Wheels of Change: Transforming Girls' Lives with Bicycles *

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February 24, 2024

Abstract

Motivated by the challenge of systemic barriers in girls'; education, such as distance and safety, our study in Zambia investigates the impact of bicycle distribution to school-going girls living over 3 km from their schools. In this randomized trial, we compared outcomes for girls receiving bicycles against a control group. Results, one year post-intervention, revealed a 35% reduction in commute time, 66% improvement in punctuality, and 27% decrease in absenteeism. Furthermore, within a year of the intervention, the program significantly enhanced empowerment outcomes, including elevated locus of control and aspirations. Additionally, the intervention notably increased school retention two to three years later. These findings underscore the effectiveness of bicycles as a scalable tool for overcoming educational barriers and empowering girls in similar settings.

Keywords: Bicycles, Commute time, Girls' Education, Female Empowerment, Safety, Zambia.
JEL Codes: H42, I21, I25, J16, O15.

*We are grateful for comments provided at various points during this study by Prashant Bharadwaj, Lasse Brune, Alex Cohen, Alexander Coutts, Manisha Desai, Taryn Dinkelman, Esther Duflo, Thomas Fujiwara, Siddharth George, Rachel Glennerster, Rema Hanna, Caroline M. Hoxby, Macartan Humphreys, Lakshmi Iyer, Tarun Jain, Matthew E. Kahn, Dean Karlan, Daniel Keniston, Gaurav Khanna, Asim Khwaja, Michael Kremer, Matt Lowe, Robert Metcalfe, Nirajana Mishra, Ahmed Mushfiq Mobarak, Dilip Mookherjee, Priya Mukherjee, Karthik Muralidharan, Nathan Nunn, Daniele Paserman, Gautam Rao, Deepak Saraswat, Alexandra Scacco, Frank Schilbach, Danila Serra, Christopher Udry, Anjali P. Verma and seminar and conference participants at COSME Gender Economics Workshop (2022), NBER's Education Meetings (2022), NBER's Children Program Meeting (2021), EEA-ESEM (2021), NEUDC (2021), Symposium on Economic Experiments in Developing Countries (2021), KDI School-World Bank DIME Conference on Impact Evaluation in Sustainable and Inclusive Development, 2021 Asia Impact Evaluation Conference, Bogota Experimental Economics Conference (2021), CSAE Conference (2019), 3^{rd} Annual Economics Conference (Ashoka University), 15^{th} Annual Conference on Economic Growth and Development, Universidad Autónoma de Madrid, RWI–Leibniz-Institute for Economic Research, Universidad del Rosario, Indian Institute of Management Ahmedabad, Indian Institute of Management Bangalore, University of Rochester, Boston University, Emory University, Harvard Kennedy School (WAPPP), University of Notre Dame, Reserve Bank of India, Louisiana State University, University of St. Andrews, University of South Florida, Shiv Nadar University, ifo Institute, University of Wisconsin-Madison, University of Wisconsin-La Crosse, International Food Policy Research Institute, and The World Bank. We thank Mahesh Dahal, Margarida Ortigão, and Olivia Siegl for excellent research assistance and the study participants for generously giving their time. We thank Emma Lambert-Porter, the field officers at Innovations for Poverty Action Zambia, and all the staff from World Bicycle Relief Zambia, especially Alisha Meyers, and Lawrence Banda. The study was preregistered with the AEA Trial Registry (RCT ID: AEARCTR-0003339). Funding for this study was provided by Innovations for Poverty Action, World Bicycle Relief, UBS Optimus Foundation, German Research Foundation, and the University of Connecticut. Any errors are entirely our own.

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"Even if my parents decide to use it [the bicycle], if they have not asked for permission from me first, they cannot use it." - **Girl from a treatment school**

1 Introduction

Despite considerable progress in closing the gender gap in education, there exist several barriers to human capital accumulation for girls in developing countries.[1] These barriers include distance to school (Muralidharan and Prakash, 2017; Tigre et al., 2017; Contreras et al., 2018; Ganimian and Murnane, 2016), existing facilities in schools (Adukia, 2017), safety and violence (Evans et al., 2023; Borker, 2020), lack of agency and autonomy (Dupas, 2011; Field and Ambrus, 2008; Jensen, 2012), and deep-rooted cultural norms that perpetuate gender inequality (Jayachandran, 2015). Addressing these gender-specific barriers to human capital accumulation is a major policy goal for developing countries due to their far-reaching implications on the well-being of women (Lundberg and Pollak, 1993; Duflo, 2012) and country's growth and development (UNDP, 2008; Antonopoulos, 2009).

In many developing countries, policymakers have identified school construction programs as a potential solution for improving access to education and reducing gender disparities (Burde and Linden, 2013; Kazianga et al., 2013). While this approach has the potential to improve education outcomes, it is expensive and time-consuming to implement. In this paper, we aim to examine the impact of a bicycle distribution program, an alternative approach that addresses the issue of access to schools by reducing the distance and safety costs associated with attending school, on the educational outcomes of adolescent girls in rural Zambia.[2]

In rural Zambia, nearly 98% of girls in our sample walk to school, spending approximately 110 minutes commuting one way, with 35% reporting experiencing sexual harassment during their commute.[3] Previous non-experimental research has shown that distance to school negatively impacts girls' enrollment (Muralidharan and Prakash, 2017; Hawke, 2015), while lack of safety hinders women's human capital attainment (Borker, 2020; Evans et al., 2021b), and personal safety concerns have long-term effects on women's employment choices.[4] Furthermore, walking long distances to school can affect

[1] Women have more education today than fifty years ago in every country the world (Barro and Lee, 2013), however, adult women still have less education than men in more than two-thirds of the world's countries (Evans et al., 2021a).

[2] Other programs have tried to address this problem by providing other types of transportation like buses, constructing roads, boarding opportunities in existing schools, etc.

[3] Although long distances to school is a problem for everyone, it is especially critical for adolescent girls who reach puberty around this age and face additional risks, including the danger of being assaulted on the way to school (Hawke, 2015).

[4] Women are willing to receive a lower payment in an experiment that involves a potentially dangerous journey to the lab (Becerra and Guerra, 2021) or working fewer hours after working hours in academia (Trawalter et al., 2020).

learning for those girls enrolled through its impact on student absenteeism and punctuality.[5]

In this paper, we conduct a randomized controlled trial in the Southwest province of Zambia to test the efficacy of a bicycle distribution program that directly addresses the issue of girls' access to schools. This study aimed to estimate the impact of reducing the distance and safety costs associated with attending school on the *educational* and *empowerment* outcomes[6] of adolescent girls. First, by decreasing the daily distance cost of school attendance via a reduction in commuting time, we expect an impact on access to education.[7] Further, we expect the reduction in commuting time to also make it relatively safer to travel to school, which could further improve girls' access to education and empowerment outcomes.[8] Second, we expect the provision of bicycles – owning a relatively valuable asset – to have an impact on empowerment outcomes for girls. Scholars of the women's suffrage movement, including Susan B. Anthony in the United States, have highlighted the role that bicycles played in empowering women in the 19^{th} century by enhancing their independence, control, self-reliance, self-respect, mobility, and freedom (Macy, 2011). There is also a sizable literature that suggests that providing assets to adolescent girls improves their empowerment outcomes through increased self-confidence, raising their aspirations, and increasing their autonomy over important life decisions.[9] And, finally, we expect improvements in education outcomes and female empowerment to bolster each other (Samarakoon and Parinduri, 2015; Cannonier and Mocan, 2018; Kaffenberger et al., 2018; Ashraf et al., 2020; Kjelsrud et al., 2021).

We implemented this randomized controlled trial in 100 schools in collaboration with the World Bicycle Relief (WBR) and the Ministry of General Education in Zambia. WBR provided bicycles to adolescent girls in grades 5, 6, and 7 who live more than 3 kms away from school. The bicycle was given on the condition that it be used primarily for attending school.[10] We randomly divided the schools into two treatment groups and a control group. The two treatment groups differ in the obligation of the parents/guardians to pay a small upfront payment at the time of receiving the bicycle. In the

[5]National Assessment results in Zambia show that the longer the distance pupils traveled to school, the lower their learning achievement (UNICEF, 2014).

[6]To measure *empowerment*, we collect measures of bargaining power, aspirations, preferences of fertility, locus of control, self-image, prosociality, and mobility.

[7]Muralidharan and Prakash (2017) study a state-wide cycling program in the Indian state of Bihar and find that the policy increased girls' enrollment in secondary schools by 32% and reduced the gender gap by 40%. The study also finds an 18% increase in the number of girls who appear for the high-stakes secondary school certificate exam and a 12% increase in the number of girls who pass the exam.

[8]Borker (2020) finds that women in Delhi, India choose worse education outcomes for themselves in order to avoid street harassment and that they are willing to incur an additional expense of USD 310 per year to travel by a route that is one standard deviation safer.

[9]See Dickson and Bangpan (2012) for a review of the papers examining the impact of providing economic assets to young girls in low- and lower-middle-income countries on their psychological well-being.

[10]This is enforced by a Bicycle Supervisory Committee (BSC), which includes members of the community and the school, using their own set of rules.

'Payment Arm' (25 schools), the parents pay a small upfront amount ($\approx$ \$5)[11] to cover replacement parts, while in the 'No Payment Arm' (20 schools), parents pay nothing (\$0) to cover replacement parts. Across the two treatment groups, 2,471 girls received the bicycles, with compliance of 99.99%.[12] There was no differential take-up by the two treatment groups. The remaining 55 schools are in the control group.

Our motivation for having two treatment arms stems from a desire to understand optimal pricing of goods with large spillovers, which is of considerable policy interest, as prices affect not just the demand for goods, but also their utilization (Morgan, 2010). Proponents of the "cost-sharing" method argue that usage intensity will be higher for the group sharing costs of the intervention, as charging positive prices can have a psychological effect on consumers, inducing them to commit the "sunk cost fallacy" (Arkes and Blumer, 1985; Thaler, 1980).[13] Thus, charging a small upfront cost in the 'Payment Arm' could induce parents to encourage their girls to use the bicycle more. Alternatively, in this context, a signal of parents' willingness to invest in girls' education, which could independently improve the girls' self-worth and related empowerment outcomes. On the other hand, charging a "zero price" and taking away the monetary aspect of the transaction can induce feelings of social and moral relationship, thereby reducing improper usage of the product (Ariely et al., 2018). Specifically, it is likely to reduce occurrences of misuse of bicycles by the girls' parents, making the ownership of bicycles more salient for the girls in the 'No Payment Arm'. As such, conceptually, it is not obvious if the impact of the program will be larger for the 'Payment Arm' or the 'No Payment Arm'.

The intervention had an impact on several outcomes that were *pre-registered* at AEARCTR-0003339 (details in Section 4.4.4). First, we measure the impact of the intervention on four first-stage outcomes, those that relate to the reduction of distance cost of school attendance, both in terms of distance and safety concerns. One year after the intervention, the girls in the pooled treatment group ('Payment Arm' + 'No Payment Arm') were 88% more likely to have access to a bicycle vis-à-vis girls in the control group. The girls' commuting time to school decreased by about 35 minutes one way, which translates to a 34% decrease from the baseline commuting time.[14] The intervention improved the perception of safety for the girls in the treatment schools by 0.11 standard deviation ($s.d$) and improved self-reported measures of safety. In particular, the intervention reduced the probability of girls being teased or

[11] This is about 6.5% of the average rural monthly household expenditure in the Southern Province (CSO, 1991-2017).

