress an indicator variable for whether parents report that it is difficult to get a girl to do chores on the negotiation treatment; parents of girls in the negotiation treatment are marginally significantly less likely to report difficulty getting daughters to do chores. These two regressions reveal that though girls ask for more from parents after the negotiation treatment, they may also find ways to reciprocate and repay parents (e.g. doing chores).

In columns (3)-(5), we regress parents' reports of girls' behavior on the negotiation outcome. Columns (3) and (4) show that parents are no more likely to report that a girl has difficulty controlling her temper or is rude. However, according to column (5), parents of girls in the negotiation treatment are significantly more likely to report that girls are respect-

ful. These results indicate that negotiation did not negatively affect girls' relationships with their parents – one of our key concerns when we designed the curriculum. Instead, column (5) indicates that negotiation may have even strengthened these relationships.

Finally, in columns (6) and (7), we consider an alternative mechanism through which negotiation could affect educational outcomes. It is possible that negotiation skills led parents to believe that daughters were higher ability, incentivizing them to invest in the treated daughters, or that it allowed daughters to inform parents about their ability, reducing the misallocation of schooling investments (as in Dizon-Ross (2016)). To test for these two possibilities, we regress a measure of parents' perceptions of a daughter's ability (relative to her class mates) on a proxy for the daughter's ability[14] and negotiation (column (6)) and the interaction between the ability factor and negotiation (column (7)). In column (6), we see that, while the ability factor is highly correlated with a parent's perception of a daughter's ability, negotiation has no effect on that perception. Similarly, in column (7), we find that negotiation does not lead a daughter's ability to be more correlated with the parent's perception of her ability. Thus, we find no evidence that negotiation changed parents' perceptions of daughters' abilities or provided them with more information on daughters' abilities. Overall, our findings in table 7 appear to be consistent with the idea that negotiation allows daughters and parents to improve their joint outcomes, with daughters asking for more food and repaying parents by being more respectful and doing more chores.

6 Effect of Negotiation on Human Capital

In this section, we estimate the effects of the negotiation treatment on both the average daughter in the administrative data and the average daughter who attended the investment game. We find positive effects of negotiation in both the full sample and in the sample of girls who attended the investment game, with stronger effects on the girls who attended the investment game. In section 7, we confirm that the larger effects of negotiation on girls who attend the investment game are consistent with the fact that negotiation has a larger effect for girls whose ability levels put them on the margin of receiving more education. Indeed, the sample of girls in the investment game is of higher ability on average than the full sample. In section 7, we show that this result is fully consistent with the theoretical framework in section 4.

[14]We proxy for a daughter's ability by doing a factor analysis of our indicator variables for whether a daughter speaks and writes Nyanga and English well or excellently and predicting the first factor.

6.1 Effect on the Average Daughter

In this section, we estimate the effects of the negotiation, safe space, and information treatments on the educational and related outcomes of daughters. Our regressions take the form

$$y_i = \beta_0 + \beta_1 negotiation_i + \beta_2 safe_space_i + \beta_3 information_i + \mathbf{\Gamma X_i} + \epsilon_i, \tag{2}$$

where, as before, $negotiation_i$ and $safe_space_i$ are indicator variables for receiving the negotiation and safe space treatments, and $\mathbf{X_i}$ is the same set of controls as in tables 3-6. Our main outcome variables are: (1) an indicator variable for whether parents paid all secondary school fees at the end of 8th grade, (2) an indicator variable for whether a daughter took the national exam in 9th grade, which she was required to take and pass to continue her education, (3) an indicator variable for whether a daughter scored above the approximate threshold to be guaranteed a place in secondary school in math, (4) an indicator variable for whether a daughter scored above the approximate threshold to be guaranteed a place in secondary school in English,[15] (5) average enrollment rate over term 3 of grade 8 and terms 1 and 2 of grade 9, (6) average percent of days attended in terms 2 and 3 of grade 8 and terms 1 and 2 of grade 9, and (7) an indicator variable equal to 1 if a girl became pregnant prior to term 1 of 2016.

Additionally, to increase our statistical power, we form two indices that combine these measures. These are a human capital index, which only includes the direct human capital measures (1-6), and a full index, which includes measure 7 as well. For these indices, we standardize each of the measures and then take the average to create a new outcome variable (flipping the sign of pregnancy so that all the effects are in the same direction).[16] Following Clingingsmith et al. (2009), we also report the aggregate effect across the components of the indices by reporting the average effect sizes.[17] Unlike the index, which is missing for girls for whom data on every outcome is not available, the average effect sizes use the full data set and equally weight each outcome. Both the index and the average effect size approaches are also less sensitive to multiple hypothesis testing since they provide single tests of whether the aggregate negotiation and safe space effects are significant across related outcomes.

[15] Since only the top 25% of test-takers were assigned to public secondary schools, we set this threshold at the top 25th percentile. This outcome is set to 0 for girls who did not take the exam, since they could not have scored in the top 25th percentile. This avoids issues of selection that would arise if we only estimated the effect of negotiation among exam-takers.

[16] Using these indices as outcomes, we also verify that the number of tokens sent in the investment game is related to educational outcomes, using the same set of controls as in equation 2. We find that sending an additional token is associated with scoring 0.014 higher on the human capital index and 0.015 on the full index ($p < 0.05$ in both cases).

[17] We arrive at average effect sizes by running stacked regressions of the outcomes on the explanatory variables, allowing the explanatory variables' coefficients to vary with the outcome variable. We then take the average of the negotiation coefficients (dividing each by the standard deviation of the outcome for the control group to standardize the units) to arrive at the aggregate effect size.

Table 8 reports the results of these regressions. In the first two columns of Panel A, we report the effects of negotiation and safe space on the human capital index and the average effect size of negotiation across the sub-components of the index. The remaining columns report the effects for each of the components of the human capital index. In panel B, we report the effects of negotiation and safe space for the full index that now includes pregnancy as an outcome (column (1)), the average effect size including the educational outcomes and pregnancy (column (2)), and for pregnancy separately (column (3)). While negotiation does not have significant effects on the individual measures of human capital besides average enrollment, we find that it increases the human capital index by 0.057 ($p < 0.05$) standard deviations and increases the full index by 0.053 standard deviations ($p < 0.10$). Similarly, the average effect size for the educational outcomes is 0.062 standard deviations ($p < 0.01$) and is 0.060 standard deviations ($p < 0.01$) for the full set of outcomes. For comparison, the association between having a biological mother who has completed secondary school (relative to primary school) and the full and human capital indices is roughly the same magnitude (0.070 for the human capital index and 0.057 for the full index). The similarity between the estimates using the indices and the average effect sizes suggests that the significant results in the index are not driven by the restriction of the sample to individuals with non-missing outcome data across all outcomes. Moreover, the fact that the *aggregate* measures of the effects of negotiation on human capital are sizable and significant suggests that these statistically significant results are not driven by multiple hypothesis testing.

In contrast to negotiation, safe space has smaller, insignificant effects on both the outcome index and in the case of the average effect sizes, we can reject that the negotiation and safe space effects are the same. In appendix table A6, we further test whether information had any effect on the outcomes and find that the information treatment had no significant effect.[18] In other regressions (not shown), we also find that there is no meaningful interaction effect between negotiation and information.

6.2 Effect on the Daughters Who Attended the Investment Game

Recalling that only 70% of girls attended the investment game, we now estimate the effects of negotiation and safe space for this sub-sample in table 9. Negotiation now has stronger effects on both the human capital and full index, as well as several of the original outcomes. For this sub-sample, negotiation increases the likelihood of taking the national exam by 3.6 percentage points (4%, $p < 0.05$), increases the average attendance rate by 1 percentage point (2%, $p < 0.10$), increases the average enrollment rate by 4.2 percentage points (6%,

[18] While information has effectively increased educational attainment in other contexts (for example, see Jensen (2010)), our informational treatment may not have been intensive enough to change girls' behavior.

$p < 0.05$), and reduces the likelihood of becoming pregnant over the study period by 1.7 percentage points (45%, $p < 0.10$). When the measures are combined into indices, negotiation increases the human capital index by 0.110 standard deviations ($p < 0.01$) and the full index by 0.108 standard deviations ($p < 0.01$). Similarly, the average effect size for the human capital outcomes is 0.104 standard deviations ($p < 0.01$) and for the full set of outcomes is 0.101 standard deviations ($p < 0.01$).[19] The fact that we identify strong effect sizes for the aggregate measures – particularly the average effect sizes – again suggests that these results are not driven by multiple hypothesis testing. In fact, the p-value for the average effect size of the components of the human capital index is so small (0.00002), that this measure would pass the conservative Bonferroni correction for multiple hypothesis testing, which stipulates using a significance threshold of α divided by the number of tests, where α is desired significance level. This correction indicates that the cut-off for 5% significance across the 22 tests in tables 8 and 9 would be 0.002.[20]

Table 9 also indicates that, among this sample, the safe space treatment also had a positive and significant effect about one-half the size of the negotiation effect. However, for both the average effect sizes and indices in human capital, we can reject that the safe space and negotiation effects are the same. While safe space may have a positive effect on girls' outcomes, perhaps through an empowerment effect, our findings here and in the investment game suggest that negotiation affects girls' outcomes through other mechanisms beyond empowerment.

There are two possible explanations for why the effect sizes in table 9 are larger and more precise than those in table 8. First, there could be differential attrition by negotiation status, leading to biased estimates in table 9. Second, negotiation could have heterogeneous effects and have larger effects for the selected sample that attended the investment game. In appendix table A3, we examine which characteristics predict selection into the midline and how these characteristics interact with negotiation status. Negotiation alone has no significant effect on attending the midline. The interactions between negotiation and other characteristics do suggest that living with a biological parent and living with both parents are less positively predictive of negotiation girls attending the midline. However, these are 2 of 15 interaction coefficients of interest and if anything, indicate that negotiation girls who appear in the midline may be negatively selected relative to non-negotiation girls. This

[19] Since the average effect size estimates are typically similar in magnitude and are as or more statistically significant as the indices of outcomes, in the remaining tables we only report the outcomes with the indices rather than both the indices and the average effect sizes.

