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Abstract

Using a randomized control trial, we examine whether offering adolescent girls non-material resources – specifically, negotiation skills – can improve educational outcomes in a low-income country. In so doing, we provide the first evidence on the effects of an intervention that increased non-cognitive, interpersonal skills during adolescence. Long-run administrative data shows that negotiation training significantly improved educational outcomes over the next three years. The training had greater effects than two alternative treatments (offering girls a safe physical space with female mentors and offering girls information about the returns to education), suggesting that negotiation skills themselves drive the effect. Further evidence from a lab-in-the-field experiment, which simulates parents' educational investment decisions, and a midline survey suggests that negotiation skills improved girls' outcomes by moving households' human capital investments closer to the efficient frontier. This is consistent with an incomplete contracting model, where negotiation allows daughters to strategically cooperate with parents.

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1 Introduction

Why do two children with similar abilities and financial resources receive very different levels of education and have vastly different outcomes? Much of the literature attributes the variation in life outcomes that remains after accounting for observable characteristics to non-cognitive skills. While these skills often develop in the critical period before age 5, neuroscience and developmental psychology have now identified a second critical period in early adolescence during which crucial non-cognitive skills related to interpersonal communication develop most quickly (Choudhury et al., 2006). In this paper, we study how a specific type of non-cognitive skill – negotiation skills – taught during this second critical period can affect the educational outcomes of girls in a particularly vulnerable environment.

Specifically, we study adolescent girls in Zambia, a country where female secondary school enrollment is low (39%), as in much of Sub-Saharan Africa (31%[1]). For these girls, like many girls in developing countries, early adolescence is a period of heightened challenges: school dropout rates peak, and girls begin to menstruate and contend with future marriage and motherhood. We find that teaching non-cognitive, interpersonal skills during this critical period matters: girls who are taught negotiation skills have significantly better educational outcomes in the following three years.

While much of the literature on educational investment has focused on the role of policies that alleviate resource constraints such as offering scholarships (for a review, see Banerjee et al. (2013)), we show that even in a very financially resource-constrained environment, offering girls *non-material* resources that do not directly expand the household's budget constraint can meaningfully affect their human capital attainment. Moreover, our experimental design allows us to unbundle the mechanisms through which negotiation training affects economic outcomes, isolating the effect of skills from that of role models or information. Through household surveys and lab-in-the-field games, we also illustrate how negotiation skills employed by daughters facilitate strategic cooperation within the household, helping the family overcome the incomplete contracting problem at the heart of educational investment.

We first measure the longer-term effects of negotiation skills by collecting multiple years of administrative data on enrollment, attendance, school fee payments, and girls' scores on a high stakes exam at the end of 9th grade that determines whether they graduate junior secondary school and where they will attend senior secondary school. We find that negotiation training reduces the hazard of dropout by 10 percentage points and that it increases aggregate measures of human capital attainment across the full set of human capital

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[1]World Bank (2017).

outcomes.

We next unbundle whether the negotiation treatment effects are driven by the acquisition of negotiation skills themselves.[2] To do so, we compare the negotiation effects to a "placebo" intervention – a safe space treatment[3] that offered girls a safe physical space to spend time after school with the same Zambian, female mentors who taught the negotiation program. This treatment was designed to capture the possible ancillary benefits of engaging in the negotiation training, such as having time in a girls-only space, or exposure to a role model (as well as the very small in kind transfers that were part of the program, such as lunch after school). We also cross-randomize the negotiation treatment with another treatment that provided girls with information about the returns to education. The comparison with this treatment can shed light on whether the negotiation treatment's effects are partly driven by information about the returns to schooling communicated by examples in the curriculum or the aspirations effect of interacting with an educated mentor, rather than the skills themselves. We virtually always estimate larger effects for negotiation than the alternative treatments, and for our aggregate measures of human capital attainment, the effects are statistically significantly different, suggesting that negotiation skills themselves matter rather than just the ancillary benefits of the treatment. Additionally, using mediation analysis, we show that a direct measure of a girl's negotiation ability explains a large portion of the negotiation treatment effect, further suggesting that the skills matter.

We then investigate the mechanisms through which negotiation skills lead to human capital investment in the household. Our main findings motivate a simple theoretical framework that allows us to investigate behavior that is often opaque: how families make decisions about educational investment. 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, 2009), children and parents may also have different preferences (Bursztyn and Coffman, 2012). In our data from 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 across generations 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 incomplete contracting problems within the household, imperfectly altruistic parents may under-invest in their children's education (Banerjee, 2004). A theoretical literature in relational contracting (Chassang, 2010

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[2]The focus on skills encompasses both the effect of the training on the returns to educational investment as a complementary, non-cognitive input, as well as girls' usage of the skills with parents, teachers, peers, and other negotiating partners to both transfer information and achieve different intra-household allocations.

[3]The safe space program followed common female empowerment treatments throughout the world (Baldwin, 2011) by providing a safe, physical space for girls to gather with a role model.

and Watson, 1999) shows that dynamic strategic cooperation can lead to joint outcomes that are closer to the efficient frontier. The negotiation curriculum we designed emphasized identifying one's own and one's negotiating partner's deeper interests and using this information to brainstorm solutions that benefited both parties, creating routines of cooperation. In economic terms, therefore, the curriculum is more closely aligned with increasing strategic cooperation across many interactions, rather than increasing a girl's bargaining power in one-shot negotiations.

Our theoretical framework outlines different forces, consisting of altruism, expected returns, and expected reciprocity, that will affect parents' educational investment in girls on the margin of this investment. Guided by the theory, we investigate the channels through which negotiation skills could push these marginal girls "over the threshold," leading them to be educated. First, using midline survey data collected two months after the negotiation training took place, we find evidence of increased reciprocity and routines of cooperation within the household. Relative to both safe space and control girls, negotiation girls ask their parents for more food, consistent with asking for more investment, but parents also report that it is easier to get daughters to do chores and that daughters are more respectful, demonstrating reciprocity. Additionally, negotiation girls are more likely to do chores on Fridays, when they are least likely to conflict with studying and less likely to do chores on other weekdays, consistent with households reallocating time in more efficient ways. Finally, consistent with our framework, our analysis shows that higher ability girls experienced the largest benefits from the negotiation training, suggesting that the benefits of the treatment indeed accrued to girls who were on the margin of receiving educational investments in a country where the female secondary school enrollment rate is low (39%).

To complement the evidence from the midline survey on day-to-day interactions between parents and children, we also invited parents and children to play a lab-in-the-field investment game when we administered the midline survey. This game allows us to test for strategic cooperation between parents and children in a controlled environment where the household's efficient frontier is precisely defined. In the game, parents are given tokens that could be converted into cell phone airtime;[4] any tokens sent to their daughters were doubled, and then daughters decided how many tokens to return to their parents and how many to redeem for prizes. We randomly allowed some girls to communicate with their parents before parents decided how many tokens to send. Since the returns to investment are high, if households are at the efficient frontier, parents should send all of their tokens. This was not the case: parents send about one-half of their tokens. However, for girls who were given the opportunity to communicate, parents of girls who received negotiation training sent more tokens, and thus

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

moved closer to the efficient frontier. As these effects only appear when negotiation girls are given the opportunity to communicate, one possible mechanism is that girls who received the negotiation training used their negotiation skills to credibly commit to return more tokens to their parents. Using a random income shock given to some girls, we can confirm that negotiation girls had a higher propensity to return the marginal token that they received. Altogether, the findings from both the midline survey and the lab-in-the-field investment game suggest that negotiation led parents and girls to cooperate more to increase their joint gains *when they could communicate,* highlighting strategic cooperation as a pathway through which negotiation could increase educational investment.

In addition to shedding light on how negotiation skills affect intrahousehold decision-making regarding education, this paper is the first to establish a causal link between negotiation training and economic outcomes – despite the large amount of resources and time spent on such training at business and law schools around the world.[5] This paper is also the first that can isolate the effect of negotiation skills from potential role-model or information effects.[6] Training on negotiation is usually available to the most economically advantaged. If these skills are indeed effective at changing economic outcomes, as we begin to demonstrate in this paper, then providing this powerful tool only to those who start out life the most privileged could perpetuate inequality. While there has been recent interest in expanding access to these skills to other populations, there is no evidence on their efficacy in such constrained environments. Our results suggest that negotiation skills could be taught to and can aid people in a less privileged positions as well, such as young, female Zambians.[7]

This paper also contributes to two literatures. First, it contributes to a growing literature on the importance of non-cognitive skills (Heckman and Rubinstein, 2001; Heckman et al., 2006; Alan et al., 2016; Attanasio et al., 2015; Adhvaryu et al., 2016). By unbundling the different components of a treatment that is designed to improve non-cognitive skills, we show that the specific *skills* aspect of the intervention is important, rather than other components of the intervention, and that these skills affect human capital investment in ways that may alter a young woman's long-run trajectory. Additionally, we provide new evidence on what kinds of specific non-cognitive skills matter for an individual's outcomes and on how to

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[5]Negotiation training reaches more than 200,000 MBA and Executive students in the United States alone, and in over 16,000 business schools worldwide (Murray, 2011)

[6]The sparse evidence of negotiation training's efficacy rests on measures indicating the participant's ability to identify mutually-beneficial trades within simulated negotiations following training or on subjective measures of negotiation efficacy (e.g., Gist et al., 1991; Movius, 2008; Nadler et al., 2003; Zerres et al., 2013). Studies on negotiation measuring behavioral outcomes examine either only very short-term measures inside the lab (e.g., Small et al. (2007) measure effects on propensity to negotiate research subject payment) or find no support for negotiation training effects on behavioral measures (e.g., Hobfoll et al. (2002) find no effect on rates of sexually transmitted diseases).

[7]The program now has been adapted and incorporated by the Ministry of Education into the national life skills curriculum for all grade 8 students across Zambia.

build the capacities that make an individual successful. Beyond unbundling the negotiation training effect, we also provide evidence on specifically how these non-cognitive skills are used. Importantly, from a policy perspective, we show that it is not too late to teach these skills in adolescence, suggesting that these skills could be taught directly within the school system.

Second, this paper contributes to a growing literature on intra-household bargaining and inefficiencies in investment within the household (Udry, 1996; Ashraf, 2009; De Mel et al., 2009; Bobonis, 2009; Doepke and Tertilt, 2014; Robinson, 2012; Schaner, 2015; Angelucci and Garlick, 2016; Corno and Voena, 2016; Bergman, 2015; Giustinelli, 2016; Jensen and Miller, 2017; Ashraf et al., 2016; Bau, 2016). While much of this literature has focused on spouses rather than parents and children, this paper shows that, in a context where parents and children have different preferences over educational investment, household members can learn skills that facilitate strategic cooperation, helping households get closer to the efficient frontier, in the spirit of Chassang (2010).

This paper is organized as follows. Section 2 describes the negotiation treatment and the experimental design in more detail. Section 3 measures the effect of the negotiation treatment on human capital outcomes. Section 4 unbundles the negotiation effect by comparing it to the effects of two alternative treatments, information and safe space. Section 5 establishes a theoretical framework to guide our search for the mechanisms driving the negotiation treatment effect and uses the midline survey and the lab-in-the-field investment game to test for the different mechanisms. Section 6 tests for spillovers of the negotiation treatment on untreated children. Section 7 concludes.

2   Experimental Design & Data

2.1   Experimental Design and Timeline

We designed the experiment to both measure the effects of the negotiation treatment and to unbundle whether those effects are due to the negotiation skills themselves – including any effect of these skills on the returns to educational investment – or due to auxiliary benefits of the treatment, as well as to shed light on the mechanisms underlying these effects. To accomplish our first objective of measuring the effect of negotiation training – which provided girls with non-material resources, but did not directly affect financial constraints – on educational outcomes, we randomly provided girls with the training and then tracked them over the ensuing four years.

Unbundling the drivers of the negotiation treatment's effect is important since the effect

could be driven by the empowerment or aspiration effects of spending time in an all-female space with a college-educated, female mentor, by improved information about the returns to education from interacting with that mentor, or through elements of the curriculum that used education as an example rather than by improved negotiation skills alone. To unbundle the treatment effects, we study the effects of two alternative randomized interventions – a safe space treatment that provided girls with the same female mentor and all-female space but did not teach the negotiation curriculum, and a short information intervention that provided girls with information about the returns to education (in addition to information on HIV prevention). We run a horse race between the negotiation treatment and the safe space treatment, which non-overlapping samples of girls received, to test whether the negotiation curriculum itself had effects beyond any empowerment or aspirational effects. The information treatment, on the other hand, was cross-randomized with the negotiation treatment so that we could compare both the raw negotiation and informational effects and test for any interaction effects in case negotiation skills allowed girls to better pass information on to their parents.[8]

Finally, to understand *how* negotiation skills affect girls' long-term outcomes, we invited the girls and their parents or guardians to play a lab-in-the-field game with one-another that tested their degree of strategic cooperation under different treatments, allowing us to directly test for cooperative routines between parents and children in a controlled environment where the efficient frontier of investment is clearly defined. Additionally, we collected data from the girls and their parents on variables like the timing of chores and asking for food, which help capture the degree of day-to-day strategic cooperation at home.

The randomized controlled trial targeted 8th grade girls at 41 primary schools throughout Lusaka, Zambia. Of the girls approached to take part in the experiment at these schools, 67% received permission from their guardians to participate (and agreed themselves), and our randomization is within this group. At 29 schools, we conducted a girl-level randomization into three groups: (1) control group, (2) safe space group, and (3) negotiation group. This treatment assignment was cross-randomized with the brief informational treatment, delivered during the baseline survey. An additional 12 schools were assigned to be "pure control" schools, to assist in measuring spillovers, using a matched-pair randomization strategy. Baseline data was collected from all girls whose parents consented, the interventions then took place, and then two months later, midline data was collected at the same time as the lab-in-field experiment was conducted. We then continued to collect administrative data

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[8]Information was provided on health and the returns to education, 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.

on the girls' educational and pregnancy outcomes for the next three years. Figure 1 documents the timeline 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 private away 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. Appendix A provides a more detailed description of the information treatment.