[12] Only 1 girl out of the 2,471 selected refused the cycle as reported by the World Bicycle Relief.

[13] There are also additional selection effects of charging positive prices, which can increase usage intensity by screening individuals with the greatest propensity to consume (Oster, 1995; Ashraf et al., 2010), but can also dampen demand and reduce program coverage substantially (Cohen et al., 2010). These effects are not applicable in our context since take up is 100% in all treatment arms.

[14] This is a self-reported measure asking the girls how much time it took them to get to school in the previous week.

whistled at on the way to school by 22%, and the probability that a girl missed school or left for home early from school due to concerns of safety by 39%.

Second, we find that the provision of bicycles led to an increase in empowerment outcomes for the girls in the treatment schools. Overall, the index of empowerment improved by 0.12 *s.d* for girls in the pooled treatment group compared to those in the control group. Specifically, the intervention improved the girls' locus of control, bargaining, pro-sociality, and self-image, but did *not* have a significant impact on their mobility, aspirations, or desired fertility. We find that the index of locus of control increased by 0.18 *s.d*, bargaining by 0.21 *s.d*, and pro-sociality by 0.15 *s.d* for girls in the treatment schools for the pooled treatment ('Payment Arm' + 'No Payment Arm'). Furthermore, the intervention improved the index of aspirations by 0.12 *s.d*, and desired fertility and age of marriage by 0.18 *s.d* for the girls in the 'Payment Arm' but *not* for those in the 'No Payment Arm'.

Third, the intervention improved several measures of educational outcomes for girls in the treatment schools. We find that the intervention reduced overall self-reported absenteeism in the previous week by 29%, which translates to an addition of about 5 school days per academic year. Furthermore, the intervention also reduced the self-reported number of days the girls arrived late to school in the previous week by 1.45 days, which translates to a 66% reduction vis-à-vis the girls in the control group. Finally, we find that the intervention improved Mathematics test scores by 0.11 *s.d*, but had no impact on English test scores.[15] The effect size on Mathematics test scores is consistent with the literature on conditional and unconditional cash transfer, which concludes that the effects of these interventions on student achievement are small at best (Baird et al., 2013).[16]

Fourth, we collected administrative data on attendance, dropouts, and grade transitions two, three, and four years after the distribution of the bicycles, i.e., in 2019, and when the schools reopened after COVID-19 in 2020 and 2021. We find the impact on school attendance to persist several years after the intervention. Although we did not find an impact on dropouts and grade transition one year after the intervention in 2018 as measured at the endline (the mean for dropouts was 6% and for grade transition 95% in the control group), we find a 17% decline in dropouts in 2021 (37% in 2019, 21% in 2020), and a 19% increase in grade-transition in 2021, using administrative data from the schools. These impacts are not only sizeable but especially important from a policy standpoint due to the overall worsening

[15] Although moving test scores is non-trivial in the education literature, the strong association of the intervention with Mathematics test scores is consistent with the literature that finds Mathematics achievement to be more responsive to interventions changing curriculum or instruction time (see Cronin et al. (2005)).

[16] The meta-analysis by Baird et al. (2013) suggests pooled effect sizes in the range of 0.04–0.08 standard deviations, respectively, for Unconditional Cash Transfer and Conditional Cash Transfer interventions.

impact of COVID-19 on human capital accumulation.

Fifth, we collected data on time use and found that the intervention led to a decrease in time spent on income-generating activities for the girls in the treatment schools. We believe this is driven by the positive income effects a bicycle generates for the family (e.g. parents can use the bicycle to sell milk on weekends) and/or the change in value a family places on education vis-à-vis engaging in short-term income-generating activities.

Finally, we examine the heterogeneity in the impact of the program by baseline time taken to travel to school. Results suggest that the intervention relaxed the distance constraints for girls living further away from school,[17] who experience a greater reduction in time taken to travel to school and in the number of days they arrive late to a school than girls in the control group. However, it is the girls in the middle tercile who experience the greatest reduction in absenteeism. Overall, these results suggest that the distance costs are still large for girls living the furthest from the school or that these girls face additional constraints.

This paper contributes to the broader literature that examines the impact of access to school on girls' educational outcomes. A standard policy response to address the problem of school access has been to construct schools. Several studies have shown the positive impact of school construction programs on enrollment and completion (Birdsall et al., 1985; DeTray, 1988; Lillard and Willis, 1994; Lavy, 1996; Duflo, 2001; Burde and Linden, 2013; Kazianga et al., 2013; Azam and Saing, 2017; Khanna, 2023). More narrowly, we contribute to the handful of papers that estimates the impact of commute time on educational outcomes. Contreras et al. (2018) estimates the impact of commute time on student achievement in Chile and finds that it negatively affects student achievement, however, the impact is not always significant. Similarly, Tigre et al. (2017) uses Brazilian data to estimate the impact of commute duration on students' performance and finds a negative effect on academic achievement. However, we are not aware of any study in a low-income setting that shows that experimentally varying commuting costs affect educational outcomes.[18] Although Muralidharan and Prakash (2017) studies the impact of a cycle program in India on girl's enrollment and school completion; and Kjelsrud et al. (2021) studies the impact of the same program on the agency of non-targeted females, this paper verifies through a proof of concept that difficulties of commuting to school impact girls' empowerment,

[17] This includes girls in the middle and top tercile in terms of distance from school.

[18] In a related paper Asahi (2014) finds a large negative effect of school-subway distance reduction on Mathematics test scores in Santiago (Chile). In a different context, Koolwal and van de Walle (2013) estimates the role of reducing women's time in water collection on their participation in market-based income-earning activities and does not find any evidence of improvement on off-farm work for women. However, the paper finds that in countries with large gender gaps in schooling, a reduction in the time needed to collect water improved both boys' and girls' enrollments.

safety, and education outcomes. Furthermore, the studies in India use the differences-in-differences approach instead of the random variation this paper exploits.

Second, we contribute to the literature aimed at finding solutions to empower adolescent girls in low-income settings. While much of the literature has focused on the adult female population, only a handful of papers have aimed at improving the empowerment of adolescent girls.[19] Some of these interventions have looked at the impact of relaxing women's human capital constraints in Uganda (Bandiera et al., 2020), teaching adolescent girls with negotiating skills training in Zambia (Ashraf et al., 2020), teaching life skills to girls in India (Edmonds et al., 2021), or effect of role models (La Ferrara et al., 2012; Riley, forthcoming) on measures of economic empowerment, fertility, education, and labor force participation. Although the above-mentioned studies attempt to directly change women's empowerment, ours does so in a less-salient way through an improvement in education, locus of control, bargaining, pro-sociality, and self-image. We further contribute to a small set of papers that show the impact of safety on women's behavioral response (Keane, 1998), human capital accumulation (Borker, 2020; Evans et al., 2021b), and mobility patterns (Hsu, 2011; Porter et al., 2011). More specifically, we are not aware of any intervention that studies the impact of providing an asset, such as a bicycle, to adolescent girls in a resource-constrained environment on female empowerment and safety.

Third, we contribute to the large number of papers studying the impact of conditional transfers on schooling outcomes. The majority of the well-identified studies find a positive impact on girls' enrollment and attainment (Fiszbein and Schady, 2009; Barrera-Osorio et al., 2011; Baird et al., 2011; Filmer and Schady, 2011; Heath and Mobarak, 2015; Chaudhury and Parajuli, 2010); however, the evidence of impact on test scores is weak (Baird et al., 2013).[20] Taken together, our findings suggest that policies such as providing bicycles that reduce the daily commute time to attending school can be a viable short-to-medium run solution, especially when the trade-off between school access and scale is of first-order concern. The heterogeneity results based on distance suggest that these types of programs will benefit more those girls that are far, but not too far from school, so might not be effective for those girls for which distance is too large to cover with a bicycle.

Finally, this paper also contributes to the growing debate on external validity around experimen-

[19] Heckman and Mosso (2014) argues that interventions targeting adolescent girls are likely to have higher returns than later-timed interventions.

[20] See Fiszbein and Schady (2009); Rawlings and Rubio (2005) for a review of this literature, and Baird et al. (2013) for a review on the relative effectiveness of conditional and unconditional cash transfers for schooling outcomes in developing countries.

tal and non-experimental studies (Dehejia et al., 2019; Gechter, 2015; Vivalt, 2019; Kowalski, 2019). Although Muralidharan and Prakash (2017), which was a non-experimental study of a large-scale cycling program for adolescent girls in India, only studies the impact on enrollment and school completion, the similarity of results for schooling outcomes suggests that identifying underlying mechanisms can play an important role in understanding the challenges around replicability and external validity in international development.

2 Context

Zambia is a landlocked country and home to over 17 million people. The national education system in Zambia is divided into the following levels of education. Primary education begins at age 7 and has a duration of 7 years [divided into lower basic grades (1-4) and middle basic grades (5-7)]. The entry age for lower secondary education (upper basic education) is 14 years, and it lasts 2 years. Upper secondary education begins at the age of 16 years and lasts 3 years. Finally, tertiary or post-secondary education begins at age 19.

Although Zambia has made remarkable progress in improving access and equity in education, and provides close to universal education at the primary level, with a gross enrollment ratio (GER) of 108% in 2013 (UNESCO, 2016), there exist considerable disparities in terms of absenteeism, dropout, and performance for girls at above primary level (Mwanza, 2015). For example, Zambia's GER for girls drops to 61% in the lower secondary level, and, starting in grade 6, significantly more girls leave school than boys (Bank, 2015). Student performance is among the lowest in the region according to the 2007 Southern Africa Consortium for Monitoring Educational Quality (SACMEQ). Transitioning to and completing secondary schooling is challenging for girls due to several barriers including, school fees, opportunity costs, and most importantly, long distances to school. The transition also coincides with a lack of menstrual hygiene facilities in the schools, parents underestimating the returns from secondary education, teenage pregnancy, and child marriage (UNICEF, 2014).

Overall, despite striking progress in increasing overall enrollment, Zambia needs to reduce gender gaps in absenteeism, dropout, and educational attainment, for adolescents, especially at the above primary level. This will require addressing both the supply of and demand for education constraints. In this study, we address both constraints. We partnered with World Bicycle Relief (WBR) and the Ministry of General Education in Zambia to test the impact of a program that provided a bicycle to

a school-going girl on measures of education and empowerment. This intervention mimics a conditional in-kind transfer program and has features of both demand and supply-side interventions (Muralidharan and Prakash, 2017). WBR provided bicycles if a student was enrolled, which is similar to demand-side conditional cash transfer programs, but access to bicycles reduces the daily distance cost and improves the safety of girls while going to school, which is similar to supply-side interventions.

3 Design and Methods

3.1 Treatment Arms

This experiment is a multi-treatment design with 100 schools randomly allocated to one of two treatment groups or a control group.