[20] We note that this correction is very conservative because its cut-off p-value will achieve a type 1 error rate below α if the tests are not independent of one another. In our setting, tests of the effect of negotiation on different measures of human capital are unlikely to be independent of one another, and tests that aggregate across all the outcomes, like the indices and average effect sizes, are not independent by design.

would lead to negative rather than positive omitted variable bias. Thus, we find the first explanation for the different results across the two samples unlikely.

Nonetheless, to further rule out the possibility of omitted variable bias, we follow the procedure of Heckman (1979) to correct the estimates in table 9 for selection bias. If girls and their parents agreed to participate in the investment game, they were assigned a day to show up for the game. Among the group who agreed to participate, whether it rained on the day that a girl and her parent or guardian were assigned to show up is a significant predictor of attendance. Therefore, we use whether it rained as an instrument for appearing in the sample. Appendix table A7 reports the selection-corrected estimates. Column (1) of appendix table A7 verifies that an indicator variable for whether it rained predicts attendance: rain reduced the likelihood of attendance by almost 9 percentage points ($p < 0.01$). The remaining columns report the estimates of the effects of negotiation and safe space on daughters' outcomes. The selection-corrected effects of negotiation are even larger and more significant than before, with negotiation increasing the human capital index by 0.121 standard deviations ($p < 0.01$) and the full index by 0.115 standard deviations ($p < 0.01$), consistent with the idea that any omitted variable bias is negative.

These results suggest that the differences between the effect sizes for the full sample and the investment game-only sample are driven by heterogeneous effects rather than omitted variable bias. Given the positive selection of the investment game-only sample, this may suggest that negotiation has larger effects on girls who are already somewhat more advantaged. In the next section, we further explore the drivers of these heterogeneous effects.

7 Sources of Heterogeneity

Given our findings in section 6, we expect that there is important heterogeneity in the effects of negotiation on girls' outcomes. To search for this heterogeneity in a principled way, we draw upon the machine-learning, honest causal tree methodology proposed by Athey and Imbens (2016). This procedure requires splitting our small data set so that separate samples are used to partition the data set (training data) and to estimate the treatment effects and confidence intervals (estimation data). The intuition behind the causal tree methodology is that, by further splitting the training sample used to identify the partitions (already one-half of our sample), we can select the partitions in one part of the training sample that maximize the out-of-sample predictive power for the other part of the training sample. This ensures that our point estimates are unbiased (Athey and Imbens, 2016), though at the loss of statistical power.

Since many of our potential covariates for this procedure are highly correlated with one-

another (such as measures of whether a girl lives with her father or mother, or measures of a girl's ability in English and Nyanja) and splitting the sample means that the intersections of these covariates could result in very small cells, we collapse some of our covariates down to indices. We create a "relatedness" or altruism index by estimating the first factor from a factor analysis of the indicator variables for a girl living with her biological father, a girl living with her biological mother, both parents being alive, and parents paying a girl's fees in the pre-treatment period. We also create an "ability index" by estimating the first factor from a factor analysis of the indicator variables for reading Nyanja excellently, speaking Nyanja excellently, reading English excellently, speaking English excellently, reading Nyanja well, speaking Nyanja well, reading English well, and speaking English well. The relatedness factor explains 91% of the variation in the relatedness variables, and the ability factor explains 86% of the variation in the ability variables.

We then randomly split our sample in half, and use half the sample (the training sample) to build a causal tree to search for heterogeneous effects of negotiation on the human capital index. We consider partitions of the data using the ability index, the altruism index, bride price status (motivated by Ashraf et al. (2016), who find that bride price positively affects female education in Zambia), and age. Ultimately, the causal tree suggests that negotiation has heterogeneous effects by ability, with the strongest effects for those whose scores on the ability factor are above 0.45. This cut point corresponds to the 60th percentile of the distribution. However, we caution that there is likely to be other heterogeneity in the effects of negotiation, which we may not have the statistical power to identify.

In table 10, we estimate the effects of negotiation on the human capital and full indices using both the estimation sample (columns (2) and (4)), which is distinct from the sample used to build the causal tree, and therefore yields valid confidence intervals, and the full sample (columns (1) and (3)). Consistent with the heterogeneity identified in the training sample, we find that negotiation has large effects in the group with ability above 0.45 (estimates of 0.106-0.131 standard deviations on the human capital index). In contrast, the treatment has smaller and not consistently significant effects at other ability levels. Since higher ability girls are more likely to attend the investment game (see appendix table A3), this result may explain the differences in the effects estimated in tables 8 and 9.

Interestingly, the heterogeneity we identify is also strongly consistent with the model we proposed in section 4. The model generates the following intuitive prediction.

Prediction 2. Increasing τ from τ^{NoNeg} to $\tau^{Neg} > \tau^{NoNeg}$ will only change educational outcomes for girls with returns to education $\frac{f_s-(1-\delta)\tau^{NoNeg}}{\delta} > R_i > \frac{f_s-(1-\delta)\tau^{Neg}}{\delta}$.

Proof. See appendix C. □

Figure 4 illustrates the intuition for the prediction. Figure 4 plots the distribution of the returns to education (R_i), which vary with ability. When $\tau = \tau^{NoNeg}$, all girls with ability $R_i > R^*$ are educated. The increase in τ to τ^{Neg} reduces the minimum ability a girl needs to be educated to $R^{**} < R^*$. As a result, the share of girls who are educated grows, but education outcomes are only affected for those girls who have returns to education between R^{**} and R^*.

This prediction just formalizes the intuition that only girls with returns to education *almost* high enough that they would be educated without the negotiation treatment will have their educational outcomes change due to negotiation. Even if τ is relatively high, parents will not educate very low ability girls for whom the cost of education still outweighs the returns. Therefore, we expect the negotiation treatment to affect girls who are on the margin of being educated. From the 2012 DHS, we calculate that a girl who completes 8th grade (all girls in our sample enrolled in 8th grade), has a 61% chance of completing 10th grade and a 44% chance of completing 12th grade. This indicates that girls at the 56th percentile – almost exactly the group our machine learning exercise identifies as benefiting the most from the negotiation treatment – are those on the margin of continuing in school. This suggests that negotiation skills increased education where efficiency gains were the highest by increasing education for girls with relatively high returns from school.

Alternatively, we might see this heterogeneous effect of negotiation on the human capital and full indices if only high ability girls are able to learn negotiation skills from the training. In appendix table A8, we test whether this is the case by estimating the heterogeneous effects of the negotiation training on knowledge of negotiation in the estimation sample for low and high ability girls. As the results in appendix table A8 show, there is no differential effect of the negotiation training on knowledge by ability, suggesting that this alternative explanation does not drive the results in table 10. Additionally, in appendix table A9, we test whether we find evidence of the same heterogeneity by ability in the effects of negotiation among the girls in the communication treatment of the trust game. If the heterogeneity in the administrative outcomes is driven by which girls are at the margin of receiving more education, we would not expect to see the same heterogeneity and indeed, we do not find that girls in upper part of the ability distribution were sent relatively more tokens.

8 Evidence on Spillovers

One concern is that, while negotiation made the treated girls better off, if parents have limited resources to invest in education, it may have made untreated children worse off. Similarly, the negotiation treatment could have led teachers to devote more resources to treated girls

at the expense of their classmates. The design of our experiment makes it difficult to test for negative spillovers because the characteristics of the pure control and treatment schools were not balanced and because we did not collect administrative data on siblings' outcomes. However, we are able to provide some evidence on spillovers using three different methods. First, we use propensity score matching to match the pure control and treated students and estimate the effects of being in a treatment school on the administrative outcomes of the girls who did not receive safe space or negotiation.[21] Second, we use data on grade 9 male drop out rates in the Zambian school census from 2001-2014 and a difference-in-differences methodology to estimate the effect of being a treated school in 2014 on male drop out. Finally, we estimate the effects of negotiation on siblings' outcomes that were reported by parents in the midline to see if parents treat siblings differently or expect them to finish fewer years of schooling.

Table 11 reports the results of these tests. In columns (1) and (2), we regress the human capital and full index on indicator variables for the negotiation treatment, safe space treatment, and for being in a treated school, which captures the spillover effect. In both cases, we find no significant spillovers. However, we do replicate our estimates of the negotiation treatment effect, though the coefficients are less precisely estimated. Column (3) reports the effect of being a negotiation school on male drop out rates from the difference-in-differences regression and again, the effect is small and insignificant.

The remaining columns of table 11 estimate the effect of negotiation on parents' responses to questions about the closest male and female sibling in age to the treated girl in the midline. Parents were given 20 tokens that represented their resources and were asked how they would divide them between the treated girl, her female sibling, and her male sibling. In column (4), the outcome is the number of tokens the parent allocated to the male sibling, and in column (5) it is the number she or he allocated to the female sibling. In both cases, negotiation had no effect on the allocation of the tokens. Parents were then asked how much time the male and female sibling spent doing chores and spent doing school work. Columns (6), (7), and (8) show that negotiation had no effect on the time the siblings spent doing chores and did not affect the amount of time the male sibling spent doing school work. Column (9) shows that negotiation had a marginally significant, positive effect on the amount of time the female sibling spent doing school work. Parents were also asked if, after the intervention,

[21]Since pregnancy and enrollment were not collected in the pure control schools after term 2 of grade 9, we only look at these outcomes before term 2 of grade 9. Additionally, since attendance data is not typically collected by schools outside of the context of the study, the collection of the data for this project was extremely heterogeneous across schools. In fact, missing attendance data at the school-level is not balanced between the pure control and treated schools. For these reasons, we do not include the attendance measure in these regressions, where we cannot control for school fixed effects. In contrast, other measures, such as fee payment and national exam scores, are typically collected administratively by schools and are rarely missing.

they were more likely to pay girls' school fees relative to boys. In column (9), we do find that negotiation significantly increased the likelihood that they answered "yes" to this question. Finally, parents were asked, given the obstacles they faced, how many years of schooling they expected the male and female sibling to complete. Columns (11) and (12) reveal that the negotiation treatment had no effect on the number of years parents expected the siblings to complete.

