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

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Abstract

Using a randomized controlled trial, we study whether a negotiation skills training can improve girls' educational outcomes in a low-resource environment. We find that a negotiation training given to 8th grade Zambian girls significantly improved educational outcomes over the next three years, and these effects did not fade out. To better understand mechanisms, we estimate the effects of two alternative treatments. Negotiation had much stronger effects than an informational treatment, which had no effect. A treatment designed to have more traditional girls' empowerment effects had directionally positive but insignificant educational effects. Relative to this treatment, negotiation increased enrollment in higher quality schooling and had larger effects for high ability girls. These findings are consistent with a model where negotiation allows girls to resolve incomplete contracting issues with their parents, yielding increased educational investment for those who experience sufficiently high returns. We provide evidence for this channel through a lab-in-the-field game and follow-up survey with girls and their guardians.

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1 Introduction

In highly constrained environments, why do two children with similar abilities, coming from households with similar financial resources, receive very different levels of education and have vastly different later-life outcomes? To answer this question, we examine whether children's non-cognitive skills, a form of non-financial resource, can help explain this variation, focusing on adolescent girls in Zambia. Adolescent girls and their parents face enormous constraints in this environment. School fees increase and girls' dropout rates spike at the end of middle school. If parents do not fully internalize the benefits of schooling to their daughters and there is imperfect contracting over schooling investments, a girl may not be educated even if it would be efficient based on her ability. Girls may then find themselves powerless to navigate the constraints they face, and economic development and social welfare may suffer as a result.

How, then, to empower girls to overcome these constraints? Empowerment has traditionally meant the degree to which one has independent control of one's life circumstances (Bandiera et al., 2018). Thus, one way to create empowerment would be severing girls' dependence on other decision-makers. We explore an alternate source of power: utilizing the interdependence of decision-making to generate joint gains. While girls in Zambia recognize the extent to which they are reliant on their parents, they may not realize the extent to which their parents' utility is dependent on *them*. Thus, girls have a nascent form of power that is rarely emphasized. By influencing the household welfare function, girls have the power to affect their parents' decisions and, as a result, their own outcomes. This idea aligns with a growing literature that recognizes children as active agents within the household rather than passive consumption vehicles or receptacles for investment (Bursztyn and Coffman, 2012; Cosconati, 2013; Del Boca, Monfardini and Nicoletti, 2017; Sutter, Zoller and Glätzle-Rützler, 2019).

We use a randomized controlled trial to study the impact of a two week interpersonal skills training in advance of the peak period for female dropout. This training taught girls to use their agency within the household to affect overall household surplus and thus parents' decision-making. Following the tradition in the business world of training executives in the skill of reconciling different interests by looking for "win-win" solutions, we term this training "negotiation training." Indeed, the curriculum was designed to teach girls to identify their own and others' interests and develop solutions that create value for both parties. Thus, negotiation skills may alleviate incomplete contracting problems between parents and their daughters arising from different preferences over education.

The negotiation treatment is related to a movement focusing on empowering young

women to improve development outcomes.[1] Negotiation skills are designed to endow young women with a specific form of empowerment that may be particularly well-suited to an environment with strong cultural traditions of obedience and reciprocity to parents, such as exists in Zambia (Whiting and Whiting, 1973; Munroe and Munroe, 1972; Harkness and Super, 1977; Wenger, 1989).[2] At the same time, a training designed and administered exclusively to girls by female coaches may also have more traditional empowerment effects. The program may lead girls to have higher aspirations and to place a greater weight on their own utilities. We refer to this as "individualistic empowerment," and include another treatment arm, "safe space," to capture its effects. Safe space was designed to have individualistic empowerment effects without teaching negotiation skills. While individualistic empowerment may increase girls' determination to pursue education, if it also undermines norms around respectfulness and deference to parents, it could have the unintended consequence of exacerbating incomplete contracting problems. Thus, understanding the effects of this treatment is important in its own right.

We randomly assign 2,366 8th grade girls in 29 schools to be in either the negotiation, safe space, or control treatment. An additional 12 schools serve as "pure control" schools to assist in the measurement of spillovers to untreated girls. We also cross-randomize all arms with an informational intervention to test another possible means of empowering girls—arming them with information required for decision-making about educational investment. To measure the effects of these treatments, we track enrollment in the subsequent three grades. We also measure whether girls enrolled in the higher-ability schooling track, called "morning schooling," in 10th and 11th grade. This track requires girls to perform well on a national exam in order to enroll and provides higher quality educational inputs. We complement these measures with additional, shorter-term administrative data. We also collect follow-up survey data from girls and their guardians and study their behavior in a lab-in-the-field game designed to measure the effect of negotiation skills in a controlled setting.

We find that the negotiation training has large impacts on enrollment by 11th grade, reducing dropout during the critical transition to secondary school. For 11th grade, our longest-term enrollment outcome, the treatment increases enrollment by 4.4 percentage points (10%). In contrast, the negotiation treatment has little effect in 9th grade, when continued enrollment depends more on girls than their parents. Thus, for enrollment, un-

[1] In a systematic review of 77 studies of adolescent girl programs in low- and middle-income countries, Haberland, McCarthy and Brady (2018) find that 30% of the programs had girls' empowerment or leadership as an objective.

[2] Murris (2016) writes of parent-child relationships in Africa, "The idea often written about in African philosophy is that African societies are characterized by communal interdependence... Hierarchies are written into the nature of the universe, with children low in the hierarchy – subservient (obedient and respectful) to adults and ancestors. The child's place is to serve this extended family, with obedience as a prerequisite and reinforced through physical punishment."

like many other educational interventions, the effects of the negotiation training accumulate rather than fade out over time. Negotiation also has large effects on the probability of being enrolled in the higher ability track. By 11th grade, negotiation increases the probability of being enrolled in a morning program by 4 percentage points (16%). Supporting the longer-term results, negotiation also increases aggregate measures of shorter-term human capital outcomes.

The comparison with the safe space treatment provides some evidence on the role of skills versus other empowerment elements in creating this impact. The negotiation treatment has statistically (and economically) significantly larger effects on enrolling in the higher ability track and directionally larger effects for all outcomes. Results from a machine learning exercise that identifies sources of heterogeneity in the negotiation effect are also consistent with differences in the negotiation and safe space treatment effects. Girls in the top 40% of the ability distribution, who were on the margin of enrolling in 10th and 11th grade, benefited the most. Safe space does not exhibit the same heterogeneity and has zero effect on the long-term enrollment of high ability girls. The fact that the pattern of the safe space estimates is so different suggests that the individualistic empowerment elements of the negotiation treatment alone are unlikely to drive the human capital effects. Negotiation also consistently has statistically larger effects than the cross-randomized information treatment, suggesting that its treatment effects are not due to learning about the returns to education from female mentors or examples in the curriculum.

We then test a model where the educational decision is partly a strategic interaction between parents and daughters, and negotiation skills allow daughters to expand the feasible contracting space for reciprocity of investments. To do so, we use a lab-in-the-field game to allow girls to use their negotiation skills with parents in a controlled environment where the returns to investment cannot be impacted by the treatment. In the game, parents are endowed with tokens that they can choose to pass on to girls. Any tokens passed increase in value. Girls can then choose to either return or spend any tokens they receive, aiming to mimic both the return and risk of educational investments for parents. When parents and daughters communicated before parents decided, the negotiation treatment led parents to send significantly more tokens. Two additional variants of the game provide evidence that this effect is not driven by altruism or individualistic empowerment. Complementing these findings, a follow-up survey provides evidence that girls made concurrent transfers in response to greater educational investment.

This paper contributes to a growing literature on the importance of non-cognitive skills (Heckman and Rubinstein, 2001; Kautz et al., 2014; Attanasio et al., 2015; Alan, Boneva and Ertac, 2019) by showing how to build the capacities that make an individual successful.

While much of the literature has focused on non-cognitive skills that develop in the critical period before age 5, adolescence may also be a critical period. Skills related to interpersonal communication have been shown to develop most quickly in adolescence (Choudhury, Blakemore and Charman, 2006). Thus, we focus on a particularly important period for intervention in terms of both the potential for acquiring non-cognitive skills and the vulnerability of the population. By measuring the effects of the different components of a treatment that is designed to improve non-cognitive skills, we provide evidence that the specific skills aspect of the intervention is important. Moreover, we find that these skills affect human capital investment in ways that *accumulate* rather than fade out over time. From a policy perspective, we add to growing evidence that it is not too late to teach these skills in adolescence, suggesting that these skills could be taught directly within the school system.

This paper also contributes to the literature on intra-household bargaining and inefficiencies in investment within the household (Udry, 1996; Ashraf, 2009; Bobonis, 2009). While much of this literature has focused on spouses rather than parents and children,[3] this paper shows that, in a context where parents and children have different preferences over educational investments, household members can learn skills that increase the contracting space. These skills help households get closer to the efficient frontier, in the spirit of the theoretical work of Chassang (2010) and Watson (1999).

Finally, this paper establishes a causal link between negotiation skills and economic outcomes. Despite the large amount of resources spent on these trainings at business and law schools, little is known about their effects.[4,5] While there are growing efforts to expand access to these skills to other populations,[6] negotiation training is usually only available to the most economically advantaged. If these skills are indeed effective at changing economic outcomes, providing this powerful tool only to the most privileged could perpetuate inequality.

The paper is organized as follows. Section 2 describes the negotiation treatment and the experimental design. Section 3 develops a simple model to guide our analysis of the mechanisms driving the negotiation treatment effect. Section 4 measures the effects of negotiation and the alternative treatments on enrollment, morning schooling, and other human capital

[3]Exceptions that focus on intergenerational intra-household bargaining are Bergman (2015); Jensen and Miller (2017); Ashraf, Bau, Nunn and Voena (forthcoming); and Bau (2019).

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

[5]Evidence of negotiation's efficacy rests on measures of participants' ability to identify mutually-beneficial trades within simulated negotiations or on subjective measures of efficacy (e.g., Gist, Stevens and Bavetta, 1991; Movius, 2008; Nadler, Thompson and Boven, 2003; Zerres, Hüffmeier, Freund, Backhaus and Hertel, 2013). Studies on negotiation measuring behavioral outcomes examine either very short-term measures inside the lab (Small, Gelfand, Babcock and Gettman, 2007) or find no effect (Hobfoll et al., 2002).

[6]Mercy Corps has implemented over 100 conflict management programs since the 1990s. Conflict resolution training effectively reduces disputes in areas with weak rule of law (Blattman, Hartman and Blair, 2014; Hartman, Blair and Blattman, 2018).

outcomes. Section 5 uses the follow-up survey and the lab-in-the-the-field investment game to test for the different mechanisms laid out by the model. Section 6 concludes.

2 Experimental Design & Data

In this section, we document the design and timing of the negotiation, safe space, and information interventions, as well as the timing of our data collection. We then discuss our outcome variables, collected over the subsequent three years, which allow us to both measure the effects of the negotiation training and shed light on the different mechanisms underlying the negotiation effect.

2.1 Experimental Design and Timeline

We study the effects of a randomized controlled trial targeting 8th grade girls at 41 primary schools throughout Lusaka, Zambia. These 41 schools are the universe of co-ed government schools with sufficient enrollment to allow for within-school randomization. Of the girls approached to take part in the experiment at these schools, 67% received permission from their guardians to participate (and agreed themselves). Appendix Figure A1 shows the template for the letter sent to parents to invite them to participate in the study. We collected baseline data from the set of girls whose parents agreed.[7] The data collection is described in greater detail in Appendix A. At the time of the baseline, girls were randomly assigned at the individual level to receive an information treatment on health and the returns to education.[8]

We then randomly chose 29 three-arm treatment schools. Within these schools, we stratified by classroom and information treatment and randomized girls at the individual level into three groups: (1) control group (780 girls), (2) safe space group (785 girls), and (3) negotiation group (801 girls). The experimental design is shown in Figure 1. Following standard practice, we control for classroom fixed effects and the info treatment, our randomization strata, throughout our analyses of the interventions' effects within these 29 schools (Glennerster and Takavarasha, 2013). The girls were informed that the randomization would

[7]Because of the tight implementation time-line, some girls whose parents had consented were randomized into the experiment without having been surveyed with the goal of surveying them and informing them of their treatment status on the day of the intervention. Due to this, 4.6% of girls did not receive baseline surveys and usually did not learn their treatment status in time to take part in the intervention. This was not differential by treatment assignment (see Appendix Table A1), and we exclude these girls from most of our analyses. In Appendix D we demonstrate that our main results are robust to including these girls.

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

be done by a computer and that they might receive one of two programs or be assigned to receive a program later (the control group).

The remaining 12 schools were then assigned to be a "pure control" group.[9] Thus, one of our strategies to assess the extent of spillovers is to compare control girls in the treated schools to girls in the pure control schools.

Three to four months after the negotiation and safe space interventions, follow-up data was collected at the same time as the lab-in-the-field experiment was conducted. We then continued to collect administrative data on the girls' educational and pregnancy outcomes for the next three years. Figure 2 documents the timeline of the study.[10]

Table 1 reports summary statistics for the 29 treated schools, and the results of balance tests between intervention groups, controlling for classroom fixed effects. The table shows that most characteristics are balanced for the negotiation treatment relative to the safe space and control treatments, with a p-value for joint tests of whether the covariates significantly predict negotiation treatment status relative to the control of 0.311. However, there is some evidence that girls who received the negotiation treatment are lower ability. They are 4.8 and 5.7 percentage points less likely to read and speak Nyanja (the vernacular) excellently and 4.9 percentage points less likely to speak English excellently relative to the control. Given that we test balance across 14 outcomes, these may be significant by chance. If negotiation girls *are* slightly lower ability, this is likely to negatively bias our results.

Appendix Table A2 compares our intervention schools to other urban government schools in Zambia that offer 8th grade (columns 1-5), all government schools in Zambia that offer 8th grade (columns 6-8), and all Zambian schools, including private and community schools, that offer 8th grade (columns 9-11). The intervention schools are larger than other urban schools on average, 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.

[9]Treatment and pure control schools were assigned through a matched pair randomization using pre-baseline administrative data to make 12 pairs of schools that were similar on geography, number of girls, and percent on scholarships. One school in each pair was then randomly assigned to be a three-arm school, and the other was assigned to be a pure control school. We discuss this further in Appendix H.

[10]In line with our commitment to the Zambian government to offer the program to control schools and control girls, we expanded the negotiation training program to pure control schools after the grade 9 exam was taken, and to control girls from treatment schools during the 10th grade school year. For budgetary reasons, we stopped tracking girls in pure control schools after the scale-up was completed, planning to use the short-term outcome measures to look for spillovers. We continued tracking treatment school participants for an additional year after scale up, discussed further in footnote 17.

2.2 Negotiation Treatment

The negotiation 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), Mercy Corps (2009), and the classic negotiation texts of Fisher, Ury and Patton (2011) and Ury (1993).[11] Attendance rates for these sessions were high, with the average girl attending 4.8 out of 6 days. The curriculum (McGinn, Low and Ashraf, 2012) was designed to allow girls to understand their ability to potentially affect other people's decisions without violating cultural norms of deference to elders. A key component was recognizing the potential for agreements that result in joint gains in a situation where these gains are not immediately obvious. Recognizing this potential allowed girls to propose alternatives to their parents without being viewed as disrespectful.

A canonical example in the negotiation literature that helps illustrate how negotiation skills can create "win-win" solutions, which we adapted for the curriculum, goes as follows:

> *Two sisters are arguing over an orange. One says "I saw the orange first, so I should get it!" The other says, "I'm older so I should get it!" They go back and forth, getting more and more angry, until finally they compromise and cut the orange in half. One takes her half of the orange, peels it, throws away the rind, and eats the inside. The other takes her half of the orange, peels it, throws away the inside, and uses the rind to make a cake.*[12]

By using negotiation skills, the sisters could have realized that they wanted the orange for different things, and thus could both have had what they wanted, expanding the available surplus. Examples of girls being able to increase joint surplus in their real lives, in addition to reciprocating educational investments, might include agreeing with siblings about times when it is least costly for each to watch younger children or working with parents to do housework at a time that does not interfere with schoolwork.

In the negotiation literature, a distinction is made between positions and interests. Whereas positions tend to be diametrically opposed (e.g., "I want the orange," and "No, I want the orange!"), interests may be reconcilable (e.g., "I want a snack," and "I want to bake a cake."). Thus the key steps of the curriculum involve determining one's own interests, determining the other party's interests, identifying areas of overlap or profitable exchange, and then crafting a solution that creates joint gains.

These four steps were called Me, You, Together, and Build (see Appendix Figure A2), and formed the structure for the curriculum. These steps were designed to teach skills

[11] The curriculum is freely available under a creative commons license at https://hbsp.harvard.edu/girls-arise/.

[12] Adapted from Fisher, Ury and Patton (2011).

typically found in an MBA-style negotiation class, but adapted for the age of participants and the cultural context. Some activities directly mimicked real situations that girls might face, while other types of exercises were more abstract, such as games that illustrated the impact of one's own choices on long-term payoffs for both parties. We now discuss each of the four steps in greater detail.

Me. This step 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. 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 could walk away if the agreement options did not serve the girls' needs and interests.[13] Girls were also taught to focus on regulating their emotions, "taking five" when they were angry.[14]

You. This step emphasizes the importance of understanding the other party and discovering their interests. Girls learned to "step to the side" of the other party, taking their perspective. Galinsky, Maddux, Gilin and White (2008) show that individuals who can take their partners' perspectives generate more efficient solutions in cases where a deal seemed impossible. In this particular context, this is a crucial step for girls to see that their negotiating partner is not a fixed, "dogmatic" actor but rather motivated by incentives, which may be affected by the girl's actions. Understanding a parent's utility function allows a girl to see how she can make transfers or trades that would alter the parents' willingness to invest. Recognizing that such deals exist can be thought of as expanding the feasible contracting space. Typically this step is done using open-ended questions. However, since direct questions to a parent can be considered rude in the Zambian context, girls were taught to use indirect questions to identify their parents' interests.

Together. This step taught the girls to look for common ground with their negotiating partner and treat resistance as a roadblock to be solved together. First, girls were taught to recognize and emphasize the shared values with the person they were negotiating with. This removed the mindset that parents were acting from dictatorial whims, which obscures profitable exchanges. For example, instead of thinking or saying, "If you cared about me, you would pay my school fees," a girl might substitute, "We both care about education, let's find a way to make this work." Second, they were taught to see other people's decisions as a product of constraints, rather than fixed preferences. For example, a parent might say "no"

[13]Calculating a walk-away value and incorporating the walk-away value into negotiation planning and execution is fundamental to negotiation analytics (Raiffa, 1982; Walton and McKersie, 1965).

[14]Fabiansson and Denson (2012) show that such emotional regulation is important since anger hinders bargaining.

to paying for something because they needed to pay for other things, rather than because they did not care about the girl. Girls could then realize that if they were able to problem solve with their negotiating partner and help remove the roadblocks, or constraints, they might be able to change the outcome.

Build. In this step, girls learned to brainstorm solutions to roadblocks and look for "win-win" agreements that met the needs of both negotiating partners. Girls were taught ways to brainstorm together with their negotiation partner to look for new solutions to external constraints. Moreover, they were also taught to look for productive trades, where one person cares about something a lot, but it is easy to give for the other person. 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. Such solutions may not have been in the feasible contracting space in the absence of negotiation, but negotiation skills could expand that space.

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.*

The approach laid out by the curriculum focused on cooperative actions that allowed girls to get their needs met, rather than teaching them to "bargain" for all the surplus. In this way, it is related to a theoretical literature in relational contracting that demonstrates how the establishment of dynamic cooperation can lead to more efficient outcomes (Kranton, 1996; Ghosh and Ray, 1996; Chassang, 2010). Because of the emphasis on understanding the other party's utility functions, we also view it as related to breaking out of a "cursed equilibrium" (Eyster and Rabin, 2005) that can be caused by misperceptions and lack of communication. Appendix B provides more qualitative information from the girls in the sample about how they used the training in everyday life.

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 follow-up survey (three to

four months after the intervention).[15] Appendix Table A3 regresses girls' scores on different questions in the scenario 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 follow-up occurred several months after the negotiation classes, this provides evidence that the classes had persistent effects on girls' knowledge of negotiation skills and how to apply them. 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.

To test whether girls applied their negotiation skills in the household, we also designed a module to ask *guardians* about girls' behavior in the household during the follow-up survey. Appendix Table A4 shows that negotiation girls also behave in a way that is more aligned with the negotiation curriculum according to their parents or guardians. These results suggest that girls did not just know about the skills theoretically but were able to apply them in ways that were observable to their parents.

2.3 Safe Space Treatment

The safe space program was designed to have individualistic empowerment effects and the same ancillary benefits of negotiation without imparting negotiation skills. Thus, we can compare the negotiation and safe space treatment effects to determine if the *skills* component of the negotiation training is important. In the safe space program, girls met for the same number of sessions under the supervision of the same female mentors as the negotiation training.[16] However, in the place of the negotiation curriculum, the mentors launched each session with some songs and cheers and then allowed the girls to play games, do homework, or just talk with one another. We provided simple games and materials such as cards, jacks, and hula hoops. The safe space program also had all the same small, auxiliary benefits as the negotiation program (free lunch on session days, a notebook, and pens) and affected girls' time spent in an after-school program in the same way. The common individualistic empowerment benefits between the two programs are the provision of female role models and a positive, girls-only space as part of a program focused specifically on girls. However, safe space may have had a greater impact in other areas, such as building social capital among

[15] 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. According to this coding scheme, 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; and brainstorming solutions.

[16] Attendance rates were not statistically significantly different between the negotiation and safe space treatments. The average girl in the safe space treatment attended 5 days (relative to 4.8 in negotiation).

girls, since girls had more free time to spend with one another. Some girls may have also found it more fun or appealing, since it was free time rather than structured learning time. We further describe the safe space treatment in Appendix B.

2.4 Information Treatment

The information intervention was intended to measure the effects of providing information about the returns to education or health protection, which may also be unintentionally transferred through the negotiation curriculum (since it used both educational and health examples). The information treatment provided *more* information than the negotiation program, so we should not think of it as nested in the negotiation program. Rather, comparing the treatments allows us to compare pure information effects to negotiation effects. This treatment is described in more detail in Appendix B. Since negotiation was cross-randomized with information, we can also test for any complementarities between the interventions.

2.5 Schooling Outcomes

In this subsection, we describe the two main measures we use to evaluate the treatments' effects on education over the subsequent three years.

Enrollment. Enrollment is our most important measure since it allows us to capture the longer-term educational effects of negotiation. Many of our shorter-term measures can be seen as investments that need to be made to ensure a girl remains enrolled. Thus, enrollment captures the aggregate effects of both observable and unobservable investments. As girls had to be enrolled in grade 8 to participate in the program, we measure enrollment for grades 9, 10, and 11.[17]

The barriers to enrollment across grade levels vary in significant ways in our context. Institutionally, girls are very likely to progress into 9th grade conditional on being enrolled in 8th grade. There is no high-stakes national exam to pass at this transition, and schools are prohibited from barring girls from class for non-payment of fees (although they may pressure parents to pay). To progress to 10th grade, however, a girl must cross a series of important hurdles. The girl must take the national exam and receive a sufficiently high score to be admitted to a school, and parents must pay *both* any outstanding fees for 9th grade (or else a girls' scores will not be released) and the entry fee for 10th grade in order to enroll at the new school. Thus, enrollment in grades 10 and 11 captures whether a girl has passed the peak dropout period at the transition between schools.

[17]Because the program was scaled up during grade 10 at the treatment schools, grade 11 enrollment effects could be negatively biased by "catch-up" from girls in other arms. However, since the program was offered through schools, girls who had already dropped out were unlikely to be affected by the expansion.

Enrollment in 9th grade was measured administratively from the primary schools we were working with for the study. Therefore, false negatives would only occur if a girl moved away from the area but enrolled in a government school elsewhere. Enrollment for grades 10 and 11 was captured by having teams of data collectors verify girls' presence at schools either directly or through official school rosters. This is administrative data, in the sense that it comes from official status, and not self-reports, but there is no central enrollment database. Accordingly, there is some possibility for false negatives if a girl enrolled in a school in which she was not expected to enroll and was not found by the data collection team. It should also be noted that enrollment is only coded as "1" if girls enrolled in a government school program, since any other schooling is lower quality and does still reflect a worse educational outcome. This process is discussed further in Appendix A.

Schooling Type. We additionally measure enrollment in "morning school," the higher quality ability track within the Zambian school system. To officially be promised a place in 10th grade, girls must score above a threshold on their national exam, which was a score of 361 in the year our participants finished 9th grade (in the top 27% of exam takers). Girls with a lower score could potentially get a place in "afternoon school," if a school had space available.[18] These schools differ in terms of inputs, as well as students' ability. Historically, afternoon school was introduced as remedial classes – for a fee – serving students who had been denied placement in the official school system. These classes, called Academic Production Units, essentially functioned as a private school operating on government school grounds (Verspoor, 2008). In 2011, the government formally abolished APUs, and stated that all students must be incorporated into the government system (Lusaka Times, 2011). In practice, however, distinctions between the afternoon and morning program remain. In addition to the peer group being different, morning and afternoon girls in secondary school wear different uniforms, and girls typically cannot take "pure science" (essentially, STEM) in the afternoon program. Contact hours are also higher in the morning program, and teacher effort and attention are likely higher. Moreover, the afternoon program does not include exam preparation for the government exam that girls must pass to graduate 12th grade. Thus, girls morning school are much more likely to continue their education after high school.