Treatment Assignment and Intervention. Following the baseline survey, within each of the principal 29 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 (but were told it would be offered later). 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. The remaining 12 schools received no treatment.[9]

Table 1 reports summary statistics for the 29 treated schools, and the results of balance tests between intervention groups, controlling for school fixed effects. The table shows that most characteristics are balanced for the negotiation treatment relative to the safe space and control treatments, with p-values for joint tests of whether the covariates significantly predict treatment status always greater than 0.457, but there is some evidence that girls who received the negotiation treatment are lower ability. They are 4.5 and 5.6 percentage points less likely to read or speak Nyanja (the vernacular) excellently and 4.9 percentage points less likely to speak English excellently relative to the control. However, given that we test balance across 14 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,

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[9]To assign the 12 pure control schools, we created 12 matched pairs of 24 schools that were most similar to one another on academic, socioeconomic, and geographic metrics, then randomly assigned one of each pair to be a treatment school, and one to be pure control. Because only 24 schools are assigned this way, we are under-powered to detect small differences in outcomes, but can use these schools to provide suggestive evidence on the extent of spillovers. Additionally, because of the small number of pairs, there are some balance issues in the comparison between the pure control schools and their matched pairs. For this reason, when measuring spillovers, we use propensity score matching at the girl level rather than comparing schools. The negotiation training was expanded to the girls in the pure control schools following term two of 9th grade (but not the control group in the treated schools), and so the pure control schools can only be used to check for spillovers early in the program. However, with the exception of pregnancy and enrollment, all our outcomes (fee payment, attendance, and exam take-up and scores) were exclusively collected during this earlier period.

that offer 8th grade (columns 9-11). The intervention schools are larger than other urban schools on average,[10] but otherwise resemble other urban government schools in terms of the male and female dropout rates and resources. Thus, although our intervention took place in Lusaka, we expect our results to be externally valid across urban Zambia. In contrast, our intervention schools have lower dropout rates and more resources than the average school in Zambia.

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 and further details on the safe space treatment in Appendix B:

1. *Negotiation Group* (801 girls): Girls participated in six after-school sessions over two weeks with female coaches consisting of training on negotiation and interpersonal communication. Sessions consisted of about 15–20 girls. 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 with the same coaches, during which they could play games, study or do homework, or just talk with other girls. Sessions consisted of about 15–20 girls. The same female Zambian role models who taught the negotiation sessions supervised these sessions. Girls 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, led by the same coaches as the initial program, was held at each school with the negotiation and safe space groups.[11]

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 and midline survey with a parent or guardian, where they participated in a randomly chosen version of the investment game, described in more detail in Section 5.3. These games were designed to simulate the human capital investment problem in the presence of incomplete

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

[11]Booster sessions were two hours long (the same length as one of the six training sessions) and reviewed the topics and repeated some role-plays from the main curriculum. Time was also left for general Q&A on the negotiation skills they learned (but not particular applications). The safe space girls also had a "booster," which was just an additional safe space session.

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 affects parents' human capital investment decisions. Daughters and their guardians also participated in a midline survey at this time, which covered topics such as the girl's negotiating ability, the girl's behavior, and the guardians' impressions of girls' behavior.

Administrative Data Collection. From the start of the treatment until the end of 9th grade, 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.[12] We discuss each of these outcome measures in detail below, and Appendix C provides more information on the administrative data collection process.

2.2   Negotiation Treatment

The program was comprised of six, two-hour training sessions, including activities like role-play, group discussion, storytelling, and games building on materials from Curhan (1998) and Mercy Corps (2009) and the classic negotiation texts of Fisher et al. (2011) and Ury (1993). Attendance rates for these sessions were high and were not statistically significantly different between the negotiation and safe space treatments.[13] Some activities directly mimicked real situations that girls might face, such as negotiating with siblings or parents over household disagreements, 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's own outcomes and how different strategies affected long-term cooperation.

The curriculum (McGinn et al., 2012) was structured around four key principles: me, you, together, and build (see Appendix Figure A2).[14] Below, we describe each of these principles in more detail. We also use stories from the girls' notebooks (collected at the booster session in term 2 of grade 8) and stories relayed to us by the negotiation coaches to show how the girls practiced these principles at home.

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

[13]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.

[14]The curriculum is freely available under creative commons license at https://cb.hbsp.harvard.edu/cbmp/pages/content/girlsarise.

Me. This principle taught girls to understand their own interests – that is, to identify 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 recognized at what point they would not compromise and entered the negotiation with a back up plan. Then, they could walk away if the agreement options did not serve the girls' needs and interests.[15,16]

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 improved parents and daughters' relationships since, in subsection 5.2, we find that the parents considered negotiation girls 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, for example, asking open-ended questions to determine the other party's interests. Just as identifying the girl's own deeper interests is a necessary step for identifying 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 partners' perspectives uncover underlying interests and generate more efficient solutions in cases where a deal seemed impossible.

This step also included 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.[17]

Together. This principle focused on bringing the two sides together. First, girls were

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[15]One notebook 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.*

[16]Calculating a walkaway value and incorporating the walkaway value into negotiation planning and execution is fundamental to negotiation analytics (Raiffa, 1982 and Walton and McKersie, 1965).

[17]One girl describes the importance of choosing the right approach in her notebook:

> *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 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.*

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, "If you cared about me, you would pay my school fees." Second, they were taught to 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.[18]

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 partner's true interests and discover the roadblocks leading to conflict. Girls were taught to brainstorm ways to overcome external roadblocks and find productive trades with their partners. They 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 nearby. 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.*

As a whole, 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

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[18]A participating girl discusses identifying a roadblock behind her sister's refusal to do her hair:

> *One day, I asked my sister to do my hair. She refused then I tried to ask her why. She did not answer. Later when her baby was asleep I asked her why she refused. I decided to pick a time when she was not angry. I chose the approach. We shared our interests. She said she could not do it because the baby was troubling her. As we talked and shared more we decided on an agreement. She said she could do my hair if I would watch over her baby while she took her bath.*

tries 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, a scenario was included in the midline survey two months later. Girls were asked to imagine they were in the following situation: they needed to study for a test and had asked their sister to take care of their younger brother, but the sister refused, saying that she wanted to go visit a friend. The girls were asked three open-ended questions about what they would do. The responses were coded, blind to treatment, on a scale of 1-7, with "7" indicating the best answer according to the negotiation curriculum.[19] Appendix Table A2 regresses girls' scores on each of these questions and their average score across the questions on an indicator variable for whether they 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: girls in the negotiation treatment acquired an understanding of negotiation skills and how to apply them, suggesting that they could potentially apply these skills in their everyday lives. 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.

2.3   Long-term Outcome Measures

To measure the effects of the experimental treatments on girls' educational outcomes, we focus on seven different measures, each of which may capture human capital investments in different parts of the ability distribution. We discuss each of these measures below.

Paid School Fees. This measure is coded as 1 if parents had paid all school fees by the end of grade 9 and 0 otherwise. After the girls in our sample left their junior secondary experimental schools for many different senior secondary schools following grade 9, we were no longer able to collect this administrative measure. Since girls had to be enrolled in 8th grade to participate in the experiment, any variation in this measure related to the negotiation training would likely occur for the more disadvantaged girls. Indeed, among the control group, 67% of girls had a zero balance at the end of 9th grade.

Took National Exam. This measure is coded as 1 if girls took the 9th grade national exam and 0 otherwise. Passing this exam is required for girls to graduate from junior secondary

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[19]We developed a 7-point coding scheme, where 1 = no reflection of negotiation lessons and 7 = full integration of me-you-together-build. Coding was based on evidence of: attention to both parties' interests; working together to solve the problem; dealing with emotions; brainstorming solutions. The coding of the responses was blind to the treatment.

school and girls are assigned to secondary schools based on their national exam results. Because the exam was extremely high stakes, most girls (90% of the control) took the exam, suggesting that any variation for this measure caused by the negotiation treatment would be most likely to occur among the most disadvantaged girls.

Threshold Math and English. These two measures are coded as 1 if girls took the national exam and scored in the top 25th percentile in the national exam in math and English respectively and zero otherwise. Thus, these measures combine taking the exam and doing well to avoid the attrition that would occur if we only examined scores and dropped the observations of the girls who did not take the exam. These thresholds are particularly important because girls must score approximately in the top 25th percentile to be assigned to a full-time upper-level secondary school and the school that they are assigned to also depends on their score, although the government is unwilling to reveal exact thresholds. Girls who pass the exams but score lower can attend half-day schools. Thus, we view these measures as capturing the intensive margin of being allowed to attend a higher quality school and of improving effort and learning among girls who attend school. We focus on math and English rather than other subjects in the national exam because they are required subjects. In contrast to the previous two measures, these measures are more likely to capture any negotiation effects for less-disadvantaged girls.

Attendance Rate. This variable measures the average attendance rate of girls across the terms attendance rates were collected (terms 2 and 3 of grade 8 and terms 1 and 2 of grade 9) conditional on being enrolled in school. As with paying school fees, we could no longer collect this measure after girls dispersed to different upper-level secondary schools. This outcome is rarely missing due to non-enrollment because all but 2.7% of girls were enrolled at the end of grade 8 and possess at least one or two terms of attendance measures. We view this measure as both providing information on the important intensive margin of attending school rather than the extensive margin and as providing information on girls' educational investments throughout the ability distribution. Among the control girls, average attendance rates are 54% and range from 27% at the 5th percentile to 76% at the 95th percentile.

Enrollment. We measure enrollment in two ways. Having collected indicator variables for enrollment at the end of grade 8, beginning and end of grade 9, beginning of grade 10 and beginning of grade 11, we can exploit the dynamic aspect of this data and estimate hazard models for dropout. This is the only educational measure that captures long-term educational outcomes after the high stakes period of taking the national exam (in grade 9), leaving junior secondary school, and enrolling in senior secondary school. In contrast to enrollment at the end of 8th grade, which is nearly universal, only 52% of control girls are marked as enrolled in grade 10, and 42% are marked as enrolled in grade 11. Thus, we view this

measure, particularly in later grades, as capturing important, longer-term investments, likely among somewhat more advantaged girls. To make our enrollment measure comparable with the other measures, we also average across the indicator variables for enrollment to create an average time spent enrolled measure, which is 71% for control girls.

Pregnancy. Our last long-term outcome measure is an in indicator variable for whether a girl was reported to have become pregnant prior to the start of 11th grade. This could have been impacted by the negotiation training both through direct negotiations with partners, as well as through the opportunity cost of schooling channel described by Duflo et al. (2015). Reported pregnancies are relatively rare (4% of the control group), which may be reflective of under-reporting, but additionally indicates that any variation in this measure due to negotiation would be predominantly among very disadvantaged girls.

Aggregate Measures. In addition to these individual measures, we aggregate these measures into a human capital index (which excludes pregnancy) and a full index, which includes pregnancy, in two ways. First, we form an index simply by first standardizing each of the individual variables and then averaging over them.[20] Second, we follow Kling et al. (2004) and Clingingsmith et al. (2009) and estimate our treatment effects as average effect sizes.[21] As O'Brien (1984) shows and Kling et al. (2007) note, average effect sizes allow for the formation of a global test statistic with the maximum power against the alternative that all the effects are equal to 0. In our context, the use of average effect sizes and indices have two advantages. First, they reduce the possibility of false positives due to multiple hypothesis testing by allowing us to jointly test the hypothesis that the treatment affects human capital with a single test statistic. Second, these measures increase our statistical power by allowing us to combine information across multiple measures. Intuitively, if a treatment were to have positive effects on all outcomes (even if the effects are not significant), this would be unlikely to occur by chance. Using aggregate measures allows us to exploit such additional information.

3 The Effect of the Negotiation Training Program

In this section, we estimate the effects of the negotiation treatment on educational outcomes by comparing the girls treated with negotiation to the control group within the treatment

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[20]We note that treatment effects measured with this index will not be mechanically more precise due to any "double-counting" of highly correlated outcomes. If we were to average over the same outcome variable two times, we would just recover the original outcome variable.

[21]To form average effect sizes, we run stacked regressions of our outcomes on the treatment of interest, allowing the treatment to have different effects by outcome. We then scale the effect sizes by the standard deviation of the control group and take their average to arrive at the final effect size. Running the stacked regressions allows us to estimate the full covariance matrix, which can be used to test the hypothesis that the average effect size is equal to 0.

schools. There has been substantial focus on the role of material resources and financial constraints in girls' educational completion, and thus this section aims to test whether the non-material resources that negotiation provides increase human capital acquisition even without directly affecting households' financial constraints. We arrive at our estimates of negotiation effects in two ways. First, we exploit the dynamic nature of our enrollment data, the flip-side of which is dropout, to estimate a Cox hazard model for dropout. This model takes the form

$$\lambda(t|X_i) = \lambda_0(t)exp(\pi_0 + \pi_1 negotiation_i + \alpha_s + \mathbf{\Pi X_i}), \tag{1}$$

where i denotes a girl, s denotes a school, $\lambda(t|X_i)$ is the hazard rate for dropout in period t, $negotiation_i$ is an indicator variable equal to 1 if girl i was assigned to receive the negotiation treatment and 0 otherwise, α_s are school fixed effects, and $\mathbf{X_i}$ is a vector of control variables. Throughout this paper, to maximize precision, we usually include a rich set of socioeconomic controls in our regressions, consisting of controls for both parents being alive, living with one's biological father, living with one's biological mother, living with both parents, parents paying school fees at baseline, ethnicity fixed effects, and indicator variables for whether a girl reads and speaks Nyanja and English excellently or well, as well as a control for whether a girl received the information treatment.[22] However, to show that the results are not sensitive to our choice of controls, we also report results including only a parsimonious set of controls consisting of the three variables (reading and speaking Nyanja excellently and speaking English excellently) that appear to be unbalanced across the negotiation treatment and the control (see Table 1) and the information treatment. In these regressions, our coefficient of interest is β_1, the effect of negotiation, and β_1 is identified as long as there is within-school balance by treatment (as the joint tests in Table 1 suggest) and the control group is not contaminated by spillovers. In Section 6, we use our set of pure control schools to test for this contamination. In the hazard regressions, we cluster our standard errors at the individual level to account for the fact that girls' outcomes are interdependent over time.