3.1.1 Payment Arm (T1)

The first treatment used the same model as the Bicycle Education and Empowerment Program (BEEP) that WBR has rolled out in 19 districts in Zambia since 2009.[21] In BEEP (or Payment Arm), students received a bicycle on the condition that the bicycle is used primarily to travel to school.[22] Furthermore, a contract signed between WBR and the student prevents the sale of the bicycle for four years after it is signed. A Bicycle Supervisory Committee (BSC) was in charge of monitoring the program at the school level. Furthermore, a field mechanic was trained for each school, who provided maintenance checks, repairs, and cover replacement parts for a small fee (roughly 50 Kwacha ≈$5) that was borne by the beneficiary students' families. Most importantly, this fee also covered the spare parts kit for the bicycles. We randomly selected 25 schools for the Payment Arm.

3.1.2 No Payment Arm (T2)

The second treatment is a slight modification of the BEEP (or Payment Arm) intervention. Similar to the Payment Arm, students still received a bicycle on the condition that the bicycle is used primarily to travel to school, a BSC was formed, and a field mechanic was trained for each school, who provided

[21] World Bicycle Relief, an organization that has worked in Zambia since 2009, and distributed over 183 thousand bicycles worldwide.

[22] The contract that World Bicycle Relief signs with the student stipulate that "As custodian of this bicycle, I agree that this bicycle shall be used primarily for the purpose of improving access to education: students and teachers to attend school faithfully and timely; teachers to access pupils not attending school and professional district resources; and community supporters to support the efforts of the school to improve educational outcomes".

maintenance checks, repairs, and cover replacement parts. However, *no fee* was charged from the beneficiary students' families. We randomly selected 20 schools for the No Payment Arm.

3.1.3 Control Group

Students in the control group did not receive bicycles. We randomly selected 55 schools for the control group.

3.2 Sample Selection, Randomization, and Timeline

World Bicycle Relief conducted an initial needs assessment in several districts in Zambia to identify three districts where students walked long distances to school and where the program was not already being implemented. The three districts were Monze, Mazabuka, and Kalomo, in the Southern province. This province is characterized by low population density and rural settlements, which makes distance a barrier to accessing basic services.

Within the three districts, a total of 100 schools were randomly selected from all public schools that met the following criteria.[23] First, each school had at least 20 girls enrolled in grades 5, 6, and 7, who walk more than 3 kms to school. Second, schools are basic schools, i.e., their starting grade is 1 or lower and their last grade is beyond grade 7 (end of primary) up to grade 9 (last grade before secondary education). WBR worked with the schools to compile a list of students in grades 5, 6, and 7 who walked more than 3 kms to school. Teachers generally knew the distance students walk and were able to provide accurate information on where girls lived. We also confirmed the distance from the school during the baseline survey. From the list of eligible girls in each school, we randomly selected 25 of them to be part of our survey, for a total sample of 2,471 girls. The schools were randomly assigned to one of the two treatment groups or control within each district.[24] Finally, the randomization was stratified by the district.

We describe the sampling procedure and field protocols in detail in Appendix Sections B and C, respectively. In schools assigned to receive the intervention, WBR worked with the schools to select a Bicycle Supervisory Committee (BSC), consisting of 10-12 members that comprised teachers, Parents and Teachers Association (PTA) members, local leaders, and student representatives. All girls belonging to the original list of eligible girls (not only the 25 that were part of the survey) received a

[23] Monze (44 schools out of a total of 135 schools), Mazabuka (20 schools out of 81), and Kalomo (36 schools out of 124).
[24] Appendix Figure A.1 shows the distribution of the schools in the study sample across the three districts.

bicycle.[25]

Baseline data was collected during the second term of the 2017 school year (July to August 2017).[26] The bicycles were distributed to girls in schools during the third term of the 2017 school year (September to November 2017). Distribution was done with all girls at the same time in a special event organized by WBR. The endline survey was implemented one school year after the bicycles were distributed (October to December 2018). See Figure A.2 for the timeline of the intervention.

4 Data Collection and Validity

The empirical analysis uses both survey and administrative data collected from schools, students, and head teachers over the course of the study (see Appendix Section C for Field Protocols). The research team used a variety of methods to collect data from students, and head teachers (or acting head teachers).

4.1 Surveys

We administered a 40-minute face-to-face survey to girls in the sample using a tablet to collect data on primary outcomes (see Appendix Section A for list of variables collected), a paper-based survey to collect data on sustained attention (D2 Test) which lasted 10 minutes, learning assessments in English and mathematics which lasted 25 minutes, and a semi-self-administered survey to collect data on questions that were sensitive which lasted 10 minutes. Throughout the girls' surveys, particular care was taken to ensure privacy: girls were interviewed by themselves, without interference from teachers, head teachers, or classmates. Only after the face-to-face survey ended with a girl in the school did the enumerator start the next survey. Finally, we also administered a 40-minute face-to-face survey to head teachers (or acting head teachers) using a tablet to collect data on school characteristics.

[25] If there were multiple girls from the same household, only one bicycle was given to them.

[26] It took place between 5^{th} July and 10^{th} August 2017, and the team (supervisors and surveyors) worked 21.5 days in the schools, over a period of 5 weeks. The baseline was first launched in the district of Monze and our team spent 9 days visiting all of the 44 schools. The team then moved to Mazabuka and spent 4 days visiting all of the 20 schools in the district in 4 days. Finally, the team spent 8.5 days visiting all of the 36 schools in the district of Kalomo.

4.2 Administrative Data

The research team collected administrative data on student attendance, grade transition, and dropout in October-December of 2019, 2020, and 2021 (two, three, and four years after the start of the intervention). In 2020, the data on school attendance was collected for the first term of 2020, before the schools closed because of the COVID-19 pandemic. The data on grade transition and dropout was collected after the schools reopened in October 2020. The team created a data collection form, where schools' principals and teachers filled out the information manually for the girls in the study sample, identified by their first and last names. The Monitoring and Evaluation team of WBR audited the veracity of the data by going to the schools and checking with the girls, with teachers, and looking at the registers.[27] In 2019 they audited 55% of the schools; in 2020 49%, and in 2021 97%. Finally, the data was entered into an electronic form and merged with the primary dataset, using a fake identifier. This was done to ensure the privacy of the girls in the study sample and to make sure that the team in the field had no access to data from the primary survey.

4.3 Outcomes

We pre-specified the following outcomes for the endline (see Appendix Section A for list of primary outcomes) in the pre-analysis plan (PAP) registered at RCT ID: AEARCTR-0003339.

First Stage Outcomes: We first report the impact of cycles on various measures of distance cost of attending school: (*i*) *Access to the cycle*; (*ii*) *Travel time to school*; (*iii*) *Index of perceived safety*; and two measures of actual safety; (*iv*) *Probability of being teased on the way to school*; and (*v*) *Probability of missing class because of safety concerns*.

Educational Outcomes: We use four measures of educational outcomes.[28] They are (*i*) *Days absent*, which is self-reported data on days absent in the previous week; (*ii*) *Days late to school* in the previous week; (*iii*) *Dropout*, which is a dummy variable if the girl dropped from the school at the endline; and (*iv*) *Grade transition*, which is a dummy variable if the girl progressed to a higher grade, conditional on not dropping out.

Academic Performance: We measure performance using both test scores and a measure of focus. We

[27] The Monitoring and Evaluation team of WBR in Zambia conducted these visits. The WBR team visited the school to compare the data that the school had sent with the registers. Additionally, they talked to students and teachers. The auditing exercise was done once a year, a few weeks after receiving the data from the schools.

[28] We also collected administrative data on school attendance, dropout, and grade transition 2-3 years after the monitoring ended. This data was not pre-registered in the PAP.

administer (*i*) *English* and (*ii*) *Mathematics* tests to measure students' learning outcomes. Both tests were based on the tests administered at the national level by the Examination Council of Zambia for Grade 5 (see Appendix Section F for the list of questions).

Regarding the *Index of Focus*, we use the D2 test. The D2 test consists of 14 lines with 47 characters in each line. This character is a letter "d" or "p" marked with small dashes either above or below. The respondent has 30 seconds per line to circle the letter "d" with two marks, above or below in any order (see Appendix Section G for an example of the test). The D2 test measures (*i*) *Speed*, which is the total number of observations processed in the D2 test; and (*ii*) *Accuracy*, which is the correct number of observations processed in the D2 test. We also combine these to create an *Index of Focus* using Anderson (2008).

Empowerment Outcomes: We use girls' responses to 39 individual questions (Appendix Section A lists the individual variables used in the indices) on various indicators of empowerment to construct one main index. This index measures the effect on all dimensions of empowerment. We also construct several sub-indices that will give us more detailed information about the specific aspects. The sub-indices are the following: (*i*) *Index of Mobility*; (*ii*) *Index of Aspiration*; (*iii*) *Index of Locus of Control*; (*iv*) *Index of Marriage and Fertility*; (*v*) *Index of Bargaining*; (*vi*) *Index of Pro-Sociality*; and (*vi*) *Index of Pro-Sociality*.[29] These indices include variables in which the higher value indicates a better or positive outcome. We interpret a positive value in the index as higher empowerment. The variables used to measure the empowerment of girls have been validated and used by Kabeer (1999); Laszlo and Grantham (2017); Dhar et al. (2022).[30]

Each index mentioned above is constructed by aggregating responses to several individual questions into an index, which is a weighted average value of the individual variables, with weights constructed by normalizing the variables to have the same *s.d* and then recovering the weights from the inverse covariance matrix, following the approach described in Anderson (2008) (for details on the steps for index construction see Appendix Section E).

Time Use: We measure how girls spend their time on different activities during a normal weekday (Appendix Section A lists the individual variables to measure various activities). These activities are: (*i*) *School chores*; (*ii*) *Extra-curricular activities*; (*iii*) *Studying and homework*; (*iv*) *Household chores*; (*v*) *Engaging in income generating activities*; and (*vi*) *Spending time with friends*.

[29]The research team used a self-administered survey for questions related to safety, security, marriage, and fertility to allow more privacy to the respondent and to avoid social desirability bias.

[30]See Glennerster et al. (2018) for a practical guide to measuring women's and girls' empowerment in impact evaluations.

These are categorical variables that specify the amount of time spent by the girl on a particular activity. They take the value of 0 if the girl spent no time doing that activity, 1 if she spent less than 30 minutes, 2 for between 30 and 60 minutes, 3 for between 60 and 90 minutes, 4 for between 90 and 120 minutes, and 5 for more than 120 minutes.

4.4 Integrity of the Experimental Design

4.4.1 Baseline Balance

We report the baseline characteristics of the schools by treatment status in Table A.2 and find that the sample is balanced across most variables, except the number of girls' toilets in the school. The first panel reports the mean and the standard deviations of the baseline variables for the schools in the respective treatment groups, and the second-panel tests for statistical differences across the two groups. The schools in our sample on average have an enrollment of 680 students, equally split between boys and girls. They employ an average of about 13 teachers, also equally split between male and female teachers, about three-quarters of whom reside at the school premises. A large fraction of the schools ($\sim 60\%$) have a sanitation program running, which is also confirmed by the presence of separate toilets for girls and boys. Most schools have a computer lab ($\sim 95\%$). The schools are similar in the degree of "remoteness", as can be seen from their respective distances to closest the town (47 km), tarmac road (25 km), and secondary school (20 km).