2.6 Additional Human Capital Outcomes

To further investigate the effects of the experimental treatments on girls' educational outcomes, we next consider five additional measures. In addition to these measures, we also

[18] Schools offer one school day starting at 7am and a second after the standard school day has finished.

collect data on pregnancy, a health outcome that is potentially related to education. To account for multiple hypothesis testing, we introduce aggregate measures across the different human capital outcomes.

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. The data was collected directly from the experimental schools.

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 school and receive her certificate. Additionally, the results are used to assign girls to secondary schools. Most girls (90% of the control) took the exam.

Threshold Math and English. These two measures are coded as 1 if girls took the national exam and scored in the top 27% in math and English respectively and 0 otherwise. Scoring in the top 27% of the exam is the official threshold for being assigned to secondary school in Lusaka.[19] Our 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 measures may capture girls' effort in preparation, as well as educational inputs from parents, like time to study. In contrast to the previous two measures, these measures are more likely to capture variation among higher ability 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. We view this measure as providing information on the important intensive margin of actually attending school. Among the control girls, average attendance rates are 54% and range from 27% at the 5th percentile to 76% at the 95th percentile.

Pregnancy. Our last outcome measure is an 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, Dupas and Kremer (2015a). Reported pregnancies are relatively rare (4% of the control group). This may be reflective of under-reporting as well as the relatively young age of the sample.

Aggregate Measures. We aggregate the educational measures into a human capital index in two ways. First, we form an index by first standardizing each of the individual variables and then averaging over them. Second, we follow Kling, Liebman and Katz (2007) and Clingingsmith, Khwaja and Kremer (2009) and estimate our treatment effects as average

[19] We do not use girls' overall scores since total scores are computed by summing girls' top 6 subject scores. Girls can choose both what and how many subjects to take (8 or 9), but all girls must take both math and English. Other exam subjects consist of religious education, science, geography, history, civics, home economics, art, office practice, bookkeeping, music, agriculture science, French, metalwork, woodwork, and Nyanja.

effect sizes.[20] As O'Brien (1984) shows and Kling, Liebman and Katz (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. The use of average effect sizes and indices has 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.

Appendix Table A1 reports the rates of attrition for the schooling and human capital measures. Attrition is usually low and is not differential across the treatments.

2.7 Lab-in-the-Field and Follow-up Survey Measures

Our remaining outcomes, which allow us to explore potential mechanisms for negotiation's effects, come from the lab-in-the-field experiment and the follow-up survey, which occurred three to four months after the treatment. Thus, these outcomes can be thought of as providing early indicators of changes taking place in the household. The lab-in-the-field experiment was designed to directly measure the effect of girls using their negotiation skills, as well as to isolate different potential channels for the negotiation training's effect in a controlled environment. The game is discussed in detail in Section 5.1.

The follow-up survey was designed to gather suggestive evidence on possible mechanisms in households' "real," everyday behavior. The survey measures parental beliefs about girls' abilities, time and work allocation within the household, parents' perceptions of girls' behavior, and girls' own educational aspirations. Thus, the follow-up survey allows us to test several potential channels for negotiation's effects, which we will explore more fully with the theoretical framework. These include changes in parents' beliefs about daughters' ability, changes in daughters' aspirations, and increases in strategic cooperation between parents and daughters.

3 Theoretical Framework

In this section, we develop a theoretical framework outlining different forces through which negotiation training could affect educational investment. For simplicity, we model educational investment as a one-shot game. However, we view this as a reduced-form represen-

[20] 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.

tation 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 cooperating with their parents (e.g. by doing more chores or doing chores more willingly) in addition to making later transfers. Our framework is designed to capture both the individualistic empowerment effects of a negotiation training and the skills effects. For brevity, we do not explicitly discuss the information effect, as we will show that information alone does not affect schooling in our context, although the model can be generalized to accommodate it.

In the model, parents make the decision to invest in girls' education. Because parents are imperfectly altruistic, and the set of feasible contracts between girls and parents is limited, parents may decline to invest even when the return exceeds the costs. This reflects the fact that, in our follow-up survey, daughters report want significantly more education (1/10 of a year on average) than parents report wanting for them. Thus, there is scope to improve efficiency and increase educational investment if girls whose return is sufficiently high can increase their transfers to parents in order to elicit investments.

We assume girls will naturally transfer some amount of the returns to education to their parents due to a sense of "obedience" and reciprocity. However, savvy girls may also want to transfer more than they would naturally out of reciprocity due to a desire to incentivize parental investment. Negotiation skills could create added scope for this "strategic cooperation" by allowing girls to better recognize that their transfers will impact their parents' decisions, helping them find opportunities for concurrent transfers, or increasing the ability to commit to future transfers. In the model, this is just represented as an increased set of feasible contracts, but this reduced-form representation should be thought of as standing in for the many facets of successfully using negotiation skills.

3.1 Set Up

In our basic framework, an imperfectly altruistic parent can choose whether to make an investment, $E \in \{0, 1\}$, in her daughter's education, for which she experiences a cost, $\tilde{f}$. The cost is offset both by the parent's altruism toward the daughter and by the transfer she will receive from her daughter conditional on $E = 1$, τ. The parent's problem is therefore given by

$$\max_E U^p = (-\tilde{f} + \tau + \delta U^d)E, \tag{1}$$

where $\delta \leq 1$ is the altruism parameter, and U^d is the daughter's utility. Turning to the daughter, each daughter has a discounted, net-of-effort return to schooling $R_i \sim$ iid drawn

from a distribution F, which can be thought of as the ability distribution.[21]

The daughter's key choice variable is τ, which she can transfer to her parent to offset the cost of education when $E = 1$. The daughter also internalizes the cultural norm of obedience and reciprocity to her parents, and so experiences a convex loss from the distance between her transfer and her return to education, represented as:

$$c(R_i - \tau),$$

where $c'(R_i - \tau) > 0$, $c''(R_i - \tau) > 0$, $c(0) = 0$, and $c'(0) < 1$.[22]

The daughter's problem is therefore given by

$$\max_{\tau} U^d = \Big(-\tau + R_i - c(R_i - \tau)\Big)E. \tag{2}$$

First, imagine the daughter solves this problem taking the parent's educational decision as fixed. Then, the daughter will maximize utility taking E as fixed, and will transfer 0 if $E = 0$ and τ^{ns} if $E = 1$, where τ^{ns} solves $c'(R_i - \tau^{ns}) = 1$. Because τ^{ns} is ex post incentive compatible given the parent's investment decision, it does not require any commitment (or contemporaneous transfers) by the daughter. This situation is in line with girls thinking of their parents' decision-making as unchangeable, consistent with the cultural norm in Zambia of deference toward elders by youths and especially by girls.

In general, substituting the daughter's utility into the parent's problem shows that the daughter will be educated if

$$R_i > \frac{\tilde{f} - \tau(1-\delta)}{\delta} + c(R_i - \tau). \tag{3}$$

If only τ^{ns} is transferred, we see that the required R_i to be educated is decreasing in τ^{ns}. Thus, one benefit of greater internalization of cultural norms by girls is that it may make parents expect more transfers, making them more willing to educate their daughters.

Now, imagine that two criteria are met:

1. Daughters fully understand the parent's utility function.

2. Daughters have either full commitment or the ability to transfer a large enough τ to change the parent's behavior contemporaneously.

[21] We can think of R_i as having a more complicated structure, such as $R_i = \beta r_i - e_i$, where β is the discount rate, r_i is the return to schooling, and e_i is the daughter's effort to complete school. However, since these parameters will be indistinguishable, we simplify the notation by only referring to R_i.

[22] This assumption ensures that a daughter always prefers to keep at least some of the returns to investment. If $c'(0) \geq 1$, daughters would always give parents the entire returns to their investments and strategic cooperation would be unnecessary.

In this case, a daughter is willing to transfer a maximum of R_i to be educated. Substituting $\tau = R_i$ into equation (3) shows that any daughter with $R_i \geq \tilde{f}$ will then be educated. The actual transfer required to be educated is $\tau^* = \frac{\tilde{f}}{1-\delta} - \frac{\delta}{1-\delta}\Big(R_i - c(R_i - \tau^*)\Big)$, which is obtained by inverting equation (3). As long as $\tau^* \leq R_i$, a daughter is always made better off by transferring this amount to compensate her parent for her education.

Our full model nests both the cases of complete contracting and no contracting described above, allowing for imperfect contractibility. We assume that daughters may not be able to transfer τ^* because either they cannot discern parents' motives for investment, find the right things to transfer contemporaneously, or commit to future transfers.[23] Therefore, daughters are constrained in the amount they can transfer above τ^{ns} by σ_i, which captures the feasible contracting space, and the total maximum transfer is $\bar{\tau}_i = \tau^{ns} + \sigma_i$. This creates a limited contracting problem where it is possible $\tau^* > \bar{\tau}_i$, and thus a fully sophisticated daughter would want to transfer more resources to her parent in order to be educated, but she is constrained from doing so.

3.2 Equilibrium

We characterize the equilibrium with Proposition 1.

Proposition 1. *Equilibrium educational investment is characterized by*

1. *If* $R_i \geq R_i^* \equiv \frac{\tilde{f} - \tau_{ns}(1-\delta)}{\delta} + c(R_i^* - \tau_{ns})$, $\tau^* = \tau_{ns}$ *and* $E = 1$.
2. *If* $R_i^* > R_i \geq R_i^{**} \equiv \max(\frac{\tilde{f} - \bar{\tau}(1-\delta)}{\delta} + c(R_i^{**} - \bar{\tau}), \tilde{f})$, $\tau^* = \frac{\tilde{f}}{1-\delta} - \frac{\delta}{1-\delta}\Big(R_i - c(R_i - \tau^*)\Big)$ *and* $E = 1$.
3. *If* $\tilde{f} \leq R_i < R_i^{**}$, $\tau^* = 0$, *and* $E = 0$.
4. *If* $R_i < \tilde{f}$, $\tau^* = 0$, *and* $E = 0$.

Proof. See Appendix C.

In equilibrium, a girl in case 1, who has sufficiently high returns to education $R_i > R_i^*$ does not need to strategically compensate her parent to be educated and only transfers the amount that is utility maximizing due to her norms of obedience and reciprocity. A girl in case 2, with intermediate values of R_i, will not be educated unless she strategically makes a transfer to her parent. Her equilibrium transfer is less than $\bar{\tau}_i$, so she is able to make

[23] To formalize the channel of girls' awareness of parents' utility functions, one could imagine that a daughter incorrectly perceives her parent's utility function as a weighted average of the parent's taste for education that is unaffected by daughter's actions and the parent's true utility, $\tilde{U}^p = \alpha|\bar{E} - E| + (1-\alpha)(-\tilde{f} + \tau + \delta U^d)E$. If girls believe that $\alpha = 1$, they assume $E = \bar{E}$, and transfer 0 if $E = 0$ and τ_{ns} if E=1, where τ_{ns} solves $c'(R_i - \tau_{ns}) = 1$. If, however, girls believe $\alpha < 1$, they solve for a weighted average between the non-strategic τ_{ns} and τ^*.

that transfer and be educated. In case 3, a girl would like to be educated, and it would be net welfare maximizing to educate her since the returns R_i outweigh the costs $\tilde{f}$, but she is not able to strategically compensate her parent enough to be educated because τ^* is outside of the feasible contracting space, so $E = 0$ and $\tau^* = 0$. The number of girls in case 3 ($\tilde{f} < R_i < R_i^{**}$) determines how much changing σ_i can affect education. If the parent is perfectly altruistic ($\delta = 1$), then a girl for whom $R_i \geq \tilde{f}$ will always be educated, and no girl will be in case 3. Finally, in case 4, a girl would never be willing to transfer enough to be educated since the returns are less than the costs, and therefore, $E = 0$ and $\tau^* = 0$.

Proposition 1 tells us that a girl with $R_i > R^{**}$ is educated. Thus, to develop predictions about how changing the parameters of the model will affect education, we only need to consider if they affect R_i^{**} or R_i. The model therefore also indicates that the negotiation treatment should affect education the most for girls whose returns to education place them on the margin of being educated. We next consider how the negotiation and safe space treatments could affect the different parameters of the model, keeping in mind that safe space has individualistic empowerment effects, while negotiation may have both individualistic empowerment and skills effects.

Effects of Negotiation Skills. The negotiation training was designed to help girls better understand their parents' utility functions and find opportunities for pareto-improving trades. Thus, part of the training is endowing girls with game theoretic thinking. In line with this, girls could learn to make transfers either contemporaneously or in the future that increase the parent's willingness to invest in education. Thus, we think of the main intended effect of the negotiation curriculum as **increasing $\boldsymbol{\sigma_i}$**, the set of feasible transfers, and thus $\bar{\tau}_i$, the total possible transfer.

It is also possible that girls who become more sophisticated about their parents' decision-making processes and constraints are able to take actions that reduce the cost of schooling without directly making transfers. For example, a girl could provide a parent with information about when it is most effective to spend time on schoolwork versus chores, thus **decreasing $\boldsymbol{\tilde{f}}$**. This mechanism allows for the possibility that girls can take actions to increase schooling without absorbing this cost themselves.

In addition to these hypothesized channels, it is also possible for negotiation skills to affect education through other parameters, which we will test for. They could lead girls to be more persuasive or increase the parent's other-regarding preferences, **increasing $\boldsymbol{\delta}$**.[24] Negotiation skills could also increase the net returns to education if they are a complementary, non-cognitive input to educational investment, **increasing $\boldsymbol{R_i}$**.

[24]This can also be thought of as a reduced-form for the daughter's bargaining weight in the household.

Exhibit 1: Summary of Model Predictions

	Impact on Outcomes, Relative to Control							
	Human Capital		Parental Giving in Investment Game					
			Comm		Non-Comm		DG	
Mechanism	Neg	SS	Neg	SS	Neg	SS	Neg	SS
Increasing σ_i	+	0	+	0	0	0	0	0
Decreasing $\tilde{f}$	+	0						
Increasing R_i	+	+						
Decreasing $c'(\cdot)$	−	−	−	−	−	−	0	0
Increasing δ	+	+	+	+	+	+	+	+

Effects of Individualistic Empowerment. Individualistic empowerment might also have positive effects that could lead to increased education in this model. By exposing parents to a "pro-girl" mentality, the program could have reduced parents' gender bias toward daughters, **increasing $\boldsymbol{\delta}$**. Spending time in an all-female peer group with a role model may lead a girl to see herself as someone who can avoid pregnancy, complete school, enter the labor force and pursue a professional career, **increasing $\boldsymbol{R_i}$**.

At the same time, individualistic empowerment might also reduce girls' compliance with cultural norms, which would lower the parent's expectation of τ_{ns}. This is because empowerment could decrease a girl's psychic costs to deviating from gender-biased cultural expectations of obedience, perturbing c so that the marginal girl experiences a **lower $\boldsymbol{c'}$**.

Predictions for Human Capital Investment. If negotiation primarily increases σ_i, we expect the training to have positive effects on human capital investment. However, since there are many potential mechanisms, positive effects are not enough to draw definite conclusions about mechanisms. The first two columns of Exhibit 1 summarize the predictions for how the different parameters affect human capital investment and how they can lead safe space (through individualistic empowerment) and negotiation (through individualistic empowerment and skills) to affect educational investment. Appendix C provides proofs. The potentially ambiguous impact of individualistic empowerment, through a possible decrease in non-strategic transfers, highlights the importance of including the safe space arm. Doing so allows us to determine whether any possible negative effects are due to the negotiation skills channel or the common empowerment elements of the programs.

Exhibit 1 also shows how we will use a lab-in-the-field experiment to progressively shut down some of the possible mechanisms, in the hopes of more clearly identifying the mechanisms. The lab-in-the-field experiment and this approach will be described in more detail in Section 5.1.

4 Effects on Enrollment and Other Human Capital Outcomes

In this section, we first test whether the negotiation training positively affected girls' human capital outcomes and compare its effects to the other two treatments. The comparison between negotiation and safe space allows us to test whether the negotiation effects are driven by individualistic empowerment, while the comparison between negotiation and information ensures that the effects are not driven by any informational elements of the treatment. In the second subsection, guided by the theoretical framework, we use machine learning to explore sources of heterogeneity.

4.1 Treatment Effects

Empirical Strategy. Our main estimating equation for the negotiation treatment effect is

$$y_{ic} = \beta_0 + \beta_1 negotiation_i + \beta_2 safe\ space_i + \alpha_c + \mathbf{\Gamma X_i} + \epsilon_{ic}, \tag{4}$$

where i denotes a girl, c denotes a classroom, y_{ic} is the outcome of interest, $negotiation_i$ is an indicator variable equal to 1 if girl i was assigned to receive the negotiation treatment and 0 otherwise, $safe\ space_i$ is an indicator variable equal to 1 if a girl i was assigned to the safe space treatment, α_c are classroom effects, and $\mathbf{X_i}$ is a vector of control variables. In our most basic specification, $\mathbf{X_i}$ only includes a control for the information treatment, since the negotiation and safe space treatments were stratified by classroom and information. In a second specification, to maximize precision, we choose additional controls using the double lasso method introduced by Urminsky, Hansen and Chernozhukov (2016). The potential controls consist 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, indicator variables for whether a girl reads and speaks Nyanja and English excellently or well, age and age squared, and ethnicity fixed effects. To maintain a consistent sample across specifications, we restrict the sample to participants who received the baseline survey, as discussed in Footnote 7. In Appendix D, we relax this restriction. Throughout our regressions, we cluster our standard errors at the classroom-level, resulting in 141 clusters.

The negotiation and safe space treatment effects – our estimates of interest – are identified as long as there is within-classroom balance by treatment (as the joint tests in Table 1 suggest) and the control group is not contaminated by spillovers. Appendix H uses a variety of strategies to test for spillovers.

Schooling Outcomes. Panel A of Table 2 reports the estimates from equation (4), when the outcomes are enrollment in 9th, 10th, and 11th grade. The point estimates indicate that negotiation positively affects enrollment, with larger effects in the grades after the transition to secondary school. The negotiation treatment increases 10th and 11th grade enrollment by 3.5-4.4 percentage points, depending on the specification. This means negotiation increases enrollment by approximately 10% in the crucial upper secondary years. Negotiation's larger effect in later grades aligns with the fact that most dropout occurs after 9th grade (91% of the sample continue to 9th grade, while less than 50% are observed in 10th and 11th grade). Strikingly, this means that negotiation's effects do not fade out. Rather, negotiation contributes to girls' educational attainment more than a year later when parents must make decisions about enrolling girls in a new level of schooling. Turning to the comparison with safe space, we cannot reject that safe space had zero effect on enrollment, but neither can we reject that the safe space effects are equal to the negotiation ones.[25]

Panel B of Table 2 uses the same specifications to measure negotiation's effect on being in a morning program, the higher ability track, in grades 10 and 11 of secondary school.[26] Being in the negotiation training program significantly increased the likelihood that girls were enrolled in morning programs by 11th grade. In 11th grade, negotiation increases the likelihood of enrolling in morning school by up to 4.0 percentage points (an increase of 16% from the control group level of 25% enrollment). Because the magnitude of the total enrollment and enrollment in morning school effects are similar, one possible explanation is that enrollment increased for girls who were able to enroll in morning school due to their test scores. This is consistent with higher ability girls who faced external constraints from their parents being most affected by the treatment.

In Panel B, we find meaningfully different effects of negotiation and safe space on morning schooling. The estimated safe space effect is negative and very close to zero. With the double lasso specification, we can reject that safe space and negotiation are equal at the 10% level for 11th grade, and the two-sided p-value 0.105 for 11th grade in the baseline specification. This suggests that even if safe space had positive effects on enrollment, safe space and negotiation appear to operate through different mechanisms. Since only high ability girls are able to enroll in morning schools, this provides preliminary evidence that negotiation has larger effects on higher ability girls. This is consistent with our hypothesized mechanism of impact, detailed in the theoretical model, where only girls who have returns that make it efficient to invest will be able to use negotiation skills to elicit greater investments. We

[25] As discussed in Appendix D, Appendix Tables A5 and A6 re-estimate enrollment effects including participants who did not take part in the baseline survey.

[26] We did not collect data on morning school for 9th grade since all girls who were enrolled in our program were in morning schooling in 8th grade. It is unlikely that they would switch to afternoon school in 9th grade since there is no test to transition between 8th and 9th grade.

further explore this possibility in the heterogeneity analysis in Section 4.2.

As an additional exercise to get a sense of the magnitude of our enrollment results across all years, we also estimate a Cox hazard model for dropout.[27] According to the estimates of this model, reported in Appendix Table A7, negotiation reduced the yearly dropout hazard by a statistically significant 10 percentage points. As before, safe space has insignificant effects, though we cannot reject that the effects on dropout are the same as for negotiation.

Finally, we compare the effects of negotiation on schooling outcomes to information in Appendix Table A8. Columns 1-9 of Appendix Table A8 show that information had no effect on enrollment or morning school. Information's interactions with negotiation are also insignificant. Indeed, for 10th and 11th grade enrollment and grade 11 morning schooling, we can always reject that negotiation and information have same-sized effects. Information alone is insufficient to alter girls' enrollment outcomes.

Additional Human Capital Outcomes. In Table 3, we re-estimate equation (4) with the additional, shorter-term human capital outcomes documented in Section 2, using double lasso to select the controls. 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. For the individual outcomes, negotiation is positively related to paying school fees, taking the national exam, scoring above the "assignment threshold" for math (which is marginally statistically significant) and English, and attending school. It is negatively associated with pregnancy. To summarize these results, we construct a human capital index and average effect size (columns 1–2).[28] For the human capital aggregates, negotiation has positive and significant or marginally significant effects. Table 3 estimates the safe space effects for our additional human capital outcomes. We again cannot reject that safe space had zero effect on the outcomes, even when we aggregate across all the human capital index components. We also cannot reject that negotiation and safe space had the same effects at conventional significance levels. The last columns of Appendix Table A8 show the effects of information and its interaction with negotiation on the human capital index. Information again does not have a significant effect, and we can reject that the effect is the same size as the negotiation effect at the 5% level.

[27] This model takes the form

$$\lambda(t|\mathbf{X_i}) = \lambda_0(t)exp(\pi_0 + \pi_1 negotiation_i + \pi_1 safe\ space_i + \alpha_c + \mathbf{\Pi X_i}), \tag{5}$$

where i denotes a girl, c denotes a classroom, $\lambda(t|\mathbf{X_i})$ is the hazard rate for dropout in period t, This model has two caveats: (1) hazard model coefficients may be biased even in the context of a RCT, and (2) it assumes the effect of negotiation on dropout hazard is the same in all years, when actually the effect size appears to increase.

[28] The construction of these measures is described in Section 2.

Finally, to account for the possibility of multiple hypothesis testing across our outcomes, we form an average effect size over *all* the non-aggregate outcomes in Tables 2 and 3. The point estimate is 0.056 sd, and it is significant at the 1% level. Overall, we conclude that providing adolescent, Zambian girls with non-material resources by teaching them negotiation skills in school increases human capital over the subsequent years. These human capital effects are driven both by improvements on the intensive margin (better school quality and higher test scores) and the extensive margin (greater enrollment).

4.2 Heterogeneity

Recalling that our theoretical framework suggests that negotiation will have the largest effect on girls on the margin of educational investment, we now explore heterogeneity in our negotiation treatment effect. 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 E provides the details of this procedure, but several points are worth highlighting. First, to identify this heterogeneity, we split the data into two non-overlapping, randomly chosen samples and use one subsample to determine the heterogeneity and the other to estimate our point estimates and confidence intervals. This ensures that our confidence intervals are valid and we are not "over-fitting." Second, consistent with the drivers of educational investment in our theoretical framework in Section 3, we specifically search for heterogeneity in the negotiation effect by child ability and parental altruism.[29] We also include age as a potential source of heterogeneity, since it is the only baseline variable that is not included in either the ability or altruism proxy. Finally, we use the machine learning procedure to search for heterogeneity in the effect on enrollment in 11th grade, our longest-run outcome.

According to the machine learning exercise with the training sample, negotiation has heterogeneous effects by ability, with the strongest effects for those in the top 40% of the ability distribution. Thus, when we turn to the analysis sample, we allow both the negotiation and safe space treatments to have different effects on enrollment and morning schooling for girls in the top 40% of ability and the bottom 60%.[30]

Table 4 shows that in grade 9, both negotiation and safe space if anything had larger (though only marginally significant) effects for lower ability girls. These results align with the educational transition process in Zambia, where 9th grade is part of the same schooling

[29]We proxy for ability by taking the first factor of a factor analysis of the Nyanja and English ability variables. Altruism is proxied with 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.

[30]The sample is restricted to the distinct sample that was not used to identify the heterogeneity, resulting in half as many observations as in Table 3.

level as 8th grade, and thus dropout is rarer and would be concentrated among those in poor academic or disciplinary standing.

In 11th grade, however, negotiation had greater effects for high ability girls. The negotiation treatment had an effect of 11 percentage points for high ability girls, while having a null effect on low ability girls. In contrast, the safe space treatment had no effect on high ability girls' enrollment in 11th grade, and we reject at the 10% level in the double lasso specification that safe space and negotiation had the same effects on high ability girls in 11th grade enrollment. Panel B shows that the heterogeneity is even more striking for morning schooling, which increases by 14 percentage points for high ability negotiation girls. Safe space has no effect on either group, and the two treatments' effects on high ability girls are statistically significantly different at the 5% level.