Second, for each outcome and the human capital and full indices, which average over the individual outcomes, we estimate the negotiation effects with the following regression

$$y_{is} = \beta_0 + \beta_1 negotiation_i + \alpha_s + \mathbf{\Gamma X_i} + \epsilon_{is}, \tag{2}$$

where y_{is} is our outcome of interest and $\mathbf{X_i}$ is defined as before.

Table 2 reports the estimates from equation 1 with the full set of controls (column 1)

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[22]Girls' ability to read Nyanja and English was assessed by asking them to read a sentence aloud. The girls' speaking ability was assessed by asking them to describe the steps to make a cup of tea in English and Nyanja.

and the parsimonious set (column 2). The estimates with both sets of controls are almost quantitatively identical, and both indicate that the hazard of dropout for negotiation girls is 10 percentage points lower than for control girls. Table 3 reports the results for the full set of administrative outcomes. Panel A reports the estimates for the human capital index and its components, while Panel B reports the estimates for the full index and pregnancy, its additional component, and Figure 2 visually reports the same estimates of the negotiation effect as in panel A. The coefficients are reported in both the natural units of the outcomes and in standard deviations of the control group, so that they are comparable to the magnitudes of the index estimates and average effect sizes. Consistent with the findings from the hazard model, negotiation also positively and significantly affects average enrollment, increasing it by 0.097sd. Negotiation also marginally significantly affects attendance, increasing average attendance by 0.061sd. While the effects on the remaining outcomes are statistically insignificant, they are consistently in the expected direction, and this is captured by the fact that effects of negotiation on the human capital and full indices and average effect sizes are all positive and significant. Furthermore, the p-value for the human capital average effect size is 0.001, indicating that even under the most conservative correction for multiple hypothesis testing – the Bonferroni correction, which assumes the outcome variables in the regressions are uncorrelated with one another – the effects of negotiation are statistically significant.[23,24]

In Table 4, we further examine whether the average effect size estimates are robust to the inclusion of different controls or varying the components used in the estimation of the human capital average effect size. The second row of Table 4 shows that including only the parsimonious set of controls leads to similar estimates to the full set. The remaining rows show that no one component of the index drives the results. Even when we omit enrollment as an outcome, the average effect size is 0.060sd (relative to 0.066 with the full set of outcomes). In general, the average effect size estimates are quite stable when we omit each component of the index individually, as well as both test score components, with estimates ranging between 0.060sd and 0.074sd. Thus, we conclude that providing adolescent, Zambian girls with non-material resources by teaching them negotiation skills within-school increases human capital over the subsequent years. These human capital effects are driven both by improvements on the intensive margin (higher test scores and school attendance for enrolled girls) and the

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[23]Under the Bonferroni correction, to keep the probability of type 1 error constant as the number of independent regressions increases, one applies a cut-off value for a significant p-value of $\frac{\alpha}{N}$, where N is the number of regressions and α is the desired level of significance. Therefore, for the 11 regressions here, the value for 5% significance would be 0.005. We note that this correction is particularly conservative in this case since, by definition, indices and average effect sizes are not independent of their components.

[24]As an additional test, we also perform a Westfall-Young permutation test across all the regressions for the human capital index. In the test, we permute the treatment 2,500 times and each time calculate the p-values for each regression, including the index and the AES. We then note the percent of the time the minimum p-value across all tests falls below the p-value for the human capital AES. The implied p-value for our AES in this permutation test is 0.007.

extensive margin (greater enrollment).

4   Unbundling the Treatment Effect

4.1   Negotiation vs. Safe Space and Information

The previous section showed that the negotiation training, a non-material intervention that did not directly affect household's budget constraints, positively affected girls' educational outcomes. The next question is whether this effect was driven by negotiation skills themselves or by ancillary components of the treatment. Thus, in this section, we compare the effects of the negotiation treatment to the effects of two alternative treatments, information and safe space. The safe space treatment is designed to capture all the other benefits offered by the negotiation program apart from the skills training itself, including female mentorship, the time in a safe environment, and the small in kind transfers of daily lunch and a notebook. The information treatment is designed to capture any possible informational transmission that could have occurred through the negotiation training, either by exposure to female mentors who were themselves educated (thus indicating the value of education) or through examples used in the training on the topic of education. Additionally, the information treatment was cross-randomized to test for a positive interaction between a treatment that emphasized information and the negotiation skills which could be used to convey this information, but we find no such interaction effect.[25]

Although the safe space treatment was designed as an analogue to the "placebo control" in medical trials, because it involved the same amount of time, but without the need to learn negotiation skills during that time, it could have also carried benefits orthogonal to those provided by the negotiation training (e.g., time to do homework and interact informally with other girls). Similarly, the information treatment may have contained more dense and useful information than the negotiation treatment. Thus, differences between the negotiation training effect and the safe space and informational effects may be a lower-bound on the marginal impact of the specific negotiation skills.

Table 5 replicates the regressions in Table 3 but now includes the full sample of students in treated schools and reports the estimates of the safe space and information treatment effects, as well as p-values for one-sided tests of whether the negotiation, safe space, and information treatments have different effects. We find no evidence that information about the returns to education alone affects human capital in this context. We do estimate positive effects of safe space, but only the enrollment effect is (marginally) significant. Additionally, the safe

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[25]This table is available on request.

space coefficients are consistently smaller in magnitude than the negotiation ones and for the average effect sizes, which maximize statistical power, we reject that safe space has the same or larger effects than negotiation. In Appendix Table A3, we repeat the robustness exercise in Table 4 with the full sample of girls in treated schools and report coefficients for safe space and information as well. As the table shows, the average effect size of negotiation is always significantly greater than the information treatment. For the comparison to safe space, the point estimates of negotiation are always larger, and in most of the specifications, the effects of negotiation are significantly greater. While we are under-powered to differentiate the negotiation, safe space, and information effects on most individual outcomes, the weight of the evidence from our aggregate measures suggests that the negotiation treatment had greater effects on human capital than the safe space and information treatments.

These results suggest that the skills taught in the negotiation treatment positively affected girls' later human capital attainment, rather than the ancillary elements of the treatment. To more directly test whether this is the case, we perform a suggestive mediation analysis. In Appendix Table A4, we rerun the specifications in Table 5 for the human capital and full indices, including measures of girls' knowledge of negotiation as explanatory variables. For this analysis and the analysis in the next subsection, we focus on the indices rather than average effect sizes both because indices provide us with a single outcome variable that is simpler to work with and because generally, our results using the indices as outcome variables are quantitatively similar to those with the average effect sizes, if less precise.[26] We measure knowledge of negotiation in two ways. First, we average over girls' scores on the three negotiation measures coded from the girls' responses to the negotiation scenario questions presented at midline (See Section 2.2 above). Second, to reduce measurement error, we instrument for girls' scores on the third measure with their scores on the first two measures. Since only 70% of girls came to the midline survey, we also replicate our estimates without these knowledge controls for this different sample, though as Appendix Table A5, which reports the correlates of attending the midline survey, shows, the negotiation treatment did not predict attending the midline.

There are two key results in Appendix Table A4. First, among the girls who attended the midline survey, the effects of the negotiation treatment on the human capital and full indices are 80%-90% larger than for the full sample of girls. While negotiation training does not predict attending the midline survey, girls who appeared in the midline survey were more advantaged on average than those who did not (Appendix Table A5). Thus, the larger treatment effect among this sub-sample suggests that the negotiation training had hetero-

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[26]For the results for the OLS regressions in Appendix Table A4, using the AES indeed delivers quantitatively similar but more precise estimates of the negotiation and knowledge of negotiation effects.

geneous effects – a finding that we explore further in the next subsection. Second, though our measures of negotiation knowledge are unlikely to fully capture negotiation skills, the mediation analysis indicates that negotiation knowledge plays a substantial role in explaining the treatment effects. We find that including the mean knowledge measure reduces the negotiation coefficient for the human capital index (column 2) by 19%, while including the IV measure, which reduces measurement error, reduces it by 41%.

4.2   Heterogeneity in the Negotiation Effect

Based on the findings in the previous subsection, we now further explore heterogeneity in the negotiation treatment effect on the human capital index. 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). Appendix D provides the details of this procedure, but three points are worth highlighting. First, we split the data into two non-overlapping, randomly chosen samples, so that we use one subsample to determine the heterogenity and use a distinct subsample to estimate our point estimates and confidence intervals. This ensures that our confidence intervals are valid and that we are not merely identifying spurious relationships by "over-predicting" random variation in the data. Second, consistent with the drivers of educational investment in our theoretical framework in the next section, we specifically search for heterogeneity in the negotiation effect by ability and parental altruism, where ability is proxied by the first factor of a factor analysis of the Nyanga and English ability variables and altruism is proxied as the first factor of a factor analysis of the variables that capture whether a child lives with her biological parents and has parents paying school fees at baseline. Third, while our machine learning procedure identifies sources of heterogeneity in the data, it does not necessarily identify all sources of heterogeneity.

The results of the machine learning exercise on the training sample show that negotiation has heterogeneous effects by ability, with the strongest effects for those in the top 40th percentile of the ability distribution. In Table 6, we estimate the effects of negotiation on the human capital index and its components in the distinct estimation sample, allowing these effects to vary by whether girls are in the top 40th percentile of the ability distribution. We find that negotiation has large effects for the high ability group, increasing the human capital index by 0.131sd and improving attendance rates by 0.188sd and enrollment rates by 0.228sd. In contrast, the treatment had smaller effects at lower ability levels. The heterogeneity we find is consistent with the outcomes for which we see the strongest effects of the negotiation treatment in the full sample – for example, in the full sample, we find strong effects of negotiation on enrollment, an outcome that varies among higher ability girls in 10th and

11th grade. In contrast, we see less of an effect on outcomes like paying fees in 9th grade and taking the national exam, which are likely to only vary for more disadvantaged girls. Additionally, since higher ability girls are more likely to attend the midline survey (see Appendix Table A5), this result may explain the differences in the effects estimated in Table 3, which uses the full sample of girls, and Appendix Table A4, which uses the sample for whom measures of knowledge of negotiation were available. Finally, the fact that girls in the top 40 percent of the ability distribution are most affected by the negotiation training accords with the fact that a girl who completes 8th grade in the Zambian DHS has a 44% chance of completing 12th grade, suggesting that girls in this ability range are on the margin of receiving more educational investments and continuing in school. Our model in the next section formalizes this intuition.[27,28]

5   Mechanisms

In this section, to fix ideas about what mechanisms could drive the negotiation effect, we first develop a simple theoretical framework. Guided by this framework, we then test for these mechanisms in two ways. We first report results from our midline survey, which allows us to document the effects of negotiation on interactions within the household. We then report results from a lab-in-the-field investment game that girls played with their parents or guardians and which was designed to capture elements of the schooling investment decision in a controlled environment where the efficient frontier for household investment is precisely defined.

Both the midline survey and the investment game took place two months after the treatment implementation. A letter was sent home with girls asking them to come to school with their parent or guardian to participate in the midline survey and game, and 70% of participating girls attended with a parent or guardian. Despite this incomplete sample, there was no significant selective attrition by negotiation or safe space status (see rows 1 and 2 of Appendix Table A5 for the associations between appearing in the sample and the negotiation and safe space treatments).[29]

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[27]In Appendix Table A6, we replicate the analyses in Table 6 but now allow for heterogeneity in safe space's effect. We do not find evidence of the same heterogeneity in the safe space effects, with safe space – if anything – having larger effects for the lower ability girls. The fact that we do not see the same heterogeneity in the safe space effect further suggests that negotiation operates through different channels than safe space.

[28]The results for information interacted with high and low ability are also available on request. We do not report them here since, as for the average effects, we never see significant nor systematically positive effects of the information treatment on outcomes.

[29]In the majority of cases (57%), girls attended the game with their biological mothers.

5.1 Theoretical Framework

Based on our findings in the previous section, which suggest that negotiation training affects girls who are on the margin of receiving more educational investment the most, we develop a theoretical framework outlining different forces through which the treatment could affect marginal girls. While, for simplicity, we model educational investment as a one-shot game, we view this as a reduced-form representation of a reality where small educational investments (such as a allowing a daughter to study) are made over time and daughters can reciprocate these investments by strategically cooperating with their parents (e.g. by doing more chores or doing chores more willingly).

5.1.1 Set Up

The parents' problem is given by

$$\max_E U^p = E(-\tilde{f} + \tau + \beta\delta\tilde{R}_i^p - \delta\tau),$$

where $E \in \{0,1\}$ is 1 if parents invest in education and 0 otherwise, $\tilde{f}$ is the perceived cost of schooling, β is the discount rate, $\delta \in [0,1]$ is the parent's altruism toward the daughter, $\tilde{R}_i^p$ is the parent's perceived returns to education for a daughter i, and $\tau \in [0,\bar{\tau}]$ is the amount the daughter can transfer to the parent to reciprocate the investment, and is a choice variable. The fact that τ is bounded by $\bar{\tau}$, even though daughters may want to transfer more than $\bar{\tau}$ to parents, captures the fact that there is imperfect commitment between daughters and parents. In particular, the ability to commit to future transfers is limited. We note that $\tilde{f}$ might differ from the minimum cost of schooling if there are ways to reduce the cost of schooling that parents and their children do not realize.