Similarly, we report the baseline characteristics of the girls in the two treatment groups in Appendix Table A.3 and find that the sample is balanced across most variables, except grade, number of times girls eat meat in their meals, socio-economic index,[31] and self-esteem index. The first panel reports the mean and the standard deviations of the baseline variables for the girls in the respective treatment groups, and the second panel tests for statistical differences across the two groups. The average age for girls in our sample is 13, with about 15% of them currently engaged or married, and 5% being pregnant. The average household size of girls in the sample is 6 members, and 80% of them have both parents alive. A third of the girls have repeated a grade in the past. These girls come from extremely impoverished backgrounds, where they have not had enough food to eat in 1 out of the last 7 days.[32] The girls are similar across the treatment groups in terms of the indices on the locus of con-

[31] The socio-economic index is a Principal Component Analysis (PAC) index constructed by variables that indicate asset ownership in the household.

[32] Section 8 in the Appendix presents the tables with the main results controlling for the imbalanced variables and other demographic characteristics.

trol, and gender attitudes. These indices have been constructed using Principal Component Analysis. Appendix Section A has a description of the variables in each of these indices, and their spread across the two treatment groups.

We present a detailed description of the sampling of schools and girls in Appendix Section B. Overall, we conclude that the randomization was successful.

4.4.2 Compliance

The allocation of the bicycles was carried out by World Bicycle Relief in partnership with the Ministry of General Education in Zambia in the 45 schools in the treatment group. The distribution took place within the schools and was considered successful by WBR and the research team. Only 1 girl out of the 2,471 selected refused the cycle.[33]

4.4.3 Attrition

In this study, we follow a two-stage tracking method during the endline to limit attrition as suggested by Glennerster et al. (2013). This method consists of a first step in which we attempt to interview the girls in the school where they were enrolled during baseline interviews. We found 72% of the girls in this phase. Of those not found in their initial schools, we randomly selected 50% to track in their households, villages, and, if necessary, in other districts. We weighted the answers from those girls interviewed in the second phase depending on the probability of being sampled in the analysis. This method allows us to maximize the resources available and keep the effective attrition rate as low as possible. In the first line of Table A.4, we present the attrition rate without considering the weight of the girls found in the second stage of tracking by the treatment arm. In the last row, we show the effective rate of attrition, using the method described. The tracking rate of girls in the control group is lower (90%) than the one in the two treatment arms (94% and 92%, respectively).[34] We find significant differential attrition between the control group and the No Payment Arm. However, this difference is not a concern given that the tracking rate is above 90% for all the groups. Overall, the attrition on outcome measures is below 9%.

Nevertheless, we checked whether the attritors are different from those that we interviewed in the endline in any of the observable characteristics from the baseline. In Table A.5, we see that none

[33]This information was provided by WBR monitoring team.

[34]In Figure A.3 we present the numbers of girls that we interviewed in different phases of the project by treatment.

of the indices at the baseline (socio-economic characteristics of the household, index of self-esteem, index of locus of control, and index of gender attitudes) are significantly different for those students interviewed at baseline and for those missing. Finally, we also estimate Lee bounds of the treatment effects using Lee (2009) to consider the possibility that other non-observable variables are endogenous to the treatments. We present the results in Tables A.6–A.8 and find minimal differences between the upper and lower bounds of the treatment effects. Overall, due to the low attrition, our main results are robust to Lee bounding.

4.4.4 Data Analysis

We evaluate the impact of providing a bicycle to a school-going girl on various measures of empowerment, safety, and educational outcomes. We collected a large number of outcomes through a primary survey to study the impact of the intervention. For complete transparency, we follow the Pre-Analysis Plan (PAP) which is available and timestamped at RCT ID: AEARCTR-0003339.[35] The PAP specifies the variables to be analyzed, construction of indexes, how we plan to address multiple inferences, the empirical specifications we plan to use, and our approach for tracking and the handling of attrition. The empirical analysis reported in this paper follows the PAP, however, we report results where the analysis deviated from the PAP.[36] In such cases, we report these deviations and provide reasons for them while discussing those results.[37]

5 Empirical Strategy

5.1 Reduced-Form Specifications

We estimate the intent-to-treat (ITT) impact of the cycles program for both the treatments together (Payment Arm and No Payment Arm combined, relative to the control), and separately using the following specification:

$$Y_{i,s,t=1} = \alpha_0 + \alpha_1 T_s + \alpha_2 Y_{i,s,t=0} + e_{i,s} \quad (1)$$

[35] PAP registered at AEA RCT Registry: `https://www.socialscienceregistry.org/trials/3339/history/34596`

[36] A recent paper by Banerjee et al. (2020) discusses the costs and benefits of adhering to PAP and recommends that the final research paper be written and judged as a distinct object from the "results of the PAP".

[37] In Appendix H, we used a non-survey method to elicit girls' behavioral decisions. However, during the implementation of the behavioral games, we encountered challenges related to the cultural aspects of giving students goods for free. In this region of Zambia, receiving goods for free can be seen as an unfriendly act and some of the families were becoming suspicious. Thus, we decided to discontinue the games not to harm the rest of the data collection exercise. Before we discontinued the implementation, we had covered ten schools in the district of Monze. In this Appendix, we describe the details of the behavioral games for transparency.

where $Y_{i,s,t=1}$ is the outcome variable of student *i* in school *s* as measured in post treatment, and $Y_{i,s,t=0}$ is its baseline value. T_s is an indicator for the school assigned to the treatment group, and $e_{i,s}$ is the error term.[38] We cluster the standard errors at the school level in order to account for unobservable correlations in girls at the same school, and also because the treatment was assigned at the school level. For outcomes where we do not have baseline values, we do not include $Y_{i,s,t=0}$ in the estimation. Finally, β_1 is our main coefficient of interest and provides the ITT effect, which is the effect of being given a bicycle in 2017 on the outcome variable.

We further present the ITT impact of the cycles program by the two treatment arms, relative to the control using the following specification:

$$Y_{i,s,t=1} = \beta_0 + \beta_1 Payment\ Arm_s\ (T1) + \beta_2 No\ Payment\ Arm_s\ (T2) + \beta_3 Y_{i,s,t=0} + e_{i,s} \qquad (2)$$

where $Payment\ Arm_s\ (T1)$ and $No\ Payment\ Arm_s\ (T2)$ are indicators for being assigned to each of the treatment groups and all other variables are the same as in Equation 1. Finally, β_1 and β_2 will provide the ITT effects for each of the two treatment groups.

5.2 Accounting for Multiple Comparisons

This study entails the analysis of different outcome variables; therefore, it is important to address the concern of false positives. We address this by computing standardized indexes using Anderson (2008) for several primary and secondary outcome outcomes. Furthermore, we include the tables with multiple hypothesis testing corrections in Tables A.9–A.13. To do that, we use the Benjamini and Hochberg (1995) False Discovery Rate correction (as specified in the PAP).[39] We show results with the corrected p-values, where the corrections are made within outcome groupings.

5.3 Minimum Detectable Effect Sizes

We present the minimum detectable effect sizes (MDEs) in Table A.14 for our primary outcome variables. Due to very low intra-cluster correlation, the MDEs are generally very low. We are powered to observe changes at or well below 20% for all outcomes.

[38] This is a deviation from the PAP where we had pre-specified our main analysis with controls. However, we include a vector of individual level controls and present the results Tables B.1–B.6. Including controls does not change the main results.

[39] In addition, we also use Westfall et al. (1993) and Holm (1979) to address the concern of false positives (not specified in the PAP).

6 Results

6.1 Impact of Cycles on First Stage Outcomes

We first report the impact of cycles on first-stage outcomes, those that we expect the cycles to affect directly: (*i*) *Access to the cycle*; (*ii*) *Travel time to school*; (*iii*) *Index of perceived safety*; and two measures of actual safety: (*iv*) *Probability of being teased on the way to school*; and (*v*) *Probability of missing class because of safety concerns* in Table 1.[40]

Panel A in Table 1 reports the results for the pooled treatment estimated using Equation 1. We find that the girls in the treatment group were 88% (Column 1) more likely to have access to a bicycle vis-à-vis girl in the control group. Most importantly, the intervention reduced the one-way commute time to school by 35 minutes (Column 2). This corresponds to a one-third reduction in commuting time.

In columns (3) to (5) we investigate the intervention's impact on perceived and actual safety. The index of perceived safety includes questions related to whether the girl feels safe moving around in the village and traveling to school. We find that the intervention improved the perceived safety of girls in the treatment schools by 0.11 s.d. (Column 3). To get at the hard outcomes, we also asked questions on two self-reported measures of safety.[41] We find that the intervention reduced the probability that a girl is teased or whistled on the way to school by 22% (Column 4). Furthermore, we also find that the intervention reduced the probability that a girl missed school or left for home early from school due to concerns of safety by 38% (Column 5).[42]

Panel B reports the impact of the intervention by the two treatment groups separately using Equation 2. We do not find any statistical difference in the outcome measures between the two treatment groups. This is not surprising since the impact of cycles on first-stage outcomes should not vary by Payment and the No Payment treatment arms. All estimated impact remains significant when the p-values are corrected for multiple hypotheses testing (Table A.9).

The results on girls' safety are particularly important from the policy standpoint as approximately

[40] Table A.1 provides the means and standard deviations of the variables in the estimation sample.

[41] These two outcomes were not pre-specified in the PAP.

[42] These results are consistent with the findings in the qualitative survey conducted by the research team. The following quotes corroborate the results estimated in Table 1: *"[When you are harassed], you can just ride your bicycle faster...because you can ride the bicycle faster if he is walking." (Girl, Treatment School, Mazabuka)"; "The bicycle has contributed to their safety in the sense that when they are done with school, they reach home faster and spend less time on the way. What used to happen before, was that they reached home late and sometimes they could stop along the way to rest, which increased the chances of them being harassed." (Female parent, Treatment School, Mazabuka)".*

35% of the girls report having been sexually harassed on their way to school in the baseline. There is strong evidence that violence against women, including sexual harassment, has a negative impact on psychological costs (Langton and Truman, 2014), human capital investments (Borker, 2020), labor force participation (Siddique, 2018), and mobility (Hsu, 2011; Porter et al., 2011). Also, harassment by strangers strongly impacts women's perception of safety across social contexts (Macmillan et al., 2000). We believe that the improvements in safety due to bicycles are likely to have a far-reaching impact on girls' well-being.

Overall, it is reassuring to find that the intervention had a large and statistically significant impact first stages outcomes.[43]

6.2 Impact of Cycles on Time Use

Given that girl in the treatment, the school saves about one hour per day in transport, we wanted to explore the impact on measures of time-use: (*i*) *School chores*[44]; (*ii*) *Extra-curricular activities*; (*iii*) *Studying and homework*; (*iv*) *Household chores*; (*v*) *Engaging in income generating activities*; and (*vi*) *Spending time with friends*.[45]

We estimate the impact of the intervention on changes in the way the girls spend their time on different activities during a normal weekday and present the results in Table 2, where Panel A shows the results for the pooled treatment, and Panel B reports the same separately, by the two treatment groups. The dependent variables are categorical variables, specifying the amount of time spent by the girl on a particular activity.[46] The estimated coefficients are odds ratios from an ordered logit specification. We do not find any impact of the intervention on most of the measures of time use, except for time spent Engaging in income-generating activities (Column 5). The odds of spending time Engaging in income-generating activities for the girls in treatment schools is 0.71 times that of girls in the control group, i.e., girls in treatment are less likely to be engaged in income-generating activities.