The fact that we do not see the same heterogeneity in the safe space and negotiation effects further suggests that though safe space may have some positive effects, these do not operate through the same mechanisms as the negotiation training. The positive effects of both treatments on lower ability girls in 9th grade are somewhat suggestive that the common "individualistic empowerment" elements of the two treatments may have helped low ability girls who were on the margin of dropout due to internal constraints (such as lack of motivation or avoiding disciplinary trouble) remain in school. By 11th grade, however, when parental investment becomes a key constraint, the two treatments' effects are different. The heterogeneity we identify also aligns with our model, in which high ability girls whose parents are insufficiently altruistic to invest without transfers are able to use skills to resolve incomplete contracting problems in the household, enabling them to continue in school. Thus, altogether, the heterogeneity in the negotiation effect and its comparison to safe space provides additional evidence that the skills component of the negotiation training matters for girls' education.[31]

5 Understanding Mechanisms

To explore the specific mechanisms in the model through which negotiation skills and empowerment may affect parental investment, we now turn to two additional sources of evidence: the lab-in-the-field investment game and the follow-up survey. The follow-up and investment game took place three to four months after the training. Girls were asked to bring a parent

[31]For completeness, Appendix Table A9 also reports estimates of heterogeneous effects for whether girls are above or below the median for the parental altruism index and age. According to the point estimates, negotiation has larger effects on girls with lower altruism measures, consistent with the idea that negotiation matters more in households with greater incomplete contracting problems. Negotiation also has larger effects on younger girls, who are likely to be higher ability since a younger age means a girl has repeated fewer grades.

or guardian to school to take part in the follow-up and the game, and the majority (57%) brought their biological mothers. 70% of girls in the sample attended the follow-up/lab-in-the-field game,[32] and attrition was not differential by treatment status (see Appendix Table A10).

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

5.1.1 Experimental Design & Link to the Model

The lab-in-the-field experiment was designed to measure the effect of girls using their negotiation skills with parents in a controlled environment, as well as to isolate the different mechanisms outlined in the model. The principal game is an investment game with communication, which was designed to most closely mirror the everyday household interactions that could lead girls to receive greater human capital investments (whether time to do homework, money for school fees, or other forms of parental support). Except, in this setting, the cost of investing for parents and the returns to investment are fixed. This allows us to more cleanly test for the remaining mechanisms in the model – increasing the contracting space, the daughter's cultural fealty toward parents, and parental altruism. In addition to the principal version, we assigned some girls to two other variants of the game that allow us to further isolate mechanisms: an investment game without the opportunity for communication and a basic "dictator" game. Exhibit 1, which we will discuss in detail, shows how the different games allow us to isolate different parameters from the model, and Appendix Table A11 reports the number of girls assigned to each variation.

Investment Game With Communication. In the investment game with communication, parents were endowed with ten tokens, worth about $2, which either could be redeemed for cell phone air time[33] or sent to daughters. Any tokens sent to daughters were doubled and combined with a random income shock of 2 or 4 tokens. The size of the income shock was not revealed to the girls, which served two purposes. First, the income shock obscured the parent's decision and ensured that no girl was left with zero tokens. Second, it created random variation in the girl's tokens, which can be used to identify her propensity to return tokens. Girls could then choose how many tokens to send to parents and redeem the remaining tokens for girl-specific "prizes."[34]

[32] 70 girls came to the follow-up survey but did not bring a parent or guardian. In this case, we administered the follow-up survey to them, but the girls did not take part in the lab-in-the-field experiment.

[33] Airtime is fungible and serves as pseudo currency for survey compensation.

[34] 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

After these rules were explained to girls and parents, but before any decisions were made, girls and parents were given the opportunity to communicate with one another. The surveyors implemented this by pausing and allowing the girl and her guardian to meet before returning to their "stations" to make their decisions privately. Parents and girls were not required to communicate, mirroring the fact that in the real world, girls can choose to communicate with their guardians if they wish, and negotiation skills may aid in initiating these communications.

This version of the game allows us to directly test whether girls are able to use their negotiation skills to elicit higher "investments" from their parents. If they are, since the return to investment and the cost of investment is fixed by the game, it will provide evidence that an increase in the contracting space (σ_i) may be a possible channel. However, it is also possible that parents exhibit higher altruism (δ) or have different expectations about non-strategic transfers (τ_{ns}) in the negotiation arm. Thus, the total prediction about the effect of negotiation on the number of tokens sent by parents is ambiguous, as shown in columns 7 and 8 of Exhibit 1. Specifically, while we expect negotiation to increase the scope for strategic cooperation, increasing tokens sent, it may also increase "individualistic empowerment," decreasing tokens sent. Similarly, it could increase parental altruism, increasing tokens sent (or decrease altruism if there is a backlash effect).

Thus, to further untangle the three channels in the investment game with communication, we introduced two additional versions of the game, which shut down or vary the strength of these channels.[35]

Investment Game Without Communication. The second version of the game follows the investment game protocol but with no communication between girls and their guardians. Thus, a guardian will make decisions based on her expectations of how much a daughter will return in the absence of the opportunity to negotiate and her altruism toward her daughter. Since our hypothesis is that negotiation allows girls to increase the feasible contracting space explicitly through communication, we expect the σ_i channel to be less active here than in the version with communication.

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 (games, hair bands, and candy), school supplies (pencils and notebooks), and personal items (socks and 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.

[35]There was also 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. 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. The results from the word game are discussed in detail in Appendix F, and its experimental protocols are included in Appendix G.

If the σ_i channel is indeed less active in the non-communication game, then this version would be relatively more affected by parents' expectations of non-strategic return, τ_{ns} and parental altruism, δ. It is possible that the individualistic empowerment elements of both the negotiation and safe space treatments could reduce either of these parameters. In particular, the model provides a channel through which individualistic empowerment could decrease τ_{ns} by making girls less sensitive to costs associated with deviating from cultural norms of reciprocity. The fact that altruism effects could be positive, however, makes the overall prediction ambiguous, as shown in columns 5 and 6 of Exhibit 1.

Dictator Game. The last version of the game allows us to separate channels that depend on parents' expectation of reciprocity from parental altruism by eliminating the stage where girls return tokens. Parents simply choose how many tokens to send, knowing that any that are sent will be doubled and then used by girls for prizes. This allows us to see to what extent effects in the other variants could have been driven by negotiation's effects on parental altruism, δ, alone. If girls' empowerment increases altruism, we would expect both the safe space and negotiation treatment to have positive effects, as shown in columns 7 and 8 of Exhibit 1. If negotiation skills enhance altruism – for example, if girls convince their parents to put a higher weight on their utility – the negotiation effect will be positive. If the negotiation or safe space treatments caused parents to be annoyed with their daughters, these treatments would have negative effects.

In our analyses of the effects of the different games, our main outcome variable is the number of tokens sent by parents, which is the analogue of educational investment in the real-world. To confirm the connection between the game and real-world outcomes, in Appendix Table A12, we regress the enrollment variables and human capital index on the number of tokens parents sent. We find that tokens sent are positively related to the human capital index, grade 10 and 11 enrollment, and grade 10 and 11 morning schooling. The number of tokens parents send in the investment game also serves as a measure of the daughter-parent's distance from the efficient frontier. Since tokens will be doubled and can be fully returned to parents, full efficiency requires that the parent sends all the tokens. In fact, only 2.4% of households do so, suggesting that parents and daughters have limited contractibility, and that parents treat the stakes of the game as "real."

5.1.2 Lab-in-the-Field Game Results

Tokens Sent to Daughter. For each variant of the investment game, we estimate the effects of negotiation and safe space on our main outcome of interest, the number of tokens

parents sent to daughters. In addition, we pool the investment games with and without communication and estimate the effect of the interaction between communication and the two treatments.

Table 5 reports our results for the number of tokens that parents sent to daughters. Columns 1-2 report the results in the investment game where parents and girls could communicate before parents made their allocation choices. Girls in the negotiation treatment receive 0.4 more tokens than control girls in this game. Safe space girls receive about the same number of tokens as the control. While we cannot reject that safe space and negotiation girls received the same number of tokens in this version with 2-sided F-tests, a 1-sided test indicates that negotiation girls received marginally significantly more tokens in columns 1 and 2. Thus, the treatment had positive effects when parents alone make investment decisions, rather than only when the girl directly controls investment. This provides additional evidence that negotiation's educational effects are not merely due to increased motivation on the part of the girls and affirms that the positive effects of negotiation can stem from channels other than increasing the returns to education. As this finding also provides initial evidence that strategic cooperation could increase parental investment, we next turn to the remaining variants of the game to isolate σ_i from the other potential channels.

Columns 3-4 pool the game with communication and the game without communication to show that there is a strong positive interaction between the communication variant of the game and the negotiation treatment. When girls with negotiation skills are allowed to communicate, they receive 0.8 more tokens than when they are not. Because the main wedge between the two games is the scope for the girls to communicate strategic cooperation (affecting σ_i in the theoretical framework), this suggests that this channel is important for the positive effects in the investment game. The interaction between safe space and communication is approximately half the size. Consistent with our findings in Table 5, in Appendix Table A13, we also find that knowledge of negotiation interacts positively with communication.

Interestingly, as shown in Panel B of Table 5, parents give fewer tokens to negotiation and safe space girls in the non-communication game. The negative effect of negotiation and safe space in columns 1-2 of Panel B suggests that, in the absence of communication, the common element of the two treatments, individualistic empowerment, negatively affected parental giving. Linking these results to the model, empowerment may have either decreased parents' expectations of transfers or decreased parental altruism toward girls. In this case, having the safe space treatment for comparison is crucial for interpreting the results, as it means the negative effect is unlikely to be driven specifically by negotiation skills.[36]

[36]While it is ex ante possible that negotiation on its own further decreased or increased parental altruism

Turning to the last version of the game, in columns 3-4 of Panel B, we see a statistically insignificant but directionally positive effect of being in the negotiation or safe space arms on parental giving in the dictator game, where girls do not return tokens. Thus, it is unlikely that negotiation and safe space's effects on pure altruism are responsible for the negative impact in the non-communication investment game. Indeed, both negotiation and safe space's effects in the non-communication game are statistically significantly different from the effect in the dictator game in both of our specifications. Given the apparent lack of a negative effect on altruism, the model suggests that parents reduced the number of tokens sent in the non-communication game because the individualistic empowerment elements of the negotiation and safe space treatments led parents to expect a lower return from the girls. That is, they expected that the girls would spend more tokens on themselves. In the model, this is consistent with the channel of individualistic empowerment lowering the marginal girl's sensitivity to $c'(\cdot)$, her cost associated with deviating from the cultural norm of reciprocity toward parents.

To summarize, combining the evidence from the game with the predictions of the model suggests that the channel of increasing expected strategic cooperation played a role in increased parental giving in the game with communication. In contrast, when girls could not communicate, we do not see an increase. In fact, giving decreased in both the negotiation and safe space arms, indicating that parents expected girls to be less reciprocal in the absence of communication in both treatments. Results from Appendix Table A14, which examines how daughters spent the tokens, further support this interpretation. When negotiation girls could not communicate they spent more on consumption goods like candy and make-up. When they could communicate, they spent more on household items and school supplies.

Tokens Returned to the Parent. Our findings in Table 5 suggest that negotiation girls may have been sent more tokens because they were able to increase parents' expectations of reciprocity when they were allowed to communicate. If this is the case, and parents have rational expectations, girls in the negotiation $\times$ communication cell should be more likely to send parents back a marginal token.

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, since this will be confounded by the fact that girls whose parents sent more tokens were different from those who were sent less. Instead, we take advantage of the fact that daughters received a random windfall of two or four tokens before deciding how many tokens

toward girls, given the similarity of the effects of negotiation and safe space in the non-communication game, we view this as unlikely.

to send to their parents, leading some girls to exogeneously receive more tokens. Using the sample of girls in the investment game, we use this random shock to estimate the daughter's rate of pass-through of a marginal token to the parent.

Columns 1-2 of Table 6 report that girls in the negotiation treatment playing the communication game passed through between 0.475 and 0.480 more of a marginal token. The analogous effect for the safe space treatment, added in columns 3-4, is less than one-third the size, and statistically insignificant.[37]

Using the coefficients from Table 6, 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. Control girls pass-through one-third of a token, while safe space girls pass through one-fifth. 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 girls' ability to commit to reciprocate investments (σ_i).

In Appendix F, we also report additional analyses of the lab-in-the-field game, including estimating the effects of negotiation and safe space on the number of tokens with which girls end the game and providing a visual representation of how distance to the efficient frontier is affected by negotiation. The former results confirm that negotiation girls in the communication game not only receive more tokens, they end the game with more tokens despite a higher marginal propensity to return tokens (Appendix Table A15). Safe space girls in the communication variant, in contrast, end the game with fewer tokens. This effect is marginally statistically significantly different from that of negotiation under a two-sided test. Thus, negotiation girls are made better off by the training in a controlled environment where the return to investment is fixed. Altogether, the results provide evidence that negotiation increases σ_i, the feasible contracting space between parents and daughters. Thus, increasing σ_i is a potential mechanism for the negotiation treatment's positive human capital effects.

[37]The direction of the point estimates also suggests that parents of negotiation and safe space girls would be right to expect these more empowered girls to send back fewer tokens in the absence of communication. The coefficients for $extra \times negotiation$ and $extra \times safespace$ are both negative. However, when negotiation girls can communicate, they appear to alter their behavior to be consistent with their communications to their parents about reciprocity.

5.2 Effects of Negotiation Within the Household: The Follow-up Survey

In this sub-section, we turn to the follow-up survey to further explore how negotiation and safe space affected intra-household behavior, including the costs and expected returns of schooling ($\tilde{f}$ and R_i). Table 7 reports the effect of negotiation on girls' and parents' behavior in the follow-up survey. Altogether, these suggestive results provide further evidence that the negotiation treatment increased girls' ability to strategically cooperate within the household but did not increase parental altruism (δ) or parents' perceptions of daughters' ability (R_i). The follow-up results also shed light on an additional possible channel for negotiation's human capital effects that is consistent with the negotiation curriculum – girls working with parents to reduce the effective cost of schooling, $\tilde{f}$.

Column 1 reports that negotiation girls were 6.5 percentage points more likely to ask for food,[38] while column 2 indicates that parents were 2.6 percentage points less likely to report it was difficult to get negotiation girls to do chores, although the effect is not significant. Recalling Appendix Table A4, negotiation also led parents to report daughters were more respectful and that they cared more about other household members. This pattern of results is consistent with increased strategic cooperation; negotiation led daughters to ask for more investment and to reciprocate in return.

In columns 3-4, we test whether negotiation affected girls' behavior in other ways that might affect parents' views of daughters. Columns 3 and 4 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). Altogether, this set of results indicates that negotiation did not negatively affect girls' relationships with their parents.

In columns 5-7, we consider the possibility that negotiation affected parents' or daughters' perceptions of daughters' abilities, equivalent to altering R_i in the theoretical framework. Negotiation skills may have either led parents to believe that daughters were higher ability, incentivizing them to invest in the treated daughters, or they may have allowed daughters to inform parents about their ability.[39] 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 5) and the interaction between negotiation and the ability factor (column 6). In column 5, we see that negotiation has no effect on parents' perceptions, and in column 6, we find that negotiation does not lead a daughter's measured ability to be more correlated with the parent's perception of her ability. Finally, in column 7, we regress the number of years

[38] In our baseline survey, one-third of girls report not having enough food to eat at least one day in the last week. In Zambia, if there is not enough food for everyone to have enough, it is common that men and boys will eat first or to take more protein while others eat vegetables.

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

of schooling a daughter reported wanting to complete on the treatment. If negotiation increased a daughter's perceived returns to education, including by 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.

Panel B of the table provides suggestive evidence that negotiation allowed households to find less costly ways to make educational investments. Directionally, negotiation girls spend less time on chores (measured in hours) before and during school hours and more time on chores after school.[40] In column 4, 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 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 relative to 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.

5.3 Summary of Findings

Exhibit 2: Summary of Model Predictions: Findings

	Possible effect			
Mechanism	Neg	SS	Finding	Source
Increasing σ_i	+	0	✓	Investment game with comm. and survey
Decreasing $\tilde{f}$	+	0	✓	Chore "swaps" in survey
Increasing R_i	+	+	no	No evidence in survey
Decreasing $c'(\cdot)$	−	−	✓	Investment game with no comm.
Increasing δ	+	+	no	No evidence in dictator game or survey

To summarize our findings, Exhibit 2 reports the empirical evidence from the lab-in-the-field game and follow-up survey on each of the model's possible mechanisms. For the negotiation but not the safe space group, the evidence is consistent with an increase in σ_i and a decrease in $\tilde{f}$. Girls reciprocate parental investments by sending more tokens in the investment game and by being more respectful in the follow-up survey. We find no evidence of an increase in parents' estimation of R_i or an increase in δ. For both treatments, there is evidence of a decrease in the sensitivity to $c(\cdot)$, indicating that individualistic empowerment alone may cause girls to value their own utility relatively more than the parents'. Altogether, we conclude that the positive human capital effects of the negotiation training are most likely driven by an increase in the feasible contracting space with parents.

[40]Time spent on chores was calculated using an extensive time diary rather than merely asking girls how much time was spent on chores, and it is therefore unlikely to be affected by experimenter demand.

6 Discussion & 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. We provided a randomly chosen group of Zambian 8th graders with negotiation skills training. The training significantly increased school enrollment and educational investment, even though it did not relieve households' financial constraints. In addition to increasing enrollment, negotiation increased girls' enrollment in high quality "morning" schooling, and both of these positive effects grew rather than fading out over time.

Like any training intervention, negotiation had multiple components, each of which could have affected girls' outcomes. Beyond measuring the effect of negotiation training, we also examine which elements of the training were effective and particularly, the impact of negotiation *skills* themselves. To do so, we compare negotiation to two other treatments, information and safe space. We find that information had no effect, and its effect is statistically different from negotiation's. Thus, incidental communication of information about schooling is not the source of the negotiation effect.

The safe space treatment affects individualistic empowerment, which may also have been affected by the negotiation training, without imparting negotiation skills. Although the safe space treatment on its own does not have statistically significant positive effects, we generally cannot reject that the two treatments have the same effect on the average girl for our enrollment measures and shorter-term human capital indices. Thus, the safe space treatment alone could be an effective intervention, although we also cannot reject that it had zero effect.[41] Nonetheless, several pieces of evidence lead us to speculate that the negotiation training's effects were due to different mechanisms than any safe space effect. First, negotiation had a statistically significantly larger effect on morning schooling, while the safe space treatment had zero effect. Enrolling in morning schooling is more likely to lead girls to continue their education, as only morning school girls do the test preparation necessary to proceed to college. Second, exploring heterogeneity detected by machine learning, we find that the negotiation treatment has statistically significantly larger effects on higher ability girls' enrollment in 10th and 11th grade. Negotiation's differential effect on high ability girls suggests that negotiation may have helped girls who were good candidates academically to continue in school but were constrained by external forces to resolve these constraints. In

[41] While it may seem like safe space is a possible lower-cost intervention, the cost of running the safe space intervention in this case was very similar to the negotiation program. The same high-skilled "coaches" served as the supervisors for the safe space program. Any positive effects of the safe space program could be due to prolonged exposure to these educated, dynamic role models in an informal setting with more opportunity for interpersonal exchange (versus the structured negotiation program), which may not replicate with lower-skilled facilitators.

contrast, both negotiation and safe space appear to have stronger effects on lower ability girls in 9th grade, when parents are unlikely to pull a girl out of school. Thus, it is possible that the common individualistic empowerment elements of the two treatments helped girls who were at risk of dropping out due to internal constraints enroll in 9th grade.

Guided by the theoretical framework, we further disentangle the mechanisms underlying the negotiation effect, focusing on the possibility that the treatment may have increased the feasible contracting space for girls to reciprocate their parents' educational investments. Consistent with our empirical results, this channel would affect the highest ability girls more because these are the girls for whom the ability to make transfers is pivotal for parental investment. Further evidence from the lab-in-the-field game also supports this mechanism. When girls and parents can communicate, the ability to cooperate strategically with parents appears to yield higher in-game "investments." But having the opportunity to *use* negotiation skills is important. When the ability to communicate strategically is shut down, parents' behavior suggests that they expect negotiation and safe space girls to be *less* reciprocal. Thus, individualistic empowerment on its own could reduce expectations of reciprocity by disrupting cultural norms of obedience.

The follow-up survey further allows us to open the "black box" of the household and provides additional evidence that negotiation increases the feasible contracting space through strategic cooperation in the household. Girls appear to have found strategic swaps with parents to make schooling less costly to the household, such as through doing chores at times that did not conflict with schoolwork. Parents also indicate they found negotiation girls more respectful and that these girls cared more about other household members. This again suggests that negotiation girls are changing their behavior in ways that both elicit more investment and increase household utility.

In terms of increasing schooling, the intervention was also relatively cost effective. We estimate the cost of the intervention, including staff training, as approximately \$60 per girl.[42] Calculating the total increased years of schooling through grades 9, 10 and 11 yields an estimate of 0.16 additional years of education per \$100 spent.[43] The program thus compares favorably to conditional cash transfers and other material ways of increasing schooling.[44]

[42]The \$60 cost includes the costs of training and paying facilitators, copies and supplies, lunch on school days, and management and transportation. Many of these costs could potentially be reduced for a scaled-up version, such as by having the trained facilitators reach more girls by working a full year, and participating in short refresher trainings. Thus, we expect the cost of scale-up could be lowered to \$50 a girl, and potentially further to \$35 a girl if it was taught at a time or in an environment where lunch was not needed.

[43]We equate being moved from un-enrolled to enrolled for a year as leading to 1 additional year of schooling. Although some girls may dropout before the completion of the newly enrolled grade, some girls may have also dropped out earlier in the previous year. Note that this result does not account for additional benefits beyond grade 11, and hence, this is a lower bound estimate for the treatment's cost effectiveness.

[44]Evidence from the PROGRESA program in Mexico, for example, shows that schooling increased by 0.01 additional years per \$100 spent (Schultz, 2004). Another conditional cash transfer program in Malawi led to 0.09 additional years per \$100 spent (Baird, McIntosh and Özler, 2011). Among interventions that

Moreover, because the intervention affected the highest ability girls, it increased educational investment for those who were likely to have the highest returns. Failing to educate this group might present the largest welfare loss to society. Taking the theoretical model seriously suggests that negotiation will only affect education when educational investment is efficient. Only then will a girl be willing to make sufficient transfers to offset the cost to her parents. Thus, negotiation may also be a more attractive choice for increasing education relative to subsidies or conditional cash transfers (which could potentially lead to misallocation via *over*-investment) in environments where the supply of schooling is constrained.

In sum, we conclude that it is possible to empower girls to change their educational outcomes through interpersonal skills, even in highly constrained environments. Teaching girls non-cognitive interpersonal skills appears to lead to greater human capital investment in part because these skills allow young women to solve inefficiencies within the household. Reflecting these positive findings, the Zambian Ministry of Education has begun adapting elements from our training into the national life skills curriculum for all grade 8 students. However, several important questions for policy remain. First, since our curriculum was taught by highly trained and skilled facilitators, it is important to understand if the same results can be achieved at national scale, and moreover inside the bounds of a traditional classroom. Second, we know little about the optimal timing of these negotiation trainings. In our setting, adolescence may have been a critical period for the development of interpersonal skills. Given the timing of the intervention, girls had the opportunity to practice and develop their skills in lower-stakes negotiations with siblings and parents during 9th grade, in advance of the peak period for dropout between 9th and 10th grade. Thus, it is important to understand if the girls' negotiation abilities themselves strengthened over time, or if they were simply deployed to greatest impact at the point of the secondary school transition. Finally, if teaching daughters negotiation skills can increase intra-household efficiency, endowing negotiating partners with these skills (e.g., parents) could yield further gains. However, if part of our educational gains are from girls extracting the surplus they create, the gains to girls may be dampened by training other parties. More broadly, while we showed that training girls in negotiation increased their educational outcomes, the potential for negotiation skills to increase economic surplus both within and beyond the household by reducing other inefficiencies is an exciting avenue for future research.

affect schooling by reducing costs specifically, evidence from Kenya show that providing school uniforms generates on average 0.09 additional years for $100 spent (Duflo, Dupas and Kremer, 2015b), while offering scholarships for secondary school in Ghana generated 0.17 additional years per $100 spent (Duflo, Dupas and Kremer, 2017).