Overall, these results suggest that the negotiation program did not have strong negative spillovers on either the treated girls' siblings or their classmates. We only find evidence of spillovers on two outcomes (time spent on school work and whether parents would pay boys' school fees), and in the school work case, the spillover is positive. While parents reported they were relatively less likely to pay boys' school fees over girls', the question was asked in a leading way (specifically mentioning the girl's participation in the program) and parents did not expect male siblings to complete fewer years of schooling. At first, it may seem surprising that increased educational investment in the treated girl did not negatively affect her siblings. However, that might occur if either the increased investment came out of parents' consumption or if the girls and their parents were able, following the tenants of the negotiation curriculum, to arrive at solutions that increased overall family welfare.

9 Conclusion

In this paper, we study the effect of negotiation and interpersonal skills on female education in Zambia, a context where – as in much of sub-Saharan Africa – female secondary school enrollment is low. To study the effects of negotiation, we provided a randomly chosen group of Zambian 8th graders with negotiation skills training. Then, to understand how negotiation can affect girls' outcomes, we invited the girls to participate in a lab-in-the-field investment game with a parent that simulates the educational investment decision in a setting where there is incomplete contracting between parents and children. Additionally, we collected data on girls' longer-term educational outcomes, consisting of whether parents pay school fees, attendance rates, school enrollment, national exam scores, and whether girls took the national exam. We find that negotiation skills increase educational investment, particularly for higher ability girls who are likely on the margin of enrolling in and completing secondary school. The results of our investment game suggest that negotiation increases girls' educational attainment by allowing them to promise to transfer more of the gains from education to their parents. These transfers do not need to occur later in life and could come from a girl's behavior today. For instance, we find some evidence that parents find it easier to get girls in the negotiation treatment to do chores and view these girls as more respectful.

Our paper adds to an important literature on intra-household bargaining. This literature has largely focused on bargaining problems between spouses, but bargaining between parents and children over human capital investment is just as important given the potentially high returns to education (Duflo, 2001). In standard spousal bargaining models such as Chiappori (1992), the efficient action is always undertaken and spouses merely bargain over the surplus. In contrast, when imperfectly altruistic parents make human capital investment decisions for children who cannot commit to repay them, the efficient outcome may not be achieved (Banerjee, 2004; Becker et al., 2016; Ashraf et al., 2016; Bau, 2016). Therefore, negotiation skills can play an important role by helping parents and children achieve a more efficient, pareto-improving outcome. Moreover, we find that this incomplete contracting problem can be partially resolved through a simple in-school intervention.

Additionally, this paper complements the growing literature on how non-cognitive skills improve long-term outcomes. We provide – to our knowledge – the first estimates of the educational returns to a specific type of non-cognitive skill, interpersonal skills. By focusing on interpersonal skills, which develop in adolescence (Choudhury et al., 2006), instead of other non-cognitive skills, which are typically best influenced when children are very young, we discover an opportunity to influence non-cognitive skills in the school system. Since it is arguably easier to intervene within schools than within households, this finding may inform policies that aim to increase students' non-cognitive skills.

Going back to Nash's seminal paper on bargaining, one of the clearest ways to increase an individual's share in the bargaining surplus is to increase that individual's outside option if bargaining breaks down (Nash, 1950). However, increasing a child's outside option may be impossible. In contrast, if a household is not at the efficient frontier, and the parental human capital investment decision is not a zero-sum game, then there is an opportunity to increase a daughter's welfare (and her parents') without improving the daughter's outside option. Guided by relational contracting models (for example, Chassang (2010)), we show that this is the case. We provide evidence both that there is a policy opportunity to facilitate welfare-improving human capital investments and that teaching girls negotiation skills can help achieve this outcome. Finally, we speculate that these negotiation skills may also help facilitate more efficient decisions when there are incomplete contracting problems between spouses (as may be the case in Udry (1996)).

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Figures

Figure 1: Experimental Time Line

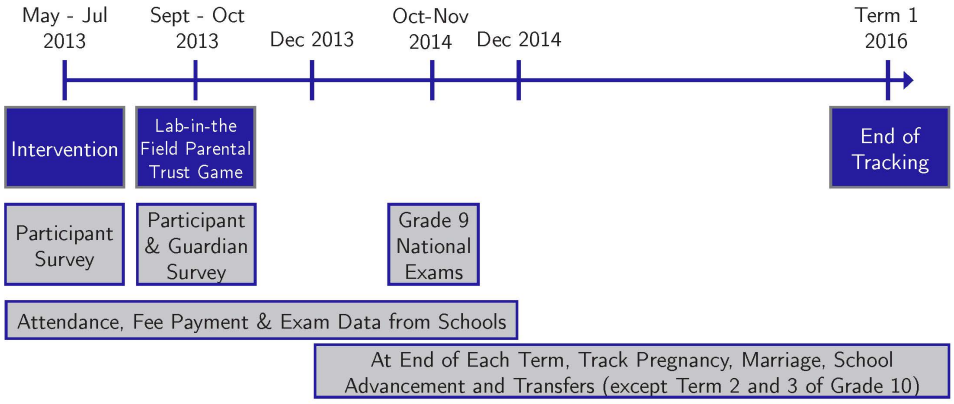


Figure 2: Negotiation Curriculum

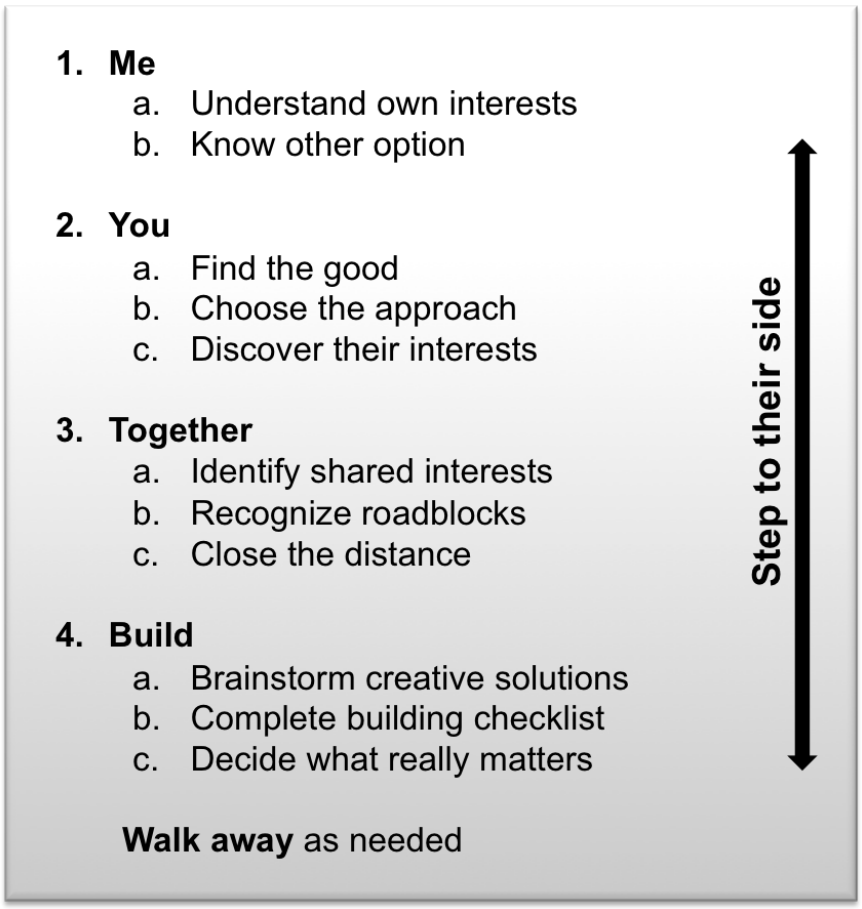


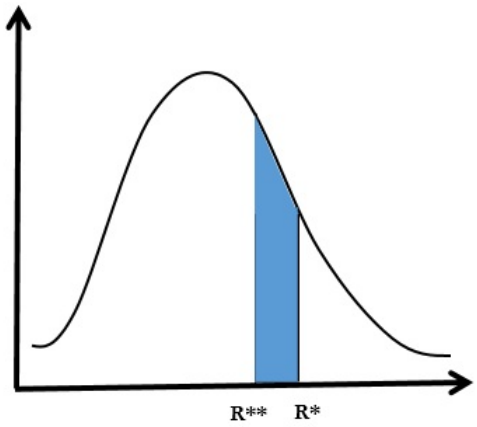
Figure 3: Prizes from the Chuck E. Cheese's Store







Figure 4: Who Benefits From an Increase in τ?