The daughter's problem is given by

$$\max_\tau U^d = E(-\tau + \beta\tilde{R}_i^d),$$

where $\tilde{R}_i^d$ is the daughter's belief about the returns to education. For simplicity, we assume that the daughter always believes the returns to education are at least as high as the parents believe they are, consistent with the idea that parental investment is a binding constraint on daughters' educational outcomes and with the fact that daughters report wanting more education than their parents report wanting for them in our data. Then, a parent will educate a daughter if

$$(1-\delta)\tau + \beta\delta\tilde{R}_i^p \geq \tilde{f},$$

while, from the daughter's perspective, the benefits of her education will outweigh the costs if

$$\beta \tilde{R}_i^d \geq \tilde{f}.$$

If $\tilde{R}_i^p \geq \tilde{R}_i^* = \frac{\tilde{f}}{\beta\delta}$, the daughter will choose $\tau = 0$ and be educated. Define $\tau^* = \max(\frac{\tilde{f} - \beta\delta\tilde{R}_i^p}{1-\delta}, 0)$ as the minimum τ needed such that parents educate a daughter. Daughters for whom $\tilde{R}_i^p < \tilde{R}_i^*$ and $\tau^* < \bar{\tau}$, the highest possible transfer to parents, will choose to reciprocate τ^* to their parents. Therefore, girls for whom $\tilde{R}_i^p > \tilde{R}_i^{**}$ are also educated, where

$$\tilde{R}_i^{**} = \frac{\tilde{f} - (1-\delta)\bar{\tau}}{\beta\delta}.$$

Girls for whom the cost of education is less their perceived discounted returns to education (so that education is efficient from their perspective), such that $\beta\tilde{R}_i^d \geq \tilde{f}$, will always be willing to reciprocate enough to make the parent willing to educate them since their maximum willingness to pay is $\beta\tilde{R}_i^d$ and the maximum compensation totally selfish parents would need to educate their daughter is $\tilde{f}$. Thus, as long as

$$\tau^* > \bar{\tau} \quad \text{and} \quad \beta\tilde{R}_i^d \geq \tilde{f}$$

for some girls i, there will be girls who desire to strategically cooperate with their parents so that they are educated but cannot because they are limited by their inability to commit to greater transfers than $\bar{\tau}$. Note that if parents are perfectly altruistic ($\delta = 1$), compensation will not affect parents' educational decisions since they will fully internalize the cost to the daughters of strategically cooperating. If the above two expressions are satisfied, the cut-off perceived returns by the parent for a girl to be educated will be given by $\tilde{R}_i^{**}$ and girls who would be willing to compensate their parents for their education if it were possible will not be educated. Therefore, an important aspect of the model is that, if parents are imperfectly altruistic ($\delta < 1$), they will invest less than is efficient in their daughters' education from their daughters' perspective (Banerjee, 2004).

5.1.2 Potential Mechanisms in the Model

The components of R_i^{**} illustrate several potential mechanisms through which negotiation could affect education. Below, we consider each of these components, which if changed, could reduce R_i^{**}, increasing education levels.

Increasing $\bar{\tau}$. If negotiation skills allow girls to strategically cooperate with parents, allowing them to reciprocate their parents' investments with a greater maximum τ would reduce

the minimum returns to education needed to be educated and increase education. Examples of such strategic cooperation could include girls offering to do more chores (or performing chores more willingly) around the household. We view this mechanism as moving daughters and parents closer to the effective efficient frontier for the household.

Reducing $\tilde{f}$. If negotiation skills allow girls to reduce the effective cost of education by providing information to parents about the timing of effective investment (e.g. moving chores to Fridays or weekends so that a girl can study on weekdays rather than not doing those chores altogether), this will reduce the minimum returns to education needed to be educated, increasing the share of the girls who are educated. We view this as negotiation skills expanding the effective efficient frontier (from the point of view of parents and their daughters) and thus moving household investments closer to the true efficient frontier.

Increasing $\tilde{R}_i^d$ or $\tilde{R}_i^p$. If negotiation training increases either parents' beliefs about the returns to education or the true returns of education (as a complementary, non-cognitive input), $\tilde{R}_i^p$, this would move girls on the margin of the threshold returns to education needed to be educated (as perceived by the parent) above the threshold needed to be educated. Increasing $\tilde{R}_i^d$ alone could also affect parents' investment by affecting the set of girls willing to compensate parents for their investments. Given that the information treatment had no effect, we view information about the returns to education to be unlikely to be driving the results, although negotiation girls may be more able to communicate the value of education to their parents. Moreover, this does not rule out the possibility that parents perceived the treatment itself as increasing the returns to education, which would increase the set of girls whose returns to education are greater than the threshold even without affecting strategic cooperation.

Increasing δ. If negotiation training led girls to use persuasive tactics to increase parents' other-regarding preferences (that is, to value their utility more), this would reduce the minimum returns to education needed to be educated, increasing education levels. δ can also be interpreted as a reduced-form representation of increasing the daughter's bargaining weight in the household, which will appear observationally equivalent to increased parental altruism.

The first two mechanisms most closely align with the negotiation curriculum, which emphasizes identifying gains from trade and finding solutions that make both parties better off rather than pure persuasion. Both these two mechanisms move households closer to the true efficient frontier. In contrast, increasing the returns to education expands the efficient frontier. Still, ex ante, all these mechanisms are plausible, and in the next section, we use both the results of the midline survey and the lab-in-the-field investment game to test for evidence of each of these mechanisms.

Lastly, the model is highly consistent with the heterogeneity indicated by the machine learning exercise, which indicated that negotiation's effects are greatest among girls whose ability puts them on the margin of being educated. Assuming ability is positively related to the returns to education, our model tells us that changing any of these components should affect higher ability girls (e.g. those with returns to education right below R_i^{**}). Thus, from the model and consistent with the machine learning, negotiation increases education among the group where the efficiency gains are the highest by increasing education among girls for whom the returns to schooling are relatively high.[30]

5.2 Effects of Negotiation Within the Household: The Midline Survey

Table 7 reports the effect of negotiation on girls' and parents' behavior within the household in the midline survey. It provides evidence in favor of two of the mechanisms for the negotiation effect in the model – strategic cooperation between parents and girls and the reduction of the effective cost of education. Additionally, the survey assists in ruling out alternative mechanisms and effects of the treatment, including that the treatment affected parents' perceptions of girls in a negative way, that parents either increased their estimation of girls' abilities or better aligned these expectations with true ability, and that girls own desire for education was increased due to a change in the returns to education.

Columns 1 and 2 provide suggestive evidence for the first mechanism – that negotiation fostered strategic cooperation between parents and daughters, leading daughters to ask for more investment and to reciprocate in return. Column 1 reports that negotiation girls were 7.7 percentage points more likely to ask for food,[31] while column 2 indicates that parents were 3.7 percentage points less likely to report it was difficult to get negotiation girls to do chores, although the effect is only marginally significant.

Columns 3-6 provide evidence in favor of our second mechanism – that negotiation allowed households to find less costly ways to make educational investments. Columns 3-5 provide suggestive (if mostly statistically insignificant) evidence that negotiation girls spend less time on chores (measured in hours) before and during school hours and more time on chores after school. In column 6, we exploit the fact that girls were asked how many hours they spent on chores on the last weekday when they were surveyed, introducing random variation in the

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[30]To verify that this interpretation of the heterogeneous effects is not confounded by an alternative, plausible interpretation – that higher ability girls were better able to learn negotiation skills – we regress knowledge of negotiation on receiving the negotiation training interacted with variables for high and low ability. Appendix Table A7 reports these results and provides evidence that low and high ability girls did not differ in their ability to learn negotiation skills.

[31]Conversations with our negotiation coaches confirm that hunger was a major issue for adolescent females in Lusaka.

day about which they were asked. We find that negotiation girls spend more hours doing chores on Fridays and less time doing chores on other week days than other girls. Since Friday is the day girls least need to do homework or study for exams, this suggests that negotiation girls are able to allocate time spent on chores to times when school work has lower returns.

In columns 7-9, we test whether negotiation affected girls behavior in other ways that might affect parents' views of daughters. Columns 7 and 8 show that parents are no more likely to report that a girl has difficulty controlling her temper (indicator variable) or is rude (1–4 scale), and according to column 9, parents of girls in the negotiation treatment are significantly more likely to report that girls are respectful (1–4 scale), which is also consistent with the hypothesis that negotiation increased daughters' ability to reciprocate their parents' investments. Altogether, this set of results indicates that negotiation did not negatively affect girls' relationships with their parents – one of our key concerns when we designed the curriculum. Instead, column 9 indicates that negotiation may have even strengthened these relationships.

In columns 10-12, we consider the possibility that negotiation affected parents' or daughters' perceptions of daughters' abilities. Negotiation skills may have led parents to believe that daughters were higher ability, incentivizing them to invest in the treated daughters, consistent with the idea that it may have increased the perceived returns to education in the model, or these skills may have allowed daughters to inform parents about their ability.[32] To test for these two possibilities, we regress the parent's 1-5 rating of the daughter's ability relative to her classmates on negotiation (column 10) and the interaction between negotiation and the ability factor (column 11). In column 10, we see that negotiation has no effect on parents' perceptions, and in column 11, we find that negotiation does not lead a daughter's measured 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. Finally, in column 12, we regress the number of years of schooling a daughter reported wanting to complete on the treatment. If negotiation increased a daughter's perceived returns to education, including by actually increasing her real returns to education, negotiation should positively affect the number of years of school a daughter wants to complete. We see no evidence that this is the case.

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[32]This would reduce the misallocation of schooling investments, as in Dizon-Ross (2016).

5.3 Effect of Negotiation in a Controlled Environment: The Lab-in-the-Field Experiment

While the midline survey provides suggestive evidence on how girls used their negotiation skills within the household, the lab-in-the-field experiment was designed to simulate parents' educational investment decision-making and provide a controlled environment in which to isolate the different mechanisms outlined in the model.

Girls were randomly assigned to one of three variations of the game:[33] a basic "dictator" game, an "investment" game, and an investment game with the opportunity for communication. The investment game with the opportunity for communication, similar in protocol to a "trust game" that is often used in lab experiments, is designed to most closely mirror the everyday household interactions that could lead girls to receive increased human capital investments (whether time to do homework, money for school fees, or other forms of parental support). In both versions of the investment game, parents were endowed with ten tokens, that either could be redeemed for cell phone air time, which is fungible and serves as pseudo currency for survey compensation, or sent to daughters. Any tokens sent to daughters would be doubled, combined with a random income shock of 2 or 4 tokens, and then given to daughters, who could then choose how many tokens to send back to parents and redeem the remainder for prizes, as described below. The amount of the income shock was not revealed to the girls, and served two purposes. First, it allowed parents to make their decision about what to send with incomplete monitoring by the girls, reducing contractibility. Second, as we discuss in more detail later, it provides exogenous variation in a girl's number of tokens received, which will allow us to identify unbiased estimates of the effect of negotiation on reciprocity by daughters. In the communication variant, after parents and girls learned these rules, but before tokens were sent, they were given the opportunity to communicate. This opportunity was provided by having the surveyors implementing the game pause and allow the girl and guardian to meet before returning to their "stations" to make their decisions privately. Parents and girls were not required to communicate, and in many cases, no information was shared. This mirrors the fact that, in the real world, girls can choose to communicate with their guardians if they wish, and that negotiation skills may aid in initiating these communications.

In the "dictator" variation of the game, the protocol was the same, except girls did not return any tokens to their parents, and there was no communication. This variant was designed to measure whether the treatment impacted parental altruism, δ, toward girls (or

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[33]Because lab experiments typically require a smaller sample than field experiments for sufficient power, and we did not want to confuse girls or their guardians by playing multiple versions, we divided our sample among the three versions.

the girls' bargaining weight within the household, which is not observationally separable from altruism), since there was no direct return to parents themselves from sending tokens to their daughters.

In contrast to the dictator game, the number of tokens parents sent in the investment version of the game depended on parents' expectation of how many tokens daughters would return, or daughters' reciprocity, τ. Thus, by estimating the effects of negotiation on the number of tokens parents sent, we can estimate how negotiation affected parents' expectations of daughters' reciprocity. Additionally, by administering this version of the game with and without communication, we can isolate the effect of the girls' negotiation skills themselves – which they are able to use in the communication version of the game – on expected reciprocity. Because parents' expectations about the number of tokens daughters will return could have been influenced in multiple ways by the treatment, as we discuss more below, we do not have clear predictions about the effect of negotiation on the number of tokens parents will send when communication is not possible. However, if treated girls were able to use their negotiation skills to increase expected reciprocity, this would manifest as a strong positive interaction between the communication variant and the negotiation treatment.

The investment game with communication closely aligns with the theoretical framework, where increased reciprocity by daughters allows parents and daughters to move closer to the efficient frontier in educational investment. In the standard investment game, it is efficient for parents to send all their tokens to their daughters since there is a high, certain rate of return from investment (100%). If parents could contract with their daughters, they would achieve this outcome. In fact, households are far from the efficient frontier: the average parent sent half her tokens to her daughter in the investment game. Only 2.4% sent all their tokens. Thus, just as in the theoretical framework, strategic cooperation could lead to more efficient outcomes in the context of the game. If a treatment increases the number of tokens that a parent sends, it mechanically increases the total number of tokens available, moving the household toward the efficient frontier.

There was one additional cross-randomized variation of the game. For a subset of girls, the tokens sent to girls were only doubled if they successfully completed a word search. This was intended to allow the returns to parental investment to vary based on daughters' ability (allowing $\tilde{R}_i^p$ to affect the investment decision).[34] However, parents' investments did not respond to the potential variation created by the word game, and so we pool it with other versions for our main analyses. Altogether, a total of 333 girls participated in the

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[34]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 increase parents' perceptions of her ability.

dictator game, 682 girls participated in the investment game without communication, and 647 participated in the investment game with communication. Appendix Table A8 reports the number of girls assigned to each variation.[35]

Unlike in typical lab games, which are played by strangers, the results of a game between daughters and parents could easily be undone after the game if daughters received cash. To solve this problem, daughters redeemed their tokens for prizes at a "store" (a table in the game room), displaying and selling girl-specific items that parents would not value for themselves, including consumption items (e.g., hair bands, candy), school supplies (e.g., pencils, notebooks), and personal items (e.g., socks, menstrual pads). Appendix Figure A3 shows the store and the prizes. Parents had no control over how daughters spent the tokens, though we acknowledge that resource allocations from the game might still be undone ex post since parents can control daughters' later consumption. However, parents at least believe that a portion of the allocation is binding (or requires some cost to undo), since otherwise they would pursue the fully efficient allocation of sending 100% of tokens instead of 50%.