A plausible explanation could be that the intervention had positive income effects for the family,

[43] Primarily motivated by the likely decrease in time taken to reach school due to cycles, we present the impact of the intervention on an index of focus in Table A.15. This index measures (*i*) *Speed*, and (*ii*) *Accuracy* using a D2 test. We do not find a statistically significant impact on the index of focus, however, the signs are positive.

[44] Activities include cleaning the classrooms, brushing the compound, if there are animals helping feed them, and cleaning the blackboards, etc.

[45] These outcomes were registered as secondary outcomes in the PAP.

[46] The dependent variable takes the value 0 if the girl spent no time doing that activity, 1 if she spent less than 30 minutes, 2 for between 30 and 60 minutes, 3 for between 60 and 90 minutes, 4 for between 90 and 120 minutes, and 5 for more than 120 minutes.

thereby not requiring the girl to engage in any income-generating activity, or the intervention changed the value that the family places on education vis-à-vis engaging in short-term income-generating activities, which resulted in a decline in the amount of time spent by the girls in this activity. However, we do not see any increase in the time spent by girls studying, which is likely due to the very coarse intervals we use to measure time use.

6.3 Did Bicycles Transform Girls' Lives?

One of the primary outcomes of the cycle intervention was to study its impact on female empowerment given the historical importance of bicycles in empowering women in the United States in the 19^{th} century (Macy, 2011). We present the results on various measures of empowerment in Table 3.[47] Panel A reports the results for the pooled treatment, and Panel B reports the same separately, by the two treatment groups.

We find that the intervention increased the index of female empowerment by 0.12 *s.d* (Column 1). Furthermore, we report the impact of bicycles on various sub-indices of empowerment in columns 2–8. Estimates from the pooled treatments (Panel A in Tables 3) suggest that the intervention did not have any impact on the indices of mobility,[48] aspiration, and marriage and fertility. However, the bicycles improved the indices of locus of control, bargaining, pro-sociality, and self-image. The index of locus of control improved by 0.18 *s.d* (Column 4), bargaining improved by 0.21 *s.d* (Column 6), pro-sociality improved by 0.15 *s.d* (Column 7), and self-image improved by 0.13 *s.d* (Column 8) for girls in the treatment schools.[49]

These are important results as the index of locus of control measures the degree of control the girls believe they have over outcomes in their lives, and how satisfied they are with their life in general. Similarly, the index of bargaining measures whether girls have access to and control over small amounts of resources, the clothes they wear, the food they eat, and whether they are able to discuss matters pertaining to their lives with their parents.[50] The index of pro-sociality measures the partic-

[47] In the PAP, the *Index of Mobility and Safety*, *Index of Aspiration*, *Index of Locus of Control*, and *Index of Marriage and Fertility* are registered as primary outcomes and *Index of Bargaining*, *Index of Pro-Sociality*, and *Index of Pro-Sociality* as secondary outcomes.

[48] This index is constructed slightly different from the one reported in the PAP. We separate the results of mobility from the ones of safety, which is presented as a first-stage outcome in Table 1.

[49] We report the table with p-values corrected for multiple hypotheses testing (Table A.11).

[50] This index is slightly different from the one we registered in the PAP. In particular, we excluded two variables that measure rebellion. The estimated coefficient for the bargaining index with the two variables included was: 0.10 (0.06) and statistically significant at 10 percent for the pooled treatment, 0.07 (0.07) but not statistically significant for Payment Arm, and 0.14 (0.07) and statistically significant at 5 percent for No Payment Arm.

ipation of girls in local clubs, their willingness to help out their friends, and their knowledge of the local leadership, while the index of self-image measures what girls think of themselves vis-à-vis their peers in terms of academic achievement, and their probability to succeed in future.

Although it is surprising that the intervention did not improve the mobility of girls in the treatment schools, upon further investigation, we found out that parents of the girls in the treatment schools as well as the members of the Bicycle Supervisory Committee (BSC) considered the bicycle to be a precious asset, to be used by the girls only for the purpose of traveling to school.[51] These restrictions on how the bicycle should be used by the girls explain why we do not find an impact on girls' mobility.

Panel B in Table 3 reports the impact by Payment Arm and No Payment Arm separately. The intervention improved the index of empowerment (Column 1) for girls in the Payment Arm by 0.2 *s.d*. We find similar patterns for the sub-indices of aspiration, fertility, and marriage for girls in the Payment Arm. We find that the bicycle intervention improved the index of aspiration by 0.12 *s.d*, and the index of fertility and marriage by 0.18 *s.d* (Columns 3 and 5, respectively in Panel B of Table 3). The index of aspiration measures the girls' aspirations with regard to the years of education they want to receive, and their future participation in the workforce. Similarly, the index of fertility and marriage measures the girls' desired fertility behavior and preferences on the age of marriage.

Improvements in the indices of aspiration, fertility, and marriage suggest that the girls in the Payment Arm aspire to continue their education and participate in the workforce in the future, and have a decreased preference to get married early and have many children. In our context, when a credit-constrained household spends money ex-ante to acquire bicycles for their girl child, this is likely to be a signal to the girls in the Payment Arm that their parents are invested in their education and general well-being. Furthermore, charging a small upfront cost in the Payment Arm is likely to induce parents to push their girls to use the bicycle more. This is consistent with the "sunk cost fallacy" which argues that usage intensity is higher when consumers are charged positive prices (Arkes and Blumer, 1985; Thaler, 1980).

Taken together, these results provide the *first causal evidence* on the transformative role a bicycle can play on women's empowerment, which was first highlighted by scholars of the women's suffrage movement.[52] The positive impact of the intervention on the empowerment of girls in rural Zambia

[51]The research team did an extensive qualitative survey with parents, teachers, girls, and community leaders to further understand the changes we estimate.

[52]In 1895, at the age of 80, suffragist leader Elizabeth Cady Stanton claimed that "the bicycle will inspire women with more courage, self-respect, self-reliance..." Stanton predicted the power of the bicycle in transforming the lives of women, realizing that the independence women were gaining because of this invention would allow for growth in other areas of

is likely to have a lasting impact on their future living standards as argued by Duflo (2012). More importantly, the results on measures of empowerment directly contribute to the policy debate on interventions that can improve female empowerment, which is a priority for policymakers in developing countries (UN Sustainable Development Goal No. 5).[53]

6.4 Impact of Cycles on Educational Outcomes

Table 4 reports the impact of the intervention on educational outcomes.[54] Panel A presents the results for the pooled treatment and Panel B reports the results for the two treatment groups separately.

We find that the cycle intervention reduced overall absenteeism in the previous week (Column 1 in Panel A) by 29%, with the effect sizes being identical for the two treatment arms (Panel B). The impact is sizable, as this translates to an addition of about 5 additional school days for girls in the Southern Province, where students in primary school miss an average of 18 days in a school year (DHS, 2002). It also reduced the number of days that girls arrived late to school the previous day by more than a day, which represents a 66% difference when compared to the control group (Column 2). We do not find any impact on dropouts (Column 3) and grade transition (Column 4), although the sign of the coefficient is negative and in the right direction. The result on dropouts is not surprising given the low level of dropout in the study sample (the control group means is 6%). Similarly, grade transition is automatic up to grade 7 in our sample schools. Although statistically insignificant, taken together, the results suggest that girls in treatment are more likely to be enrolled in school, but they do not necessarily graduate to higher grades. Given that grade transition is automatic up to grade 7, this implies that the intervention is successful in keeping those girls in school who would have otherwise dropped out during the transition to secondary school from grade 7 to grade 8.

We administered tests in English and Mathematics to girls in the baseline and the endline.[55] We

their character. Having the ability to be fully self-reliant, often for the first time in their lives, would encourage women to be more courageous in other areas, such as demanding voting rights. One century later, our qualitative survey echoes similar findings. The following quotes further corroborate what has been documented in the 19^{th} century in the United States and the results estimated in Table 3: *"We see their confidence just by the way they say bye to us when they get on their bicycle going to school ... In addition, a child who has a bicycle and one who hasn't ... you are able to tell that indeed, this ... has brought so much confidence* [*in the beneficiaries*] *... Those that have bicycles walk differently. There is just the way they walk compared to those who don't have* (*Male parent, Treatment school, Mazabuka*)*"; "I felt important because it was my first time having this kind of a bicycle* (*Girl, Treatment school, Kalomo*)*".*

[53]The Gender Parity Goals of the UN Sustainable Goal No. 4 aims to achieve gender equality and empower all women and girls. Similarly, Goal No. 4 aims to ensure inclusive and equitable quality education and promote life-long learning opportunities for all.

[54]We pre-specified *Days Absent*, *Dropout* and *Grade Transition* are registered as primary outcomes; and test scores on *English* and *Mathematics* as secondary outcomes in the PAP registered at RCT ID: AEARCTR-0003339.

[55]The English and Mathematics tests were based on tests administered at the national level by the Examination Council of Zambia for grade 5.

find modest increase in girls' Mathematics scores by 0.11 *s.d* (Column 5), bit not on English scores (Column 6). Results in Panel B suggest that the improvements in learning outcomes are driven by girls in the Payment Arm, although there is not enough evidence to support the differential treatment effect between the two treatment arms.

6.5 Medium Run Impact of Cycles on Educational Outcomes

To study the medium-run impact of the intervention on girls' education, we collected administrative data on students' attendance, dropout, and grade transition two (2019), three (2020), and four (2021) years after the cycle intervention.

Table 5 reports the impact of the intervention on girls' attendance in term I and term II of 2019, term I of 2020, and term I of 2021. Panel A presents the results for the pooled treatment and Panel B reports the results separately by the two treatment groups. The analysis is done with the sub-sample of students that are enrolled at the time of the measurement. Thus, we see differences in the sample size between years. Overall, we find that the intervention continued to improve girls' attendance two, three, and four years after the intervention. Girls in the treatment schools missed 3.77 days less (45% reduction), and 3.4 days less (40% reduction) in the term I and term II of 2019, compared to the girls in the control schools (*See* Panel A of Table 5). Similarly, girls in the treatment schools missed 2.18 days less (29% reduction), and 4.80 days less (55% reduction) in terms I of 2020, and 2021, respectively. We do not find the impact to vary by the two treatment groups (*See* Panel B of Table 5).[56] These results are encouraging and provide credible evidence that the intervention was successful in mitigating the negative consequences of the pandemic on girls' education.

Table 6 reports the impact on girls' dropout and grade transition. Panel A presents the results for the pooled treatment, while Panel B reports the results separately by the two treatment groups. Similarly to the previous table, the analysis for the grade transition is conditional on the student being enrolled, resulting in differences between the sample sizes.[57] We find that the girls in the treatment schools were 9% less likely (37% reduction) to drop out of school than the girls in the control schools in 2019. In 2020 and 2021, we collected data on dropout and grade transition for term III (November-December), when the schools reopened after the closure due to COVID-19. In Columns 1, 3, and 5 of

[56] We only have attendance data from term 1 of 2020 since the schools were closed due to COVID-19. We collected attendance data in 2021 after schools reopened after COVID-19.