Tables

Table 1: Summary Statistics

	(1) Mean	(2) SD
Both Parents Alive	0.737	0.440
Live With Bio Dad	0.548	0.498
Live With Bio Mom	0.701	0.458
Live With Mom and Dad	0.499	0.500
Parents Pay Fees	0.763	0.425
Read Nyanja Excellently	0.399	0.490
Speak Nyanja Excellently	0.480	0.500
Read English Excellently	0.697	0.459
Speak English Excellently	0.412	0.492
Bride Price	0.249	0.432
Age	14.419	1.461
Read Nyanja Well	0.637	0.481
Speak Nyanja Well	0.885	0.320
Read English Well	0.899	0.301
Speak English Well	0.789	0.408

This table reports summary statistics collected during the baseline survey for the girls from the 29 treatment schools who participated in the experiment.

Table 2: Balance of Characteristics by Negotiation Treatment

	(1) **Mean**	(2) **Coefficient**	(3) **SE**	(4) **N**
Both Parents Alive	0.732	-0.018	0.017	2,254
Live With Biological Father	0.544	-0.014	0.022	2,254
Live With Biological Mother	0.689	0.008	0.019	2,254
Live With Both Parents	0.493	-0.018	0.020	2,254
Parents Pay School Fees	0.754	0.023	0.015	2,249
Read Nyanja Excellently	0.396	-0.048**	0.017	2,254
Speak Nyanja Excellently	0.482	-0.047*	0.027	2,254
Read English Excellently	0.692	-0.021	0.018	2,254
Speak English Excellently	0.409	-0.025	0.018	2,254
Bride Price	0.278	-0.012	0.021	2,127
Age	14.447	0.046	0.053	2,254
Read Nyanja Well	0.631	-0.016	0.021	2,254
Speak Nyanja Well	0.885	-0.003	0.017	2,254
Read English Well	0.896	-0.005	0.012	2,254
Speak English Well	0.785	-0.022	0.018	2,254

This table reports tests of the within-school randomization of the negotiation treatment. Each row is a regression of a child/household characteristic on a indicator for whether the girl was included in the negotiation treatment, controlling for school fixed effects. Standard errors are heteroskedasticity robust.

Table 3: Effect of Negotiation Treatment on Parents' Behavior in the Investment Game

	(1) **Tokens Sent**	(2) **Tokens Sent**	(3) **Tokens Sent**	(4) **Tokens Sent**
Communication × Negotiation			0.785***	0.783***
			(0.263)	(0.263)
Communication × Safe Space			0.450*	0.445*
			(0.263)	(0.263)
Negotiation	0.390	-0.086	-0.469***	-0.467***
	(0.275)	(0.130)	(0.180)	(0.180)
Safe Space	0.271	-0.176	-0.397**	-0.396**
	(0.252)	(0.131)	(0.181)	(0.181)
Communication			-0.092	-0.087
			(0.194)	(0.194)
Word Game				-0.099
				(0.104)
Sample	DG Only	IG Only	IG Only	IG Only
Number of observations	297	1,224	1,224	1,224
Adjusted R^2	0.085	0.019	0.033	0.033

This table reports the effects of the negotiation treatment on parents' behavior in a lab-in-the-field investment game. In the dictator game (DG, column 1), parents decided how many tokens to send to their daughters and the tokens were doubled (plus a random component), but daughters did not return tokens to their parents. In other versions of the game (IG), parents decided how many tokens to send to daughters, and tokens sent to daughters were doubled (plus a random component). Daughters then decided how many tokens to return to guardians. In the communication treatment, daughters were allowed to communicate with guardians before guardians sent the tokens. In the word game treatment, the tokens were only doubled if the girl had found at least half the words in a word game. Column 1 only includes girls who participated in the dictator game. The remaining columns only include those who participated in the investment game. All columns include the information treatment, ethnicity fixed effects, school fixed effects, and socioeconomic controls, consisting of variables for both parents alive, lives with the biological father, lives with the biological mother, lives with both mother and father, parents were paying school fees in the pre-treatment period, reading Nyanja excellently, speaking Nyanja excellently, reading English excellently, speaking English excellently, reading Nyanja well, speaking Nyanja well, reading English well, and speaking English well. Standard errors are heteroskedasticity robust.

Table 4: How Girls Spent the Tokens

	(1) **School Supplies**	(2) **In Between**	(3) **Pure Consumption**	(4) **School Supplies**	(5) **In Between**	(6) **Pure Consumption**	(7) **School Supplies**	(8) **In Between**	(9) **Pure Consumption**
Negotiation	-0.324	-0.144	0.486	0.029	-0.123	0.115	-0.132	-0.406**	0.575**
	(0.495)	(0.436)	(0.516)	(0.160)	(0.151)	(0.189)	(0.207)	(0.201)	(0.255)
Safe Space	-0.858*	-0.416	1.243**	-0.073	-0.002	0.094	-0.183	-0.117	0.342
	(0.474)	(0.415)	(0.495)	(0.158)	(0.154)	(0.190)	(0.206)	(0.212)	(0.260)
Communication							-0.021	-0.193	0.226
							(0.239)	(0.219)	(0.275)
Negotiation × Communication							0.329	0.577*	-0.939**
							(0.323)	(0.299)	(0.378)
Safe Space × Communication							0.224	0.232	-0.501
							(0.313)	(0.310)	(0.380)
Total Tokens	0.345***	0.161***	0.497***	0.264***	0.207***	0.523***	0.260***	0.203***	0.531***
	(0.055)	(0.046)	(0.058)	(0.024)	(0.023)	(0.030)	(0.024)	(0.023)	(0.030)
Sample	DG Only	DG Only	DG Only	IG	IG	IG	IG	IG	IG
Number of observations	297	297	297	1,221	1,221	1,221	1,221	1,221	1,221
Adjusted R^2	0.226	0.056	0.333	0.208	0.095	0.356	0.208	0.096	0.359

This table reports the effect of the negotiation, safe space, and communication treatments on how girls spent their tokens at the "Chuck-E-Cheese" store. School supplies is the total spending on school supplies (colored pens, math books, notebooks, pencils, erasers, rulers, and pencil sharpeners). Pure consumption is the total spending on hair ties, scarves, bracelets, lip balm, lollipops, biscuits, jiggies, and snakes and ladders games. In Between is the total spending on sanitary pads and socks. All columns include controls for the information treatment, ethnicity fixed effects, school fixed effects, and socioeconomic controls, consisting of variables for both parents alive, lives with the biological father, lives with the biological mother, lives with both mother and father, parents were paying school fees in the pre-treatment period, reading Nyanja excellently, speaking Nyanja excellently, reading English excellently, speaking English excellently, reading Nyanja well, speaking Nyanja well, reading English well, and speaking English well. Standard errors are heteroskedasticity robust.

Table 5: The Effect of Negotiation and Communication on a Daughter's Propensity to Return an Additional Token to her Parent

	(1) **Tokens Returned**	(2) **Tokens Returned**
Negotiation × Num. Tokens × Communication	0.653**	0.836**
	(0.305)	(0.415)
Num. Tokens × Communication	-0.340*	-0.536
	(0.199)	(0.361)
Negotiation × Num. Tokens	-0.355*	-0.554
	(0.209)	(0.347)
Num. Tokens	0.566***	0.774**
	(0.170)	(0.337)
Safe Space × Num. Tokens		-0.339
		(0.386)
Safe Space × Num. Tokens × Communication		0.319
		(0.418)
Number of observations	1,219	1,219
Adjusted R^2	0.167	0.127
Pass-Through Rates for Girls in Communication Investment Game		
Control Pass Through	0.226**	0.238*
	(0.099)	(0.134)
Negotiation Pass Through	0.524***	0.520***
	(0.174)	(0.173)
Safe Space Pass Through		0.219
		(0.141)

The top panel of this table reports the effects of the negotiation treatment on daughters' propensity to return additional tokens to parents in the lab-in-the-field investment game. We instrument for the daughter's number of tokens with an indicator variable for whether a daughter randomly received a high income shock before deciding how many tokens to return to her parent (a shock of 4 vs. 2). Column (1) includes controls for negotiation, safe space, communication, the daughter's number of tokens and its interactions with negotiation, the communication game, and the triple interaction of the daughter's number of tokens, negotiation, and the communication game. Column (2) adds the double and triple interactions between safe space, communication, and the daughter's number of tokens. All columns include the information treatment, ethnicity fixed effects, school fixed effects, and socioeconomic controls, consisting of variables for both parents alive, lives with the biological father, lives with the biological mother, lives with both mother and father, parents were paying school fees in the pre-treatment period, reading Nyanja excellently, speaking Nyanja excellently, reading English excellently, speaking English excellently, reading Nyanja well, speaking Nyanja well, reading English well, and speaking English well. Standard errors are heteroskedasticity robust. The bottom panel reports the pass through of giving a daughter an extra token to parents in the communication version of the investment game, calculated using the regression coefficients.

Table 6: Effect of Negotiation Treatment on Daughters' Outcomes in the Investment Game

	(1) **Daughter Total**	(2) **Daughter Total**	(3) **Daughter Total**
Communication × Negotiation		0.954**	0.953**
		(0.478)	(0.478)
Communication × Safe Space		0.239	0.231
		(0.484)	(0.484)
Negotiation	-0.073	-0.537*	-0.535*
	(0.235)	(0.306)	(0.307)
Safe Space	-0.402*	-0.524*	-0.523*
	(0.241)	(0.308)	(0.308)
Communication		0.364	0.373
		(0.343)	(0.345)
Word Game			-0.153
			(0.193)
Number of observations	1,219	1,219	1,219
Adjusted R^2	0.022	0.037	0.037

This table reports the effects of the negotiation treatment on daughters' outcomes in a lab-in-the-field investment game. In the investment game, parents decided how many tokens to send to daughters, and coins sent to daughters were doubled (plus a random component). Daughters then decided how many tokens to return to guardians. In the communication treatment, daughters were allowed to communicate with guardians before guardians sent the tokens. In the word game treatment, the tokens were only doubled if the girl had found at least half the words in a word game. All columns include the information treatment, ethnicity fixed effects, school fixed effects, and socioeconomic controls, consisting of variables for both parents alive, lives with the biological father, lives with the biological mother, lives with both mother and father, parents were paying school fees in the pre-treatment period, reading Nyanja excellently, speaking Nyanja excellently, reading English excellently, speaking English excellently, reading Nyanja well, speaking Nyanja well, reading English well, and speaking English well. Standard errors are heteroskedasticity robust.