To confirm that the game is perceived as having real stakes, and that it has explanatory power for real-world outcomes, in Appendix Table A9, we regress the human capital index on the number of tokens parents sent. We do not find a significant connection between tokens sent in the dictator game and real-world investment (although this may be due to a lack of power, as the coefficient is similar to the coefficient for the investment game), but we do find a significant relationship between tokens sent in the investment game and real-world investment. Moreover, we find that this connection is most strongly driven by the communication variant of the game, affirming our view that this version most closely mirrors the real-world setting in which parents make investment decisions.

5.3.1   Results

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

$$
\begin{aligned}
y_i = {} & \beta_0 + \beta_1 negotiation_i + \beta_2 safe\_space_i + \beta_3 communication_i \\
& + \beta_4 negotiation_i \times communication_i + \beta_5 safespace_i \times communication_i \\
& + \mathbf{\Gamma X_i} + \epsilon_i,
\end{aligned} \tag{3}
$$

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[35]These numbers differ slightly from the sample sizes in our regressions since data entry and non-response for control variables lead some observations to be missing in the regressions.

where y_i is the outcome of interest, $negotiation_i$ 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 equal to 1 if girls were allowed to communicate with their parents, and $\mathbf{X_i}$ is our standard set of controls. As before, since randomization took place within schools, all the regressions control for school fixed effects, and we control for ethnicity fixed effects and a rich set of controls for socioeconomic status.

Tokens Sent to Daughter. Table 8 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, which aims to isolate the altruism channel. 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 suggest that the negotiation treatment did not generally affect 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 have a strong, positive interaction. When girls with negotiation skills are allowed to communicate, they receive 0.785 more tokens than when they are not. The interaction between safe space and communication (0.450) is half the size. 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, a result which we discuss in more detail below. When we combine these coefficients, we find that among girls who were allowed to communicate, girls in the negotiation treatment receive a marginally significant 0.316 more tokens than control girls, while girls in the safe space treatment receive almost exactly the same number of tokens as the control girls. Appendix Figure A4 graphs the density of tokens sent by parents when daughters are and are not allowed to communicate for the safe space, negotiation, and control girls. Consistent with the results from the regressions, in the game with communication, the distribution of tokens sent for safe space and control girls is almost identical, while the density of tokens sent for negotiation girls is lower in the middle and fatter in the upper tail relative to the game without communication.

The results in column 3 suggest that parents believe that girls in the negotiation treatment are more reciprocal than control girls when they are allowed to communicate.[36] This suggests that, when daughters can communicate and therefore, specifically use their negotiation skills,

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[36]We cannot rule out an alternate explanation for this finding, which is that while the returns to investment in terms of tokens are known, negotiation girls who are allowed to communicate transmit information about the subjective returns to the items they could buy at the store. For example, a girl might tell her parents that she really needs pencils or notebooks for school. This mechanism maps onto theoretical work on Bayesian persuasion by Kamenica and Gentzkow (2011).

they affect parents' expectations of reciprocity. The fact that communication is needed for negotiation to positively affect parents' behavior provides further evidence that negotiation skills themselves matter rather than other aspects of the treatment. The findings in the communication version of the game also provide evidence that fostering strategic cooperation (affecting τ in the theoretical framework), rather than merely affecting parental altruism or bargaining weights, is an important element of the effect of the negotiation treatment on girls' outcomes. Indeed, these results show that – when communication is possible – negotiation (marginally significantly) led to more efficient outcomes in the investment game, and that the interaction between the negotiation treatment and the opportunity to communicate is strongly positive and significant.

Column 4 reports the average effect of the word game, and shows that the word game variation had no impact on parental giving. We therefore pool the word game with the other treatments in most specifications.[37]

Why did parents send fewer tokens in the game without communication in both the negotiation and safe space treatments? We can speculate that this effect reflects the fact that the female empowerment elements of the negotiation and safe space treatments led parents to expect less reciprocity from the girls in the absence of communication. That is, they expected that the girls would spend more tokens on themselves.[38] However, when negotiation girls could communicate, they were able to use their negotiation skills to increase parents' expectations of reciprocity, leading them to receive more tokens. We further explore this hypothesis later in this section. Importantly, while the results without communication are similar between the negotiation and safe space treatments, the results with communication are different (albeit marginally significantly so), indicating that the negotiation treatment specifically affected girls' ability to *communicate* reciprocity.

To verify that the negotiation skills themselves play an important role in increasing the number of tokens daughters are sent when daughters can communicate, we next include the girl's total score on the knowledge of negotiation test and its interaction with the communication treatment in the regressions (Appendix Table A11). We find that knowledge has no effect when girls are not allowed to communicate, but when girls are allowed to communicate, those with more knowledge are sent significantly more tokens, even controlling for the interaction of the negotiation treatment with communication. This is again consistent with negotiation girls specifically using their negotiation skills when they were allowed to communicate with their parents.

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[37]In Appendix Table A10, 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.

[38]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.

Tokens Returned to the Parent. Our findings in Table 8 suggest that negotiation girls may have been sent more tokens because they were able to increase parents' expectations of reciprocity (by committing to send them back more tokens) when they were allowed to communicate. If this is the case, and parents have rational expectations, girls in the negotiation × 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 deciding how many tokens to send to their parents. We can use this random shock to estimate the pass-through of an additional token sent to the daughter to the parent with the following regression

$$
\begin{aligned}
tokens\_returned_i = {}& \beta_0 + \beta_1 negotiation_i + \beta_2 safe\_space_i + \beta_3 communication_i \\
& + \beta_4 negotiation_i \times communication_i + \beta_5 safe\_space_i \times communication_i \\
& + \beta_6 shock_i + \beta_7 shock_i \times negotiation_i \times communication_i \\
& + + \beta_8 negotiation_i \times shock_i + \beta_9 communication_i \times shock_i + \alpha_t \\
& + \alpha_s + \mathbf{\Gamma X_i} + \epsilon_i,
\end{aligned}
$$

where i denotes a daughter, $tokens\_returned_i$ is the number of tokens a daughter sends her parent, $shock_i$ takes the value of the shock to the number of tokens the daughter received (2 or 4), α_t is a fixed effect for the number of tokens parents sent daughters, and $negotiation_i$ $safe\_space_i$, $communication_i$, and $\mathbf{X_i}$ are defined in the same way as before. Then β_7, the relative increase in the pass-through of the marginal token given to a daughter to the parent in the negotiation treatment and the communication variant of the game, is one of our key coefficients of interest.

Column 1 of Table 9 reports the results of this regression. We estimate that β_7 is 0.464, 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 $communication_i$, $shock_i$, and $safe\_space_i$. We find that the coefficient of $communication_i \times safe\_space_i \times num\_tokens_i$ is less than one-third the size of β_7 and statistically insignificant.

Using the coefficients from Table 9, in the bottom panel of the table, 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 is allowed to communicate receives an additional token based on our coefficient estimates. Control and safe space girls pass-through about one-fifth

of a token. In contrast, negotiation girls pass through one-half of an additional token. While we do not have enough statistical power to rule out the possibility that the overall pass-through rate when communication is allowed is the same for both safe space and negotiation girls, the pattern of the point estimates is consistent with the idea that negotiation (in the presence of communication) increases parental investment by increasing the expectation of reciprocity (τ). Interestingly, in the non-communication treatment, both negotiation and safe space girls have a lower (albeit not statistically significantly different) pass-through rate, consistent with the idea that these treatments partly empowered girls to look after their own interests. This also suggests that girls in the negotiation treatment playing the communication game aligned their behavior to be consistent with their communication to their parents about reciprocity.

Daughter's Welfare. Next, in Appendix Table A12, we investigate whether negotiation made daughters better off in the investment game. The regressions in Appendix Table A12 duplicate the specifications in columns 2-4 of Table 8, but the outcome variable is now the final number of tokens that the daughter finishes the game with, which is tokens sent (plus random income shock) minus tokens returned. The results in Appendix Table A12 echo those in Table 8. 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 8. Negotiation girls who were allowed to communicate with their parents end the game with 0.954 more tokens than girls who could not communicate. Moreover, among girls who were allowed to communicate, negotiation girls end the game with 0.417 more tokens than control girls, while safe space girls end the game with 0.285 *fewer* tokens. The estimates are consistent with the idea 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 girls' welfare.[39]

Figure 3 visually confirms that this is the case. The figure plots the density of parent-child pairs by percent of total potential tokens received by the guardian and the daughter. The red, diagonal line plots the efficient frontier. Outcomes closer to this line are nearer to the efficient frontier, while outcomes in the top left of the picture are better for daughters. The figure is consistent with communication leading to outcomes that are both better for daughters and closer to the frontier among negotiation girls.

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[39]As in Table 8, the word game has no significant effect on the number of tokens daughters received (see column 3).

How Girls Spent the Tokens. Finally, in Appendix Table A13, 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); personal household items, consisting of socks and sanitary pads; and pure consumption (hair ties, scarves, bracelets, lip balm, snacks, and snakes and ladders games). We speculate that girls exhibiting more empowered behavior, without the strategic reciprocity balancing it out, may exhibit more "me-first" expenditures. Consistent with this, negotiation and safe space girls spend more in magnitude (and sometimes significantly more) on pure consumption relative to control girls in both the non-communication investment and dictator games (columns 3 and 9). However, when negotiation girls are allowed to communicate with their parents, they spend less on pure consumption (column 9) and more on personal items that are useful for the household (column 8). These results provide further suggestive evidence that the communication game allows strategic cooperation between parents and girls in the negotiation treatment. They are able to credibly communicate their intentions to both return more tokens and spend tokens in a more household-oriented way.

Putting the results together from this and the previous subsection, we find evidence that negotiation affected $\bar{\tau}$, increasing reciprocity and strategic cooperation in the household and moving human capital investments closer to the efficient frontier. This is evidenced both by the reciprocity in the midline survey – girls are more respectful and it is easier to get them to do chores, but they ask for more food – and the results from the investment game, which suggest that negotiation girls achieved more efficient outcomes when they could communicate. Additionally, we find evidence that negotiation affected $\tilde{f}$ by allowing girls to negotiate with their parents to move their chores so that they could study or attend school at times when doing so was most effective, again moving households closer to the efficient frontier.

6   Evidence on Spillovers

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 or led parents to reallocate investments within the household (Das et al., 2013). The design of our experiment only allows for suggestive evidence on the presence of spillovers, because the characteristics of the pure control and treatment schools were not fully 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 in the control group.[40] Second, we use data on grade 9 male dropout 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 dropout.[41] 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 10 reports the results of these tests. Columns 1 and 2 report the effect of being in a treated school on the control girls. In both cases, we find no significant spillovers. Column 3 reports the effect of being a negotiation school on male dropout rates from the difference-in-differences regression and again, the effect is small and insignificant.

The remaining columns of Table 10 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 on the last weekday. 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, 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. In Appendix Table A14, motivated by our stronger treatment effects on high

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[40]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.

[41]We regress school-year level 9th grade male dropout rates for the universe of Zambian junior secondary schools on school and year fixed effects, and an indicator variable set to equal 1 if a school was a program school in 2014. Standard errors are clustered at the school-level.

ability girls, we further check for differential spillovers on the siblings of high ability versus low ability girls and find no evidence of heterogeneous effects.

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', parents did not expect male siblings to complete fewer years of schooling. While it may seem surprising that increased educational investment in the treated girl did not negatively affect her siblings, this could be because the increased investment came out of parents' consumption or because girls used negotiation to arrive at solutions that increased family welfare, consistent with the theoretical framework.

7 Conclusion

In this paper, we study the effect of non-cognitive, 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, as well as two alternative treatments – information and safe space – which isolate potential ways through which the training could affect educational outcomes. Then, to understand how negotiation can affect girls' outcomes, we invited the girls to participate with a parent in a lab-in-the-field investment game that simulates the educational investment decision in a setting where there is incomplete contracting between parents and children and to take part in a midline survey.

We find that negotiation skills increase educational investment despite the fact that they do not relieve households' financial constraints, particularly for higher ability girls who are likely on the margin of enrolling in and completing secondary school. Running a horse-race between negotiation and our alternative treatments suggests that neither the aspirations or mentorship effects of the negotiation treatment nor information about the returns to education explain the full negotiation effect. Instead, the results of our investment game and midline survey suggest that negotiation girls increased their education by strategically cooperating with parents, reciprocating their educational investments, and by moving the timing of chores to conflict less with school work and studying, reducing the effective costs of education. We find evidence that parents find it easier to get negotiation girls to do chores and view these girls as more respectful, although girls ask for more food, and we also find evidence that negotiation girls shift their chores to Fridays when studying is less valuable.

Both these mechanisms move educational investment closer to the efficient frontier.

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. Indeed, we speculate that these skills could also facilitate more efficient decisions when there are incomplete contracting problems between spouses (as may be the case in Udry (1996)).

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, negotiation 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 necessarily improving the daughter's outside option. Guided by relational contracting models (for example, Chassang (2010)), we show that this is the case, providing 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.