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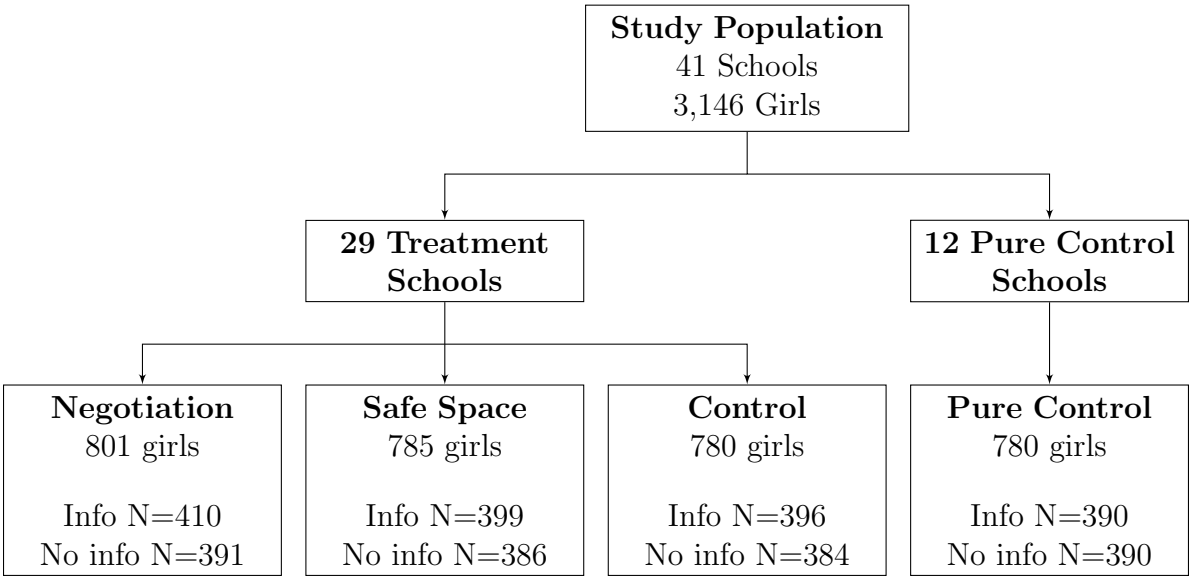
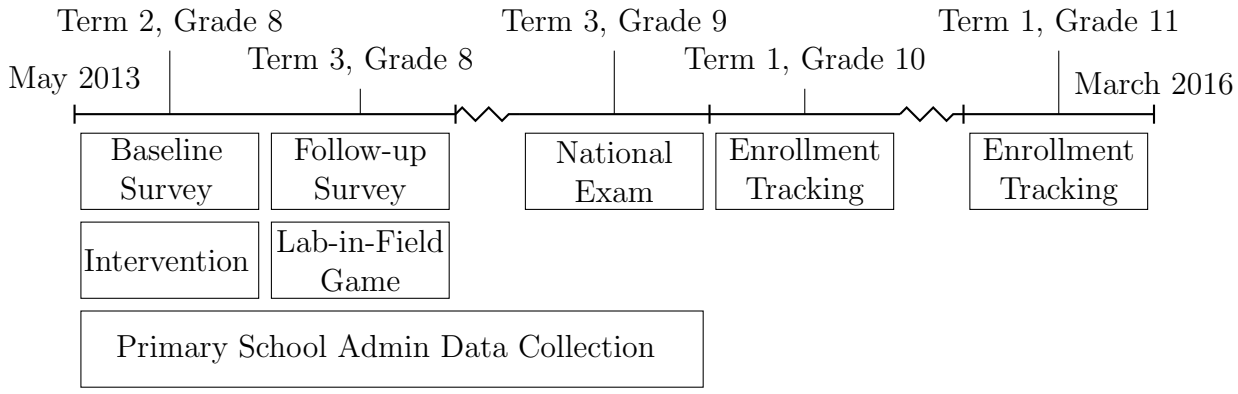
Figures

Figure 1: Experimental Design



This figure details the design of the experiment and the number of schools and individuals assigned to each treatment.

Figure 2: Experimental Timeline



This figure details the timeline for the baseline data collection, the initiation of the experiment, the follow-up data collection and the lab-in-the-field game, and the subsequent administrative data collection.

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 Error**	(9) **Number of Obs.**
Both Parents Alive	0.737	0.440	-0.019	0.021	-0.017	0.022	-0.005	0.021	2,254
Live With Bio Dad	0.548	0.498	-0.020	0.025	-0.004	0.025	-0.009	0.025	2,254
Live With Bio Mom	0.701	0.458	0.011	0.023	0.007	0.024	0.002	0.022	2,254
Live With Mom and Dad	0.499	0.500	-0.023	0.024	-0.009	0.025	-0.010	0.025	2,254
Parents Pay Fees	0.763	0.425	0.032	0.020	0.017	0.022	0.015	0.022	2,249
Read Nyanja Excellently	0.399	0.490	-0.048*	0.026	-0.046**	0.021	0.001	0.028	2,254
Speak Nyanja Excellently	0.480	0.500	-0.057**	0.027	-0.037	0.023	-0.015	0.025	2,254
Read English Excellently	0.697	0.459	-0.019	0.023	-0.026	0.021	0.008	0.026	2,254
Speak English Excellently	0.412	0.492	-0.049*	0.028	-0.002	0.022	-0.043	0.029	2,254
Read Nyanja Well	0.637	0.481	-0.026	0.022	0.003	0.027	-0.028	0.024	2,254
Speak Nyanja Well	0.885	0.320	0.000	0.017	-0.006	0.017	0.001	0.014	2,254
Read English Well	0.899	0.301	-0.008	0.014	0.000	0.016	-0.009	0.013	2,254
Speak English Well	0.789	0.408	-0.020	0.023	-0.025	0.021	0.003	0.022	2,254
Age	14.419	1.461	0.058	0.067	0.035	0.064	0.011	0.068	2,254
P-value (joint test)			0.311		0.183		0.920		

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 balance between the negotiation, safe space, and control groups. For the coefficient columns, each row is a regression of a child/household characteristic on a indicator for whether the girl was included in the treatment of interest, controlling for classroom 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 classroom 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 clustered at the class level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table 2: The Effects of Negotiation and Safe Space on Enrollment

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Enrolled in Government School					
	Grade 9	**Grade 9**	**Grade 10**	**Grade 10**	**Grade 11**	**Grade 11**
Negotiation	0.011	0.013	0.035	0.040*	0.040*	0.044**
	(0.014)	(0.013)	(0.023)	(0.022)	(0.022)	(0.022)
Safe Space	0.011	0.011	0.027	0.029	0.028	0.029
	(0.014)	(0.014)	(0.025)	(0.025)	(0.027)	(0.027)
Controls	Baseline	Double Lasso	Baseline	Double Lasso	Baseline	Double Lasso
Mean of Dep. Var.	0.912	0.912	0.486	0.486	0.424	0.424
Neg. vs. SS. (p-value)	0.979	0.883	0.749	0.633	0.642	0.580
Number of observations	2,239	2,239	2,244	2,244	2,244	2,244
Adjusted R^2	0.011	0.026	0.072	0.100	0.065	0.089
	Panel B: Enrolled in Morning School					
			Grade 10	**Grade 10**	**Grade 11**	**Grade 11**
Negotiation			0.024	0.031	0.037*	0.040**
			(0.020)	(0.020)	(0.020)	(0.019)
Safe Space			-0.003	-0.001	-0.002	-0.002
			(0.023)	(0.023)	(0.023)	(0.022)
Controls			Baseline	Double Lasso	Baseline	Double Lasso
Mean of Dep. Var			0.279	0.279	0.251	0.251
Neg. vs. SS. (p-value)			0.205	0.138	0.105	0.078
Number of observations			2,170	2,170	2,196	2,196
Adjusted R^2			0.085	0.110	0.071	0.101

Panel A reports the effect of the negotiation and safe space treatments on being enrolled in the indicated grade for the full sample of treatment schools. Panel B reports the impact on being enrolled in morning school, the more academic track of the Zambian school system. Baseline controls consist of classroom fixed effects and an information treatment control, our stratification variables. The double lasso specification uses double lasso to choose additional controls among variables 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, reading and speaking English excellently and well, reading and speaking Nyanja excellently and well, age and age-squared, and ethnicity fixed effects. Individuals for whom baseline data is not available are dropped from the regressions. Standard errors are clustered at the class level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table 3: The Effects of Negotiation and Safe Space on Additional Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Educational Outcomes					Health Outcome
	HC Investment Index	**HC AES**	**Paid All Fees, Year 9**	**Took National Exam**	**Threshold Math**	**Threshold Eng.**	**Avg. Attendance Rate**	**Ever Pregnant**
Negotiation	0.045*	0.054**	0.013	0.015	0.039*	0.029	0.005	-0.004
	(0.025)	(0.027)	(0.021)	(0.014)	(0.020)	(0.021)	(0.005)	(0.008)
SD Negotiation			0.027	0.051	0.091	0.066	0.034	-0.023
Safe Space	0.014	0.019	0.008	0.011	0.017	-0.012	0.005	-0.008
	(0.026)	(0.026)	(0.022)	(0.015)	(0.020)	(0.020)	(0.005)	(0.009)
SD Safe Space			0.017	0.038	0.039	-0.027	0.032	-0.042
Mean of Dep. Var.	-0.014		0.673	0.901	0.236	0.262	0.534	0.034
Neg. vs. Safe Space (p-value)	0.268	0.232	0.834	0.819	0.242	0.062	0.939	0.683
Number of observations	2,174		2,203	2,232	2,232	2,232	2,225	2,244
Adjusted R^2	0.191		0.137	0.045	0.117	0.165	0.614	0.031

This table reports estimates of the effect of the negotiation and safe space treatments on outcomes collected in the shorter-term 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. In column 1, the outcome is a human capital index constructed by standardizing each of the outcomes in columns 3-7 and taking their average. In column 2, the estimate is the average effect size over columns 3-7. 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 columns 5 and 6, the outcome is 1 if the student received greater than the threshold required for morning school placement in math and English, respectively, on the national exam. 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 1 if the student is reported to be pregnant. All regressions include controls for the stratification variables (classroom fixed effects and information treatment). In addition, double lasso is used to choose additional controls among variables 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, reading and speaking English excellently and well, reading and speaking Nyanja excellently and well, age and age-squared, and ethnicity fixed effects. The Neg. vs. Safe Space row reports the 2-sided p-value from a F-test of the equality of the safe space and negotiation coefficients. Participants for whom baseline data was missing were dropped from the regressions. Standard errors are clustered at the class level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table 4: The Effects of Negotiation and Safe Space on Enrollment by Pre-Treatment Ability

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Enrolled in Government School						
	Grade 9	**Grade 9**	**Grade 10**	**Grade 10**	**Grade 11**	**Grade 11**
Negotiation × High Ability	0.021	0.023	0.079	0.082	0.112**	0.114**
	(0.028)	(0.028)	(0.060)	(0.059)	(0.055)	(0.056)
Negotiation × Low Ability	0.054*	0.056*	-0.007	-0.009	-0.026	-0.030
	(0.030)	(0.030)	(0.044)	(0.043)	(0.046)	(0.044)
Safe Space × High Ability	-0.000	0.005	0.041	0.025	0.027	0.010
	(0.029)	(0.029)	(0.057)	(0.059)	(0.057)	(0.060)
Safe Space × Low Ability	0.044	0.048*	0.000	0.011	0.021	0.026
	(0.027)	(0.027)	(0.048)	(0.047)	(0.053)	(0.050)
Controls	Baseline	Double Lasso	Baseline	Double Lasso	Baseline	Double Lasso
Mean of Dep. Var.	0.901	0.901	0.513	0.513	0.452	0.452
High Ability Neg. vs. Low Ability Neg. (p-value)	0.421	0.418	0.293	0.256	0.072	0.062
High Ability Neg. vs. High Ability S.S. (p-value)	0.414	0.490	0.499	0.313	0.138	0.076
Number of observations	1,137	1,137	1,139	1,139	1,139	1,139
Adjusted R^2	0.025	0.041	0.076	0.106	0.053	0.072
Panel B: Enrolled in Morning School						
			Grade 10	**Grade 10**	**Grade 11**	**Grade 11**
Negotiation × High Ability			0.137**	0.142***	0.143***	0.144***
			(0.055)	(0.054)	(0.054)	(0.053)
Negotiation × Low Ability			-0.035	-0.035	-0.030	-0.033
			(0.042)	(0.040)	(0.042)	(0.042)
Safe Space × High Ability			0.024	0.014	0.015	0.011
			(0.047)	(0.049)	(0.053)	(0.054)
Safe Space × Low Ability			-0.022	-0.012	-0.001	0.003
			(0.044)	(0.044)	(0.046)	(0.046)
Controls			Baseline	Double Lasso	Baseline	Double Lasso
Mean of Dep. Var.			0.297	0.297	0.269	0.269
High Ability Neg. vs. Low Ability Neg. (p-value)			0.030	0.020	0.023	0.020
High Ability Neg. vs. High Ability S.S. (p-value)			0.032	0.021	0.012	0.013
Number of observations			1,103	1,103	1,116	1,116
Adjusted R^2			0.102	0.123	0.081	0.099

Panel A reports the heterogeneous effects of negotiation and safe space on girls in the top 40% and bottom 60% of the ability distribution on being enrolled in the indicated grade. Panel B reports the same heterogeneity analysis for the impact on being enrolled in morning school, the more academic track of the Zambian school system. The heterogeneity examined here was determined by a causal tree machine learning analysis. 50% of the data were used to determine the sources of heterogeneity. The effects in this table are then estimated on the remaining, distinct 50% of the data. Baseline controls consist of classroom fixed effects and an information treatment control, our stratification variables. The double lasso specification uses double lasso to choose additional controls among variables 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, reading and speaking English excellently and well, reading and speaking Nyanja excellently and well, age and age-squared, and ethnicity fixed effects. The High Ability Neg. vs. Low Ability Neg. row reports the 2-sided p-value from a F-test of the equality of the negotiation coefficients for high and low ability girls. The High Ability Neg. vs. High Ability S.S. row reports the 2-sided p-value from a F-test of the equality of the negotiation and safe space coefficients for high ability girls. The data set is restricted to participants with baseline data. Standard errors are clustered at the class level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table 5: Effects of Negotiation and Safe Space on Tokens Sent by Guardians

	(1)	(2)	(3)	(4)
Panel A: Investment Game with Communication and Comparison to Non-Communication Game				
	Dependent Variable: Tokens Sent by Parents			
	Comm.	**Comm.**	**Pooled**	**Pooled**
Game Type:	**Investment**	**Investment**	**Investment**	**Investment**
Negotiation	0.396**	0.404**	-0.476***	-0.445**
	(0.193)	(0.195)	(0.177)	(0.176)
Safe Space	0.092	0.092	-0.447***	-0.399**
	(0.200)	(0.195)	(0.167)	(0.172)
Negotiation × Comm.			0.835***	0.813***
			(0.257)	(0.255)
Safe Space × Comm.			0.526*	0.490*
			(0.270)	(0.271)
Controls	Baseline	Double Lasso	Baseline	Double Lasso
Mean of Dep. Var.	5.279	5.279	5.360	5.360
Neg. vs. Safe Space (p-value)	0.197	0.191	0.266	0.246
Number of observations	630	630	1,290	1,290
Adjusted R^2	0.071	0.100	0.040	0.054
Panel B: Alternate Game Results to Isolate Mechanisms				
	Dependent Variable: Tokens Sent by Parents			
	No Comm.	**No Comm.**		
Game Type:	**Investment**	**Investment**	**Dictator**	**Dictator**
Negotiation	-0.451**	-0.456**	0.645	0.569
	(0.195)	(0.206)	(0.398)	(0.386)
Safe Space	-0.380**	-0.357*	0.484	0.429
	(0.180)	(0.188)	(0.357)	(0.345)
Controls	Baseline	Double Lasso	Baseline	Double Lasso
Mean of Dep. Var.	5.441	5.441	4.952	4.952
Neg. vs. Safe Space (p-value)	0.703	0.610	0.643	0.670
Number of observations	660	660	321	321
Adjusted R^2	0.033	0.047	0.065	0.081

This table reports the effects of the negotiation and safe space treatments on the parent's behavior in the lab-in-the-field investment game. The dependant variable is the number of tokens sent by the parent out of her 10 tokens. Panel A, columns 1-2 use the sample that participated in the main game, the Investment Game with Communication. Columns 3-4 pool this game with the Investment Game with No Communication to isolate the effect of communication. Panel B, columns 1-2 restrict the sample to girls who participated in the Investment Game with No Communication, while columns 3-4 restrict the sample to those who participated in the Dictator Game. Baseline controls consist of classroom fixed effects and an information treatment control, our stratification variables. The double lasso specification uses double lasso to choose additional controls among variables 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, reading and speaking English excellently and well, reading and speaking Nyanja excellently and well, age and age-squared, and ethnicity fixed effects. The Neg. vs. Safe Space row reports the 2-sided p-value from a F-test of the equality of the safe space and negotiation coefficients, except in the case of columns 3-4 of Panel A. In these columns, it reports the p-value for a test of the equality of the interaction terms Negotiation × Comm. and Safe Space × Comm. The sample is restricted to participants for whom baseline data was available. Standard errors are clustered at the class level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table 6: Effects of Negotiation and Safe Space on Tokens Returned

	(1)	(2)	(3)	(4)
	Dependent Variable: Tokens Returned by Girls			
Extra Tokens	0.448***	0.452***	0.583***	0.580***
	(0.091)	(0.092)	(0.151)	(0.150)
Comm.×Extra×Negotiation	0.480**	0.475**	0.554**	0.565**
	(0.229)	(0.227)	(0.258)	(0.252)
Negotiation×Extra	-0.298*	-0.323*	-0.437**	-0.446**
	(0.169)	(0.167)	(0.205)	(0.201)
Comm.×Extra	-0.159	-0.136	-0.227	-0.234
	(0.127)	(0.127)	(0.202)	(0.204)
Safe Space×Extra			-0.259	-0.239
			(0.219)	(0.213)
Comm.×Extra×Safe Space			0.126	0.163
			(0.333)	(0.334)
Negotiation	0.283	0.354	0.416	0.463*
	(0.242)	(0.245)	(0.270)	(0.273)
Safe Space	0.092	0.111	0.343	0.346
	(0.210)	(0.208)	(0.303)	(0.301)
Communication Dummy	-0.206	-0.193	-0.143	-0.090
	(0.228)	(0.233)	(0.277)	(0.283)
Comm.×Negotiation	-0.524	-0.569	-0.591	-0.647*
	(0.368)	(0.363)	(0.390)	(0.388)
Comm.×Safe Space	0.260	0.218	0.143	0.039
	(0.302)	(0.303)	(0.417)	(0.425)
Controls	Baseline	Double Lasso	Baseline	Double Lasso
Mean of Dep. Var.	4.564	4.564	4.564	4.564
Neg. vs. S.S. for Triple Interaction (p-value)			0.163	0.195
Number of observations	1,285	1,285	1,285	1,285
Adjusted R^2	0.251	0.258	0.251	0.256
	Implied Pass-Through Rate			
Control	0.289***	0.316***	0.356***	0.367**
	(0.099)	(0.100)	(0.138)	(0.143)
Negotiation	0.471***	0.468***	0.473***	0.468***
	(0.151)	(0.154)	(0.151)	(0.154)
Safe Space			0.223	0.267
			(0.166)	(0.169)

This table reports the effects of the negotiation and safe space treatments on daughters' propensity to return additional tokens to parents in the two versions of the investment game, with and without communication. The sample excludes girls who were assigned to the dictator game. The bottom panel calculates the implied pass-through rate of a marginal token (i.e., the portion of one additional token that girls returned to parents) in the game with communication using the coefficient estimates from the same column. Baseline controls consist of classroom fixed effects and an information treatment control, our stratification variables. The double lasso specification uses double lasso to choose additional controls among variables 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, reading and speaking English excellently and well, reading and speaking Nyanja excellently and well, age and age-squared, and ethnicity fixed effects. The Neg. vs. S.S. for Triple Interaction (p-value) row reports the 2-sided p-value from a F-test of the equality of the coefficients for Comm.×Extra×Negotiation and Comm.×Extra×Safe Space. The sample is restricted to participants with non-missing baseline data. Standard errors are clustered at the class level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table 7: Evidence on Mechanisms From the Follow-Up Survey

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Outcomes in the Follow-Up Survey							
	Asks for Food	**Difficulty Getting to do Chores**	**Difficulty Controlling Temper**	**Girl is Rude**	**Natural Ability**	**Natural Ability**	**Grade Wants to Complete**
Negotiation	0.065**	-0.026	0.006	-0.042	-0.086	-0.081	-0.005
	(0.027)	(0.021)	(0.011)	(0.074)	(0.065)	(0.065)	(0.066)
Safe Space	0.016	-0.006	-0.016	-0.022	-0.081	-0.073	-0.062
	(0.027)	(0.020)	(0.010)	(0.071)	(0.065)	(0.065)	(0.069)
Negotiation×Ability						-0.054	
						(0.089)	
Safe Space×Ability						-0.122*	-0.068
						(0.074)	(0.079)
Negotiation vs. Safe Space (p-value)	0.111	0.338	0.040	0.748	0.947	0.356	0.477
Mean Dep. Var.	0.284	0.134	0.022	0.598	3.667	3.667	15.254
Number of observations	1,665	1,564	1,575	1,566	1,562	1,562	1,660
Adjusted R^2	0.016	0.052	0.011	0.003	0.102	0.102	0.053
Panel B: Timing of Chores							
	Chores Before School	**Chores During School**	**Chores After School**	**Total Weekday Chores**			
Negotiation	-0.046	-0.069	0.041	-0.287			
	(0.032)	(0.090)	(0.089)	(0.181)			
Safe Space	0.027	0.038	0.133	0.103			
	(0.034)	(0.090)	(0.095)	(0.181)			
Negotiation×Friday				0.593**			
				(0.288)			
Safe Space×Friday				0.253			
				(0.299)			
Negotiation vs. Safe Space (p-value)	0.035	0.175	0.328	0.190			
Mean Dep. Var.	0.488	1.226	2.315	4.029			
Number of observations	1,665	1,665	1,665	1,665			
Adjusted R^2	0.167	0.260	0.111	0.228			

This table reports the effect of negotiation and safe space on outcomes in the follow-up survey. In Panel A, Column 1 reports girls' reports of ever asking for more food (0/1). Columns 2-4 report parental assessments of girls' behavior on a 0/1 scale. Column 5 reports the parent's assessment of girls' ability relative to classmates on a 1-5 scale. Column 6 has the same dependent variable but interacts negotiation with the girl's measured ability (based on a factor analysis of baseline ability measures) to see if parental ability estimates become better aligned with true ability. In column 7, the outcome variable is the years of schooling a girl reports wanting to complete. Panel B reports results from a time diary exercise from the girls' survey. The first column is the hours spent doing chores before school, column 2 is hours spent doing chores during school hours, and column 3 is hours spent during after school hours. Column 4 is the total number of hours spent doing chores on the most recent weekday. All regressions include controls for the stratification variables (classroom fixed effects and the information treatment). Double lasso is used to choose additional controls among variables 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, reading and speaking English excellently and well, reading and speaking Nyanja excellently and well, age and age-squared, and ethnicity fixed effects. The row Negotiation vs. Safe Space reports the two-sided p-value from a test of the equality of the safe space and negotiation coefficients. The sample is restricted to participants for whom baseline data is available. Standard errors are clustered at the classroom level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Appendix A: Data Appendix

In this appendix, we provide more details on the timing and implementation of the baseline survey, as well as how we collected administrative data on participants' outcomes.

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.

Shorter-Term Administrative Data Collection. 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, enrollment, and school type) 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, school type, 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.

Longer-Term Enrollment and Pregnancy Data. Data on whether students were still enrolled in school and whether they had become pregnant were collected from school administrators at the end of year. Beginning in 2014 (term 1 of grade 9), we cross-checked this data with data collected by the class monitors. We also tracked whether participants in intervention schools enrolled in grade 10 and 11 by contacting the basic schools in our study sample, as well as visiting upper secondary schools in the Lusaka area. Depending on the score from the grade 9 national exam, pupils are assigned to enroll in particular secondary schools. We first gathered information from their basic schools to determine whether our participants had enrolled in grade 10, and if so, at which secondary school. In order to confirm that our participants enrolled at a particular school, we visited the secondary schools they were assigned to throughout Lusaka and verified if they were indeed enrolled.

When pupils were found, they provided us with information on their peers' secondary enrollment statuses, as well as their own. We used this information to visit other secondary schools that were not listed by the basic schools within Lusaka and search for any participants from our intervention. If we found girls at these schools, we collected enrollment and pregnancy statuses. Although multiple schools within Lusaka were searched for each girl, schools outside Lusaka or outside the government schooling system were not searched; any of these outcomes would indicate a less preferred enrollment outcome. Thus, an enrollment status of "zero" indicates "not verifiably enrolled in a Lusaka government school" rather than not continuing schooling in any form. In 2016, we went back to the same secondary schools for additional verifications on data collected in 2015, as well as to collect information on girls' statuses in grade 11.

Appendix B: Treatment Details

This appendix provides additional details on the negotiation, safe space, and information treatments.

Negotiation Treatment. As the negotiation curriculum has already been described in depth, we use this space to share additional quotes from girls on how they used the different elements of the curriculum. Each of the quotes below illustrates at least one of the principles of the curriculum.

Me. 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.*

You. 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.*

Together & Build. A participating girl discusses identifying a roadblock behind her sister's refusal to do her hair and brainstorming a solution that made both young women better off:

> *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.*

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 brainstorming 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. This final element of the intervention is in line with Dupas (2011), who found that educating teenagers about the prevalence of HIV among older men reduced risky sexual behavior.

Safe Space Treatment. 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. 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 (and the safe

space program may have seemed more "fun" to girls after a long school day than the learning that took place during the negotiation program). Despite this potential, we chose 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 negotiation program.

Appendix C: Mathematical Appendix

This appendix provides proofs of Proposition 1 and the model's key predictions. Without loss of generality, for simplicity, we suppress the i subscripts with the exception of the subscript on R_i.

Proof of Proposition 1.

Proof. This proof proceeds by considering each of the four cases described in proposition 1.

1. $R_i \geq R_i^* \equiv \frac{\tilde{f}-\tau_{ns}(1-\delta)}{\delta} + c(R_i - \tau_{ns})$, $\tau^* = \tau_{ns}$ and $E = 1$. It is clear that if a parent chooses $E = 1$ when $\tau = \tau_{ns}$, the daughter will transfer τ_{ns} since this is her utility maximizing transfer if $E = 1$ and she has no strategic incentive to transfer more. From equation (3), we see that $E = 1$ if $R_i \geq \frac{\tilde{f}-\tau_{ns}(1-\delta)}{\delta} + c(R_i - \tau_{ns})$. Finally, to show we can define a threshold returns to education R_i, we show there is single-crossing in equation (3) in R_i if $\tau = \tau_{ns}$. Applying the implicit function theorem to $c'(R_i - \tau_{ns}) = 1$ shows that $\frac{\partial \tau_{ns}}{\partial R_i} = 1$. Note that equation (3) is a re-arrangement of $U^p_{E=1} > 0$, which can be re-written as

$$-\tilde{f} + \tau^* + \delta U^d_{E=1}(R_i - \tau^*) > 0,$$

Clearly, the derivative of the right-side with respect to R_i is 0. Differentiating the left-side with respect to R_i gives

$$\frac{\partial LS}{\partial R_i} = \frac{\partial \tau^*}{\partial R_i} + \delta\Big(- \frac{\partial \tau^*}{\partial R_i} + 1 - c'(R_i - \tau^*) + c'(R_i - \tau^*)\frac{\partial \tau^*}{\partial R_i}\Big)$$

If $\tau = \tau_{ns}$, under the envelope theorem, $\Big(- \frac{\partial \tau^*}{\partial R_i} + 1 - c'(R_i - \tau^*) + c'(R_i - \tau^*)\frac{\partial \tau^*}{\partial R_i}\Big) = 0$, though we can also see this by substituting in $\frac{\partial \tau_{ns}}{\partial R_i} = 1$. Thus, $\frac{\partial LS}{\partial R_i} = \frac{\partial \tau^*}{\partial R_i} = 1$. As this is always greater than 0, there is single-crossing in R_i.