Table 7: Additional Evidence on the Mechanisms

	(1) **Ask For More Food**	(2) **Difficulty Getting to do Chores**	(3) **Difficulty Controlling Temper**	(4) **Girl is Rude**	(5) **Girl is Respectful**	(6) **Natural Ability**	(7) **Natural Ability**
Negotiation	0.077***	-0.037*	0.008	-0.034	0.082**	-0.095	-0.092
	(0.029)	(0.021)	(0.010)	(0.065)	(0.041)	(0.065)	(0.065)
Safe Space	0.015	-0.020	-0.011	-0.033	0.049	-0.105	-0.097
	(0.029)	(0.021)	(0.008)	(0.065)	(0.041)	(0.067)	(0.067)
Negotiation × Ability							-0.068
							(0.077)
Safe Space × Ability							-0.116
							(0.079)
Ability						0.620***	0.683***
						(0.206)	(0.214)
Number of observations	1,573	1,476	1,484	1,477	1,477	1,473	1,473
Adjusted R^2	0.021	0.051	0.005	0.018	0.036	0.095	0.095

This table reports the effect of negotiation and safe space on other outcomes in the midline survey. The outcome in column 1 is coded as 1 if parents report girls asking for more food and 0 otherwise. The outcome in column 2 is coded as 1 if parents reported difficulty getting girls to do chores and 0 otherwise. The outcome in column 3 is coded as 1 if parents reported that the girl has difficulty controlling her temper and 0 otherwise. The outcome in column 4 is coded as 1 if parents say the girl is rude and 0 otherwise. The outcome in column 5 is coded as 1 if parents say the girl is respectful and 0 otherwise. In columns 6 and 7, the outcome is the parent's ranking of a girl's ability relative to her classmates from 0-4. All columns include the information treatment, ethnicity fixed effects, school fixed effects, and socioeconomic controls, consisting of variables for both parents alive, lives with the biological father, lives with the biological mother, lives with both mother and father, parents were paying school fees in the pre-treatment period, reading Nyanja excellently, speaking Nyanja excellently, reading English excellently, speaking English excellently, reading Nyanja well, speaking Nyanja well, reading English well, and speaking English well. Standard errors are heteroskedasticity robust.

Table 8: Effect of Negotiation on Outcomes in Administrative Data

Panel A. Human Capital Index

	(1) **Human Capital Index**	(2) **Human Capital AES**	(3) **Paid All Fees**	(4) **Took National Exam**	(5) **Threshold Math**	(6) **Threshold English**	(7) **Attendance Rate**	(8) **Enrollment Rate**
Negotiation	0.057**	0.062***	0.012	0.017	0.035	0.025	0.008	0.026**
	(0.026)	(0.019)	(0.018)	(0.016)	(0.026)	(0.023)	(0.005)	(0.011)
Safe Space	0.018	0.021	0.002	0.011	0.005	-0.024	0.007	0.025*
	(0.023)	(0.018)	(0.020)	(0.013)	(0.022)	(0.018)	(0.005)	(0.012)
Number of observations	2,059		2,082	2,112	2,112	2,112	2,107	2,122
Adjusted R^2	0.046		0.004	0.016	0.003	0.056	0.015	0.037

Panel B. Full Index and Pregnancy

	(1) **Full Index**	(2) **Full AES**	(3) **Ever Pregnant**
Negotiation	0.053*	0.060***	-0.009
	(0.026)	(0.019)	(0.018)
Safe Space	0.024	0.025	-0.009
	(0.024)	(0.017)	(0.012)
Number of observations	2,059		2,122
Adjusted R^2	0.050		0.017

This table reports estimates of the effect of the negotiation and safe space experimental treatments on outcomes collected in the administrative data. **Panel A:** In column (1), the outcome is a human capital index constructed by standardizing each of the outcomes in columns (3)-(8) and taking their average. In column (2), the effect sizes are the average effect sizes from columns (3)-(8). In column (3), the outcome is an indicator variable equal to 1 if parents paid 9th grade school fees and 0 otherwise. In column (4), the outcome is an indicator variable equal to 1 if the student took the national exam at the end of grade 9. In column (5), the outcome is 1 if the student received greater than the 75th percentile on the national exam in math and 0 otherwise and in column (6), it is the same for English. In column (7), the outcome is the students' average post-treatment attendance rate in grade 8 and terms 1 and 2 of grade 9. In column (8), the outcome is the student's average enrollment rate in term 3 of grade 8 and terms 1 and 2 of grade 9. **Panel B:** In column (1), the outcome is the full index including the pregnancy dummy (with its sign flipped), and in column (2), it is the average effect size including pregnancy. In column (3), the outcome is an indicator variable equal to 1 if the student is ever observed to be pregnant in the post-treatment period. All columns in both panels include the information treatment, ethnicity fixed effects, school fixed effects, and socioeconomic controls, consisting of variables for both parents alive, lives with the biological father, lives with biological mother, lives with both mother and father, parents were paying school fees in the pre-treatment period, reading Nyanja excellently, speaking Nyanja excellently, reading English excellently, speaking English excellently, reading Nyanja well, speaking Nyanja well, reading English well, and speaking English well. Standard errors are heteroskedasticity robust.

Table 9: Effect of Negotiation on Girls' Outcomes in the Investment Game Sample

Panel A. Human Capital Index

	(1) **Human Capital Index**	(2) **Human Capital AES**	(3) **Paid All Fees**	(4) **Took National Exam**	(5) **Threshold Math**	(6) **Threshold English**	(7) **Attendance Rate**	(8) **Enrollment Rate**
Negotiation	0.110***	0.104***	0.022	0.036**	0.065**	0.037	0.010*	0.042***
	(0.030)	(0.023)	(0.018)	(0.017)	(0.031)	(0.031)	(0.005)	(0.014)
Safe Space	0.046	0.048**	0.023	0.022	0.026	-0.010	0.003	0.029**
	(0.029)	(0.021)	(0.026)	(0.019)	(0.023)	(0.021)	(0.005)	(0.014)
Number of observations	1,490		1,506	1,515	1,515	1,515	1,511	1,521
Adjusted R^2	0.045		0.006	0.014	0.003	0.047	0.021	0.031

Panel B. Full Index and Pregnancy

	(1) **Full Index**	(2) **Full AES**	(3) **Ever Pregnant**
Negotiation	0.108***	0.101***	-0.017*
	(0.026)	(0.021)	(0.010)
Safe Space	0.049	0.050***	-0.012
	(0.030)	(0.021)	(0.015)
Number of observations	1,490		1,521
Adjusted R^2	0.051		0.017

This table reports estimates of the effect of the negotiation and safe space experimental treatments on outcomes collected in the administrative data, restricting the sample to girls who attended the investment game. **Panel A:** In column (1), the outcome is a human capital index constructed by standardizing each of the outcomes in columns (3)-(8) and taking their average. In column (2), the effect sizes are the average effect sizes from columns (3)-(8). In column (3), the outcome is an indicator variable equal to 1 if parents paid 9th grade school fees and 0 otherwise. In column (4), the outcome is an indicator variable equal to 1 if the student took the national exam at the end of grade 9. In column (5), the outcome is 1 if the student received greater than the 75th percentile on the national exam in math and 0 otherwise and in column (6), it is the same for English. In column (7), the outcome is the students' average post-treatment attendance rate in grade 8 and terms 1 and 2 of grade 9. In column (8), the outcome is the student's average enrollment rate in term 3 of grade 8 and terms 1 and 2 of grade 9. **Panel B:** In column (1), the outcome is the full index including the pregnancy dummy (with its sign flipped), and in column (2), it is the average effect size including pregnancy. In column (3), the outcome is an indicator variable equal to 1 if the student is ever observed to be pregnant in the post-treatment period. All columns in both panels include the information treatment, ethnicity fixed effects, school fixed effects, and socioeconomic controls, consisting of variables for both parents alive, lives with the biological father, lives with biological mother, lives with both mother and father, parents were paying school fees in the pre-treatment period, reading Nyanja excellently, speaking Nyanja excellently, reading English excellently, speaking English excellently, reading Nyanja well, speaking Nyanja well, reading English well, and speaking English well. Standard errors are heteroskedasticity robust.

Table 10: Estimates of Negotiation Effects by Ability

	(1) **Human Capital Index** Full Sample	(2) **Human Capital Index** Estimation Sample	(3) **Full Index** Full Sample	(4) **Full Index** Estimation Sample
Negotiation × Low Ability	0.035	0.081*	0.036	0.081*
	(0.028)	(0.040)	(0.027)	(0.038)
Negotiation × High Ability	0.106**	0.131**	0.092**	0.124**
	(0.030)	(0.042)	(0.031)	(0.043)
Number of observations	2,059	1,021	2,059	1,021
Adjusted R^2	0.047	0.027	0.051	0.034

This table reports the effects of negotiation for individuals who scored less than 0.45 on the ability index (low ability) and those who score more than 0.45 (high ability). The ability index is the first predicted component of a factor analysis of indicator variables for reading Nyanja excellently, speaking Nyanja excellently, reading English excellently, speaking English excellently, reading Nyanja well, speaking Nyanja well, reading English well, and speaking English well. The cut-offs are determined using the honest causal tree methodology of Athey and Imbens (2016) on a randomly chosen training sample (one-half of the total sample) to find sources of heterogeneity in the negotiation treatment effect. The effects are estimated in both the full sample (columns (1) and (3)) and the estimation sample (columns (2) and (4)), which does not include the training sample. All columns include ethnicity fixed effects, school fixed effects, and socioeconomic controls, consisting of variables for both parents alive, lives with the biological father, lives with the biological mother, lives with both mother and father, parents were paying school fees in the pre-treatment period, reading Nyanja excellently, speaking Nyanja excellently, reading English excellently, speaking English excellently, reading Nyanja well, speaking Nyanja well, reading English well, and speaking English well, as well as indicator variables for being high ability. Standard errors are heteroskedasticity robust.