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Figures

Figure 1: Experimental Timeline
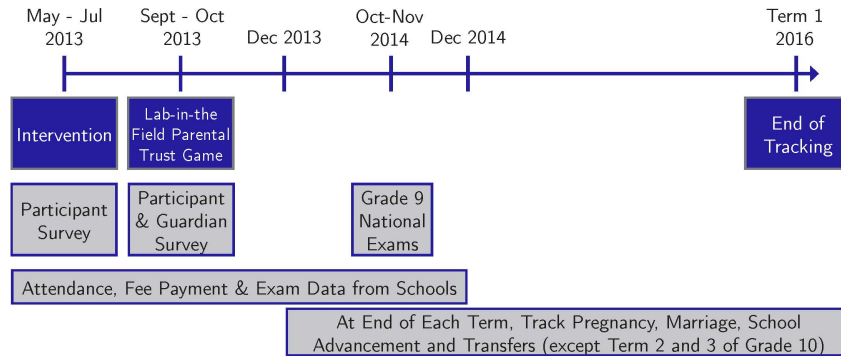
This figure details the timeline for the baseline data collection, the initiation of the experiment, the midline data collection and the investment game, and the subsequent three years of administrative data collection.

Figure 2: Estimates of the Effect of Negotiation on Human Capital
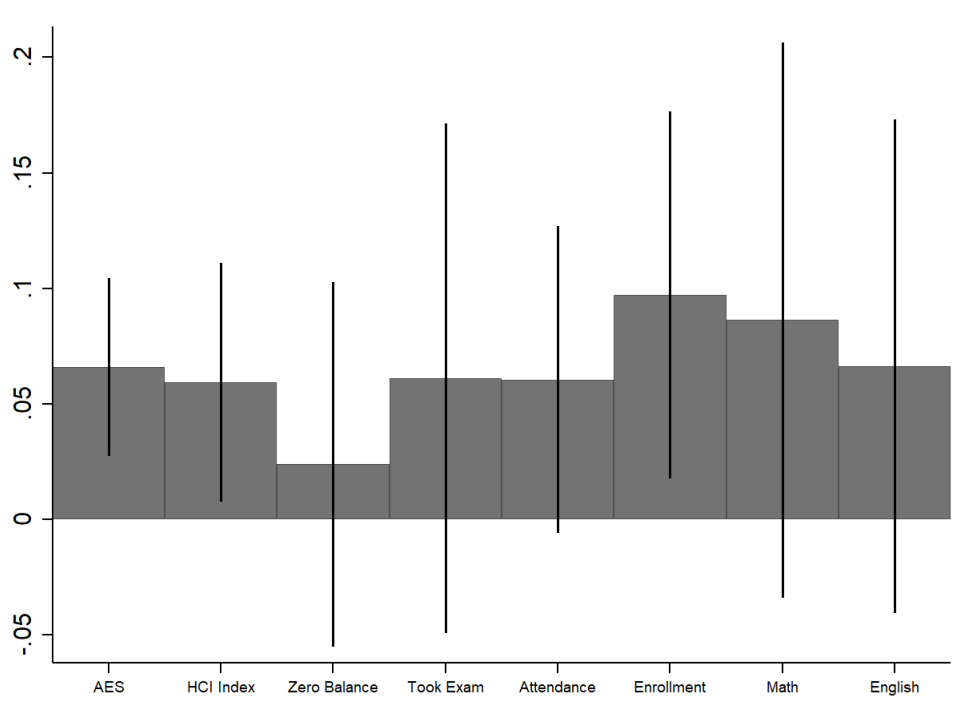
This figure reports the estimates of the coefficient of the negotiation effect (relative to the control) on different human capital outcomes. The estimates are the same estimates as those reported in Table 3. "AES" refers to the average effect size coefficient, "HCI Index" refers to the human capital index, "Zero Balance" refers to having a zero balance for school fees at the end of 9th grade, "Took Exam" refers to taking the national exam, "Attendance" refers to average attendance conditional on being enrolled in school in 8th and 9th grade, "Enrollment" refers to the average enrollment rate, and "Math" and "English" refer to scoring in the top 25th percentile in these subjects on the national exam at the end of 9th grade. The vertical bars denote 95% confidence intervals.

Figure 3: Distribution of Parent-Child Outcomes and Distance to the Efficient Frontier for Negotiation Girls With and Without Communication
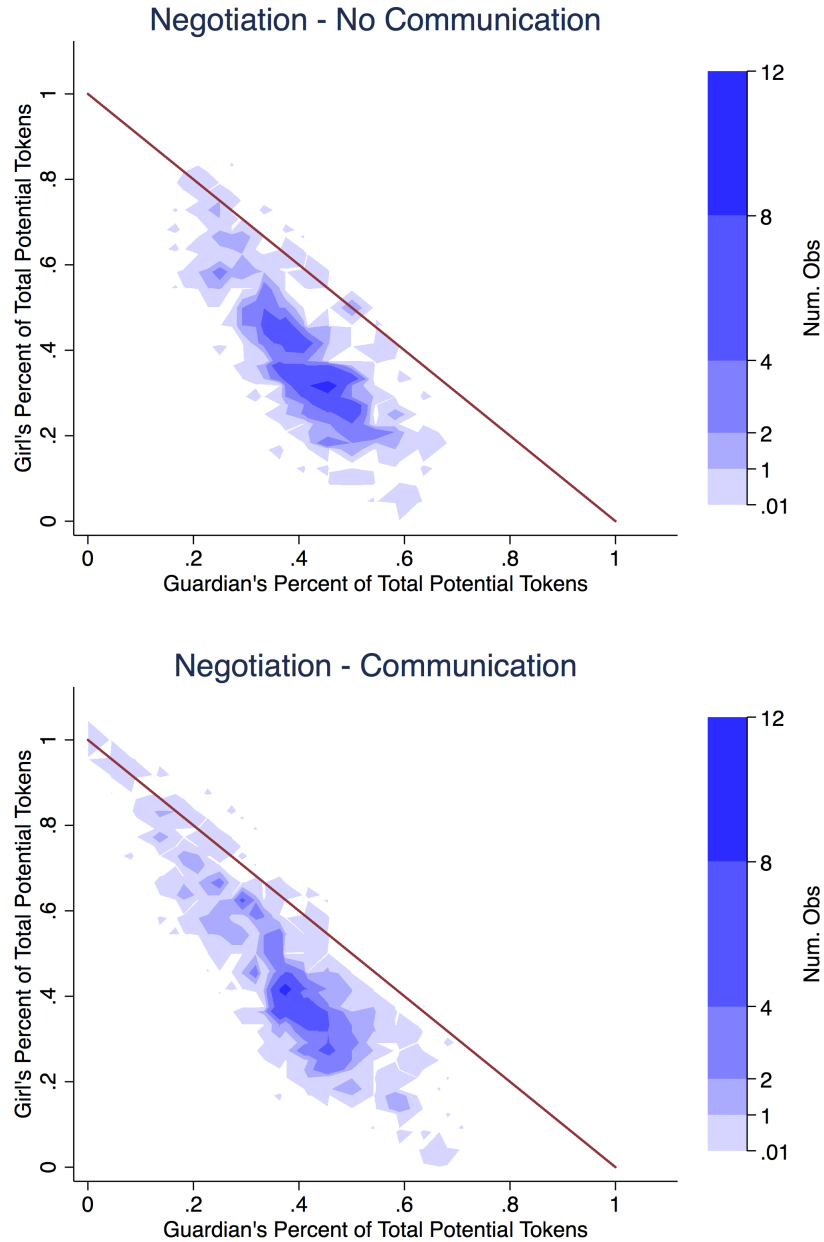
This figure simultaneously plots the percent of the potential total number of tokens a household (daughter plus guardian) received (distance from the efficient frontier) and the allocation of tokens at the end of the investment game between parents and daughters for girls in the negotiation treatment in the non-communication variant of the game (top graph) and the communication variant (bottom graph). The darkness of the color indicates how many observations have a given allocation between the daughter and her parent for a given percent of the total potential tokens acquired. The total number of potential tokens is the number of tokens the household would receive if a parent sent all her tokens to the daughter (22 or 24 depending on the income shock). Households are on the efficient frontier if the household received all the tokens (on the diagonal line). Daughters are better off if the allocation is to the left, indicating daughters ended the game with more tokens.

Tables

Table 1: Summary Statistics and Balance of Characteristics by Negotiation Treatment

	(1) Mean	(2) SD	(3) Coeff. (Neg vs Control)	(4) SE	(5) Coeff. (Neg vs. SS)	(6) SE	(7) Coeff. (SS vs Control)	(8) SE	N
Both Parents Alive	0.737	0.440	-0.018	0.023	-0.019	0.023	-0.005	0.023	2,254
Live With Bio Dad	0.548	0.498	-0.018	0.025	-0.009	0.026	-0.008	0.026	2,254
Live With Bio Mom	0.701	0.458	0.011	0.023	0.008	0.023	0.003	0.024	2,254
Live With Mom and Dad	0.499	0.500	-0.021	0.025	-0.012	0.026	-0.009	0.026	2,254
Parents Pay Fees at Baseline	0.763	0.425	0.032	0.022	0.015	0.021	0.018	0.022	2,249
Read Nyanja Excellently	0.399	0.490	-0.045**	0.025	-0.048**	0.025	0.004	0.025	2,254
Speak Nyanja Excellently	0.480	0.500	-0.056**	0.025	-0.039	0.025	-0.014	0.025	2,254
Read English Excellently	0.697	0.459	-0.019	0.023	-0.025	0.023	0.009	0.023	2,254
Speak English Excellently	0.412	0.492	-0.049**	0.025	-0.002	0.025	-0.042	0.025	2,254
Age	14.419	1.461	0.052	0.073	0.033	0.073	0.012	0.073	2,254
Read Nyanja Well	0.637	0.481	-0.029	0.024	-0.002	0.025	-0.026	0.025	2,254
Speak Nyanja Well	0.885	0.320	-0.002	0.016	-0.006	0.016	-0.001	0.016	2,254
Read English Well	0.899	0.301	-0.010	0.015	-0.003	0.016	-0.007	0.015	2,254
Speak English Well	0.789	0.408	-0.021	0.021	-0.025	0.021	0.002	0.020	2,254
P-value (joint test)			0.457		0.550		0.923		

This table reports summary statistics collected during the baseline survey for the girls from the 29 treatment schools who participated in the experiment, as well as tests of the within-school randomization of the negotiation and safe space treatments relative to one another and the control. For the coefficient column, 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. The final row regresses indicator variables for negotiation (columns 3 and 5) or safe space (column 7) on the full set of covariates and school fixed effects using a sample of either negotiation and control girls (column 3), negotiation and safe space girls (column 5), or safe space and control girls (column 7), and reports the p-value from a joint test of the significance of the covariates. Standard errors are heteroskedasticity robust. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table 2: Hazard Model of the Effect of Negotiation on Dropout

	(1) Dropout	(2) Dropout
Negotiation	0.896**	0.900**
	(0.050)	(0.048)
Percent Control Eventually Dropout	0.587	0.594
Controls	Full	Parsimonious
Number of observations	6,174	6,524
Clusters	1,429	1,508
Pseudo R^2	0.011	0.007

This table reports estimates of the effect of negotiation on dropout in Cox hazard regressions. Coefficients are reported as hazard ratios. The full controls consist of controls for ethnicity fixed effects, both parents being alive, living with one's biological father, living with one's biological mother, living with both parents, parents paying school fees at baseline, and indicator variables for whether a girl reads and speaks Nyanja and English excellently or well, as well as a control for whether a girl received the information treatment and school fixed effects. The parsimonious controls only include controls for whether a girl reads or speaks Nyanja excellently and whether she speaks English excellently, school fixed effects, and the control for the information treatment. The standard errors allow interdependence over observations for the same individual by clustering at the individual level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table 3: Effect of Negotiation Relative to Control 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	0.059**	0.066***	0.011	0.018	0.036	0.028	0.009*	0.027**
	(0.026)	(0.020)	(0.019)	(0.017)	(0.026)	(0.023)	(0.005)	(0.011)
SD			0.024	0.061	0.086	0.066	0.061*	0.097**
Control Mean	-0.010		0.675	0.897	0.215	0.227	0.534	0.709
Number of Observations	1,388		1,404	1,422	1,422	1,422	1,419	1,429
Adjusted R^2	0.043		-0.002	0.012	0.006	0.073	0.015	0.038

	Panel B. Full Index and Pregnancy		
	(1) Full Index	(2) Full AES	(3) Ever Pregnant
Negotiation	0.056**	0.064***	-0.010
	(0.027)	(0.018)	(0.010)
SD			-0.053
Control Mean	-0.010		0.036
Number of observations	1,388		1,429
Adjusted R^2	0.049		0.020

This table reports estimates of the effect of the negotiation relative to the control girl in the treated schools on outcomes collected in the administrative data. Estimates are reported both in the natural units of the data and in terms of standard deviations of the control group, so that the effects are in the same units as the average effect sizes. **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 of the effects 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 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 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. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table 4: Robustness of Human Capital Average Effect Size

	(1) Negotiation Coef.	(2) Se
Baseline	0.066***	0.020
Parsimonious Controls	0.051***	0.019
No Paid Fees	0.074***	0.022
No Took National Exam	0.067***	0.021
No Attendance	0.067***	0.023
No Enrollment	0.060***	0.022
No Math Threshold	0.062***	0.020
No English Threshold	0.066***	0.021
No Test Scores	0.061***	0.021