2. $R_i^* > R_i \geq R_i^{**} \equiv \max(\frac{\tilde{f}-\bar{\tau}(1-\delta)}{\delta} + c(R_i - \bar{\tau}), \tilde{f})$ and $E = 1$. From equation (3), a parent will invest if $R_i \geq \frac{\tilde{f}-\bar{\tau}(1-\delta)}{\delta} + c(R_i - \bar{\tau})$. From equation (2), a daughter for whom $R_i < R_i^*$ will be willing to transfer up to R_i to be educated. Re-arranging equation (3), the minimum transfer needed to be educated is characterized by $\tau^* - \frac{\delta c(R_i - \tau^*)}{1-\delta} = \frac{\tilde{f}-R_i}{1-\delta}$, with the constraint that $\tau^* \leq \min(\bar{\tau}, R_i)$. The daughter chooses the minimum transfer since for $\tau^* > \tau_{ns}$, $c'(R_i - \tau^*) < 1$ and $\frac{\partial U^d_{E=1}(R_i - \tau^*)}{\partial \tau} < 0$. The final step is to show that the transfer needed to be educated, τ^*, is always falling in R_i. This implies that there is a single-crossing in R_i, where, as R_i increases, τ^* crosses $\bar{\tau}$, and above that point, all girls are educated. To show this, we use implicit differentiation to show that $\frac{\partial \tau^*}{\partial R_i} = \frac{\delta(c'(R_i - \tau^*) - 1)}{1 - \delta + \delta c'(R_i - \tau^*)}$. Since $c'(R_i - \tau^*) < 1$, if $\tau^* > \tau_{ns}$, the numerator is negative and

since $\delta < 1$, the denominator is positive. Therefore, $\frac{\partial \tau^*}{\partial R_i} < 0$.

3. $R_i < R_i^{**}$, $E = 0$. In this case, $\tau^* > \bar{\tau}$, and a daughter will not be educated.

4. $R_i < \tilde{f}$, $E = 0$. In this case, a daughter would have to compensate her parent more than $\tilde{f}$ to be educated and she would never be willing to do this since $\tilde{f} > R_i$.

Proofs of Predictions.

Predictions

1. **Increasing** R_i. Increases education by increasing the likelihood a girl passes the threshold returns to education needed to be educated.

2. **Increasing** δ. Decreases R_i^* and R_i^{**}, increasing education.

3. **Increasing** σ, thus increasing $\bar{\tau}$. Decreases R_i^{**}, increasing education.

4. **Decreasing effect of** c **for a marginal girl**. Any perturbation that makes the cost function, c, less steep for a girl on the margin of being educated will increase R_i^* and R_i^{**}, reducing education.

5. **Reducing** $\tilde{f}$**.** Decreasing $\tilde{f}$ decreases R_i^* and R_i^{**}, increasing education.

Proof.

1. **Increasing** R_i. Follows from Proposition 1.

2. **Increasing** δ. Differentiating R_i^* and R_i^{**} with respect to δ produces $\frac{\partial R_i^*}{\partial \delta} = \frac{\partial R_i^{**}}{\partial \delta} = -\frac{1}{\delta} < 0$.

3. **Increasing** σ. Differentiating R_i^{**} with respect to σ_i produces $\frac{\partial R_i^{**}}{\partial \sigma_i} = \frac{\delta - 1}{\delta} - \alpha c'(R_i^{**} - \bar{\tau}) < 0$.

4. **Decreasing effect of** c **for a marginal girl**. The marginal girls are at R_i^* and R_i^{**}. We first consider the marginal girl at R_i^*. To prove this prediction, we replace c with $\hat{c}(R_i^* - \tau_{ns}) = (1-\alpha)c(R_i^* - \tau_{ns}) + \alpha g(R_i^* - \tau_{ns})$, where $\alpha = 0$, $g'(R_i^* - \tau_{ns}) < c'(R_i^* - \tau_{ns})$, and $g(R_i^* - \tau_{ns}) = c(R_i^* - \tau_{ns})$. Replacing c with $\hat{c}$ at R_i^*, we differentiate R_i^* with respect to α:

$$\frac{\partial R_i^*}{\partial \alpha} = -\frac{\partial \tau_{ns}}{\partial \alpha}\frac{1-\delta}{\delta} + g'(R_i^* - \tau_{ns})\Big(\frac{\partial R_i^*}{\partial \alpha} - \frac{\partial \tau_{ns}}{\partial \alpha}\Big) - c'(R_i^* - \tau_{ns})\Big(\frac{\partial R_i^*}{\partial \alpha} - \frac{\partial \tau_{ns}}{\partial \alpha}\Big) \quad (6)$$

Next, differentiating $U^d_{E=1}$ with respect to τ shows that $1 = (1-\alpha)c'(R_i^* - \tau_{ns}) + \alpha g'(R_i^* - \tau_{ns})$, and with implicit differentiation, we find that

$$\frac{\partial R_i^*}{\partial \alpha} - \frac{\partial \tau_{ns}}{\partial \alpha} = \frac{c'(R_i^* - \tau_{ns}) - g'(R_i^* - \tau_{ns})}{c''(R_i^* - \tau_{ns})} > 0$$

Differentiating R_i^* with respect to α, substituting $\frac{c'(R_i^*-\tau_{ns})-g'(R_i^*-\tau_{ns})}{c''(R_i^*-\tau_{ns})}$ for $\frac{\partial R_i^*}{\partial \alpha} - \frac{\partial \tau}{\partial \alpha}$ and $\frac{c'(R_i^*-\tau_{ns})-g'(R_i^*-\tau_{ns})}{c''(R_i^*-\tau_{ns})} - \frac{\partial R_i^*}{\partial \alpha}$ for $-\frac{\partial \tau_{ns}}{\partial \alpha}$ leads to the expression

$$\frac{\partial R_i^*}{\partial \alpha} = \delta\Big(\frac{c'(R_i^* - \tau_{ns}) - g'(R_i^* - \tau_{ns})}{c''(R_i^* - \tau_{ns})}\Big)\Big(\frac{1-\delta}{\delta} + c'(R_i^* - \tau_{ns})\Big) > 0,$$

indicating that the perturbation makes R_i^* greater, reducing education.
Now consider the case of the marginal girl at R_i^{**}. The proof is very similar:

$$\frac{\partial R_i^{**}}{\partial \alpha} = -\frac{\partial \bar{\tau}}{\partial \alpha}\frac{1-\delta}{\delta} + g'(R_i^* - \bar{\tau})\Big(\frac{\partial R_i^*}{\partial \alpha} - \frac{\partial \bar{\tau}}{\partial \alpha}\Big) - c'(R_i^* - \bar{\tau})\Big(\frac{\partial R_i^*}{\partial \alpha} - \frac{\partial \bar{\tau}}{\partial \alpha}\Big).$$

Recognizing, that $\bar{\tau} = \sigma_i + \tau_{ns}$ and that σ_i is fixed, we see that $\frac{\partial \bar{\tau}}{\partial \alpha} = \frac{\partial \tau_{ns}}{\partial \alpha}$. Thus, the above expression is the same as in equation (6) and $\frac{\partial R_i^*}{\partial \alpha} = \frac{\partial R_i^{**}}{\partial \alpha} > 0$.

5. **Reducing $\tilde{f}$.** Differentiating R_i^* and R_i^{**} with respect to $\tilde{f}$ produces $\frac{\partial R_i^*}{\partial \delta} = \frac{\partial R_i^{**}}{\partial \delta} = 1$.

Appendix D: Participants with Missing Baseline Data

In this appendix, we explore the sensitivity of our estimates in Table 2 to the inclusion of participants who did not participate in the baseline. There were a small number of girls (4.6%) who were randomized into the experiment even though a baseline survey was never conducted with them. This was because there was an extremely tight schedule to hold parent meetings, conduct baseline surveys, randomize, and then start the intervention at each school. As a result, if a girl's parents had consented but she had not yet been surveyed, our implementing partner allowed girls to be placed in the randomization pool (which took place over the weekend) for the intervention starting on Monday with the goal of conducting the baseline survey during the school day before the girl learned her treatment status. In reality, some of these girls could not be found on Monday. These girls would have been unlikely to learn their treatment status, as the implementers told them their treatment status at a school meeting on Monday afternoon, with the first day of the intervention beginning immediately after.

These girls are evenly distributed across the treatments, as this attrition took place before treatment assignment was communicated to girls. In most randomized experiments, only individuals with baseline data are randomized into the experiment, and according to our original study protocol, those girls who had not completed baseline surveys should not have been randomly assigned to a treatment arm. In fact, they were unlikely to be treated given that they were not present at school to learn their treatment status (60% of negotiation girls in this category received 0 days of treatment). Thus, for our main specification, we treat this sample as outside of the experiment. However, this appendix explores the sensitivity of our results to the inclusion of this sample.

In Appendix Table A5, to include these additional participants, we replicate the specifications in Table 2, recoding our control variables to take the value 0 if they are missing and including an indicator variable for missing baseline data. With the inclusion of participants without baseline data, negotiation continues to have marginally significant effects on 11th grade enrollment and significant effects on 11th grade morning schooling. However, the effect sizes are smaller than the estimates in Table 2, consistent with the inclusion of individuals coded as having been treated who did not actually receive the treatment. To evaluate whether the fact these individuals were less likely to receive the treatment could be driving the differences in the point estimates, in Appendix Table A6, we also instrument for attending at least 5 sessions (the average negotiation girl with non-missing baseline data attended 4.9 sessions). The resulting point estimates are consistent with the estimates that restrict to the sample that took part in the baseline survey.

Appendix E: Machine Learning

This appendix provides more details on the machine learning exercise used to identify heterogeneity in the negotiation treatment effect. The machine-learning procedure requires splitting our data set so that separate samples are used to identify the sources of heterogeneity (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 maximize 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 altruism 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 11th grade enrollment. When we split the sample, we treat a classroom as an independent unit

rather than an individual to reflect the fact that error terms may be correlated within a classroom. We consider partitions of the data using the ability index, the altruism index, and age.

Appendix F: Additional Lab-in-the-Field Game Results

In this appendix, we report additional results from the lab-in-the-field game.

Connection Between Tokens Sent and Human Capital Outcomes. To evaluate whether the number of tokens parents sent in the investment game is related to "real world" outcomes, we regress different human capital measures on the number of tokens sent. Since we are interested specifically in whether the investment game reflects the educational investment decision, we drop the sample that took part in the dictator game. The results from this exercise are reported in Appendix Table A12. We find a positive connection between the number of tokens parents sent and educational investment. An additional token sent by guardians is associated with a 0.03 sd increase in the human capital index, a 1.3 percentage point greater chance of being enrolled in 11th grade, and a 2.0 percentage point greater chance of enrolling in morning school in 11th grade.

Word Game. The two investment game variants were cross-randomized with conditional doubling depending on the girl's successful completion of a word search. This word search variant was initially designed to create variation in the return on investment connected to girls' ability. Through this, we hoped to test whether strategic communication was more effective when girls had (private) information to share on their ability. We hoped this would let us test whether aligning parental expectations of the returns on investment with the girl's own expectations was one possible channel for the negotiation treatment's effects.

Parents and girls participating in this variation were told that girls would be given a word search with 6 words. If they found at least 3 of them, the tokens sent to the girl would be doubled. Otherwise the tokens would be passed on to the girl without doubling. In practice, 85% of girls found at least 3 words. Thus, in the vast majority of cases, the number of tokens was doubled. As shown in Table A16, there is no significant difference between giving with and without the word game, no interaction with negotiation, and no gradient by ability. It's possible that the word search variation's non-effect is because the game did not create sufficient variation in parental investment to be used to identify variation in investment based on the daughter's ability. Because we do not find evidence that the word game interacted with the treatments to affect investment decisions, we pool the version of the investment games with and without the word search variant in all of our main tables.

How Girls Spent the Tokens. In the non-communication version of the game, if negotiation and safe space girls are more empowered, we expect them to spend more tokens on their own consumption. In the communication version, if negotiation girls are cooperating with their parents, we expect them to spend more tokens on goods (such as household and educational goods) that parents would value relatively more. To test if this is the case, in Appendix Table A14, 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 two categories: (1) non-consumption items consisting of school supplies (colored pens, math books, notebooks, pencils, erasers, rulers, and pencil sharpeners) and household items (socks and sanitary pads) and (2) pure consumption items (hair ties, scarves, bracelets, lip balm, snacks, and snakes and ladders games). We label spending on the first set of items as spending on "non-consumption" goods, while spending on the second set is spending on "pure consumption goods."

Consistent with negotiation and safe space girls' more individualistically empowered behavior, these girls spend more in magnitude on pure consumption relative to control girls in both the non-communication investment and dictator games. However, when negotiation girls are allowed to communicate with their parents, they spend less on pure consumption and more on school and household items (columns 3 and 4). Communication does not have significant effects on safe space girls' spending, and in a 1-sided F-test, we find that the interactions of safe space and negotiation with communication are marginally significantly different. These results provide further suggestive evidence that the communication game allows for strategic cooperation between parents and girls in the negotiation treatment. Negotiation girls are able to credibly communicate their intentions to both return more tokens and spend tokens in a more household-oriented way.

Knowledge Interaction. We next test whether the skills component of negotiation matters specifically. We re-run the main investment game analysis in Table 5 but now include a control for our aggregate measure of knowledge of negotiation (from the follow-up survey) and its interaction with the communication indicator variable. Appendix Table A13 reports these results. Consistent with the idea that negotiation skills matter for increasing the contracting space in settings where girls can communicate, we find that knowledge leads girls to receive more tokens in the communication game but has no effect in the non-communication game. In the pooled analysis, we also test for the interaction between knowledge and communication, and find negotiation knowledge interacts positively and significantly with communication. Thus, this table provides further evidence that skills themselves matter, and they matter specifically in the communication version of the investment game, where there is scope to use these skills to foster strategic cooperation with parents.

Ability Interaction. We interpret the heterogeneous effects of negotiation on high ability and lower ability girls' human capital outcomes as being driven by the fact that high ability girls are on the margin of greater educational investment. An alternative interpretation is that high ability girls are better able to learn and use negotiation skills than lower ability girls. We evaluate whether this the case in Appendix Table A17. This table repeats the main investment game analysis in Table 5 but now estimates separate effects for negotiation and

negotiation × communication on high and low ability girls. As with the machine learning exercise, ability is the first factor of a factor analysis of the baseline variables assessing speaking and writing skills in Nyanja and English. A girl is considered high ability if she is in the top 40% for this measure and low ability if she is in the bottom 60%. The results in Appendix Table A17 show that academic ability does not create heterogeneity here, which makes sense, since actual returns are fixed and only transfers can vary. Thus, the negotiation skill results are not proxying for ability.

Daughter's Welfare. Next, in Appendix Table A15, we investigate whether negotiation made daughters better off in the investment game. The regressions in Appendix Table A15 duplicate the full control specifications of Table 5, but the outcome variable is now the final number of tokens with which the daughter finishes the game.[45] The results in Appendix Table A15 echo those in Table 5. Negotiation girls in the communication treatment end the game with marginally significantly more (0.649) tokens than control girls, while safe space girls end with directionally fewer. These differences are marginally significant in a 2-sided F-test. Column 2 shows that the effect of communication on negotiation girls' final token count is statistically significantly positive, indicating that despite the higher propensity to return marginal tokens, negotiation girls still ended the game with more resources. Column 3 shows that girls who received the safe space and negotiation treatments and were not allowed to communicate with their parents end the game with fewer tokens, consistent with Table 5. Thus, the lower propensity of these girls to return tokens does not cancel out the lower number of tokens sent by parents. The point estimates for the dictator game (column 4) are positive but not significant. On net, the estimates are consistent with the idea that negotiation – with communication – not only increased the total size of the surplus but also provide some evidence that it increased girls' welfare.

Movement Toward the Efficient Frontier. Appendix Figure A4 demonstrates that the communication treatment led to allocations that were closer to the efficient frontier for negotiation girls. 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. With communication, the parent-child pairs from the negotiation treatment have more density both closer to the efficient frontier and closer to the upper-left side of the graph. Consistent with the results in the tables, visually, communication among negotiation girls appears to lead to allocations that are closer to the red-line (more efficient) and closer to the upper-left of the graph (better for daughters).

[45] This is the number of tokens sent multiplied by two (plus the random income shock) in the dictator game. It is net of the number of tokens returned in the investment game.

Appendix G: Lab-in-the-Field Protocols

In this section, we include the protocols for the guardian's and girl's sides of the investment game with communication. To include details on all possible variants, we include the protocol for the most complex game, the communication game with the word search. Only half of the communication games were played with the word search. For the version without the word search, tokens were doubled automatically. For the version without communication, the communication break was removed. For the dictator game, the opportunity for girls to return tokens was removed.

Communication + Word Search Game: GIRL protocol

Please follow the instructions on this sheet as you begin the experiment. The text in italics is instructions for you; the boxed should be read to the respondent, including any headings within the boxes. As you go through the experiment, please remember to:

- *Follow the instructions exactly as they are written*
- *Only go to the next step when the respondent understands the previous directions*
- *Avoid influencing the respondent's answer*
- *Where it says <guardian> if you know it is the mom/dad/aunt/uncle, say that, otherwise just say "guardian."*

As you go through the activity, make sure you are recording the answers under the correct question numbers on your answer sheet.

(Check that the ID on the answer sheet matches the ID for the participant. Check the activity color code on the answer sheet and ensure it matches the color code of the protocol you are using. Check whether this is a communication activity or not. Label your keep and send envelopes with the ID number.)

INTRODUCTION *[approach alongside guardian experimenter and together greet both the guardian and girl with the text below]*

- Good afternoon, how are you? My name is_____ . Nice to meet you. Thank you for waiting.
- We are now ready to do the short activity with prizes we mentioned before. I'll be doing the activity with you, while my colleague here will do the activity with <your guardian>
- I'll explain more once we go sit down together to do the activity.
- We'll let them go first, then you'll follow me to our place.

After waiting for the guardian to see the prize table separately and head to the guardian room, take the girl to see the prize table, ensuring that the prices are not on the table at this point. Let her view the items, but not go up and closely touch or inspect the items. Be aware the guardian and the girl are not together when looking at prizes.

- These are some of the prizes you can use your tokens for at the end of the activity. We call this area the "store".

Then take the girl back to your area

EXPLAIN THE ACTIVITY

- There are three possible activities that the guardians and girls may do as part of this survey. For each guardian-girl pair, one of these three activities was chosen randomly, or by chance, for them to participate in. For you and your <guardian>, the red activity has been chosen. During this activity, you will have an opportunity to get some prizes. You will do

the activity with your <guardian>. We are going to ask your <guardian> to make some decisions. At the end, the prizes you get will be determined by the decisions **you** made **and** the decisions **your <guardian>** made, in addition to a small amount of chance/luck.

- You and I are doing the activity in a different space than your <guardian> to give each of you privacy to make your decisions. Our discussion here is also private, meaning just between us. *[If spot-checker is present this day add:]* If another colleague of mine joins during our discussion, she is just coming to help me, and any discussions we have will also just be between the three of us.
- As I explain the steps of the activity, I will ask a few questions as we go along, just to make sure you are understanding. I will be reading the instructions from this script, so that I make sure everyone is told the instructions in the same way, and can make their decisions based on the same information. Are you ready to start?

- We will do the activity with these tokens. *[show the girl a token]* At the end of the activity, each token can be traded in for 1 kwacha worth of prizes.
- Your <guardian>starts the activity with 10 tokens, while at first you start with zero tokens.

- **Step 1: The first step of the activity is your Guardian's choice**
- Of the tokens your <guardian> starts with, they can choose how many to keep and if they would like to send some to you. The tokens he/she keeps can be traded for airtime cards at the end of the activity.
- The tokens he/she sends, if any, MAY be doubled before they are given to you so a token that he or she sends may become two tokens when it is given to you. *[show one token, then hold up a second token next to it as you say "may become two tokens"]* (I will explain how the tokens may be doubled in a moment.) The tokens they keep will not double, but rather each remain as one token.
- Later, you will have a chance to send some tokens back, so your <guardian> knows this doubling may help him/herself as well as you.
- It is completely up to your <guardian> how many tokens to keep and how many to send.

- To review: What is the choice your <guardian> makes with his or her tokens in this first step? *(Choose how many to keep and how many to send)*

- **STEP 2: You do a word search**
- To determine whether any tokens sent to you will be doubled, you will be asked to do a word search.
- The word search will involve searching for words in a puzzle, with a limited period of time. This word search will be at your grade level, so a typical girl in Grade 8 would find it challenging, but possible to find at least some of the words. Here is a sample of what the word search looks like. *(Show word search)*
- If you can find at least half of the possible words, the tokens sent to you will be doubled before they are given to you. If not, you will still get the tokens, but they will not be doubled.
- Note, you will receive any tokens sent after you complete the word search, so you will not know how many tokens were sent before you do the word search.
- To review: What happens if you find at least half the words? *(the tokens sent are doubled)* What happens if you do not find half the words? (*I still get the tokens, but they are not doubled).* Do you know how many tokens were sent before doing the word search? *(I do not know before the word search; I find out after)*
- **Step 3: You receive tokens**
- You will then receive the tokens from your guardian, if any, and they will be either doubled or not, plus…
- In addition to those tokens sent to you, if any, you may receive up to 4 extra tokens. The exact amount added will depend on chance, so it is not the same for every girl, but it will not be more than 4. These "chance" tokens will be added to the amount your <guardian> sent, so that you will not know exactly how many tokens were sent by your guardian.
- Only the tokens your <guardian> sends, if any, can be doubled.
- To review: Where can you get tokens? *(From my guardian and from the extra tokens, which are given based on chance)*

- **Step 4: Your choice**
- Once you have gotten your tokens, you can decide what to do with them:
- From your total number of tokens, you can choose a number of tokens to send back to your guardian, if any
- You can spend your remaining tokens on prizes from the "store", which you saw on the table.
- This decision is completely up to you.
- Remember, that when deciding whether to send tokens to you, your <guardian> **knew that any tokens sent to you could be doubled** if you found at least half of the hidden words in the word challenge, whereas if <she/he> kept them to <him/her>self, the tokens would not double. Your <guardian> also knew that you could send some tokens back.
- To review: What can you do with your tokens? *(Choose how many to send to my guardian and choose how many to spend in the store)*

- **Step 5: Guardian receives tokens**
- We will then give any tokens sent from you to your guardian.
- Your <guardian> can then use his/her tokens, those he/she kept and any you sent, for airtime from his/her choice of network in exchange.
- To review: What tokens does your <guardian>get at the end? *(Those they kept and any I sent)*

UNDERSTANDING CHECK

- Great—let's go through an example to make sure we understand how the activity works. Remember the steps:
- Step 1: Your <guardian>can choose how many tokens to keep and how many to send to you. Any tokens sent by your <guardian> can be doubled.
- Step 2: You complete the word search. If you correctly identify half the hidden words, the tokens sent will be doubled before they are given to you.

- Step 3: You get those (possibly doubled) tokens from your guardian, if any, and up to 4 extra tokens at the same time.
- Step 4: You can then choose how many tokens to keep, if any, **and how many tokens to send back to your guardian**, if any. Tokens you keep can be spent at the store.
- Step 5: Your <guardian>gets the tokens he/she kept and those sent by you, if any, to spend on airtime.
- Here is a table showing how the tokens work. Let's go through some examples together, and then I'll ask you some questions.

Show the girl the table on a separate sheet (with blanks). Go through 10-0, 7-3, and 2-8 with her, like this "If your guardian keeps ___, and sends ___, you will get ___+chance to keep or send IF you find half the words in the word search, and ___+chance to keep or send if you do not find enough words. Then your guardian will get airtime worth ___, the amount he/she kept, plus any amount you might choose to send back." Then let the girl explain to you the blanks on the table. Be sure to probe, so if the girl answers "I get 2" ask, "plus what?" until she says "2 plus chance", and for the amount the guardian gets, ensure they mention both the "keep" amount and the return from the girl.

		I get to keep or send from:		
If my guardian keeps:	**And sends:**	**If I find enough words in the word search**	**If I do not find enough words**	**And he/she gets airtime worth:**
10	**0**	**0 + chance**	**0 + chance**	**10 + ? from me**
9	1	2 + chance	1 + chance	9 + ? from me
8	2	4 + chance	2 + chance	8 + ? from me
7	**3**	**6 + chance**	**3 + chance**	**7 + ? from me**
6	4	8 + chance	4 + chance	6 + ? from me
5	5	10 + chance	5 + chance	5 + ? from me
4	6	12 + chance	6 + chance	4 + ? from me
3	7	14 + chance	7 + chance	3 + ? from me
2	**8**	**16 + chance**	**8 + chance**	**2 + ? from me**
1	9	18 + chance	9 + chance	1 + ? from me
0	10	20 + chance	10 + chance	0 + ? from me

If they guess wrong, explain how that step works again, using the words from the protocol.