Table 11: Evidence on Spillovers

	(1) **Human Capital Index**	(2) **Full Index**	(3) **G9 Male Drop Out Rate**	(4) **Pile, Male**	(5) **Pile, Female**	(6) **Chores, Male**	(7) **Chores, Female**	(8) **Schoolwork, Male**	(9) **Schoolwork, Female**	(10) **Pay for Girls Over Boys**	(11) **Years Schooling, Male**	(12) **Years, Schooling Female**
Treated School	-0.034	-0.037	0.007									
	(0.059)	(0.051)	(0.018)									
Negotiation	0.054*	0.052*		0.047	0.024	-0.022	-0.011	0.039	0.139*	0.041***	0.017	-0.007
	(0.028)	(0.029)		(0.154)	(0.156)	(0.117)	(0.070)	(0.098)	(0.084)	(0.013)	(0.118)	(0.109)
Safe Space	-0.003	0.008		0.025	-0.169	0.037	0.048	0.069	0.112	0.017	-0.063	-0.005
	(0.024)	(0.025)		(0.152)	(0.157)	(0.117)	(0.074)	(0.094)	(0.085)	(0.011)	(0.118)	(0.110)
School FE	N	N	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Standard Controls	Y	Y	N	Y	Y	Y	Y	Y	Y	Y	Y	Y
Number of observations	2,731	2,731	26,301	1,160	1,205	1,203	1,166	885	945	1,476	1,094	1,148
Adjusted R^2	0.049	0.052	0.114	0.099	-0.016	0.006	-0.007	0.021	0.014	0.007	0.048	0.041

This table reports the results of tests for spillovers from the negotiation program. Columns 1 and 2 estimate the effects of negotiation and safe space and of being a control girl in a treated school by including the pure control schools in the regression. Treated and pure control girls were matched using propensity score matching and students for whom there was no match were not included in the regression. Column 3 estimates the effect of being in a negotiation school on the grade 9 boys' drop out rate using a difference-in-differences strategy on the Zambian school census data. In all three cases, standard errors are clustered at the school level. The remainder of the columns estimate the effects of negotiation and safe space on outcomes in the midline survey using our main specification. For columns 4 and 5, parents were asked to divide up 20 tokens to represent how they would allocate resources to the treated girl and her nearest male (column 4) and female siblings (column 5). In columns 6 and 7, parents were asked how much time the male and female siblings spent on chores. In columns 8 and 9, they were asked how much time they spent on school work. In column 10, they were asked if they were now more likely to pay girls' school fees over boys'. In columns 11 and 12, parents were asked how many years of schooling the male and female siblings were likely to attain. Standard controls include the information treatment, ethnicity fixed effects, school fixed effects, and socioeconomic controls, consisting of variables for both parents alive, lives with the biological father, lives with the biological mother, lives with both mother and father, parents were paying school fees in the pre-treatment period, reading Nyanja excellently, speaking Nyanja excellently, reading English excellently, speaking English excellently, reading Nyanja well, speaking Nyanja well, reading English well, and speaking English well. Standard errors for the midline outcomes are heteroskedasticity robust.

Appendix A: Information Treatment Appendix

This appendix describes the details of the information treatment. The information treatment provided during the baseline survey addressed the following points: the benefits for girls from staying in school, job opportunities in Zambia, options for families to finance education, HIV transmission, and HIV relative risk and prevention. In the education section of the information session, the discussion leader started the discussion by asking girls to think about ways in which education could help them in their lives. After a brain storming session, the leader provided information on the link between maternal education and health of the child, the positive effect of education on a woman's own health, and how education could improve a girl's ability to care for her family.

Following the section on the benefits of education for health, the girls engaged in another activity where they were asked to look through job advertisements in a newspaper and identify required education for the positions, as well as earnings. This was done to make opportunities that require a secondary school degree salient to the girls. This section concluded with information on organizations that offer scholarships and other forms of assistance for secondary school education.

The second part of the treatment focused on the prevention of HIV. The girls were first provided basic information on what HIV is, its prevalence in Zambia, ways to get tested for it, and how to cope with HIV. Then, the discussion leader asked girls to identify ways in which HIV could be transmitted from a list of behaviors and activities on a flip chart. This exercise was followed by explanations of abstinence and condom use. The session concluded with the discussion leader providing information on risky behaviors for contracting HIV, such as sexual contact with older men, who have a higher positive HIV rate, and having multiple partners.

Appendix B: Data Appendix

In this appendix, we discuss how we collected administrative data on participants' outcomes. Administrative data collection started in mid-2013 when participants were in grade 8, two weeks before the start of the intervention, and continued (in the case of pregnancy and enrollment) through 2016.

While the girls were enrolled in the sample schools in grades 8 and 9, collectors visited the schools twice in every academic term, at the beginning and at the end. At the end of each term, they collected attendance registers from the term and left the registers for the following term in advance, so that they could be used to collect attendance data in the first week of school. They also dropped off data forms for exam results, fee payment, and student status tracking, which they then collected at the start-of-term visit. In each school, a teacher was appointed as the "contact teacher," as a point of reference for our collectors and a mediator between the school administration, the collectors, and the class monitors. After the girls aged out of the sample schools, we continued to collect their enrollment and pregnancy data, as we detail below.

Attendance Data: Daily attendance records were not collected on a regular basis prior to the intervention, so our data collectors selected and trained pupils ("class monitors") to fill out attendance register forms that we provided. Recording started approximately two weeks before the baseline survey, on the same day the invitation letters for parents to participate in the experiment were delivered to the girls in school. Data collection covered grade 8 and terms 1 and 2 of grade 9.

Fee Payment Data: Data on payments were collected from school administrators for each term and each subject, starting in term 2 of grade 8. As before, the data collection covered grades 8 and 9.

Exam Data: At the end of grade 9, girls could take the national exam and decide whether to enroll in secondary school. In addition to the data we collected from the junior secondary schools, we also collected the girls' examination results for the grade 9 national exam, which is a high-stakes, standardized assessment, held in October-November 2014. The results of the national exam determine whether pupils can enroll in grade 10 and at which school. In order to facilitate the process of matching exam scores to participants, we collected examination numbers for all pupils prior to the exam in term 3 of 2014.

Enrollment and Pregnancy Data: Data on whether students were still enrolled in school and whether they had become pregnant were collected from school administrators at the end of year. Beginning in 2014 (term 1 of grade 9), we cross-checked this data with data collected by the class monitors. We also tracked whether participants in intervention schools enrolled in grade 10 and 11 by contacting the basic schools in our study sample, as well

as visiting upper secondary schools in the Lusaka area. Depending on the score from the grade 9 national exam, pupils are assigned to enroll in particular secondary schools. We first gathered information from their basic schools to determine whether our participants had enrolled in grade 10, and if so, at which secondary school. In order to confirm that our participants enrolled at a particular school, we visited the secondary schools they were assigned to throughout Lusaka and verified if they were indeed enrolled.

When pupils were found, they provided us with information on their peers' secondary enrollment statuses, as well as their own. We used this information to visit other secondary schools that were not listed by the basic schools within Lusaka and search for any participants from our intervention. If we found girls at these schools, we collected enrollment and pregnancy statuses. In 2016 we went back to the same secondary schools for additional robustness checks on data collected in 2015, as well as to collect information on girls' statuses in grade 11.

Appendix C: Mathematical Appendix

In this section, we prove predictions 1 and 2.

Prediction 1. Increasing τ will increase the share of girls who are educated.

Proof. We can write the minimum returns a girl needs to be educated as

$$R^* = \frac{f_s - (1-\delta)\tau}{\delta}.$$

If increasing τ reduces the minimum returns to education a girl needs to be educated, R^*, then increasing τ increases the share of girls who are educated. We can verify this by differentiating R^* with respect to τ:

$$\frac{\partial R^*}{\partial \tau} = \frac{\delta - 1}{\delta} < 0.$$

□

Prediction 2. Increasing τ from τ^{NoNeg} to $\tau^{Neg} > \tau^{NoNeg}$ will only change educational outcomes for girls with returns to education $\frac{f_s-(1-\delta)\tau^{NoNeg}}{\delta} > R_i > \frac{f_s-(1-\delta)\tau^{Neg}}{\delta}$.

Proof. From the proof of prediction 1, we see that when $\tau = \tau^{NoNeg}$, a girl will be educated iff

$$R^* = \frac{f_s - (1-\delta)\tau^{NoNeg}}{\delta},$$

and if $\tau = \tau^{Neg}$, a girl is educated iff

$$R^{**} = \frac{f_s - (1-\delta)\tau^{Neg}}{\delta}.$$

Since increasing τ increases the returns to parents of educating all girls, a girl's educational status only changes if she was not educated under τ^{NoNeg} and she is now educated under τ^{Neg}. Then, this is the case if

$$R^* > R_i > R^{**}.$$

□

Appendix Figures

Figure A1: Invitation to Participate in the Experiment





Dear Parents and Guardians of Grade 8 Girls,

<NAME> Primary School is partnering with Innovations for Poverty Action Zambia (IPA) for a research study and after-school program for Grade 8 girls, called "**Girls Arise!**" Girls will participate in different activities this year. Some girls will be able to participate in the program from <DATES>, while others will have the opportunity to participate in a second round later. The program provides a safe space for girls to meet after school, have lunch, and do activities for six sessions.