This table reports estimates of the human capital average effect size under different specifications, using the sample of negotiation and control girls in the treated schools. Row 1 reports the baseline specification from Table 3. Row 2 reports the results for a specification that only controls for whether a girl reads and speaks Nyanja excellently and whether she speaks English excellently, as well as a control for whether a girl received the information treatment and school fixed effects. Row 3 reports the AES omitting whether parents paid school fees as a component. Row 4 omits taking the national examin, row 5 omits attendance as a component, row 6 omits enrollment, row 7 omits the math threshold, row 8 omits the English threshold, and row 9 omits both test scores. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table 5: Effect of Negotiation Relative to Safe Space and Information 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)
SD			0.025	0.057	0.083	0.060	0.051	0.093**
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)
SD			0.005	0.035	0.012	-0.058	0.046	0.088*
Information	-0.018	-0.017	-0.026	-0.005	0.001	-0.003	0.003	-0.013
	(0.024)	(0.016)	(0.017)	(0.014)	(0.019)	(0.016)	(0.005)	(0.010)
SD			-0.055	-0.018	0.002	-0.007	0.020	-0.046
One-sided p-value (Neg vs SS)	0.073	0.015	0.308	0.361	0.110	0.005	0.448	0.459
One-sided p-value (Neg vs Info)	0.031	0.002	0.076	0.174	0.191	0.166	0.261	0.007
Control Mean	-0.010		0.675	0.897	0.215	0.227	0.534	0.709
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)	(0.018)	(0.010)
SD			-0.048
Safe Space	0.024	0.025	-0.009
	(0.024)	(0.017)	(0.012)
SD			-0.048
Information	-0.016	-0.014	-0.002
	(0.023)	(0.015)	(0.008)
SD			-0.009
One-sided P-value (Neg vs SS)	0.140	0.023	0.497
One-sided P-value (Neg vs Info)	0.046	0.002	0.287
Control Mean	-0.010		0.036
Number of Observations	2,059		2,122
Adjusted R^2	0.050		0.017

This table reports estimates of the effect of the negotiation, safe space, and the information treatments in the treated schools on outcomes collected in the administrative data. Estimates are reported both in the natural units of the data and in terms of standard deviations of the control group, so that the effects are in the same units as the average effect sizes. **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 of the effects 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. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table 6: Heterogeneous Effects of Negotiation by Ability

	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 × High Ability	0.131**	0.166***	0.020	0.071	0.049	0.067*	0.029**	0.063**
	(0.061)	(0.043)	(0.049)	(0.043)	(0.043)	(0.038)	(0.014)	(0.028)
SD			0.043	0.236	0.117	0.159*	0.188**	0.228**
Negotiation × Low Ability	0.081*	0.080***	0.020	0.060*	0.011	0.004	0.019*	0.031
	(0.020)	(0.031)	(0.025)	(0.033)	(0.035)	(0.027)	(0.011)	(0.020)
SD			0.042	0.200*	0.026	0.011	0.124*	0.011
Control Mean	-0.018		0.693	0.864	0.532	0.711	0.211	0.220
Number of Observations	1,021		1,034	1,047	1,048	1,054	1,047	1,047
Adjusted R²	0.027		-0.002	0.014	0.021	0.023	-0.006	0.051

	Panel B. Full Index and Pregnancy		
	(1) Full Index	(2) Full AES	(3) Ever Pregnant
Negotiation × High Ability	0.124***	0.126***	-0.026
	(0.060)	(0.042)	(0.027)
SD			-0.133
Negotiation × Low Ability	0.081*	0.052	-0.018
	(0.043)	(0.034)	(0.016)
SD			-0.094
Control Mean	-0.019		0.041
Number of Observations	1,021		1,054
Adjusted R²	0.034		0.030

This table reports estimates of the effect of the negotiation by whether or not girls' ability factor is greater than 0.45 (the partitions of the treatment effect indicated by the machine learning estimation) on outcomes collected in the administrative data. An ability factor above 0.45 corresponds to the top 40th percentile of the ability distribution. The data set is a randomly chosen subsample of the full data which was held back and not used in the machine learning that established partitions of the data. Estimates are reported both in the natural units of the data and in terms of standard deviations of the control group, so that the effects are in the same units as the average effect sizes. **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 of the effects 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. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table 7: Evidence on the Mechanisms in the Midline Survey

	(1) Ask For More Food	(2) Difficulty Getting to do Chores	(3) Chores Before School	(4) Chores During School	(5) Chores After School	(6) Total Weekday Chores	(7) Difficulty Controlling Temper	(8) Girl is Rude	(9) Girl is Respectful	(10) Natural Ability	(11) Natural Ability	(12) Years Want to Complete
Negotiation	0.077***	-0.037*	-0.064*	-0.066	0.064	-0.285*	0.008	-0.034	0.082**	-0.095	-0.092	-0.033
	(0.029)	(0.021)	(0.035)	(0.082)	(0.090)	(0.169)	(0.010)	(0.065)	(0.041)	(0.065)	(0.065)	(0.078)
Safe Space	0.015	-0.020	0.025	0.049	0.155*	0.172	-0.011	-0.033	0.049	-0.105	-0.097	-0.063
	(0.029)	(0.021)	(0.037)	(0.086)	(0.093)	(0.178)	(0.008)	(0.065)	(0.041)	(0.067)	(0.067)	(0.078)
Friday						0.263						
						(0.232)						
Friday × Negotiation						0.620**						
						(0.296)						
Friday × Safe Space						0.176						
						(0.297)						
Negotiation × Ability											-0.068	
											(0.077)	
Safe Space × Ability											-0.116	
											(0.079)	
Control Mean	0.275	0.138	0.493	1.225	2.306	4.105	0.021	0.591	3.460	3.666	3.666	15.242
Number of Observations	1,573	1,476	1,573	1,573	1,573	1,573	1,484	1,477	1,477	1,473	1,473	1,569
Adjusted R²	0.021	0.051	0.173	0.267	0.096	0.228	0.005	0.018	0.036	0.095	0.095	0.060

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 third column is the hours spent doing chores before school, the fourth is hours spent during school hours, and the fifth is hours spent during after school hours on the last weekday. The sixth is the total number of hours spent doing chores on the last weekday. The outcome in column 7 is coded as 1 if parents reported that the girl has difficulty controlling her temper and 0 otherwise. The outcome in column 8 is coded as 1 if parents say the girl is rude and 0 otherwise. The outcome in column 9 is coded on a 1–4 scale, with 4 denoting most respectful. In columns 10 and 11, the outcome is the parent's ranking of a girl's ability relative to her classmates from 1–5. In column 12, the outcome is the number of years of school a girl reported wanting to complete. 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 mother, lives with the biological father, 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. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table 8: Effect of Negotiation Treatment on Parents' Behavior in the Investment and Dictator Games

	(1) Tokens Sent DG Game	(2) Tokens Sent IG Game	(3) Tokens Sent IG Game	(4) Tokens Sent IG Game
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)
Control Mean	4.949	5.362	5.362	5.362
Number of observations	297	1,224	1,224	1,224
Adjusted R^2	0.085	0.019	0.033	0.033

Effects of Negotiation and Safe Space in the Communication Arm of the Experiment

Negotiation & Communication			0.316*	0.316*
			(0.188)	(0.188)
Safe Space & Communication			0.054	0.049
			(0.186)	(0.189)
One-sided Test (P-value)			0.079	0.075

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 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. The bottom panel reports the net effect of being in the negotiation or safe space treatment and being allowed to communicate based on the coefficients in the same column, and the final row reports the p-value from a one-sided test of whether the negotiation and communication effect is greater than the safe space and communication effect. Standard errors are heteroskedasticity robust. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table 9: 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
Communication × Negotiation × Shock	0.464**	0.535*
	(0.234)	(0.273)
Negotiation	0.568	0.797
	(0.492)	(0.582)
Safe Space	-0.021	0.426
	(0.190)	(0.640)
Communication	0.166	0.378
	(0.454)	(0.595)
Communication × Negotiation	-1.369*	-1.582*
	(0.738)	(0.848)
Communication × Safe Space	0.363	-0.054
	(0.281)	(0.910)
Shock	0.437***	0.514***
	(0.097)	(0.137)
Negotiation × Shock	-0.202	-0.279
	(0.154)	(0.186)
Communication × Shock	-0.201	-0.272
	(0.141)	(0.194)
Safe Space × Shock		-0.150
		(0.200)
Communication × Safe Space × Shock		0.140
		(0.296)
Control Mean	4.575	4.575
Number of observations	1,219	1,219
Adjusted R^2	0.243	0.242

Pass-Through Rates for Girls in Communication Investment Game		
Control Pass Through	0.236**	0.242*
	(0.104)	(0.138)
Negotiation Pass Through	0.498***	0.498***
	(0.142)	(0.147)
Safe Space Pass Through		0.232
		(0.168)
One-sided Test for Neg vs. SS (P-value)		0.117

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. Column 1 includes controls for negotiation, safe space, communication, the communication game, fixed effects for the number of tokens parents sent the daughter, the triple interaction of the daughter's number of tokens, negotiation, and the communication game, and the relevant double interactions. 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. 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. Standard errors are heteroskedasticity robust. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table 10: 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)
Control Mean	0.006	0.003	0.033	6.668	7.065	0.618	1.405	0.827	0.790	0.022	14.997	14.978
School FE	N	N	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Standard Controls	Y	Y	Y	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 R²	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 and no longer including school fixed effects in the estimation. 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 on the last weekday. In columns 8 and 9, they were asked how much time they spent on school work on the last weekday. 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. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

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: Safe Space Treatment Appendix

The safe space program was designed to mirror as much as possible the elements of the negotiation program other than the actual content of the lessons themselves. Thus, because the negotiation program involved brief "ice breakers" at the start of each class, these were included in the safe space program too. Additionally, because the negotiation program had fun elements, girls were given opportunities to play games with one another during the safe space program. And, because the negotiation program included access to a female mentor, the *same* female "coaches" who taught the negotiation program served as the supervisors for the safe space program. However, other than the initial ice breakers, their role in the safe space program was passive. In the case of the safe space program, the supervisors were instructed to take a non-interventionist role. They would distribute lunches, begin the program with an ice breaker, and then allow the girls to play games or do homework with one another. The supervisors would maintain their presence for the same length of time as in the negotiation program. In order to encourage the girls to interact with one another, small games and items such as cards, jacks, and hula hoops were provided. We cannot rule out that the time to do homework and the unstructured interaction with other girls in a safe space provided benefits over and above what the negotiation program provided, since girls in the negotiation program did not have those benefits. However, we wanted to keep the total time spent in the program constant, in case our effects were driven by girls being kept from negative activities during that time period. Therefore, any additional effects of the negotiation program versus the safe space program should be interpreted as the lower bound of the marginal effects of the skills portion of the program only.

Appendix C: 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 start

of the 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 searched 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 D: Machine Learning

The machine-learning procedure requires splitting our 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 causal tree methodology chooses partitions of the training sample (e.g. girls with above or below a given value or set of values chosen by the algorithm for one or more covariates) for whom the treatment of interest (negotiation) is allowed to have different effects. 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), the algorithm can select the partitions in one part of the training sample that maximizes the out-of-sample predictive power for the other part of the training sample. By re-estimating the heterogeneous negotiation effects indicated by the machine learning on the training sample in a distinct estimating sample, we both ensure that our confidence intervals are valid and that we are not merely identifying spurious relationships by "over-predicting" random variation in the data.

While we do not choose the cut-off values for the partitions, we do choose the set of covariates over which to search. Searching over our full set of baseline covariates is problematic since many of these covariates are highly correlated. This means (1) that, given we are already splitting the sample, the intersections of these covariates could result in very small samples, and (2) that statistical noise could lead the machine learning to identify one covariate as important in one randomly chosen sample and to identify a different, highly correlated covariate in a different sample, making the results hard to interpret. Instead, we form two indices to include in the machine learning procedure, which capture two of the key determinants of parental human capital investment – altruism and ability. These factors are also key potential sources of heterogeneity in our theoretical framework.

We create an 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 and speaking Nyanja and English well and excellently. 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.

Finally, we 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, and age.

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**

This figure shows the invitation to participate in the experiment received by parents.

Figure A2: Negotiation Curriculum

1. **Me**
   a. Understand own interests
   b. Know other option

2. **You**
   a. Find the good
   b. Choose the approach
   c. Discover their interests

3. **Together**
   a. Identify shared interests
   b. Recognize roadblocks
   c. Close the distance

4. **Build**
   a. Brainstorm creative solutions
   b. Complete building checklist
   c. Decide what really matters

   Walk away as needed

Step to their side
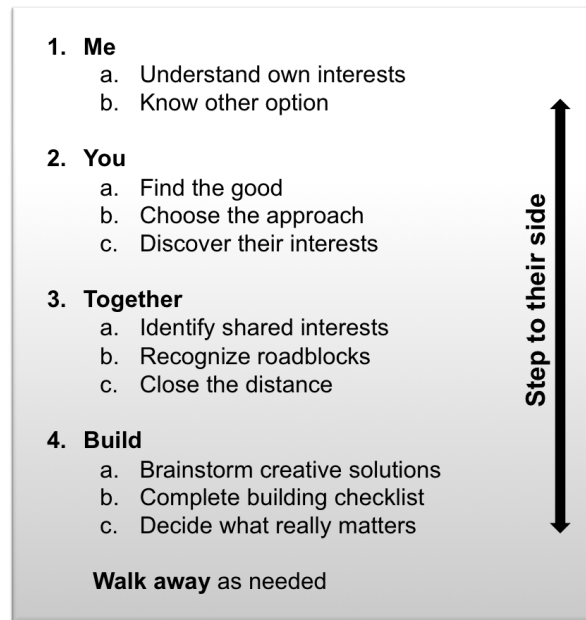
This figure shows the four key principles of the negotiation curriculum and their components.

Figure A3: Prizes from the Store in the Investment and Dictator Games

This figure shows both the prizes that the girls could purchase with tokens from the "Chuck E. Cheese"-style store in the investment game and talk-time, which the parent exchanged tokens for at the end of the game.