- Good. Now, to make sure you understand, could you please explain how the activity works to me in your own words?

(Probe to get them to explain the complete activity by saying "and then what happens?" etc. If they at any point show they have misunderstood something, take them back to the instructions for that step and read them again.)

THE ACTIVITY

- Now that you have fully understood the activity, you will have 5 minutes to discuss the activity with your <guardian> before the choices are made.

Bring the girl and the guardian together, and let them discuss with you out of hearing distance. However, keep a bit of an eye on the pair to make sure they are speaking only to each other and not to others. When the 5 minutes are over, take the girl back to sit down.

- Now let's start the activity. While your <guardian>makes his/her choices, you will do the word search. You will have 5 minutes to find as many words as possible, like the ones you see circled here. *(Refer to sample word search, and point out the words as you explain.)* The words can go up, down, or diagonal, and the can also be backwards like this one here. *(Point out and say the word*). Do you understand? *(If not, explain again. If yes, hand her the blank word puzzle to complete.)*
- So, you look for the words from this list among this scramble of letters, and circle any word you find. Ready? I'll give you 5 minutes….Start.

Give the girl the word search activity, and time her for five minutes. At the end of five minutes, count the words found (and double check that all are correct)
***R1**. Number of words found____*

Either:

- You found at least half the words, so tokens sent by your <guardian> will double.

Or:

- You did not find half the words, so tokens sent by your <guardian> will not double.

RECEIVE TOKENS

- Let me find out how many tokens you received. This may take a moment.

Get the sealed guardian envelope from the guardian surveyor. Open the envelope, count the tokens, and do the following calculations before returning to the girl.

R2.*Tokens sent by guardian*_____

- *If R1 is 3, 4, 5, or 6, multiply the guardian tokens by 2 and record in R3. If R1 is 1 or 2, simply record again the guardian tokens.*

R3*. Final tokens from guardian*_____

- *Refer to the answer sheet. If A, record 2 tokens in R4. If B, record 4 tokens in R4.*

R4. *Extra tokens*_____

- *Add together R3 and R4 and record in R5*

R5*.Total tokens to give to the girl*_____

- *Count out the total tokens and check that it equals R5. Put the tokens back in the envelope and return to the girl.*

- ***R6.*** Did you speak to your guardian during the break? What was discussed? Did you try to affect her choices? How? *Probe carefully*

- Thanks for waiting. You have received _____ tokens, which include those from your <guardian>, if any, and the extra tokens, if any.

- Now it is time for you to decide what you would like to do with your tokens.
- Any tokens you keep can be spent on prizes, and any that you send will be given to your <guardian>as airtime.
- Here are two envelopes. One labeled "Keep" and one labeled "Send" (*Point to which is which).* I will turn my back for one minute, and you can choose how many to put in each envelope. Remember, it fine to put any number from 0 to *[the amount of tokens the girl has]* in either envelope. This is your choice. Once you have made your choices, please seal the "Send" envelope like this [demonstrate sealing the envelope] and give it to me.

Surveyor, turn around. After one minute, if the respondent has not already said they are finished, ask if they have finished, but stay with back turned. If they have not, ask, "Do you need me to explain any of the instructions more, or do you just need more time?" If they say yes or seem unsure, repeat the instructions on the four steps from the understanding check, and what to do with the envelopes from the box above, and give them another 1 minute.

- Ok, we have now finished the activity. I know what you received may or may not be what you expected, but please remember this is only an activity, and that what you got partly depended on chance. You may now take your "keep" envelope to the store area and go select your prizes with my colleague there.

Either take the girl to the store, or point her toward it if it is very nearby.
Bring the sealed "send" envelope to the central meeting point and drop it off.

To be completed by check-out person

PRIZES

- Hello, good afternoon. How many total tokens do you have? *[record number of total tokens]* You may now choose whichever items you want adding up to the amount of tokens you have. Which prizes would you like? …Please keep your prizes in this sealed bag until you have left the school.

Show her the labeled values on the prizes and assist while she chooses items that total up to the amount of tokens she has (make sure not to impact her choice! Only give her help if she needs help calculating the total number of tokens-prizes she has). Then place her items in a non-see-through bag and tape it shut, recording what she has chosen.
Ask her the debriefing questions and record their responses.

R7. *What prizes were chosen?*_____
Carefully record the amount of tokens, the prizes chosen, and the token value of each prize.

Communication + Word Search Game: GUARDIAN protocol

Please follow the instructions on this sheet as you begin the experiment. The text in italics is instructions for you; the boxed should be read to the respondent, including any headings within the boxes. As you go through the experiment, please remember to:

- *Follow the instructions exactly as they are written*
- *Only go to the next step when the respondent understands the previous directions*
- *Avoid influencing the respondent's answer*
- *Where it says <girl> insert the girl respondent's name*

As you go through the activity, make sure you are recording the answers under the correct question numbers on your answer sheet.

(Check that the ID on the answer sheet matches the ID for the participant. Check the activity color code on the answer sheet and ensure it matches the color code of the protocol you are using. Check whether this is a communication activity or not. Label your keep and send envelopes with the ID number.)

***INTRODUCTION** [approach alongside girl experimenter and together greet both the guardian and girl with the text below]*

- Good afternoon, how are you? My name is_____ . Nice to meet you. Thank you for waiting.
- We are now ready to do the short activity with prizes that we mentioned before. I'll be doing the activity with you, while my colleague here will do the activity with <the girl>.
- I'll explain more once we go sit down together to do the activity.
- Please follow me.

Walk the Guardian past the prize table, let them look for 30 seconds from at least two feet away (ensure that the prices on the items are not on the table at this point), and tell them:

- Those are some of the prizes **<girl>** will be able to choose from at the end of the activity. We call this area the "store." **You** will get your prizes as **airtime**.

EXPLAIN THE ACTIVITY

- There are three possible activities that the guardians and girls may do as part of this survey. For each guardian-girl pair, one of these three

activities was chosen randomly, or by chance, for them to participate in. For you and <girl>, the red activity has been chosen.

- During this activity, you will have an opportunity to get some airtime. You will do the activity with <name of the girl>. We are going to ask you to make some decisions; <name of the girl> is also being asked to make some decisions. At the end, the amount you get will be determined by the decisions **you** made **and** the decisions **<girl>** made, in addition to a small amount of chance/luck.
- You and I are doing the activity in a different space than <girl> to give each of you privacy to make your decisions. Our discussion here is also private, meaning just between us. *[If spot-checker is present this day]* If another colleague of mine joins during our discussion, she is just coming to help me, and any discussions we have will also just be between the three of us.
- As I explain the steps of the activity, I will ask a few questions as we go along, just to make sure you are understanding. I will be reading the instructions from this script, so that I make sure everyone is told the instructions in the same way, and can make their decisions based on the same information. Are you ready to start?
- We will do the activity with these tokens. *[show the guardian a token]* At the end of the activity, each token can be traded in for 1 kwacha worth of prizes.
- You are starting this activity with 10 tokens *[show her the 10 physical tokens]*. <Girl> starts the activity with zero tokens.

- **Step 1: The first step of the activity is "Your choice"**
- You can choose how many tokens to keep for yourself and how many to send to <girl>, if any. The tokens you keep for yourself can be traded for airtime cards in the end *[show 10 kwacha in airtime, in 1 kwacha strips]*.
- The tokens you send MAY be doubled before they are given to <girl> so a token that you send to <girl> may become two tokens. *[show one token, then hold up a second token next to it as you say "may become two tokens"]* (I will explain how the tokens may be doubled in a moment.) The tokens you keep will not double, but rather each remain as one token.
- Later, <girl> will have a chance to send some tokens back, so this doubling may help you as well as her.

- It is completely up to you how many tokens to keep and how many to send. There is no way I will know what you sent, as you will put it in these envelopes that only have the "send," "keep" labels and a number on it. The "send" envelope will be passed on directly to my colleague who is working with <girl>.
- To review: What is the choice you make with your tokens in this first step? *(Choose how many to keep and how many to send)*

- **STEP 2: <Girl> does word search**
- To determine whether any tokens you may send will be doubled, <girl> will be asked to do a word search.
- The word search will involve searching for words in a puzzle, with a limited period of time. This word search will be at her grade level, so a typical girl in Grade 8 would find it challenging, but possible to find at least some of the words. Here is a sample of what the word search looks like. *(Show word search)*
- If she identifies at least half of the words in the word search correctly, the tokens you send will be doubled before they are given to her. If she does not identify half the words, she will still get any tokens you send, but they will not be doubled.
- Note she will receive any tokens you may send after she completes the word search, so she will not know how many tokens you sent before she does it. She will only know that she will double the number of tokens from you if she finds over half the words in the word search.
- To review: What happens if the girl finds at least half the words? *(the tokens I sent are doubled)* What happens if she does not find half the words? *(She still gets the tokens, but they are not doubled)*. Does she know how many tokens you sent before doing the word search? *(She does not know before, she finds out how many she got after)*

- **STEP 3: <Girl> Receives tokens**
- <Girl> will then receive the tokens you send, if any, and they will be either doubled or not, plus…
- In addition to those tokens you send, if any, the girl may receive up to 4 extra tokens. The exact amount added will depend on chance, so it is not the same for every girl, but it will not be more than 4. These "chance"

tokens will be added to the amount you send so that the girl does not know exactly how many tokens you chose to send to her.

- Note that <girl> does not start with any tokens herself. She gets the (possibly doubled) tokens you send, if any, and up to 4 extra tokens, depending on chance. Only the tokens you send can be doubled.
- To review: Does <girl> start with any tokens of her own? *(no)* Where does she get tokens? *(She can get them from me and the extra tokens, which are given based on chance)*

- **Step 4: <Girl>'s choice**
- Once she gets her tokens, <girl> can decide what to do with them.
- From her total number of tokens, she can choose a number of tokens to send back to you, if any
- She can spend her remaining tokens on prizes from the "store", which you saw on the table.
- This decision is completely up to her.
- To review: What can <girl> do with her tokens? *(Choose how many, if any, to send to me and choose how many, if any, to keep and spend in the store)*

- **Step 5: You receive tokens**
- We will then give you any tokens sent from <girl>.
- You will then take all your tokens, those you kept, if any, and those <girl> sent to you, if any, and be given airtime from your choice of network in exchange.
- To review: What tokens do you get at the end? *(Those I kept, those <girl> sent)* What do you get for these tokens? *(Airtime of my choice)*

UNDERSTANDING CHECK

- Great—let's go through an example to make sure we understand how the activity works. Remember the steps:

- Step 1: You can choose how many tokens to keep and how many to send to <girl>. Any tokens you send can be doubled.
- Step 2: She completes the word search. If she correctly finds half of the words in the word search, the tokens you sent will be doubled before they are given to her.
- Step 3: She gets any tokens from you, either doubled or not, and up to 4 extra tokens at the same time.
- Step 4: She can then choose how many tokens to keep, if any, **and how many tokens to send back to you**, if any.
- Step 5: You get the tokens you kept and any tokens sent to you by <girl>to spend on airtime.
- Here is a table showing how the tokens work. Let's go through some examples together, and then I'll ask you some questions.

Show the guardian the table on a separate sheet (with blanks). Go through 10-0, 7-3, and 2-8 with the guardian, like this "You start with 10 tokens. If you keep ___, and send ___, the girl will get ___+chance to keep or send IF she finds half the words in the word search, and ___+chance to keep or send if she does not find enough words. Then you will get airtime worth ___, the amount you kept, plus any amount the girl might choose to send back." Then let the guardian explain to you the blanks on the table. Be sure to probe, so if the guardian answers "she gets 2" ask, "plus what?" until the guardian says "2 plus chance", and for the amount the guardian gets, ensure they mention both the "keep" amount and the return from the girl.

		She gets to keep or send from:		
If you keep:	**And send:**	**If she finds enough words in the word search**	**If she does not find enough words**	**And you get airtime worth:**
10	**0**	**0 + chance**	**0 + chance**	**10 + ? from <girl>**
9	1	2 + chance	1 + chance	9 + ? from <girl>
8	2	4 + chance	2 + chance	8 + ? from <girl>
7	**3**	**6 + chance**	**3 + chance**	**7 + ? from <girl>**
6	4	8 + chance	4 + chance	6 + ? from <girl>
5	5	10 + chance	5 + chance	5 + ? from <girl>
4	6	12 + chance	6 + chance	4 + ? from <girl>
3	7	14 + chance	7 + chance	3 + ? from <girl>
2	**8**	**16 + chance**	**8 + chance**	**2 + ? from <girl>**
1	9	18 + chance	9 + chance	1 + ? from <girl>
0	10	20 + chance	10 + chance	0 + ? from <girl>

If they guess wrong, explain how that step works again, using the words from the protocol.

- Good. Now, to make sure you understand, could you please explain how the activity works to me in your own words?

(Probe to get them to explain the complete activity by saying "and then what happens?" etc. If they at any point show they have misunderstood something, take them back to the instructions for that step and read them again.)

THE ACTIVITY

- Now that you have fully understood the activity, you will have 5 minutes to discuss the activity with <girl> before you make your choices. Please speak only to <girl> rather than other parents or girls during this time.

Bring the girl and the guardian together, and let them discuss with you out of hearing distance. However, keep a bit of an eye on the pair to make sure they are speaking only to each other and not to others. When the 5 minutes are over, take the guardian back to sit down.

- Now let's start the activity. Here are your 10 tokens.
- Here are two envelopes. One labeled "Keep" and one labeled "Send" (*Point to which is which).* I will turn my back for one minute, and you can choose how many to put in each envelope. Remember, it is fine to put any number from 0 to 10 in either envelope. This is your choice. Once you have made your choices, please seal the "Send" envelope like this *[demonstrate sealing the envelope]* and give it to me.

Surveyor, turn around. After one minute, if the respondent has not already said they are finished, ask if they have finished, but stay with back turned. If they have not, ask, "Do you need me to explain any of the instructions more, or do you just need more time?" If they say yes or seem unsure, repeat the instructions on the four steps from the understanding check, and what to do with the envelopes from the box above, and give them another 1 minute.

- Thank you. I will go take this envelope to <girl>, and will return soon.

Bring the sealed envelope to the central meeting point and drop it off.

REASONS FOR CHOICE

- Thanks for waiting. While we wait to find out what choices are made by <the girl>, I would like to ask you a few questions so we can understand a bit more about how you made your choice.

- ***R1.*** How likely do you think it is that <girl> will correctly identify over half of the hidden words in the word search? [*Surveyors should just ask the question, wait for a response and then clarify with the closest options. If the response makes no sense, switch to reading out]*

a) She definitely won't
b) She probably won't
c) She probably will
d) She definitely will

- ***R1.5*** And may I ask what leads you to think that?

- ***R2.*** Of any tokens <name of the girl> received, how much do you think she will keep versus send back? [*Surveyors should just ask the question, wait for a response and then clarify with the closest options. If the response makes no sense, switch to reading out]*

a) She will keep all of it
b) She will keep most of it, but send me a bit
c) She will split it 50/50
d) She will send me most of it, and keep a bit
e) She will send me all of it

- ***R2.5.*** And may I ask what leads you to think that?
- These next questions are optional, as you may keep these things private if you like to. However, if you're willing to share, I'd like to hear a bit more about how you made your decisions.

- ***R3.*** Could you describe what things you thought about when deciding how many tokens to send and how many to keep? What things did you consider? *(please probe until they describe their reasoning)*
- ***R4.*** *(Ask each question, then pause and record, then ask the additional probes)* Did you speak to <girl> during the break? What was discussed? How did she approach you? What can you remember her saying? Did anything she said affect your decision?

RECEIVE TOKENS

- I will now go get your tokens from the girl, if any.

Retrieve the sealed envelope containing the girl's tokens. (If it takes extra time, go back to the guardian and say, "My apologies, they're not quite finished yet, so we have to wait a little longer." Then return to a neutral area and wait, rather than wait with the guardian (which might encourage them to chat). Count and record the amount.

- ***R5****. Tokens sent by girl =* _____

Go back to the guardian and hand over the envelope.

Thanks for waiting. Here are the tokens you received, which you can add to those you kept *[point towards their "keep" envelope]*. We have now finished the activity, so please take your envelopes of tokens up to the table in the front to collect your airtime from my colleague and a transport refund for coming today.

Either take the guardian to the checkout station, or point them toward it if it is very nearby. If you walk the guardian to the correct location, be sure to give the guardian some time alone before they check out if they would like to count their tokens or combine the two envelopes, so do not stand and wait with them until they checkout. (They know they need to check out to get their transport and airtime, so they won't accidentally leave or wander off!)

Thanks again, and have a nice day.

To be completed by check-out person

AIRTIME

- Hello, good afternoon. How many total token do you have? *[record number of total tokens]* What brand of airtime would you like for your tokens? *[record airtime given, by network brand and amount]* Here you go. Please keep these someplace private until after you leave the school, so that people who haven't done the activity yet don't find out about the activity before they do it.

Appendix H: Spillovers

While negotiation increased human capital for treated girls, 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). In this section, we first measure spillovers from girls to their siblings using parents' survey data from the follow-up survey. We then use both within- and across-school variation to measure spillovers from girls to their classmates.

Sibling Spillovers

To estimate spillovers from treated girls to their siblings, we regress measures of siblings' outcomes from the follow-up survey on whether a girl received the negotiation treatment. Appendix Table A18 reports these estimates. For columns 1 and 2, parents were asked how they would divide a fixed amount of resources between the treated girl, her closest age female sibling, and her closest age male sibling.[46] In column 1, the outcome is the parent's allocation to the male sibling, and in column 2, it is the allocation to the female sibling. In both cases, negotiation had no effect on the allocation of resources. 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 3-6 show that negotiation had no effect either on the time that siblings spent doing chores or the amount of time they spent doing schoolwork. For column 7, parents were also asked if, after the intervention, they were more likely to pay girls' school fees relative to boys. Here, 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 8 and 9 reveal that the negotiation treatment had no effect on the number of years parents expected the siblings to complete.

Since higher ability girls experienced larger benefits from negotiation training, we also allow spillovers to siblings to depend on whether the girl was in the top 40% or bottom 60% of the ability index. Appendix Table A19 reports these results, and we again find little evidence of spillovers. Overall, the results in both tables suggest that the negotiation program did not have strong negative spillovers on girls' siblings. 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. Thus, the results suggest that the benefits to treated girls do not come from reduced investment in their siblings.

[46] This was measured by giving parents 20 buttons representing their resources and asking them to divide up the items into 3 piles representing the girl, her nearest age sister, and her nearest age brother.

Classmate Spillovers

Classroom Variation. To measure spillovers to untreated girls within classrooms, we exploit the fact that there is random variation in the number of girls treated with negotiation in a classroom with the caveat that the amount of within-classroom variation is limited by our stratified randomization.[47] We start with this approach instead of the cross-school randomization because it allows us to compare girls' longer-term outcomes in a specification very similar to the ones used for our main results. While the treatment was stratified within classrooms and information treatment, tie-breaking in the treatment assignment means that the percent of treated girls in a classroom among study girls has some variation. Using our original sample of treated schools, we estimate within-classroom spillovers with the regression

$$y_{ics} = \beta_0 + \beta_1 negotiation + \beta_2 num\ neg_c + \mathbf{\Gamma X_{ic}} + \alpha_s + \epsilon_{ics},$$

where α_s is a school fixed effect, $num\ neg_c$ is the number of girls treated with negotiation in a classroom, and $\mathbf{X_{ic}}$ now includes an indicator variable for safe space and a control for the number of experimental girls in a class (since, otherwise, larger classes will be correlated with more girls being treated). Appendix Table A20 reports the estimates for the human capital measures using this equation. Appendix Table A21 replicates the main investment game table but now includes controls for $num\ neg_c$ and school instead of classroom fixed effects to estimate spillovers on the number of tokens parents sent in the game. In both cases, we find no evidence that untreated girls were affected by having more treated girls in their classroom.

School Variation. Our remaining two methods for identifying spillovers use across-school variation in whether a school was treated. First, to test if there are spillovers to male classmates, 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.[48] Column 1 of Appendix Table A22 reports the estimate from this regression. There is no evidence that attending a treatment school increased male dropout.

Next, we take advantage of the fact that 12 schools randomly did not receive any treatment initially, though the program was scaled up to girls in the pure control schools in the

[47] This approach is similar to the approach used by Miguel and Kremer (2004), who exploit random variation in the intensity of spillovers due to their randomized treatment to identify spillovers from deworming.

[48] 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.

last term of grade 9. Before measuring spillover effects using this cross-school randomization, we first verify that the characteristics of the schools and students are balanced. Appendix Table A23 examines whether the baseline characteristics of girls are balanced between the treated and control schools for the full sample of schools and finds that girls' characteristics do not jointly predict belonging to a treated school. Appendix Table A24 examines the balance on school characteristics provided in administrative data from the Zambian government. Though these data are incomplete, with missing information for several of the schools, we again see no evidence that the cross-school randomization is unbalanced. Thus, to maximize the sample size available to measure spillovers, we use the full sample of schools and students.

Having verified that the treatment assignment is balanced, to measure the spillovers using cross-school variation, we estimate

$$y_{is} = \beta_0 + \beta_1 Treated\ School_s + \beta_2 negotiation_i + \beta_3 safe\_space_i + \mathbf{\Gamma X_{is}} + \epsilon_{is}$$

where s denotes a school, $Treated\ School_s$ is an indicator variable equal 1 if a girl received the treatment, and $\mathbf{X_{is}}$ is our full set of socioeconomic controls. For y_{is}, we focus on whether a girl was enrolled in grade 9 during term 2. This is because the pure control schools received the program in term 3 of grade 9, making it the last enrollment measure prior to the scale-up, and following grade 9, we stopped collecting data from pure control participants due to financial constraints.[49] Standard errors are clustered at the school-level since this is the level of the treatment.

Columns 2-3 of Appendix Table A22 report the estimates for 9th grade (term 2) enrollment with the baseline and double lasso controls. We again find no evidence of spillovers. However, while the girls' baseline covariates do not jointly predict attending a treatment school, some characteristics are still imbalanced in Appendix Table A23. For robustness, we also report the effect of being in a treated school on 9th grade enrollment using estimates that stratify by propensity score. The propensity score is calculated using a logit regression for treatment status (reported in Appendix Table A25). The sample is restricted to observations with overlapping support in the propensity score. The data are divided into 5 blocks within which the socioeconomic covariates of girls are found to be balanced (at the 5% significance level),[50] and we include fixed effects for these blocks as controls in our regressions. The estimated effects with matching in the final column of Appendix Table A22 are nearly identical to those without, and we again find no evidence of spillovers.

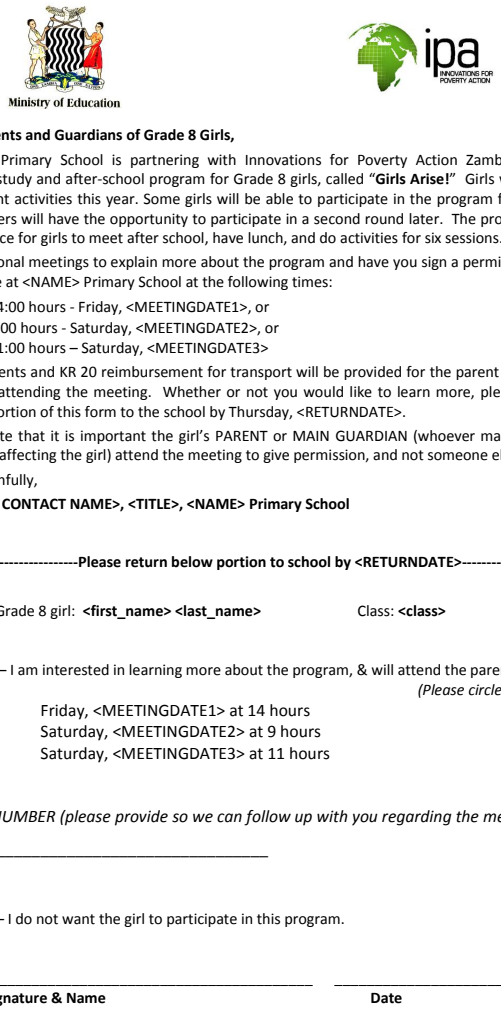
[49] In contrast, girls took the national exam after the scale-up of the program to pure control schools occurred, and most of the components of our human capital index include data from term 3 of grade 9.

[50] Balance estimates using the stratified propensity matching are reported in columns 3 and 4 of Appendix Table A23.

Finally, Appendix Table A26 estimates spillovers in the main investment game results using the cross-school randomization. As with the within-class randomization, we find no evidence of spillovers. Thus, across our sibling analysis, within-classroom analysis, and cross-school analysis, we do not find evidence of spillovers.

Appendix Figures

Figure A1: Invitation to Participate in the Experiment



Ministry of Education

ipa INNOVATIONS FOR POVERTY ACTION

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):

_____

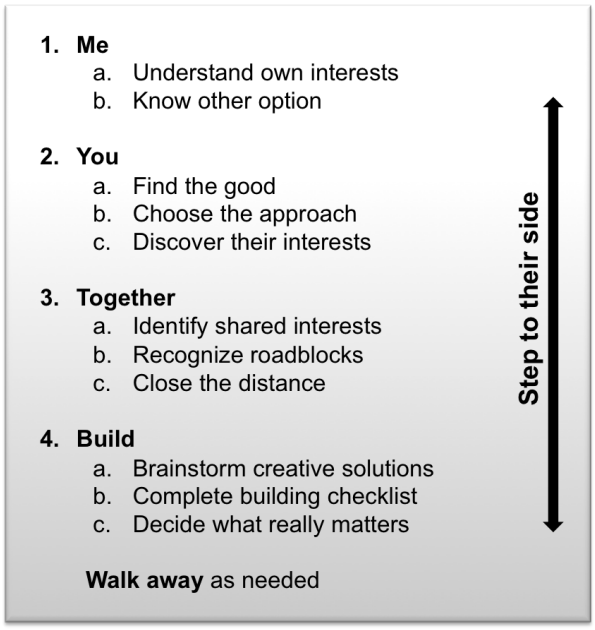
____ **NO** – I do not want the girl to participate in this program.