[57] Furthermore, the differences in the sample sizes between the conditional analysis on attendance and grade transition is because we have information about grade transition for the girls that transferred schools. We do not have data about attendance for girls that transferred.

Table 6 we observe that on average, the dropout rate in the control schools increased every year. In particular, 24% of the girls had dropped out in 2019, 42% in 2020, and 57% in 2021. We find that the girls in the treatment schools were 9% less likely (21% reduction) to drop out in 2020, and 10% less likely (17% reduction) to drop out in 2021, compared to the girls in the control schools. We do not find the impact of the intervention on grade transitions in 2019 and 2020. However, in 2021 we find that the girls' in the treatment schools were 11% more likely (19% increase) to transition to the next grade. Once again, we do not find the impact to vary by the two treatment groups as seen in Panel B of Table 6.

The results from the two through four-year follow-ups seem promising and show that the intervention has sustained effects in keeping girls in school, helping them continue their academic progression, and allowing them to attend school more regularly. Although we cannot pinpoint the underlying channel for this impact, we interpret these outcomes as realized measures of empowerment. Continuing to attend school in this scarce-resource setting, with high rates of dropouts (up to 57% in 2021), is evidence of a strong will from both the students and their families. These results are also aligned with a study that shows that an empowerment program mitigated the negative effects of the Ebola outbreak in Sierra Leone for various outcomes, including school enrollment (Bandiera et al., 2018).

6.6 Heterogeneity Analysis

Tables 8 and 7 report the heterogeneous impact of the intervention by the baseline time taken to travel to school (we interact tercile of the baseline time taken to school by the treatment groups) as prespecified in the PAP.

We do not find any heterogeneous impact of the intervention on access to cycles (Column 1 of Table 8). This is consistent with the theory of change since it implies that after a year, girls living closer or further away from the school are equally likely to still have access to the cycle. Girls in the middle tercile experience a greater reduction in absenteeism vis-à-vis the bottom tercile (Column 2 of Table 8), but there is no statistically different impact for girls in the top tercile. As expected, girls living further away from the school (as measured by the middle and top tercile) experience a greater reduction in time taken to travel to school (Column 3 of Table 8). Similarly, girls in the middle and top tercile also experience a greater reduction in the number of days they arrive late to school (Column 4 of Table 8). We cannot reject the null of no impact on absenteeism for the girls in the top tercile (p-value $= 0.59$). Finally, we do not find statistically different heterogeneous impacts on learning

outcomes (Columns 5–6 of Table 8), but the direction of the impact is consistent with our results on school absenteeism.

We further estimate the heterogeneous impact of the intervention by the baseline time taken to travel to school on measures of empowerment. We find that this intervention improved the *index of locus of control* (Column 3 of Table 7) and *bargaining* (Column 5 of Table 7) for girls living in the bottom and middle tercile. However, we do not find an impact on girls in the top tercile.

Overall, the intervention relaxed the distance constraints for the girls living furthest away from the schools, which translates to improvements in attendance for those girls enrolled in school but does not necessarily translate to increasing enrolment for girls in the top tercile. It seems like that for girls living furthest from the school the distance costs are still binding, or they face additional constraints, and a possible long-term solution would be school construction.

7 Cost Effectiveness

In this section, we undertake cost effectiveness exercise of this program by ascertaining the increase in years of schooling for every $1000 spent. We found that in the medium term, girls who received the treatment were absent, on average, for 4.16 fewer days in Term 1 and 3.74 fewer days in Term 2 of 2019. Therefore, on average, girls who received bicycles were absent for approximately 8 fewer days than the control group students in 2019. This translates to 0.044 years of schooling per student (we assume an average academic year length is 180 working days). Data from World Bicycle Relief shows that the cost of a cycle (including delivery, training of the mechanic, and implementation costs) is $155 per bicycle. Using these figures, we estimate the increase in the years of learning per thousand dollars spent to be 0.283.[58] This is larger than estimates from some of the closely related papers in the conditional cash transfer literature (see Draeger (2021); Evans et al. (2012); Levy and Ohls (2010))

An important caveat in considering the above results is that the analysis underestimates the cost-effectiveness of our study since it only considers improvements in attendance rates. The intervention has also improved empowerment measures like locus of control (0.18 s.d), bargaining (0.21 s.d), pro-sociality (0.15 s.d), and self-image (0.13 s.d). In the medium run, we also see a decrease in dropouts. Furthermore, in terms of hard outcomes, the cycles reduced the probability that a girl is teased or whistled on the way to school by 22% and the probability that a girl missed school or left for home early

[58] Years of schooling per $1000 = (Years of schooling per student)/(Cost of one cycle)*1000 = (0.044*1000)/155=0.283.

from school due to concerns about safety by 38%. Anecdotally, girls with bicycles may be motivated to share this resource with siblings, neighbors, and classmates, potentially amplifying the attendance benefits.

Therefore, while being reasonably cost-effective, especially compared to studies focused only on improving female attendance, this program has a wide range of positive implications for the lives of school-going girls in Zambia. Once we incorporate this, we believe the bicycles program has the potential to unlock scalable solutions for bolstering girls' education and reshaping their future in low-income settings.

8 Conclusion

In this paper, we find that a conditional kind transfer of a bicycle can be a useful policy tool to transform the lives of girls. *Girls in the treatment schools reported feeling more empowered, they reported less commute time to school, absenteeism, late arrival to school, improved test scores and safety, time use, grade transition, and dropout*.

Scholars of the women's suffrage movement, including Susan B. Anthony in the United States, have highlighted the role played by bicycles in empowering women in the 19^{th} century. Consistent with this historical perspective, we find that the provision of bicycles improved girls' empowerment through *improved locus of control, bargaining, pro-sociality, and self-image*. Policies that aim at improving female empowerment have limited success due to the deeply rooted cultural norms that lead to discrimination against women in all spheres of life (Jayachandran, 2015). Work by Duflo (2012) suggests that improving female empowerment may also have a lasting impact on women's future living standards. It is worth highlighting that finding positive impacts on both educational and empowerment outcomes is perhaps the most unique result of this intervention.

In rural Zambia, approximately 35% of the girls report having been teased on their way to school. We find that the intervention improved the safety of girls in the treatment schools. It reduced the probability of whether girls were teased or whistled on on the way to school by about 22% and reduced the probability that a girl missed school or left for home early from school due to concerns of safety by about 38%. Given the negative impact of sexual harassment on women's access to education and learning (Borker, 2020; Evans et al., 2021b), improvement in safety due to bicycles is likely to have a far-reaching impact on girls' well-being.

Results from the two treatment arms – Payment vs. No Payment – suggest that girls who received bicycles with a small cost to their family had higher levels of aspirations, self-image, and a desire to delay marriage and pregnancy. Surprisingly, we do not find these effects in the zero-cost treatment. We believe these results are due to girls perceiving the payment from the family as a desire to increase future investment in them. Although we do not have measures of how the parents feel about investment in the girls, based on discussions with parents we believe that these expectations from the girls are likely correct.

While an intervention like providing bicycles to students is likely too expensive for most governments and there are more cost-effective ways to increase schooling for girls, such as paying school fees, we believe our results have several important policy implications. First, we show that a policy like the provision of bicycles that improves access to school through a reduction in distance costs can improve educational outcomes, at least in the short run. This is especially important since discriminatory social norms that limit girls' access to education and labor force participation remain a challenge in many parts of the world. In addition, school construction programs, a default approach to address access to school problems, are expensive, take a long time to complete, and might not be cost-effective. Second, we demonstrate that the intervention improved girls' empowerment, which directly contributes to the "Gender Parity" objective of the UN Sustainable Development Goals 2030. Third, we did not find any differential impact between the two treatment arms that is statistically significant. Many policies are designed with "conditionality", which is administratively burdensome and increases the overall cost, especially from both implementation and monitoring points of view. In our context, we did not find evidence that a small upfront payment had any "unintended" impact on girls' outcomes. Finally, we contribute to existing research on conditional cash transfers, which are increasingly used as a policy tool to increase female schooling in developing countries, and find that a bicycle improves girls' educational outcomes.

Taken together, the results from Muralidharan and Prakash (2017) and Kjelsrud et al. (2021) – two non-experimental studies of a large-scale cycling program for adolescent girls in India, and this paper, a randomized control trial in Zambia – point to a potential policy tool that can address both the gender gap in education and improve female empowerment. More broadly, results from this paper suggest that identifying the underlying mechanisms can play an important role in understanding the challenges around replicability and external validity in international development. This is especially important since results of randomized evaluations of the same intervention vary substantially across trials (Vivalt, 2019), and even within the same location, the causal impact varies due to random

variation in conditions over time (Rosenzweig and Udry, 2020).

Future research could focus on studying the long-term impact of such policies on girls' age of marriage, fertility decisions, bargaining, and the community-level spillovers on norms and aspirations from an in-kind transfer that went to a population that does not normally receive items of relatively high value. More broadly, it is important to study how policies aimed at improving girls' education impact community dynamics and norms regarding girls' education. This is especially important since communities can have their own norms regarding girls' education. In the context of Sub-Saharan Africa, apart from problems of access, income, and information, girls face additional cultural constraints like early marriage and pregnancy. Such social expectations and gender biases can lead to certain family practices that deprive girls of not just educational opportunities but also alter their aspirations. Finding innovative ways of changing social norms might prove to be a sustainable way of tackling the problem of high dropout rates for adolescent girls in developing countries. Though social norms are slow-moving and hard to change, previous research has shown evidence of change in norms in response to exposure (Dhar et al., 2022). ■

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Main Tables

Table 1: First Stage Outcomes

Dependent variable:	Access	Time to school	Sub-Index of Safety	Teased	Missed School
	(1)	(2)	(3)	(4)	(5)
Pooled Treatment	0.88***	-35.08***	0.11**	-0.08***	-0.07***
	(0.02)	(3.01)	(0.05)	(0.03)	(0.02)
Payment Arm	0.89***	-36.84***	0.13**	-0.08***	-0.07***
	(0.03)	(3.38)	(0.07)	(0.03)	(0.02)
No Payment Arm	0.88***	-33.24***	0.09	-0.08**	-0.06**
	(0.02)	(4.06)	(0.06)	(0.04)	(0.02)
Effect size: Pooled Treatment		33.8%	35.5%	21.6%	38.8%
Effect size: Payment Arm		35.5%	41.9%	21.6%	38.8%
Effect size: No Payment Arm		32.0%	29%	21.6%	33.3%
Observations	2001	1879	1941	1954	1953
Control group mean	0.02	103.77	0.31	0.37	0.18
Payment Arm = No Payment Arm (p-value)	0.87	0.40	0.55	1.00	0.71

NOTES: Standard errors clustered at the school level (in parentheses). Regression in column (2) includes controls for the baseline value of the time traveling to school. The dependent variable in (Column 1) is a dummy indicating whether the girl has access to a bicycle, and in (2) is the time spent traveling to school (in minutes) each way, in (3) is the sub-index of perceived safety, in (4) is the probability of the girl being teased or whistled at on the way to school, and in (5) is the probability that a girl misses school or leaves early for home for safety concerns. The details on the components of the index in column (3) are in the Appendix A. The effect sizes are the % change compared with the control group means. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2: Impact on Time Use