Figure A4: Distribution of Tokens Sent by Treatment When Parents and Daughters Do and Don't Have the Opportunity to Communicate
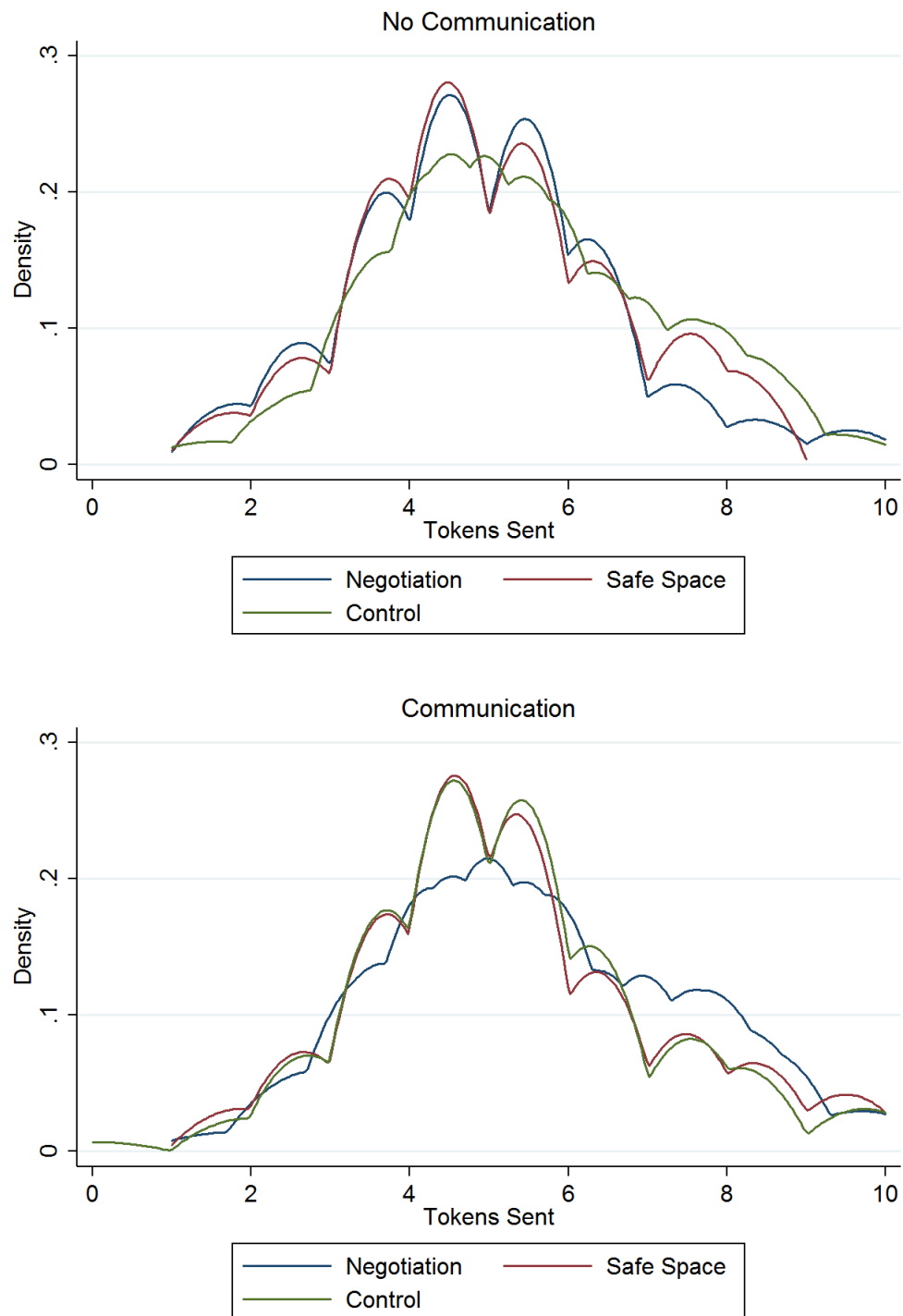
This figure graphes kernel density plots of the number of tokens parents sent to their daughters in the investment game in the safe space, control, and negotiation treatments when communication is not allowed (top graph) and when it is allowed (bottom graph).

Appendix Tables

Table A1: Comparison Between Intervention Schools and Other Zambian Schools

	(1) Intervention Schools Mean	(2) SD	(3) Urban Gov. Schools Mean	(4) SD	(5) T-test (P-value)	(6) Full Gov. Sample Mean	(7) SD	(8) T-test (P-value)	(9) Full Sample Mean	(10) SD	(11) T-test (P-value)
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
Student Teacher Ratio	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 controls for 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. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A3: Effects of Negotiation, Safe Space, and Information on Human Capital Average Effect Sizes Under Different Specifications

	Nego Coef.	Se	Safe Space	Se	One-Sided P-value Neg vs. SS	Information	Se	One-Sided P-value Neg vs. Info
Baseline	0.062***	0.019	0.021	0.018	0.020	-0.017	0.016	0.002
Parsimonious Controls	0.051***	0.019	0.019	0.017	0.039	-0.009	0.015	0.011
No Paid Fees	0.069***	0.022	0.025	0.019	0.017	-0.010	0.018	0.006
No Took National Exam	0.063***	0.021	0.019	0.019	0.010	-0.017	0.017	0.003
No Attendance	0.064***	0.022	0.017	0.020	0.013	-0.025	0.018	0.003
No Enrollment	0.055***	0.022	0.008	0.019	0.010	-0.012	0.017	0.016
No Math	0.057***	0.020	0.023	0.018	0.040	-0.021	0.017	0.008
No English	0.062***	0.021	0.037*	0.019	0.079	-0.019	0.017	0.005
No Test Scores	0.057***	0.021	0.044**	0.020	0.287	-0.025	0.018	0.003

This table reports estimates of the human capital average effect size and for negotiation, safe space, and information in the full sample of girls in treated schools under different specifications. The table also reports p-values from a one-sided test of whether the negotiation effects are greater than the information and safe space effects for each specification. Row 1 reports the baseline specification from Table 3. Row 2 reports the results for a specification that only controls for whether a girl reads and speaks Nyanja excellently and whether she speaks English excellently, as well as a control for whether a girl received the information treatment and school fixed effects. Row 3 reports the AES omitting whether parents paid school fees as a component. Row 4 omits taking the national exam, row 5 omits attendance as a component, row 6 omits enrollment, row 7 omits the math threshold, row 8 omits the English threshold, and row 9 omits both scores. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A4: Mediation Analysis of the Negotiation Effect

	(1) Human Capital Index	(2) Human Capital Index	(3) Human Capital Index	(4) Full Index	(5) Full Index	(6) Full Index
Negotiation	0.102***	0.083**	0.060*	0.101***	0.086***	0.067**
	(0.033)	(0.033)	(0.032)	(0.030)	(0.030)	(0.031)
Mean Knowledge Measure		0.024**			0.019	
		(0.011)			(0.011)	
IV Knowledge Measure			0.055**			0.044*
			(0.024)			(0.023)
First Stage F-Statistic			36.33			36.33
Number of Observations	1,482	1,482	1,482	1,482	1,482	1,482
Adjusted R^2	0.045	0.048	0.058	0.053	0.055	0.071

This table reports estimates of the effect of negotiation on the human capital and full indices for the sample of girls for whom measures of negotiation knowledge are available. Columns 2 and 5 measure knowledge of negotiation as girls' average scores across the three knowledge measures. Columns 3 and 6 reduce measurement error by instrumenting for a girl's score on the third knowledge measure with her score on the previous two. All columns include controls for 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. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A5: Association Between Girls' Characteristics and Appearing in the Midline

| | **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. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A6: Heterogeneous Effects of Safe Space by Ability

	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
Safe Space × High Ability	0.046	0.046	0.057	0.075**	-0.001	-0.038	0.001	0.013
	(0.049)	(0.039)	(0.039)	(0.034)	(0.040)	(0.043)	(0.012)	(0.022)
SD			0.122	0.249**	-0.002	-0.091	0.007	0.048
Safe Space × Low Ability	0.066	0.098***	-0.025	0.060	0.035	-0.003	0.023*	0.046*
	(0.053)	(0.036)	(0.039)	(0.037)	(0.031)	(0.035)	(0.011)	(0.024)
SD			-0.054	0.199	0.084	-0.006	0.147*	0.167*
Control Mean	-0.018		0.693	0.864	0.532	0.711	0.211	0.220
Number of Observations	1,021		1,034	1,047	1,048	1,054	1,047	1,047
Adjusted R²	0.027		-0.002	0.014	0.021	0.023	-0.006	0.051
	Panel B. Full Index and Pregnancy							
	(1) Full Index	(2) Full AES	(3) Ever Pregnant					
Safe Space × High Ability	0.052	0.022	-0.024					
	(0.052)	(0.037)	(0.024)					
SD			-0.125					
Safe Space × Low Ability	0.060	0.074**	-0.012					
	(0.052)	(0.033)	(0.018)					
SD			-0.060					
Control Mean	-0.019		0.041					
Number of Observations	1,021		1,054					
Adjusted R²	0.034		0.030					

This table reports estimates of the effect of safe space by whether or not girls' ability factor is greater than 0.45 (the partitions of the treatment effect indicated by the machine learning) on outcomes collected in the administrative data. An ability factor above 0.45 corresponds to the top 40th percentile of the ability distribution. The estimation data set is a randomly chosen subsample of the full data which was held back and not used in the machine learning that established partitions of the data. Estimates are reported both in the natural units of the data and in terms of standard deviations of the control group, so that the effects are in the same units as the average effect sizes. **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. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A7: 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 controls for 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. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A8: Number of Girls in Each Treatment in the Investment Game

	(1) DG	(2) Communication	(3) Non-Communication
No Word Game	333	318	350
Word Game	0	329	332

This table reports the number of girls assigned to each treatment arm in the lab-in-the-field investment game.

Table A9: Association Between Investment in the Lab-in-the-Field Game and Human Capital Investment

	(1) Human Capital Index	(2) Human Capital Index	(3) Human Capital Index
Tokens Sent	0.015	0.014**	
	(0.020)	(0.007)	
Tokens Sent × Communication			0.021*
			(0.011)
Tokens Sent × No Communication			0.006
			(0.012)
Control Mean	0.021	0.003	0.003
Sample	DG Only	IG Only	IG Only
Number of Observations	286	1,204	1,204
Adjusted R^2	0.041	0.037	0.037

This table reports the relationship between the number of tokens parents sent girls in the investment game and the human capital index. 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. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A10: 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 girl had found at least half the words in a word game. All columns include controls 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. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A11: 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 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. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A12: 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)
Control Mean	9.152	9.152	9.152
Number of Observations	1,219	1,219	1,219
Adjusted R^2	0.022	0.037	0.037
Effects of Negotiation and Safe Space in the Communication Arm of the Experiment			
Negotiation & Communication		0.417	0.418
		(0.360)	(0.372)
Safe Space & Communication		-0.285	-0.292
		(0.372)	(0.372)
Neg vs. SS One-Sided Test (P-value)		0.029	0.027

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 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. The bottom panel reports the net effect of being a negotiation or safe space girl in the communication game (relative to a control girl in the communication game). The last row reports the p-value of a one-sided test of whether negotiation has larger effects than safe space among girls in the communication treatment. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A13: How Girls Spent the Tokens

	(1) School Supplies DG Only	(2) Household Items DG Only	(3) Pure Consumption DG Only	(4) School Supplies IG	(5) Household Items IG	(6) Pure Consumption IG	(7) School Supplies IG	(8) Household Items IG	(9) Pure Consumption IG
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)
Mean Control	3.813	2.040	6.682	2.074	1.113	5.392	2.074	1.113	5.392
Number of Observations	297	297	297	1,221	1,221	1,221	1,221	1,221	1,221
Adjusted R²	0.226	0.056	0.333	0.208	0.095	0.356	0.208	0.096	0.359
Effects of Negotiation and Safe Space in the Communication Arm of the Experiment									
Negotiation & Communication							0.197	0.171	-0.364
							(0.247)	(0.224)	(0.280)
Safe Space & Communication							0.041	0.115	-0.160
							(0.238)	(0.225)	(0.277)

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. Household items 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 Nyanja well, speaking Nyanja well, reading English well, and speaking English well. The bottom panel reports the net effect of being a negotiation or safe space girl in the communication game (relative to a control girl in the communication game). Standard errors are heteroskedasticity robust. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

73

Table A14: Evidence on Spillovers by Daughter's Ability

	(1) Pile, Male	(2) Pile, Female	(3) Chores, Male	(4) Chores, Female	(5) Schoolwork, Male	(6) Schoolwork, Female	(7) Pay for Girls Over Boys	(8) Years Schooling, Male	(9) Years, Schooling Female
Negotiation × High Ability	0.201	0.180	-0.003	0.111	0.011	0.128	0.057***	0.064	-0.105
	(0.211)	(0.215)	(0.163)	(0.104)	(0.139)	(0.120)	(0.021)	(0.161)	(0.162)
Negotiation × Low Ability	-0.036	-0.067	-0.033	-0.077	0.054	0.146	0.032**	-0.008	0.050
	(0.188)	(0.186)	(0.142)	(0.077)	(0.116)	(0.100)	(0.015)	(0.140)	(0.127)
Safe Space	0.027	-0.170	0.037	0.050	0.069	0.112	0.017	-0.062	-0.004
	(0.152)	(0.157)	(0.117)	(0.074)	(0.094)	(0.085)	(0.012)	(0.118)	(0.110)
Control Mean	6.668	7.065	0.618	1.405	0.827	0.790	0.022	14.997	14.978
School FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Standard Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Number of Observations	1,160	1,205	1,203	1,166	885	945	1,476	1,094	1,148
Adjusted R^2	0.016	-0.016	0.005	-0.005	0.020	0.013	0.008	0.047	0.041

This table reports the results of tests for spillovers on siblings from the negotiation program by the treated girl's ability. For columns 1 and 2, parents were asked to divide up 20 tokens to represent how they would allocate resources to the treated girl and her nearest male (column 1) and female siblings (column 2). In columns 3 and 4, parents were asked how much time the male and female siblings spent on chores in hours on the last weekday. In columns 5 and 6, they were asked how much time they spent on school work in hours on the last weekday. In column 7, they were asked if they were now more likely to pay girls' school fees over boys'. In columns 8 and 9, 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. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.