_____ _____

Parent Signature & Name **Date**

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

Figure A2: Negotiation Curriculum

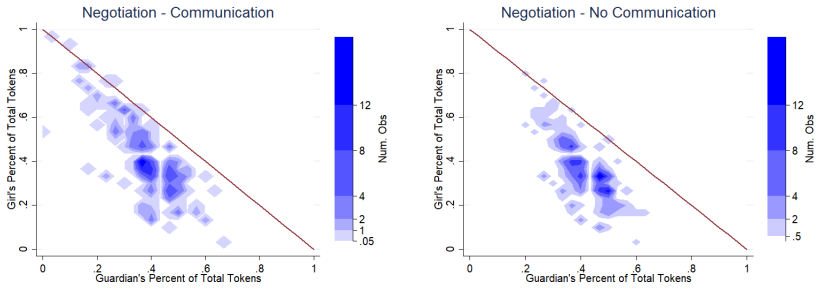


This figure shows the four key principles of the negotiation curriculum and their components.

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

The left two photos show the prizes that the girls could purchase with tokens from the "Chuck E. Cheese"-style store in the investment game. The right photo shows airtime, for which the parent exchanged her tokens at the end of the game.

Figure A4: Efficiency and Daughter Welfare for Negotiation Girls in the Investment Game With and Without Communication



This figure shows the outcomes of daughters and parents and the distance to the efficient frontier for negotiation girls in the investment game with and without communication. The x-axis is the share of the total *possible* tokens in the game a guardian ends the game with. The y-axis is the share of total possible tokens a daughter ends the game with. The red line plots the efficient frontier, showing the combinations of tokens that are efficient. More allocations near the red line indicates more efficient allocations. Allocations higher on the y-axis indicate allocations that are better for daughters.

Appendix Tables

Table A1: Attrition by Outcome

	(1) Control Mean	(2) Coef. Negotiation	(3) Standard Error	(4) Coef. Safe Space	(5) Standard Error	(6) Coef. Information	(7) Standard Error
9th Grade Enrollment	0.003	-0.003	0.002	0.001	0.002	-0.001	0.002
10th Grade Enrollment	0.000	0.000	0.000	0.000	0.000	0.000	0.000
11th Grade Enrollment	0.000	0.000	0.000	0.000	0.000	0.000	0.000
10th Grade Morning School	0.029	0.015	0.010	0.014	0.010	-0.003	0.009
11th Grade Morning School	0.035	0.002	0.009	0.011	0.009	0.008	0.008
Zero balance	0.132	0.002	0.006	0.005	0.005	-0.003	0.004
Took Exam	0.004	0.004	0.004	-0.000	0.003	-0.003	0.004
Math Score	0.004	0.004	0.004	-0.000	0.003	-0.003	0.004
English Score	0.004	0.004	0.004	-0.000	0.003	-0.003	0.004
Average Att. Rate	0.121	0.003	0.003	0.001	0.003	-0.000	0.001
Ever Pregnant	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Missing Baseline Data	0.046	-0.003	0.010	0.001	0.009	0.012	0.008

This table reports the level of attrition for each of the educational outcomes used in this paper, as well as tests of whether attrition is differential for the negotiation or safe space groups. Column 1 reports the level of attrition for the control group. The remaining columns report the coefficients and standard errors for negotiation, safe space, and information from a regression of an indicator variable for whether an outcome is missing on indicator variables for the three treatments and classroom fixed effects. Note that attrition for 10th and 11th grade is zero by construction, as all girls not found to be enrolled in Lusaka government schools were coded as not enrolled. Schooling type is only coded as missing if a girl is found to be enrolled but school type could not be verified. The final row reports the prevalence of missing baseline data and whether it is differential across treatments. Standard errors are clustered at the classroom level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A2: Comparison Between Intervention Schools and Other Zambian Schools

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Intervention Schools		Urban Gov. Schools			Full Gov. Sample			Full Sample		
	Mean	Standard Deviation	Mean	Standard Deviation	T-test (P-value)	Mean	Standard Deviation	T-test (P-value)	Mean	Standard Deviation	T-Test (P-value)
Number 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 Female Students	113.870	39.577	84.794	55.447	0.010**	33.802	35.901	0.000***	32.592	35.812	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***
Total Teachers	54.261	10.627	45.372	15.090	0.004***	18.030	15.555	0.000***	17.796	14.932	0.000***
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	0.000	0.002	0.007	0.032	0.337	0.031	0.087	0.087*	0.028	0.082	0.085*
Total Students	2,208.000	583.234	1,588.162	732.918	0.000***	753.515	550.107	0.000***	700.064	539.415	0.000***
STR	42.231	10.646	35.928	14.824	0.046**	50.207	23.301	0.115	47.645	27.117	0.302
Male Toilets/Students	0.006	0.002	0.007	0.007	0.637	0.008	0.007	0.378	0.010	0.013	0.187
Female Toilets/Students	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**
Total Classrooms	23.130	4.434	16.884	6.266	0.000***	8.985	5.940	0.000***	9.307	5.995	0.000***
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	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. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A3: The Effect of the Negotiation Treatment on Knowledge of Negotiation

	(1) **Question 1**	(2) **Question 2**	(3) **Question 3**	(4) **Combined Questions**
Negotiation	0.721***	0.819***	0.799***	0.799***
	(0.099)	(0.119)	(0.132)	(0.083)
Safe Space	-0.018	-0.120	-0.006	-0.031
	(0.081)	(0.110)	(0.128)	(0.075)
Negotiation vs. Safe Space (p-value)	0.000	0.000	0.000	0.000
Mean Dep. Var.	3.723	4.199	3.760	3.871
Number of observations	1,614	1,660	1,660	1,606
Adjusted R^2	0.063	0.079	0.058	0.116

This table reports the effect of the negotiation treatment on girls' understanding of negotiation skills in the follow-up 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 had to negotiate with her sister over who would watch their brother when she had 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 the stratification variables (classroom fixed effects and information treatment). Double lasso was used to choose additional controls among variables 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, reading and speaking English excellently and well, reading and speaking Nyanja excellently and well, age and age-squared, and ethnicity fixed effects. The row Negotiation vs. Safe Space reports the two-sided p-value from a test of the equality of the safe space and negotiation coefficients. The sample is restricted to participants with baseline data. Standard errors are clustered at the classroom level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A4: Evidence on the Use of Negotiation Skills from the Guardian

	(1) **Care for HH Mems**	(2) **Gives Advice**	(3) **Controls Neg. Emotions**	(4) **Pursues Self Interests**	(5) **Understands POV**	(6) **Being Respectful**	(7) **Negotiation Skills Index**
Negotiation	0.076*	0.021	0.080	0.016	0.055	0.103***	0.058**
	(0.040)	(0.042)	(0.051)	(0.041)	(0.047)	(0.038)	(0.025)
Safe Space	0.034	0.011	0.077	-0.058	-0.045	0.056	0.013
	(0.040)	(0.041)	(0.053)	(0.037)	(0.050)	(0.041)	(0.026)
Negotiation vs. Safe Space (p-value)	0.274	0.785	0.946	0.058	0.046	0.246	0.080
Mean Dep. Var.	3.314	3.180	2.864	3.043	3.272	3.454	3.187
Number of observations	1,565	1,557	1,564	1,564	1,565	1,565	1,555
Adjusted R^2	0.002	0.011	0.036	0.043	0.015	0.064	0.051

This table reports the effect of negotiation and safe space on the guardian's responses to a module designed to measure girls' use of negotiation skills at home. This module was administered during the follow-up survey. Each question asks the guardian to rate a girl on a 1-4 scale on how well she performs on each outcome. The final column (column 7) forms a negotiation skill index by taking a girl's average score across all the measures in the columns 1-6. All columns include controls for the stratification variables (classroom fixed effects and information treatment). Double lasso was used to choose additional controls among variables 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, reading and speaking English excellently and well, reading and speaking Nyanja excellently and well, age and age-squared, and ethnicity fixed effects. The row Negotiation vs. Safe Space reports the two-sided p-value from a test of the equality of the safe space and negotiation coefficients. The sample is restricted to participants with baseline data. Standard errors are clustered at the classroom level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A5: The Effects of Negotiation and Safe Space on Enrollment Including Individuals with Missing Baseline Data

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Enrolled in Government School						
	Grade 9	**Grade 9**	**Grade 10**	**Grade 10**	**Grade 11**	**Grade 11**
Negotiation	0.014	0.017	0.034	0.038*	0.036	0.039*
	(0.013)	(0.013)	(0.023)	(0.022)	(0.022)	(0.021)
Safe Space	0.014	0.014	0.024	0.025	0.022	0.022
	(0.014)	(0.013)	(0.024)	(0.024)	(0.026)	(0.026)
Controls	Baseline	Double Lasso	Baseline	Double Lasso	Baseline	Double Lasso
Mean of Dep. Var.	0.910	0.910	0.479	0.479	0.419	0.419
Neg. vs. SS. (p-value)	0.992	0.816	0.680	0.593	0.578	0.519
Number of observations	2,361	2,361	2,366	2,366	2,366	2,366
Adjusted R^2	0.009	0.021	0.067	0.097	0.063	0.089
Panel B: Enrolled in Morning School						
			Grade 10	**Grade 10**	**Grade 11**	**Grade 11**
Negotiation			0.021	0.027	0.033*	0.036**
			(0.019)	(0.019)	(0.019)	(0.018)
Safe Space			-0.005	-0.005	-0.002	-0.003
			(0.022)	(0.022)	(0.022)	(0.021)
Controls			Baseline	Double Lasso	Baseline	Double Lasso
Mean of Dep. Var			0.274	0.274	0.246	0.246
Neg. vs. SS. (p-value)			0.212	0.133	0.131	0.094
Number of observations			2,289	2,289	2,316	2,316
Adjusted R^2			0.081	0.112	0.066	0.100

Panel A reports the effect of the negotiation and safe space treatments on being enrolled in the indicated grade for the sample of treatment schools. Panel B reports the impact on being enrolled in morning school, the more academic track of the Zambian school system. Baseline controls consist of classroom fixed effects and an information treatment control, our stratification variables. The double lasso specification uses double lasso to choose additional controls among variables 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, reading and speaking English excellently and well, reading and speaking Nyanja excellently and well, age and age-squared, and ethnicity fixed effects. Controls are coded as 0 if baseline data is missing for an individual, and an indicator variable for missing baseline data is included in all the regressions. Standard errors are clustered at the class level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A6: IV Estimates of the Effects of Negotiation and Safe Space on Enrollment Including Individuals with Missing Baseline Data

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Enrolled in Government School						
	Grade 9	**Grade 9**	**Grade 10**	**Grade 10**	**Grade 11**	**Grade 11**
Attended Neg	0.019	0.022	0.044	0.050*	0.047	0.051*
	(0.017)	(0.017)	(0.030)	(0.029)	(0.029)	(0.028)
Attended SS	0.017	0.017	0.029	0.032	0.027	0.028
	(0.017)	(0.017)	(0.030)	(0.030)	(0.033)	(0.033)
Controls	Baseline	Double Lasso	Baseline	Double Lasso	Baseline	Double Lasso
Mean of Dep. Var.	0.910	0.910	0.479	0.479	0.419	0.419
Neg. vs. SS. (p-value)	0.944	0.763	0.630	0.536	0.532	0.469
Number of observations	2,361	2,361	2,366	2,366	2,366	2,366
Adjusted R^2	-0.000	0.012	0.003	0.034	0.003	0.030
Panel B: Enrolled in Morning School						
			Grade 10	**Grade 10**	**Grade 11**	**Grade 11**
Attended Neg Training			0.028	0.035	0.044*	0.048**
			(0.024)	(0.024)	(0.024)	(0.024)
Attended S.S. Training			-0.007	-0.006	-0.002	-0.003
			(0.027)	(0.027)	(0.027)	(0.027)
Controls			Baseline	Double Lasso	Baseline	Double Lasso
Mean of Dep. Var			0.274	0.274	0.246	0.246
Neg. vs. S.S. (p-value)			0.201	0.122	0.119	0.084
Number of observations			2,289	2,289	2,316	2,316
Adjusted R^2			-0.000	0.034	0.001	0.037

Panel A reports IV estimates of the effect of attending at least 5 out of the 6 days of negotiation and safe space trainings (the amount attended by the average participant) on being enrolled in the indicated grade for the sample of treatment schools. Panel B reports the impact on being enrolled in morning school, the more academic track of the Zambian school system. Baseline controls consist of classroom fixed effects and an information treatment control, our stratification variables. The double lasso specification uses double lasso to choose additional controls among variables 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, reading and speaking English excellently and well, reading and speaking Nyanja excellently and well, age and age-squared, and ethnicity fixed effects. Controls are coded as 0 if baseline data is missing for an individual, and an indicator variable for missing baseline data is included in all the regressions. Standard errors are clustered at the class level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A7: Hazard Model Estimates of the Effect of Negotiation and Safe Space on Dropout

	(1)	(2)
	Dropout	**Dropout**
Negotiation	0.908**	0.899**
	(0.043)	(0.043)
Safe Space	0.929	0.932
	(0.052)	(0.054)
Neg. vs. Safe Space (p-value)	0.695	0.540
Controls	Baseline	Double Lasso
Mean of Dep. Var.	0.589	0.589
Number of observations	9,720	9,720
Adjusted R^2	0.008	0.011

This table reports estimates of the effect of negotiation on dropout in Cox hazard regressions. Coefficients are reported as hazard ratios. All columns include controls for the stratification variables (classroom fixed effects and information treatment). Double lasso specifications also include additional controls selected by double lasso among variables 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, reading and speaking English excellently and well, reading and speaking Nyanja excellently and well, age and age-squared, and ethnicity fixed effects. The Neg. vs. Safe Space (p-value) row reports the 2-sided p-value from a F-test of the equality of the negotiation and safe space coefficients. The sample is restricted to participants with non-missing baseline data. Standard errors are clustered at the class level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A8: Comparing the Negotiation and Information Treatments' Effects on Human Capital Measures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
			Enrollment					Morning School				
	Grade 9	**Grade 9**	**Grade 10**	**Grade 10**	**Grade 11**	**Grade 11**	**Grade 10**	**Grade 10**	**Grade 11**	**Grade 11**	**Human Capital Index**	**Human Capital Index**
Negotiation	0.013	0.012	0.040*	0.019	0.044**	0.028	0.031	0.029	0.040**	0.040	0.045*	0.039
	(0.013)	(0.018)	(0.022)	(0.031)	(0.022)	(0.028)	(0.020)	(0.028)	(0.019)	(0.026)	(0.025)	(0.036)
Info. Treatment Assignment	0.013	0.013	-0.026	-0.040	-0.031	-0.042*	-0.009	-0.011	-0.015	-0.015	-0.010	-0.014
	(0.011)	(0.014)	(0.021)	(0.027)	(0.020)	(0.024)	(0.020)	(0.025)	(0.018)	(0.022)	(0.022)	(0.027)
Negotiation×Information		0.002		0.041		0.032		0.005		0.002		0.011
		(0.022)		(0.047)		(0.043)		(0.043)		(0.040)		(0.046)
Safe Space	0.011	0.011	0.029	0.029	0.029	0.029	-0.001	-0.001	-0.002	-0.002	0.014	0.014
	(0.014)	(0.014)	(0.025)	(0.025)	(0.027)	(0.027)	(0.023)	(0.023)	(0.022)	(0.022)	(0.026)	(0.026)
Neg. vs. Information (p-value)	0.981	0.970	0.028	0.050	0.010	0.016	0.152	0.161	0.046	0.050	0.130	0.162
Mean of Dep. Var.	0.912	0.912	0.486	0.486	0.424	0.424	0.279	0.279	0.251	0.251	-0.014	-0.014
Number of observations	2,239	2,239	2,244	2,244	2,244	2,244	2,170	2,170	2,196	2,196	2,174	2,174
Adjusted R^2	0.026	0.026	0.100	0.100	0.089	0.089	0.110	0.110	0.101	0.100	0.191	0.191

This table reports the effects of the information treatment and its interaction with negotiation on girls' human capital outcomes. All columns include controls for the information treatment and classroom fixed effects. Additional controls were selected by double lasso among variables 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, reading and speaking English excellently and well, reading and speaking Nyanja excellently and well, age and age-squared, and ethnicity fixed effects. The row Negotiation vs. Information reports the two-sided p-value from a test of the equality of the information and negotiation coefficients. The sample is restricted to individuals with non-missing baseline data. Standard errors are clustered at the classroom level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A9: Heterogeneity in the Negotiation and Safe Space Effects on Enrollment by Age and Parental Altruism

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Heterogeneity in Enrollment Effects by Parental Altruism						
	Grade 9	**Grade 9**	**Grade 10**	**Grade 10**	**Grade 11**	**Grade 11**
Negotiation×Low Altruism	0.015	0.017	0.062*	0.068*	0.075**	0.079**
	(0.022)	(0.022)	(0.036)	(0.034)	(0.036)	(0.034)
Negotiation×High Altruism	0.009	0.010	0.013	0.015	0.012	0.012
	(0.015)	(0.015)	(0.032)	(0.032)	(0.033)	(0.032)
Safe Space×Low Altruism	0.026	0.024	0.046	0.043	0.066*	0.064*
	(0.023)	(0.023)	(0.037)	(0.037)	(0.039)	(0.038)
Safe Space×High Altruism	-0.002	-0.001	0.012	0.016	-0.005	-0.002
	(0.017)	(0.017)	(0.033)	(0.032)	(0.036)	(0.036)
Controls	Baseline	Double Lasso	Baseline	Double Lasso	Baseline	Double Lasso
Neg.×Low = Neg.×High	0.816	0.799	0.333	0.286	0.232	0.183
Mean Dep. Var.	0.917	0.917	0.508	0.508	0.351	0.351
Number of observations	2,239	2,239	2,244	2,244	2,244	2,244
Adjusted R^2	0.018	0.025	0.077	0.099	0.072	0.089
Panel B: Heterogeneity in Enrollment Effects by Age						
	Grade 9	**Grade 9**	**Grade 10**	**Grade 10**	**Grade 11**	**Grade 11**
Negotiation×Low Age	-0.003	0.001	0.095*	0.100**	0.121**	0.125**
	(0.023)	(0.025)	(0.048)	(0.048)	(0.052)	(0.051)
Negotiation×High Age	0.016	0.017	0.015	0.021	0.013	0.017
	(0.018)	(0.018)	(0.028)	(0.028)	(0.028)	(0.027)
Safe Space×Low Age	-0.003	-0.002	-0.016	-0.016	0.010	0.007
	(0.023)	(0.024)	(0.052)	(0.053)	(0.057)	(0.057)
Safe Space×High Age	0.015	0.015	0.042	0.045*	0.034	0.037
	(0.017)	(0.017)	(0.027)	(0.027)	(0.029)	(0.029)
Controls	Baseline	Double Lasso	Baseline	Double Lasso	Baseline	Double Lasso
Neg.×Low = Neg.×High	0.551	0.640	0.183	0.181	0.087	0.085
Mean of Dep. Var.	0.917	0.917	0.508	0.508	0.351	0.351
Number of observations	2,239	2,239	2,244	2,244	2,244	2,244
Adjusted R^2	0.012	0.025	0.073	0.103	0.067	0.092

Panel A reports the heterogeneous effects of negotiation and safe space on girls with above and below median values of the altruism factor on being enrolled in the indicated grade. Panel B reports the heterogeneous effects of negotiation and safe space on girls with above and below median age on being enrolled in the indicated grade. Baseline controls consist of classroom fixed effects and an information treatment control, our stratification variables. In double lasso specifications, additional controls were selected by double lasso among variables 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, reading and speaking English excellently and well, reading and speaking Nyanja excellently and well, age and age-squared, and ethnicity fixed effects. The Neg.×Low = Neg.×High rows report the 2-sided p-value from a F-test of the equality of the negotiation coefficients for the two types of girls. The sample is restricted to participants with non-missing baseline data. Standard errors are clustered at the class level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A10: Association Between Girls' Characteristics and Appearing in the Follow-up

	(1)	(2)	(3)	(4)	(5)	(6)
	Likelihood in Investment Game					
	Coeff. Treatment	**Standard Error**	**Coeff. Negotiation×X**	**Standard Error**	**Coeff. X**	**Standard Error**
Negotiation	0.025	0.018				
Safe Space	0.016	0.019				
Both Parents Alive	0.040	0.026	-0.059	0.046	0.061**	0.031
Live With Bio Dad	0.045**	0.020	-0.075*	0.044	0.071***	0.025
Live With Bio Mom	0.099***	0.022	-0.045	0.045	0.114***	0.028
Live With Mom and Dad	0.076***	0.019	-0.080*	0.043	0.103***	0.024
Parents Pay Fees	0.025	0.025	-0.031	0.048	0.034	0.030
Read Nyanja Excellently	0.050**	0.020	-0.035	0.041	0.063**	0.025
Speak Nyanja Excellently	0.032	0.020	-0.030	0.038	0.044*	0.023
Read English Excellently	0.043*	0.023	0.022	0.038	0.036	0.026
Speak English Excellently	0.031	0.023	0.023	0.043	0.024	0.028
Age	-0.023***	0.008	0.001	0.013	-0.023***	0.008
Read Nyanja Well	0.057**	0.022	-0.003	0.042	0.058**	0.027
Speak Nyanja Well	0.026	0.030	0.027	0.058	0.017	0.037
Read English Well	0.067*	0.034	0.063	0.067	0.045	0.040
Speak English Well	0.042	0.028	0.064	0.049	0.021	0.034

This table reports the effect of negotiation, safe space, and negotiation's interactions with different baseline characteristics on a girl attending the investment game/follow-up survey. Each row reports the coefficients and standard errors from a regression, where the dependent variable is an indicator variable for attending the investment game. All regressions control for classroom fixed effects and the information treatment. The first two rows report the effect of negotiation and safe space on appearing in the game. The remaining rows regress appearing in the game on negotiation, a characteristic 'X' (given by the row name), and that characteristic's interaction with negotiation. The standard errors are clustered at the classroom level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A11: The Number of Girls in Each Treatment in the Investment Game

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

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

Table A12: Association Between Investment in the Lab-in-the-Field Investment Game and Human Capital Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
			Enrollment		**Morning School**	
	Human Capital Index	**Grade 9**	**Grade 10**	**Grade 11**	**Grade 10**	**Grade 11**
Tokens sent by guardian	0.029***	0.004	0.015**	0.013*	0.023***	0.020***
	(0.008)	(0.004)	(0.007)	(0.007)	(0.006)	(0.007)
Mean of Dep. Var.	-0.004	0.926	0.506	0.449	0.506	0.449
Number of observations	1,306	1,326	1,328	1,328	1,279	1,296
Adjusted R^2	0.008	-0.001	0.005	0.003	0.014	0.010

This table reports the relationship between the number of tokens parents sent girls in the investment game and different human capital measures. The sample is restricted to individuals who took part in the investment game and does not include those who took part in the dictator game. All columns include a control for whether the version of the investment game was the version with communication. Standard errors are clustered at the classroom level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A13: Effect of Knowledge of Negotiation on Parents' Behavior in the Investment Game

	(1)	(2)	(3)	(4)
	Dependent Variable: Tokens sent by parents			
	Comm. Version	**Pooled Version**	**No Comm. Version**	**DG Version**
Negotiation	0.318	-0.450**	-0.459**	0.509
	(0.195)	(0.173)	(0.193)	(0.325)
Safe Space	0.089	-0.363**	-0.344*	0.350
	(0.196)	(0.167)	(0.176)	(0.286)
Negotiation Knowledge	0.151***	-0.010	-0.020	-0.021
	(0.057)	(0.055)	(0.059)	(0.099)
Communication Dummy		-0.733**		
		(0.368)		
Knowledge×Comm.		0.174**		
		(0.078)		
Safe Space×Comm.		0.444		
		(0.273)		
Negotiation×Comm.		0.687**		
		(0.263)		
Mean of Dep. Var.	5.304	5.376	5.419	4.898
Number of observations	588	1,228	627	279
Adjusted R^2	0.123	0.061	0.034	0.059

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 and classroom fixed effects. Additional controls were selected by double lasso among variables 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, reading and speaking English excellently and well, reading and speaking Nyanja excellently and well, age and age-squared, and ethnicity fixed effects. The sample is restricted to individuals with non-missing baseline data. Standard errors are clustered at the classroom level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A14: How Girls Spent the Tokens

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent Variable: Amount of Tokens Spent on Category							
	Communication Version		Pooled Version		No Communication Version		DG Version	
	Non-Consumption	Consumption	Non-Consumption	Consumption	Non-Consumption	Consumption	Non-Consumption	Consumption
Negotiation	0.560**	-0.497*	-0.324	0.355	-0.337	0.382	-0.707	0.761
	(0.273)	(0.268)	(0.238)	(0.241)	(0.247)	(0.251)	(0.535)	(0.525)
Safe Space	0.227	-0.178	-0.202	0.248	-0.166	0.216	-0.970	0.936
	(0.321)	(0.320)	(0.228)	(0.235)	(0.240)	(0.249)	(0.659)	(0.663)
Negotiation×Comm.			0.843**	-0.870**				
			(0.357)	(0.364)				
Safe Space×Comm.			0.372	-0.416				
			(0.372)	(0.375)				
Neg. vs. Safe Space (p-value)	0.237	0.254	0.168	0.181	0.482	0.497	0.695	0.794
Mean of Dep. Var.	5.269	5.269	5.342	5.342	5.407	5.407	4.902	4.902
Number of observations	622	622	1,285	1,285	652	652	296	296
Adjusted R^2	0.359	0.362	0.333	0.391	0.298	0.440	0.347	0.342