Dependent variable:	School Chores	ECA	Home Work	Household Chores	Income Generation	Friends
	(1)	(2)	(3)	(4)	(5)	(6)
				Panel: A		
Pooled Treatment	0.75	1.05	0.94	1.00	0.71***	1.04
	(0.14)	(0.13)	(0.11)	(0.13)	(0.09)	(0.11)
				Panel: B		
Payment Arm	0.89	1.16	1.02	0.97	0.74*	1.06
	(0.20)	(0.17)	(0.14)	(0.16)	(0.12)	(0.15)
No Payment Arm	0.61**	0.92	0.83	1.01	0.70**	0.99
	(0.13)	(0.16)	(0.11)	(0.15)	(0.10)	(0.12)
Observations	1938	1925	1931	1997	2000	2005
Payment Arm = No Payment Arm (p-value)	0.15	0.24	0.16	0.83	0.78	0.64

NOTES: Standard errors in parentheses are clustered at the school level. All columns report the odds ratio from an ordered logit model. The dependent variable is the time spent by the girl doing various activities, and takes the following values: 0 - no time spent, 1 - less than 30 minutes, 2 - between 30 and 60 minutes, 3 - between 60 and 90 minutes, 4 - between 90 and 120 minutes, 5 - more than 120 minutes. The activities are: School chores like cleaning in column (1), Extracurricular activities (ECA) in column (2), Studying and homework in column (3), Household chores in column (4), Engaging in income-generating activities in column (5), and Spending time with friends in Column (6). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3: Impact on Measures of Empowerment

		Sub-index of						
Dependent variable:	Index of Empowerment	Mobility	Aspirations	Control	Fertility	Bargaining	Pro-sociality	Self-Image
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pooled Treatment	0.12**	-0.02	0.07	0.18***	0.10	0.21***	0.15**	0.13**
	(0.06)	(0.06)	(0.05)	(0.06)	(0.07)	(0.06)	(0.06)	(0.06)
Payment Arm	0.18**	-0.03	0.12**	0.20***	0.18*	0.20***	0.13**	0.15**
	(0.08)	(0.07)	(0.05)	(0.07)	(0.09)	(0.07)	(0.06)	(0.07)
No Payment Arm	0.04	-0.01	-0.01	0.15**	-0.00	0.20***	0.18**	0.11
	(0.07)	(0.07)	(0.09)	(0.07)	(0.09)	(0.07)	(0.08)	(0.08)
Observations	1871	1995	1926	2006	1948	2011	1874	1889
Control group mean	1.55	0.26	1.43	0.50	0.94	0.38	0.77	0.68
Payment Arm = No Payment Arm(p-value)	0.13	0.80	0.13	0.58	0.11	0.99	0.54	0.63

NOTES: Standard errors in parentheses are clustered at the school level. All regressions include controls for the baseline value of the dependent variable (wherever available). The dependent variable in Column (1) is the global index on empowerment, in Column (2) is the sub-index on mobility, in Column (3) is the sub-index on aspirations, in Column (4) is the sub-index on locus of control, in Column (5) is the sub-index on fertility and marriage, in Column (6) the sub-index on bargaining power, in Column (7) is the sub-index on pro-sociality, and in Column (8) the sub-index on self-image. All indices have been variance-weighted using the methodology of Anderson (2008). Endline indices contain imputed values if less than 10% of the variables in the index had missing values for an observation. The effect sizes are calculated as the % change compared with the control group means. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Impact on Educational Outcomes

Dependent variable:	Days absent	Days late	Dropouts	Grade Transition	Mathematics	English
	(1)	(2)	(3)	(4)	(5)	(6)
Pooled Treatment	-0.29***	-1.45***	-0.02	-0.03	0.11**	0.04
	(0.09)	(0.10)	(0.02)	(0.02)	(0.06)	(0.05)
Payment Arm	-0.28***	-1.39***	-0.03	-0.02	0.13*	0.08
	(0.10)	(0.11)	(0.02)	(0.02)	(0.07)	(0.06)
No Payment Arm	-0.29***	-1.53***	-0.01	-0.04	0.07	-0.01
	(0.10)	(0.11)	(0.02)	(0.02)	(0.06)	(0.07)
Effect size: Pooled Treatment	28.7%	66.2%	33.3%	3.2%		
Effect size: Payment Arm	27.7%	63.5%	50%	2.1%		
Effect size: No Payment Arm	28.7%	69.8%	16.6%	3.2%		
Observations	1952	1952	2448	1931	2001	2001
Control group mean	1.01	2.19	0.06	0.94	0.00	0.00
Payment Arm = No Payment Arm (p-value)	0.91	0.11			0.35	0.29

NOTES: Standard errors in parentheses are clustered at the school level. The regressions include controls for the baseline value of the dependent variable (wherever available). The dependent variable in Column (1) is the number of days the girl missed school in the last week, in Column (2) is the number of days the girl was late in the previous week, in Column (3) is a dummy = 1 if the girl dropped out of school, in (4) is a dummy = 1 if the girl progressed to a higher grade, conditional on not dropping out, in (5) and (6) is the standardized learning assessment score in Mathematics and English self-administered test. The effect sizes are calculated as the % change compared with the control group means. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Medium Run Impact on Girls School Attendance

Dependent variable:	Days Missed (T1 2019)	Days Missed (T2 2019)	Days missed (T1 2020)	Days missed (T1 2021)
	(1)	(2)	(3)	(4)
Treatment	-3.77***	-3.40***	-2.18***	-4.80***
	(0.92)	(0.79)	(0.76)	(1.35)
Payment Arm	-3.44***	-3.11***	-2.79***	-4.70***
	(1.11)	(0.98)	(0.71)	(1.36)
No Payment Arm	-4.16***	-3.74***	-1.11	-4.88***
	(0.87)	(0.78)	(1.37)	(1.42)
Grade girls	7, 8 & 9	7, 8 & 9	8, 9 & 10	9, 10 & 11
Effect size: Pooled Treatment	45.0%	40.3%	29.2%	55.5%
Effect size: Payment Arm	41.1%	36.8%	37.4%	54.4%
Effect size: No Payment Arm	49.7%	44.3%	14.9%	56.5%
Observations	1644	1641	983	561
Control group mean	8.37	8.44	7.46	8.64
Pay=No Pay (p-value)	0.39	0.46	0.22	0.77

NOTES: Standard errors in parentheses are clustered at the school level. All regressions include grade-fixed effects. The analysis is done with the sub-sample of girls that are still enrolled at the time of the measurement. That is the reason for the change in the sample size every year. The dependent variable in Columns (1) and (2) is the number of days missed by a girl in a term in 2019, Column (3) in 2020, and Column (4) in 2021. All the columns use data collected from the schools by the teams of District Educational Board Secretaries and coordinated by World Bicycle Relief. The Monitoring and Evaluation team of WBR audited 55% of the schools in 2019, 49% in 2020, and 97% in 2021. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6: Medium Run Impact on Girls Dropout and Grade Transition

Dependent variable:	Dropouts 2019	Grade Transition 2019	Dropouts 2020	Grade Transition 2020	Dropouts 2021	Grade Transition 2021
	(1)	(2)	(3)	(4)	(5)	(6)
Pooled Treatment	-0.09***	0.05	-0.09***	0.02	-0.10***	0.11**
	(0.02)	(0.03)	(0.03)	(0.04)	(0.03)	(0.05)
Payment Arm	-0.09***	0.04	-0.11***	0.01	-0.12***	0.14**
	(0.03)	(0.03)	(0.03)	(0.05)	(0.04)	(0.06)
No Payment Arm	-0.09***	0.05	-0.08**	0.04	-0.07	0.06
	(0.03)	(0.04)	(0.04)	(0.05)	(0.04)	(0.05)
Grade girls	7, 8 & 9	7, 8 & 9	8, 9 & 10	8, 9 & 10	9, 10 & 11	9, 10 & 11
Effect size: Pooled Treatment	37.5%	6.6%	21.4%	2.7%	17.5%	19.3%
Effect size: Payment Arm	37.5%	5.3%	26.2%	1.3%	21.0%	24.6%
Effect size: No Payment Arm	37.5%	6.6%	19.0%	5.4%	12.3%	10.5%
Observations	2467	1985	2467	1545	2467	940
Control group mean	0.24	0.75	0.42	0.74	0.57	0.57
Payment Arm = No Payment Arm (p-value)	1.00	0.82	0.58	0.58	0.17	0.24

NOTES: Standard errors in parentheses are clustered at the school level. All regressions include grade-fixed effects. The analysis for grade transition is done with the sub-sample of girls that are still enrolled at the time of the measurement. The difference between the sample size in Table 5 (on attendance) is due to the fact that we have information about the grade transition for those girls that transferred schools. The dependent variable in (1), (3), and (5) is a dummy variable that indicates whether the girl has dropped out from school in the third term of 2019, 2020, and 2021 respectively. In columns (2), (4) and (6) the dependent variable is a dummy variable that indicates if the girls transitioned grades. All the columns use data collected from the schools by the teams of District Educational Board Secretaries and coordinated by World Bicycle Relief. The Monitoring and Evaluation team of WBR audited 55% of the schools in 2019, 49% in 2020, and 97% in 2021. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7: Heterogeneous Impact on Empowerment Outcomes by Baseline Time Taken to School

Dependent variable:	Index of						
	Mobility	Aspiration	Locus of Control	Fertility & Marriage	Bargaining	Pro-Sociality	Self-Image
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.02	0.08	0.21**	0.11	0.30***	0.09	0.15*
	(0.09)	(0.08)	(0.09)	(0.09)	(0.07)	(0.06)	(0.09)
Treatment X Middle Tercile	0.04	-0.02	0.05	-0.07	-0.07	0.16*	0.04
	(0.12)	(0.10)	(0.11)	(0.11)	(0.10)	(0.10)	(0.12)
Treatment X Top Tercile	0.03	-0.01	-0.21*	0.07	-0.28**	0.05	-0.15
	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.11)	(0.12)
Middle Tercile	-0.06	0.09	-0.03	-0.09	0.06	-0.06	-0.05
	(0.08)	(0.07)	(0.08)	(0.08)	(0.07)	(0.07)	(0.09)
Top Tercile	-0.01	0.18**	0.15*	-0.01	0.21**	0.03	0.15*
	(0.09)	(0.08)	(0.09)	(0.08)	(0.08)	(0.07)	(0.08)
Observations	1939	1926	2006	1948	2011	1874	1889
Control group mean (Bottom Tercile)	0.36	1.35	0.46	1.01	0.34	0.80	0.66

NOTES: Standard errors in parentheses are clustered at the school level. All regressions include controls for the baseline value of the dependent variable (wherever available). The dependent variable in Column (1) is the index on mobility, in Column (2) is the index on aspirations, in Column (3) is the index on locus of control, in Column (4) is the index on fertility and marriage, in Column (5) is the index of bargaining (not including the variables on rebellion), in Column (6) is the index of prosociality, and in Column (7) is the index of self-image. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 8: Heterogeneous Impact on Educational Outcomes by Baseline Time Taken to School