Informational meetings to explain more about the program and have you sign a permission form will take place at <NAME> Primary School at the following times:

- 14:00 hours - Friday, <MEETINGDATE1>, or
- 9:00 hours - Saturday, <MEETINGDATE2>, or
- 11:00 hours – Saturday, <MEETINGDATE3>

Refreshments and KR 20 reimbursement for transport will be provided for the parent or guardian of each girl attending the meeting. Whether or not you would like to learn more, please return the bottom portion of this form to the school by Thursday, <RETURNDATE>.

Please note that it is important the girl's PARENT or MAIN GUARDIAN (whoever makes household decisions affecting the girl) attend the meeting to give permission, and not someone else.

Yours faithfully,

<SCHOOL CONTACT NAME>, <TITLE>, <NAME> Primary School

----------Please return below portion to school by <RETURNDATE>----------

Name of Grade 8 girl: **<first_name> <last_name>** Class: **<class>**

____ YES – I am interested in learning more about the program, & will attend the parent meeting on *(Please circle 1)*

Friday, <MEETINGDATE1> at 14 hours	Yes
Saturday, <MEETINGDATE2> at 9 hours	Yes
Saturday, <MEETINGDATE3> at 11 hours	Yes

PHONE NUMBER (please provide so we can follow up with you regarding the meeting):

_____

____ NO – I do not want the girl to participate in this program.

_____ _____

Parent Signature & Name **Date**

Appendix Tables

Table A1: Comparison Between Intervention Schools and Other Zambian Schools

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Intervention Schools		**Urban Gov. Schools**			**Full Gov. Sample**			**Full Sample**		
	Mean	SD	Mean	SD	T-test	Mean	SD	T-test	Mean	SD	T-test
Number of G8 Male Students	124.391	54.838	83.452	51.443	0.000***	36.883	34.311	0.000***	34.706	34.096	0.000***
Number of G8 Female Students	113.870	39.577	84.794	55.447	0.010**	33.802	35.901	0.000***	32.592	35.812	0.000***
Total Teachers	54.261	10.627	45.372	15.090	0.004***	18.030	15.555	0.000***	17.796	14.932	0.000***
Special Ed	0.391	0.499	0.232	0.423	0.063*	0.126	0.332	0.000***	0.121	0.326	0.001***
Female Drop Out Rate	0.017	0.049	0.018	0.048	0.922	0.087	0.148	0.025**	0.077	0.141	0.024**
Male Drop Out Rate	0.0004	0.002	0.007	0.032	0.337	0.031	0.087	0.087*	0.028	0.082	0.085**
STR	42.231	10.646	35.928	14.824	0.046**	50.207	23.301	0.115	47.645	27.117	0.302
Male Toilets/Student	0.006	0.002	0.007	0.007	0.637	0.008	0.007	0.378	0.010	0.013	0.187
Female Toilets/Student	0.008	0.003	0.007	0.007	0.948	0.008	0.007	0.778	0.011	0.015	0.338
Has Power	1.000	0.000	0.933	0.251	0.186	0.312	0.463	0.000***	0.385	0.487	0.000***
Has Protected Well	0.000	0.000	0.063	0.243	0.203	0.119	0.324	0.076*	0.120	0.325	0.054**
Has Telephone	0.522	0.511	0.494	0.501	0.787	0.280	0.449	0.010**	0.353	0.478	0.027**
Has Unprotected Well	0.000	0.000	0.044	0.206	0.291	0.194	0.396	0.018**	0.171	0.376	0.018**
Regular Hours	5.609	0.783	5.457	1.408	0.596	5.162	1.745	0.218	5.188	1.942	0.236
Library Books/Student	0.753	0.675	0.805	2.004	0.903	0.984	1.807	0.556	1.580	4.586	0.317

This table reports summary statistics for the treatment schools (columns 1 and 2), all urban government schools in Zambia (columns 3 and 4), all government schools in Zambia (columns 6 and 7), and all schools in Zambia, including private and community schools (columns 9 and 10). Column 5 reports the p-value for a t-test of the difference in means for the intervention schools and all urban government schools. Column 8 reports the p-value for a t-test of the difference in means for the intervention schools and all government schools. Column 11 reports the p-value for a t-test of the difference in means for the intervention schools and all Zambian schools. The data comes from the 2011 Zambian census of schools.

Table A2: Effect of Negotiation Treatment on Knowledge of Negotiation

	(1) **Question 1**	(2) **Question 2**	(3) **Question 3**	(4) **Combined Questions**
Negotiation	0.736***	0.776***	0.767***	0.783***
	(0.094)	(0.114)	(0.134)	(0.080)
Safe Space	-0.068	-0.172	-0.117	-0.097
	(0.086)	(0.116)	(0.134)	(0.079)
Number of observations	1,523	1,569	1,569	1,515
Adjusted R^2	0.085	0.070	0.062	0.130

This table reports the effect of the negotiation treatment on girls' understanding of negotiation skills in the midline survey. Girls were asked how they would apply negotiation skills in a scenario that the curriculum had not directly discussed. The scenario was that a girl has to negotiate with her sister over who would watch their brother when she has to study for a test. The vignette was designed to test how girls would apply their negotiation skills rather than whether they had learned the terminology from the course. Performance on each of three open-ended questions was blindly graded between 1 and 7, with 7 indicating the highest score. All columns include ethnicity fixed effects, school fixed effects, and socioeconomic controls, consisting of variables for both parents alive, lives with the biological father, lives with the biological mother, lives with both mother and father, parents were paying school fees in the pre-treatment period, reading Nyanja excellently, speaking Nyanja excellently, reading English excellently, speaking English excellently, reading Nyanja well, speaking Nyanja well, reading English well, and speaking English well. Standard errors are heteroskedasticity robust.

Table A3: Association Between Girls' Characteristics and Attending the Investment Game

	Likelihood in Investment Game			
	Coeff.	SE	Coeff. for Interaction with Negotiation	SE
Negotiation	0.024	0.020		
Safe Space	0.015	0.020		
Both Parents Alive	0.049**	0.022	-0.061	0.045
Live With Bio Dad	0.051***	0.019	-0.079**	0.040
Live With Bio Mom	0.110***	0.022	-0.052	0.045
Live With Mom and Dad	0.082***	0.019	-0.084**	0.039
Parents Pay Fees	0.044*	0.023	-0.042	0.048
Read Nyanja Excellently	0.053***	0.019	-0.040	0.040
Speak Nyanja Excellently	0.029	0.019	-0.036	0.040
Read English Excellently	0.042**	0.021	0.018	0.043
Speak English Excellently	0.035*	0.020	0.017	0.040
Age	-0.024***	0.007	0.004	0.014
Read Nyanja Well	0.058***	0.020	-0.011	0.041
Speak Nyanja Well	0.017	0.031	0.012	0.063
Read English Well	0.068**	0.033	0.058	0.068
Speak English Well	0.037	0.024	0.049	0.04)
Bride Price	0.022	0.022	0.039	0.045

The first two columns of this table report the association between different daughter characteristics and an indicator variable for whether the daughter attended the investment game. The second two columns report the coefficient on the interaction between negotiation and the characteristic of interest, controlling for negotiation and the characteristic. Each cell of the table is a regression of the indicator variable for appearing for the game on a different characteristic or the interaction between negotiation and that characteristic, controlling for school fixed effects. The standard errors are heteroskedasticity robust.

Table A4: Effect of Word Game on Parents' Behavior in the Investment Game

	(1) **Tokens Sent**	(2) **Tokens Sent**
Negotiation	-0.506**	-0.539**
	(0.225)	(0.258)
Safe Space	-0.274	-0.376
	(0.224)	(0.254)
Communication	-0.092	-0.089
	(0.195)	(0.291)
Communication × Negotiation	0.783***	0.856**
	(0.263)	(0.383)
Communication × Safe Space	0.449*	0.659*
	(0.263)	(0.382)
Word Game dummy	-0.044	-0.042
	(0.189)	(0.266)
Word × Negotiation	0.078	0.146
	(0.258)	(0.355)
Word × Safe Space	-0.252	-0.037
	(0.264)	(0.363)
Communication × Word		-0.004
		(0.389)
Communication × Word × Negotiation		-0.142
		(0.512)
Communication × Word × Safe Space		-0.442
		(0.531)
Sample	IG Only	IG Only
Number of observations	1,224	1,224
Adjusted R^2	0.033	0.032

This table reports the effects of the negotiation treatment on parents' behavior in a lab-in-the-field investment game. In the investment game (IG), parents decided how many tokens to send to daughters, and tokens sent to daughters were doubled (plus a random component). Daughters then decided how many tokens to return to guardians. In the communication treatment, daughters were allowed to communicate with guardians before guardians sent the tokens. In the word game treatment, the tokens were only doubled if the the girl had found at least half the words in a word game. All columns include ethnicity fixed effects, school fixed effects, and socioeconomic controls, consisting of variables for both parents alive, lives with the biological father, lives with the biological mother, lives with both mother and father, parents were paying school fees in the pre-treatment period, reading Nyanja excellently, speaking Nyanja excellently, reading English excellently, speaking English excellently, reading Nyanja well, speaking Nyanja well, reading English well, and speaking English well. Standard errors are heteroskedasticity robust.