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. Non-consumption spending is the sum of spending on school supplies and household items. School supplies are colored pens, math books, notebooks, pencils, erasers, rulers, and pencil sharpeners. Household items consist of socks and sanitary pads. Pure consumption is the total spending on hair ties, scarves, bracelets, lip balm, lollipops, biscuits, jiggies, and snakes and ladders games. Baseline controls consist of classroom fixed effects and an information treatment control, our stratification variables. Additional controls were selected by double lasso among variables 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, reading and speaking English excellently and well, reading and speaking Nyanja excellently and well, age and age-squared, and ethnicity fixed effects. The Neg. vs. Safe Space row reports two-sided p-values of tests of the equality of the safe space and negotiation coefficients for all columns except 3 and 4. In columns 3 and 4, it is the 2-sided p-value for a test of the equality of Negotiation×Comm. and Safe Space×Comm. Standard errors are clustered at the classroom level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A15: The Effect of the Negotiation Treatment on Daughters' Outcomes in the Investment Game

	(1)	(2)	(3)	(4)
	Dependent Variable: Daughters' Tokens			
	Comm. Version	**Pooled Version**	**No Comm. Version**	**DG Version**
Negotiation	0.649*	-0.457	-0.602*	1.156
	(0.357)	(0.309)	(0.332)	(0.832)
Safe Space	-0.066	-0.659**	-0.651**	0.678
	(0.338)	(0.297)	(0.303)	(0.751)
Negotiation×Comm.		1.085**		
		(0.442)		
Safe Space×Comm.		0.591		
		(0.474)		
Neg. vs. Safe Space (p-value)	0.064	0.324	0.874	0.497
Mean of Dep. Var.	8.841	8.716	8.552	12.762
Number of observations	621	1,283	651	321
Adjusted R^2	0.064	0.018	-0.007	0.103

This table reports the effects of the negotiation treatment on the total number of tokens daughters ended the game with in the 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. Baseline controls consist of classroom fixed effects and an information treatment control, our stratification variables. Additional controls were selected by double lasso among variables 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, reading and speaking English excellently and well, reading and speaking Nyanja excellently and well, age and age-squared, and ethnicity fixed effects. The Neg. vs. Safe Space row reports a two-sided p-value for a test of the equality of the negotiation and safe space coefficients in columns 1, 3, and 4. In column 2, it is a test of the equality of Negotiation×Comm. and Safe Space×Comm. The sample is restricted to participants with non-missing baseline data. Standard errors are clustered at the classroom level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A16: Effects of the Word Game Version of the Investment Game

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable: Tokens sent by parents					
	Comm. Version	**Comm. Version**	**Pooled Version**	**Pooled Version**	**No Comm. Version**	**No Comm. Version**
Negotiation	0.409**	0.477	-0.038	-0.041	-0.452**	-0.303
	(0.194)	(0.334)	(0.128)	(0.130)	(0.207)	(0.310)
Safe Space	0.081	0.252	-0.150	-0.153	-0.354*	-0.176
	(0.193)	(0.277)	(0.118)	(0.118)	(0.189)	(0.283)
Word Game Dummy	-0.227	-0.066	-0.051	-0.050	-0.068	0.171
	(0.219)	(0.353)	(0.142)	(0.143)	(0.162)	(0.293)
Negotiation×Word		-0.132				-0.307
		(0.494)				(0.420)
Safe Space×Word		-0.345				-0.369
		(0.432)				(0.412)
Word×Comm.			-0.149	-0.151		
			(0.250)	(0.250)		
Ability×Word				-0.003		
				(0.113)		
Mean of Dep. Var.	5.279	5.279	5.360	5.360	5.441	5.441
Neg. vs. S.S. (p-value)	0.168	0.479	0.430	0.430	0.614	0.646
Number of observations	630	630	1,290	1,290	660	660
Adjusted R^2	0.102	0.099	0.046	0.045	0.046	0.044

This table reports the effects of the word game version and its interactions with negotiation and safe space on parents' behavior in a lab-in-the-field investment game. In the investment game, 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. Baseline controls consist of classroom fixed effects and an information treatment control, our stratification variables. Additional controls were selected by double lasso among variables 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, reading and speaking English excellently and well, reading and speaking Nyanja excellently and well, age and age-squared, and ethnicity fixed effects. Ability is the first factor from a factor analysis of the speaking and reading variables. The sample is restricted to participants with non-missing baseline data. Standard errors are clustered at the classroom level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A17: Effects of Negotiation on Tokens Sent by the Guardian by Daughter's Ability

	(1)	(2)	(3)	(4)
Panel A: Effect in Investment Game with Communication and Comparison to Non-Communication Game				
	Dependent Variable: Tokens sent by parents			
Game Type:	**Comm. Investment**	**Comm. Investment**	**Pooled Investment**	**Pooled Investment**
Negotiation × High Ability	0.403	0.374	-0.633**	-0.629**
	(0.301)	(0.314)	(0.291)	(0.292)
Negotiation × Low Ability	0.405*	0.397	-0.397**	-0.415**
	(0.240)	(0.247)	(0.194)	(0.195)
Safe Space	0.104	0.093	-0.452***	-0.457***
	(0.202)	(0.206)	(0.169)	(0.170)
Negotiation × High Ability × Comm.			1.011***	1.019***
			(0.366)	(0.367)
Negotiation × Low Ability × Comm.			0.753**	0.770**
			(0.301)	(0.302)
Safe Space × Comm.			0.538*	0.553**
			(0.276)	(0.278)
Controls	Baseline	Double Lasso	Baseline	Double Lasso
High Ability Neg. vs. Low Ability Neg. (p-value)	0.996	0.952	0.533	0.546
Mean of Dep. Var.	5.279	5.279	5.360	5.360
Number of observations	630	630	1,290	1,290
Adjusted R^2	0.068	0.065	0.038	0.038
Panel B: Alternate Game Results to Isolate Mechanisms				
	Dependent Variable: Tokens sent by parents			
Game Type:	**No Comm. Investment**	**No Comm. Investment**	**Dictator**	**Dictator**
Negotiation × High Ability	-0.521	-0.542	1.140**	1.174**
	(0.335)	(0.347)	(0.463)	(0.465)
Negotiation × Low Ability	-0.415*	-0.413*	0.359	0.142
	(0.232)	(0.237)	(0.524)	(0.527)
Safe Space	-0.386**	-0.364*	0.477	0.424
	(0.182)	(0.190)	(0.357)	(0.347)
Controls	Baseline	Double Lasso	Baseline	Double Lasso
High Ability Neg. vs. Low Ability Neg. (p-value)	0.789	0.746	0.237	0.126
Mean of Dep. Var.	5.441	5.441	4.952	4.952
Number of observations	660	660	321	321
Adjusted R^2	0.030	0.044	0.066	0.100

This table reports the effects of the negotiation and treatments on parents' behavior in the lab-in-the-field investment game, allowing those effects to vary with ability. Ability is the first factor from a factor analysis of the baseline variables assessing reading and speaking skills in English and Nyanja. A girl is "low" ability if she is in the bottom 60% and high ability if she is the top 40%. These cut-offs were given by the machine learning exercise. The dependant variable is the number of tokens sent by parents out of their 10 tokens. Panel A, columns 1-2 use the sample that participated in the main game, the Investment Game with Communication. Columns 3-4 pool this game with the Investment Game with No Communication to isolate the effect of communication. Panel B, columns 1-2 restrict the sample to girls who participated in the Investment Game with No Communication, while columns 3-4 restrict the sample to those who participated in the Dictator Game. Baseline controls consist of classroom fixed effects and an information treatment control, our stratification variables. In the double lasso specification, additional controls were selected by double lasso among variables 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, reading and speaking English excellently and well, reading and speaking Nyanja excellently and well, age and age-squared, and ethnicity fixed effects. The High Ability Neg. vs. Low Ability Neg. row reports the 2-sided p-value from a F-test of the equality of Negotiation × High Ability and Negotiation × Low Ability, except in the case of columns 3-4 of Panel A. In these columns, it reports the p-value for a test of the equality of the terms Negotiation × High Ability × Comm. and Negotiation × Low Ability × Comm. The sample consists of participants with non-missing baseline data. Standard errors are clustered at the class level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A18: Evidence on Spillovers From the Follow-Up Survey

	(1) **Pile, Male**	(2) **Pile, Female**	(3) **Chores, Male**	(4) **Chores, Female**	(5) **Schoolwork, Male**	(6) **Schoolwork, Female**	(7) **Pay for Girls Rather than Boys**	(8) **Schooling Complete, Male**	(9) **Schooling Complete, Female**
Negotiation	0.033	0.092	-0.049	-0.027	0.031	0.120	0.036***	-0.006	0.025
	(0.170)	(0.164)	(0.110)	(0.060)	(0.090)	(0.079)	(0.012)	(0.117)	(0.112)
Safe Space	0.034	-0.077	0.042	0.036	-0.011	0.090	0.019	-0.043	0.020
	(0.151)	(0.166)	(0.114)	(0.063)	(0.085)	(0.087)	(0.012)	(0.130)	(0.118)
Mean of Dep. Var.	6.664	7.017	1.406	0.636	0.867	0.807	0.020	14.994	14.972
Number of observations	1,232	1,276	1,273	1,236	937	998	1,564	1,161	1,215
Adjusted R^2	0.014	-0.004	-0.014	0.014	0.033	0.034	0.014	0.051	0.045

This table reports the results of tests for spillovers on siblings from the negotiation program using data from the follow-up survey. 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 (column 2) siblings. In columns 3–6, parents were asked how much time the male and female siblings spent on chores and school work 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. Baseline controls consist of classroom fixed effects and an information treatment control, our stratification variables. Additional controls were selected by double lasso among variables 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, reading and speaking English excellently and well, reading and speaking Nyanja excellently and well, age and age-squared, and ethnicity fixed effects. The sample is restricted to participants with non-missing baseline data. Standard errors are clustered at the classroom level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A19: Evidence on Spillovers by Daughters' Ability

	(1) **Pile, Male**	(2) **Pile, Female**	(3) **Chores, Male**	(4) **Chores, Female**	(5) **Schoolwork, Male**	(6) **Schoolwork, Female**	(7) **Pay for Girls Rather than Boys**	(8) **Schooling Complete, Male**	(9) **Schooling Complete, Female**
Negotiation×High Ability	0.188	0.230	0.087	0.090	-0.056	0.035	0.057***	0.064	0.015
	(0.224)	(0.234)	(0.152)	(0.100)	(0.141)	(0.111)	(0.021)	(0.157)	(0.152)
Negotiation×Low Ability	-0.051	0.018	-0.121	-0.091	0.079	0.170*	0.025*	-0.043	0.031
	(0.186)	(0.171)	(0.130)	(0.066)	(0.099)	(0.094)	(0.013)	(0.139)	(0.127)
Safe Space	0.036	-0.076	0.043	0.038	-0.011	0.090	0.019	-0.043	0.020
	(0.143)	(0.166)	(0.114)	(0.063)	(0.084)	(0.087)	(0.012)	(0.130)	(0.118)
Mean of Dep. Var.	6.666	7.017	1.406	0.636	0.867	0.807	0.020	14.994	14.972
Number of observations	1,230	1,276	1,273	1,236	937	998	1,564	1,161	1,215
Adjusted R^2	0.011	-0.004	-0.013	0.015	0.033	0.034	0.015	0.050	0.044

This table reports the results of tests for spillovers by the treated girl's ability on siblings from the negotiation program using data from the follow-up survey. 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 3–6, parents were asked how much time the male and female siblings spent on chores and school work 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. Baseline controls consist of classroom fixed effects and an information treatment control, our stratification variables. Additional controls were selected by double lasso among variables 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, reading and speaking English excellently and well, reading and speaking Nyanja excellently and well, age and age-squared, and ethnicity fixed effects. The sample is restricted to participants with non-missing baseline data. Standard errors are clustered at the classroom level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A20: Within-School Estimates of Spillovers for Human Capital

	(1)	(2)	(3)	(4)	(5)	(6)
		Enrolled		**Morning School**		**Human Capital**
	Grade 9	**Grade 10**	**Grade 11**	**Grade 10**	**Grade 11**	**Index**
Negotiation	0.013	0.040*	0.043*	0.030	0.039**	0.045*
	(0.013)	(0.022)	(0.022)	(0.020)	(0.020)	(0.025)
Safe Space	0.010	0.030	0.030	0.000	-0.001	0.015
	(0.014)	(0.024)	(0.027)	(0.023)	(0.022)	(0.026)
Num. Negotiation Girls	0.001	-0.009	-0.011	-0.002	-0.006	0.025
	(0.007)	(0.012)	(0.014)	(0.012)	(0.012)	(0.022)
Number of observations	2,239	2,244	2,244	2,170	2,196	2,174
Adjusted R^2	0.020	0.084	0.077	0.087	0.086	0.140

This table reports estimates of the spillovers on human capital from the negotiation treatment using the within-school identification strategy. Each column regresses a human capital outcome on negotiation, safe space, the number of girls treated with negotiation in a classroom, and the number of girls in the experiment in the classroom. Baseline controls consist of classroom fixed effects and an information treatment control, our stratification variables. Additional controls were selected by double lasso among variables 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, reading and speaking English excellently and well, reading and speaking Nyanja excellently and well, age and age-squared, and ethnicity fixed effects. The sample is restricted to participants with non-missing baseline data. Standard errors are clustered at the classroom level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A21: Within-School Estimates of Spillovers in the Investment Game

	(1)	(2)	(3)	(4)
	Panel A: Tokens Sent in Investment Games With Communication			
	Dependent Variable: Tokens sent by parents			
	Comm. Version	**Comm. Version**	**Pooled Version**	**Pooled Version**
Negotiation	0.375**	0.400**	-0.446***	-0.405**
	(0.170)	(0.167)	(0.164)	(0.162)
Safe Space	0.099	0.120	-0.440***	-0.412**
	(0.176)	(0.169)	(0.156)	(0.161)
Num. Negotiation Girls	-0.088	-0.087	0.059	-0.010
	(0.087)	(0.085)	(0.092)	(0.035)
Comm.×Negotiation			0.760***	0.728***
			(0.237)	(0.230)
Comm.×Safe Space			0.482**	0.474**
			(0.236)	(0.234)
Comm.×Num. Negotiation Girls			-0.085	-0.013
			(0.157)	(0.114)
Controls	Baseline	Double Lasso	Baseline	Double Lasso
Number of observations	630	630	1,290	1,290
Clusters	118	118	123	123
Adjusted R^2	0.042	0.065	0.024	0.035
	Panel B: Tokens Sent in Investment Games Without Communication			
	Dependent Variable: Tokens sent by parents			
	No Comm. Version	**No Comm. Version**	**D.G. Version**	**D.G. Version**
Negotiation	-0.459***	-0.458**	0.282	0.267
	(0.169)	(0.179)	(0.299)	(0.283)
Safe Space	-0.446***	-0.434**	0.444	0.444*
	(0.159)	(0.166)	(0.271)	(0.267)
Num. Negotiation Girls	0.089	0.086	0.201	0.227
	(0.083)	(0.083)	(0.137)	(0.144)
Controls	Baseline	Double Lasso	Baseline	Double Lasso
Number of observations	660	660	321	321
Clusters	121	121	111	111
Adjusted R^2	0.009	0.012	0.016	0.014

This table reports estimates of the spillovers in the investment game from the negotiation treatment using the within-school identification strategy. The specifications are the same as in Table 5 except that they now also include controls for the number of negotiation girls in a classroom, the number of experimental girls in a classroom, and they replace classroom fixed effects with school fixed effects. The dependant variable is the number of tokens sent by parents out of their 10 tokens. Panel A, columns 1-2 use the sample that participated in the main game, the Investment Game with Communication. Columns 3-4 pool this game with the Investment Game with No Communication to isolate the effect of communication. Panel B, columns 1-2 restrict the sample to girls who participated in the Investment Game with No Communication, while columns 3-4 restrict the sample to those who participated in the Dictator Game. Baseline controls consist of classroom fixed effects and an information treatment control, our stratification variables. In double lasso specifications, additional controls were selected by double lasso among variables 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, reading and speaking English excellently and well, reading and speaking Nyanja excellently and well, age and age-squared, and ethnicity fixed effects. The sample is restricted to participants with non-missing baseline data. Standard errors are clustered at the class level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A22: Cross-School Estimates of Spillover Effects

	(1)	(2)	(3)	(4)
	Male Dropout Rate	**Dep. Var.: Enrolled in Grade 9, Term 2**		
				Propensity Score Matching
Treatment Year×Treated School	0.007			
	(0.018)			
Treatment School		0.001	-0.006	-0.005
		(0.016)	(0.014)	(0.014)
Negotiation		0.012	0.014	0.013
		(0.014)	(0.014)	(0.013)
Safe Space		0.013	0.012	0.010
		(0.015)	(0.014)	(0.014)
Controls	N.A.	Baseline	Double Lasso	Propensity Score
Observation	School-Year	Individual	Individual	Individual
Number of observations	26,301	2,959	2,959	2,959
Clusters	4,500	40	40	40
Adjusted R^2	0.098	-0.001	0.013	0.017

This table uses cross-school variation to measure spillovers in human capital. Column 1 reports the difference-in-differences estimate of the effect of being a treatment school in the treatment year on 9th grade male dropout. This regression controls for year and school fixed effects and uses school-year level administrative data provided by the Zambian government. Column 2 uses the experimental data and regresses an indicator variable for 9th grade enrollment (in term 2) on indicator variables for attending a treated school, negotiation, safe space, and information. In column 3, additional controls were selected by double lasso among variables 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, reading and speaking English excellently and well, reading and speaking Nyanja excellently and well, age and age-squared, and ethnicity fixed effects. Column 4 uses propensity score matching to match individuals in treatment and control schools and estimates spillover effects controlling for five propensity score strata fixed effects in addition to the controls selected by double lasso and information. The sample is restricted to participants with non-missing baseline data. Standard errors are clustered at the school level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A23: Balance of the Cross-School Randomization on Individual-Level Characteristics

	(1)	(2)	(3)	(4)
	Raw Balance		**Matched Balance**	
	Treated School Coef.	**Standard Error**	**Treated School Coef.**	**Standard Error**
Both Parents Alive	0.039*	0.022	0.021	0.023
Live With Bio Dad	0.049	0.033	0.024	0.033
Live With Bio Mom	0.065*	0.035	0.021	0.030
Live With Mom and Dad	0.057	0.035	0.029	0.034
Parents Pay Fees	0.044**	0.021	0.002	0.027
Read Nyanja Excellently	0.040	0.050	0.017	0.045
Speak Nyanja Excellently	0.006	0.057	0.019	0.056
Read English Excellently	0.037	0.049	0.013	0.042
Speak English Excellently	0.058	0.062	0.036	0.056
Read Nyanja Well	0.039	0.044	0.021	0.039
Speak Nyanja Well	-0.003	0.025	0.004	0.024
Read English Well	0.032	0.030	0.013	0.022
Speak English Well	0.054	0.053	0.021	0.042
Age	-0.197	0.139	-0.065	0.096
P-value (joint test)	0.684		0.960	

This table reports tests of the balance of the school-level assignment to treatment or pure control school using individual characteristics. The covariates were collected during the baseline survey from girls at the 41 schools, 29 of which were randomly assigned to be treatment schools. Columns 1 and 2 report the coefficient and standard error from a regression of the baseline characteristic in the row name on attending a treated school. Columns 3 and 4 report the same coefficient and standard error, but the regressions now include five fixed effects for propensity score strata. Propensity scores were estimated using a logit regression of attending a treatment school on the same set of covariates as in the table. The p-value for the joint test is obtained by regressing attending a treatment school on the full set of baseline variables and then using a F-test to jointly test if they are significant. Standard errors are clustered at the school level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A24: Balance of the Cross-School Randomization on School-Level Characteristics

	(1) **Treated School Coef.**	(2) **Standard Error**	(3) **Number of Observations**
Number Male Students	16.150	14.230	38
Number Female Students	-1.821	13.642	38
Special Ed	0.221	0.133	38
Total Teachers	-2.971	4.673	38
Female Drop Out Rate	-0.006	0.009	37
Male Drop Out Rate	-0.002	0.003	37
Total Students	-69.850	335.794	35
STR	0.865	4.691	35
Male Toilets/Students	-2.543	1.563	38
Female Toilets/Students	-0.604	2.171	39
Has Power	0.000	0.000	38
Has Protected Well	-0.100	0.097	38
Has Telephone	-0.064	0.186	38
Has Unprotected Well	0.000	0.000	38
Total Classrooms	-1.721	2.274	38
Regular Hours	-0.071	0.328	38
Library Books	-835.293	908.408	38
Joint Test (P-value)	0.584		

This table reports tests of the balance of the school-level assignment to treatment or pure control school using school-level characteristics reported in the Zambian school census. There are 41 schools, 29 of which were randomly assigned to be treatment schools. Columns 1 and 2 report the coefficient and standard error from a regression of the baseline characteristic in the row name on whether the school is a treatment school. An observation is at the school-level, so standard errors are heteroskedasticity robust. Since some of the variables are missing for each school, and we have a large number of covariates relative to the number of schools, including all the covariates in a regression together to arrive at the joint p-value is problematic. Instead, the joint p-value is the p-value of the average effect size of the effect of being a treated school across all of the covariates. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A25: Estimation of Propensity Scores

	(1) **Treated School**
Both parents are alive	0.045
	(0.133)
Lives with biological father	-0.053
	(0.195)
Lives with biological mother	0.254
	(0.231)
Living with both mom and dad	-0.024
	(0.314)
Parents pay fees	0.148
	(0.132)
Reads Nyanja excellently	0.097
	(0.167)
Speaks Nyanja excellently	-0.163
	(0.176)
Reads English excellently	-0.003
	(0.116)
Speak English excellently	0.054
	(0.196)
Age	-0.038
	(0.058)
Reads Nyanja above average	-0.008
	(0.138)
Speaks Nyanja above average	-0.124
	(0.187)
Reads English above average	0.135
	(0.171)
Speaks English above average	0.242
	(0.225)
Constant	1.246
	(1.065)
Number of observations	2,963
Adjusted R^2	0.009

This table reports the estimates used to compute the propensity scores for being in a treatment school. The coefficients are estimates from a logit regression of attending a treatment school on the baseline covariates. Standard errors are clustered at the school level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.

Table A26: Cross-School Estimates of Spillovers in the Investment Game

	(1)	(2)	(3)	(4)
Panel A: Tokens Sent in Investment Games With Communication				
	Dependent Variable: Tokens sent by parents			
	Comm. Version	**Comm. Version**	**Pooled Version**	**Pooled Version**
Negotiation	0.357*	0.384**	-0.452***	-0.446***
	(0.190)	(0.190)	(0.131)	(0.135)
Safe Space	0.101	0.143	-0.458***	-0.433***
	(0.151)	(0.154)	(0.151)	(0.153)
Treated School	-0.115	-0.157	0.194	0.164
	(0.200)	(0.191)	(0.186)	(0.168)
Negotiation × Comm.			0.808***	0.816***
			(0.261)	(0.265)
Safe Space × Comm.			0.559**	0.569**
			(0.226)	(0.225)
Comm.×Treated School			-0.162	-0.175
			(0.177)	(0.180)
Controls	Baseline	Double Lasso	Baseline	Double Lasso
Number of observations	828	828	1,697	1,697
Clusters	40	40	40	40
Adjusted R^2	0.000	0.022	0.010	0.028
Panel B: Tokens Sent in Investment Games Without Communication				
	Dependent Variable: Tokens sent by parents			
	No Comm. Version	**No Comm. Version**	**D.G. Version**	**D.G. Version**
Negotiation	-0.452***	-0.433***	0.235	0.235
	(0.131)	(0.132)	(0.300)	(0.295)
Safe Space	-0.458***	-0.441***	0.420	0.411
	(0.151)	(0.150)	(0.290)	(0.288)
Treated School	0.334*	0.307	-0.093	-0.114
	(0.196)	(0.187)	(0.362)	(0.363)
Controls	Baseline	Double Lasso	Baseline	Double Lasso
Number of observations	869	869	428	428
Clusters	40	40	40	40
Adjusted R^2	0.008	0.020	-0.000	-0.006

This table reports estimates of the spillovers in the investment game from the negotiation treatment using the cross-school identification strategy. The specifications are the same as in Table 5 except that they now also include a control for attending a treated school and they can no longer include classroom fixed effects. The dependant variable is the number of tokens sent by parents out of their 10 tokens. Panel A, columns 1-2 use the sample that participated in the main game, the Investment Game with Communication. Columns 3-4 pool this game with the Investment Game with No Communication to isolate the effect of communication. Panel B, columns 1-2 restrict the sample to girls who participated in the Investment Game with No Communication, while columns 3-4 restrict the sample to those who participated in the Dictator Game. Baseline controls consist of an information treatment control, the stratification variable that is not collinear with belonging to a treated school. In double lasso specifications, additional controls were selected by double lasso among variables 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, reading and speaking English excellently and well, reading and speaking Nyanja excellently and well, age and age-squared, and ethnicity fixed effects. The sample is restricted to participants with non-missing baseline data. Standard errors are clustered at the school level. * denotes 10% significance, ** denotes 5%, and *** denotes 1%.