Table A5: Effect of Knowledge of Negotiation on Parents' Behavior in the Investment Game

	(1) **Tokens Sent**	(2) **Tokens Sent**	(3) **Tokens Sent**
Communication × Knowledge		0.389**	0.404**
		(0.198)	(0.202)
Communication × Negotiation		0.481**	0.480**
		(0.240)	(0.240)
Communication × Safe Space		0.152	0.146
		(0.239)	(0.239)
Knowledge	0.095	-0.065	-0.072
	(0.097)	(0.118)	(0.119)
Negotiation	-0.002	-0.199	-0.198
	(0.116)	(0.147)	(0.147)
Safe Space	-0.058	-0.126	-0.126
	(0.116)	(0.146)	(0.146)
Communication		-1.047*	-1.086*
		(0.607)	(0.619)
Word Game			-0.114
			(0.104)
Sample	IG Only	IG Only	IG Only
Number of observations	1,520	1,520	1,520
Adjusted R^2	0.028	0.036	0.036

This table reports the effects of knowledge of negotiation on parents' behavior in a lab-in-the-field investment game. All columns include the information treatment, ethnicity fixed effects, school fixed effects, and socioeconomic controls, consisting of variables for both parents alive, lives with the biological father, lives with the biological mother, lives with both mother and father, parents were paying school fees in the pre-treatment period, reading Nyanja excellently, speaking Nyanja excellently, reading English excellently, speaking English excellently, reading Nyanja well, speaking Nyanja well, reading English well, and speaking English well. Standard errors are heteroskedasticity robust.

Table A6: Effect of Information on Outcomes in Administrative Data

Panel A. Human Capital Index

	(1) **Human Capital Index**	(2) **AES**	(3) **Paid All Fees**	(4) **Took National Exam**	(5) **Threshold Math**	(6) **Threshold English**	(7) **Attendance Rate**	(8) **Enrollment Rate**
Information	-0.027	-0.018	-0.026	-0.005	0.001	-0.003	0.003	-0.013
	(0.026)	(0.016)	(0.017)	(0.014)	(0.019)	(0.016)	(0.005)	(0.010)
Number of observations	2,059		2,082	2,112	2,112	2,112	2,107	2,122
Adjusted R^2	0.032		0.004	0.016	0.003	0.056	0.015	0.037

Panel B. Full Index and Pregnancy

	(1) **Full Index**	(2) **AES**	(3) **Ever Pregnant**
Information	-0.016	-0.016	-0.002
	(0.023)	(0.015)	(0.008)
Number of observations	2,059		2,122
Adjusted R^2	0.050		0.032

This table reports estimates of the effect of the information treatment on outcomes collected in the administrative data. **Panel A:** In column (1), the outcome is a human capital index constructed by standardizing each of the outcomes in columns (3)-(8) and taking their average, while column (2) computes the average effect size across those outcomes. In column (3), the outcome is an indicator variable equal to 1 if parents paid 9th grade school fees and 0 otherwise. In column (4), the outcome is an indicator variable equal to 1 if the student took the national exam at the end of grade 9. In column (5), the outcome is an indicator variable for if the student scored in the top 25th percentile on the national exam in math and column (6) is the same for English. In column (7), the outcome is the students' average post-treatment attendance rate in grades 8 and 9. In column (8), the outcome is the student's average enrollment rate in term 3 of grade 8 and terms 1 and 2 of grade 9. **Panel B:** In column (1), the outcome is the full index including the pregnancy dummy (with its sign flipped). In column (2), the outcome is the average effect size on the full set of outcomes. In column (3), the outcome is an indicator variable equal to 1 if the student is ever observed to be pregnant in the post-treatment period. All columns in both panels include controls for the negotiation and safe space treatments, ethnicity fixed effects, school fixed effects, and socioeconomic controls, consisting of variables for both parents alive, lives with the biological father, lives with the biological mother, lives with both mother and father, parents were paying school fees in the pre-treatment period, reading Nyanja excellently, speaking Nyanja excellently, reading English excellently, speaking English excellently, reading Nyanja well, speaking Nyanja well, reading English well, and speaking English well. Standard errors are heteroskedasticity robust.

Table A7: Selection-Corrected Estimates of the Effect of Negotiation in the Investment Game Sample

Panel A. Human Capital Index

	(1) **In Midline**	(2) **Human Capital Index**	(3) **Paid All Fees**	(4) **Took National Exam**	(5) **Threshold Math**	(6) **Threshold English**	(7) **Attendance Rate**	(8) **Enrollment Rate**
Rained	-0.086***							
	(0.029)							
Negotiation		0.121***	0.053*	0.029*	0.070**	0.050	0.014*	0.037**
		(0.035)	(0.031)	(0.017)	(0.032)	(0.032)	(0.008)	(0.017)
Safe Space		0.050	0.030	0.014	0.056	0.039	0.004	0.036**
		(0.036)	(0.031)	(0.017)	(0.038)	(0.038)	(0.008)	(0.017)
Number of observations	1,560	1,503	1,545	1,555	1,555	1,555	1,552	1,560
Adjusted R^2	0.029							

Panel B. Full Index and Pregnancy

	(1) **Full Index**	(2) **Ever Pregnant**
Negotiation	0.115***	-0.014
	(0.033)	(0.011)
Safe Space	0.048	-0.006
	(0.035)	(0.012)
Number of observations	1,503	1,560

This table reports estimates of the effect of the negotiation and safe space experimental treatments on outcomes collected in the administrative data. The sample is restricted to girls who appeared in the midline survey, and estimates have been corrected for selection using the methodology of Heckman (1979). An indicator variable for whether it rained on the day girls were assigned to come to the midline survey is used to predict whether girls showed up for the survey. **Panel A:** Column (1) estimates the effect of the instrument on whether a girl appears in the midline. The remaining columns report the selection-corrected estimates of the negotiation and safe space effects. In column (2), the outcome is a human capital index constructed by standardizing each of the outcomes in columns (3)-(8) and taking their average. In column (3), the outcome is an indicator variable equal to 1 if parents paid 9th grade school fees and 0 otherwise. In column (4), the outcome is an indicator variable equal to 1 if the student took the national exam at the end of grade 9. In column (5), the outcome is an indicator variable for whether a student scored in top 25th percentile on math in the national exam, and column (6) is an indicator variable for scoring in the top 25th percentile in English. In column (7), the outcome is the students' average post-treatment attendance rate in grades 8 and 9. In column (8), the outcome is the student's average enrollment rate in term 3 of grade 8 and terms 1 and 2 of grade 9. **Panel B:** In column (1), the outcome is the full index including the pregnancy dummy (with its sign flipped). In column 2, the outcome is an indicator variable equal to 1 if the student is ever observed to be pregnant in the post-treatment period. All columns in both panels include the information treatment, ethnicity fixed effects, school fixed effects, and socioeconomic controls, consisting of variables for both parents alive, lives with the biological father, lives with the biological mother, lives with both mother and father, parents were paying school fees in the pre-treatment period, reading Nyanja excellently, speaking Nyanja excellently, reading English excellently, speaking English excellently, reading Nyanja well, speaking Nyanja well, reading English well, and speaking English well. Standard errors are heteroskedasticity robust.

Table A8: Effect of Negotiation on Knowledge of Negotiation by Ability

	(1) **Question 1**	(2) **Question 2**	(3) **Question 3**	(4) **Combined Questions**
Negotiation × Low Ability	0.652***	0.767***	0.779***	0.743***
	(0.114)	(0.137)	(0.160)	(0.096)
Negotiation × High Ability	0.884***	0.790***	0.744***	0.852***
	(0.144)	(0.172)	(0.203)	(0.123)
Safe Space	-0.065	-0.171	-0.116	-0.094
	(0.086)	(0.116)	(0.134)	(0.079)
High Ability	-0.154	-0.085	-0.103	-0.173
	(0.146)	(0.184)	(0.217)	(0.129)
Number of observations	1,523	1,569	1,569	1,515
Adjusted R^2	0.085	0.069	0.061	0.130

This table reports the effect of the negotiation treatment on girls' understanding of negotiation skills in the midline survey, allowing the effect of the treatment to differ by girls' baseline ability. Girls were asked how they would apply negotiation skills in a scenario that the curriculum had not directly discussed. The scenario asked a girl to negotiate with her sister over who would watch their brother when she has to study for a test. The vignette was designed to test how girls would apply their negotiation skills rather than whether they had learned the terminology from the course. Performance on each of three open-ended questions was blindly graded between 1 and 7, with 7 indicating the highest score. All columns include ethnicity fixed effects, school fixed effects, and socioeconomic controls, consisting of variables for both parents alive, lives with the biological father, lives with biological mother, lives with both mother and father, parents were paying school fees in the pre-treatment period, reading Nyanja excellently, speaking Nyanja excellently, reading English excellently, speaking English excellently, reading Nyanja well, speaking Nyanja well, reading English well, and speaking English well. Standard errors are heteroskedasticity robust.

Table A9: Heterogeneity in Tokens Sent by Parents to Daughters by Ability

	(1) **Tokens Sent**	(2) **Tokens Sent**
Negotiation × Low Ability	0.292	0.285
	(0.225)	(0.241)
Negotiation × High Ability	0.382	0.391
	(0.287)	(0.309)
Safe Space × Low Ability		0.050
		(0.257)
Safe Space × High Ability		0.081
		(0.299)
Number of observations	598	598
Adjusted R^2	0.072	0.070

This table reports heterogeneity in the effect of negotiation and safe space on the number of tokens parents sent to daughters in the communication treatment of the trust game. All columns include ethnicity fixed effects, school fixed effects, and socioeconomic controls, consisting of variables for both parents alive, lives with the biological father, lives with biological mother, lives with both mother and father, parents were paying school fees in the pre-treatment period, reading Nyanja excellently, speaking Nyanja excellently, reading English excellently, speaking English excellently, reading Nyanja well, speaking Nyanja well, reading English well, and speaking English well. Standard errors are heteroskedasticity robust.