Dependent variable:	Bike Access	Days Absent	Time to school	Days Late	Mathematics	English
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.88***	-0.22**	-28.26***	-1.27***	0.12	0.03
	(0.03)	(0.11)	(4.25)	(0.14)	(0.07)	(0.08)
Treatment X Middle Tercile	0.02	-0.27*	-9.45*	-0.27*	0.03	0.09
	(0.03)	(0.14)	(5.58)	(0.16)	(0.08)	(0.08)
Treatment X Top Tercile	0.00	0.12	-15.75**	-0.35**	-0.09	-0.11
	(0.03)	(0.19)	(6.14)	(0.18)	(0.11)	(0.10)
Middle tercile	-0.00	0.14	11.95**	0.14	-0.07	-0.13**
	(0.01)	(0.11)	(5.32)	(0.15)	(0.05)	(0.06)
Top tercile	-0.00	-0.04	5.31	0.08	0.02	-0.03
	(0.02)	(0.13)	(7.68)	(0.15)	(0.07)	(0.08)
Observations	2001	1952	1879	1952	2001	2001
Control group mean (Bottom Tercile)	0.02	0.96	94.00	2.12	0.06	0.07

NOTES: Standard errors in parentheses are clustered at the school level. All regressions include controls for the baseline value of the dependent variable (wherever available). The dependent variable in Column (1) is a dummy for whether the girl has access to a bicycle, in Column (2) is the number of days the girl missed school in the last week, in Column (3) is the time spent traveling to school (in minutes) each way, in Column (4) is the number of days in the last week the girl arrived late to school, in Column (5) is the standardized learning assessment score in Mathematics, and in Column (6) is the standardized learning assessment score in English. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix Tables: For Online Publication

Table A.1: Descriptive Statistics

	Mean	Std.Dev.	Observations
Demographics			
Age	12.89	1.42	2461
Grade in school	6.05	0.82	2469
Ever repeated a grade	0.36	0.48	2469
Both parents alive	0.81	0.39	2467
Household size	6.39	2.91	2468
# of biological brothers	1.68	1.57	2469
# of biological sisters	1.34	1.37	2469
Currently engaged/married	0.14	0.35	2431
Ever been pregnant	0.06	0.23	2434
Mobility			
Mostly walks to school	0.98	0.13	2467
Time spent traveling to school (mins/each way)	109.16	50.52	2291
Mostly travels to school alone	0.27	0.45	2464
# of people that travel to school together	4.38	3.68	2166
Ever teased on way to school (last year)	0.35	0.48	2469
Would walk to school alone if felt safe	0.79	0.41	2464
Would walk to other places alone if felt safe	0.44	0.50	2459
Attendance			
# of days absent from school (last week)	0.88	1.29	2459
# of days arrived late to school (last week)	2.61	1.69	2412
Learning Assessment			
Learning assessment score (Overall)	0.36	0.16	2468
Learning assessment score (English)	0.30	0.17	2468
Learning assessment score (Maths)	0.44	0.20	2468

NOTES: Descriptive statistics of the girls in the estimation sample measured in baseline in 2017.

Table A.2: Balance Table: School Characteristics

	Treatment Groups			p value for test of:		
	Control (N = 55)	No Payment Arm (T2) (N = 20)	Payment Arm (T1) (N = 25)	$1 = 2$	$1 = 3$	$1 = (2 \cup 3)$
	(1)	(2)	(3)	(4)	(5)	(6)
Enrollment 2017	692.75	643.40	686.92	0.31	0.90	0.51
	(187.74)	(186.96)	(192.46)			
Enrollment girls 2017	344.85	318.70	338.04	0.28	0.77	0.41
	(93.89)	(91.98)	(95.58)			
Enrollment boys 2017	347.89	324.70	348.88	0.36	0.97	0.62
	(96.21)	(98.60)	(99.31)			
# teachers	13.47	13.45	12.72	0.99	0.54	0.72
	(6.46)	(6.44)	(4.28)			
% teachers living school	78.48	71.98	68.97	0.39	0.16	0.15
	(27.26)	(29.82)	(28.53)			
Km closest town	51.02	38.92	45.28	0.18	0.50	0.30
	(57.37)	(19.19)	(17.77)			
Km closest tarmac road	25.06	24.63	25.90	0.93	0.84	0.94
	(19.39)	(18.78)	(16.93)			
Km closest secondary school	19.31	19.82	22.10	0.92	0.58	0.64
	(15.76)	(18.78)	(22.02)			
Feeding program	0.04	0.00	0.04	0.16	0.94	0.68
	(0.19)	(0.00)	(0.20)			
Sanitation program	0.60	0.60	0.60	1.00	1.00	1.00
	(0.49)	(0.50)	(0.50)			
Other program	0.35	0.45	0.24	0.42	0.33	0.90
	(0.48)	(0.51)	(0.44)			
# toilets girls	5.78	4.70	4.88	0.09	0.14	0.05
	(2.68)	(2.34)	(2.40)			
# toilets boys	5.24	3.85	4.84	0.01	0.53	0.10
	(2.83)	(1.81)	(2.46)			
# classrooms 2017	8.44	7.95	8.08	0.58	0.59	0.51
	(3.43)	(3.30)	(2.34)			
Access to Library	0.31	0.20	0.16	0.33	0.13	0.13
	(0.47)	(0.41)	(0.37)			
Access to computers	0.95	0.95	0.92	0.94	0.69	0.80
	(0.23)	(0.22)	(0.28)			

NOTES: Balance test of the school characteristics by treatment groups. The data presented in this table was collected at baseline in 2017. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.3: Balance Table: Girls Characteristics

	Treatment Groups			p-value for test of:		
	Control (N = 1357)	No Payment Arm (T2) (N = 500)	Payment Arm (T1) (N = 614)	$1 = 2$	$1 = 3$	$1 = (2 \cup 3)$
	(1)	(2)	(3)	(4)	(5)	(6)
Age	12.88	12.96	12.85	0.27	0.62	0.79
	(1.43)	(1.44)	(1.36)			
Grade in school	6.02	6.09	6.07	0.07	0.13	0.04
	(0.82)	(0.82)	(0.81)			
Ever repeated a grade	0.36	0.37	0.36	0.59	0.81	0.66
	(0.48)	(0.48)	(0.48)			
Both parents alive	0.80	0.80	0.83	0.99	0.21	0.47
	(0.40)	(0.40)	(0.38)			
Household size	6.46	6.21	6.39	0.08	0.67	0.23
	(2.88)	(2.54)	(3.23)			
# of biological brothers	1.70	1.79	1.56	0.31	0.06	0.57
	(1.53)	(1.74)	(1.49)			
# of biological sisters	1.36	1.25	1.35	0.12	0.88	0.35
	(1.40)	(1.30)	(1.37)			
Currently engaged/married	0.15	0.16	0.16	0.85	0.76	0.80
	(0.36)	(0.36)	(0.37)			
Ever been pregnant	0.06	0.06	0.05	0.77	0.24	0.36
	(0.24)	(0.23)	(0.22)			
# of meals with Meat	1.62	1.72	1.78	0.24	0.04	0.04
	(1.52)	(1.63)	(1.58)			
# of days with more than 1 meal	5.77	5.78	5.74	0.96	0.74	0.83
	(2.36)	(2.39)	(2.41)			
# of days with not enough food	0.97	0.81	0.92	0.06	0.54	0.14
	(1.67)	(1.46)	(1.68)			
Socio-economic index (PCA)	-0.08	0.04	0.14	0.12	0.02	0.01
	(1.51)	(1.50)	(2.20)			
Locus of control index (PCA)	0.01	-0.14	0.08	0.13	0.45	0.69
	(1.90)	(1.89)	(1.87)			
Self esteem index (PCA)	-0.05	0.05	0.08	0.22	0.07	0.06
	(1.51)	(1.55)	(1.55)			
Gender attitudes index (PCA)	-0.03	0.00	0.07	0.64	0.14	0.23
	(1.36)	(1.34)	(1.34)			

NOTES: Balance test of the girls characteristics by treatment groups. The data presented in this table was collected at baseline in 2017. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.4: Tracking and Attrition

	Total	Control	Payment	No Payment
Tracked and surveyed	2,028	1,071	533	424
In school	1,789	920	481	388
Second stage tracking	239	151	52	36
Attrition Rate (not weighted)	17.9%	21.1%	13.2%	15.2%
Effective Attrition Rate (weighted)	8.7%	10.1%	7.8%	5.6%

NOTES: The sample analyzed in this table are the 2,471 potential endline respondents in 2018.

Table A.5: Determinants of Sample Attrition

	Attrited Endline
Payment Arm	-0.013
	(0.026)
No Payment Arm	-0.053*
	(0.022)
Payment Arm X Socio-Economic Index (PCA)	-0.007
	(0.010)
No Payment Arm X Socio-Economic Index (PCA)	-0.007
	(0.011)
Payment Arm X Locus of Control Index (PCA)	-0.008
	(0.012)
No Payment Arm X Locus of control Index (PCA)	-0.000
	(0.009)
Payment Arm X Self-Esteem Index (PCA)	-0.006
	(0.012)
No Payment Arm X Self-Esteem Index (PCA)	0.002
	(0.015)
Payment Arm X Gender Attitudes Index (PCA)	0.015
	(0.015)
No Payment Arm X Gender Attitudes Index (PCA)	0.018
	(0.014)
Socio-Economic Index (PCA)	0.008
	(0.007)
Locus of Control Index (PCA)	0.001
	(0.008)
Self-Esteem Index (PCA)	0.001
	(0.006)
Gender Attitudes Index (PCA)	0.001
	(0.009)
Control group mean	0.101
Payment Arm mean	0.089
No Payment Arm mean	0.049
Payment Arm = Control (p-value)	0.632
No Payment Arm = Control (p-value)	0.016
Payment Arm = No Payment Arm (p-value)	0.054
Observations	2,467

NOTES: Standard errors in parentheses and clustered at the school level. This includes the survey weights used because of the two-stage sampling procedure. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.6: Lee Bounds: Educational Outcomes

Dependent variable:	Days Absent			Days Late			Time to School			Mathematics			English		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Pooled Treatment	-0.28***	-0.43***	-0.28***	-1.45***	-1.55***	-1.45***	-34.82***	-41.99***	-32.79***	0.11*	0.08	0.15***	0.03	-0.03	0.06
	(0.08)	(0.08)	(0.08)	(0.10)	(0.10)	(0.10)	(2.94)	(2.36)	(2.92)	(0.06)	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)
Observations	1952	1923	1952	1952	1927	1952	1879	1845	1841	2001	1982	1973	2001	1963	1986
Control group mean	1.01	1.01	1.01	2.19	2.19	2.19	103.77	103.70	103.77	0.00	0.00	0.00	0.00	0.00	0.00
Lee Bounding	No	Upper	Lower	No	Upper	Lower	No	Upper	Lower	No	Upper	Lower	No	Upper	Lower

NOTES: Standard errors in parentheses are clustered at the school level. All regressions include controls for demographics and the baseline value of the dependent variable (wherever available). We follow Lee (2009) and the following two assumptions to do the Lee Bounding: (i) Random assignment of treatment, and (ii) Monotonicity assumption about selection mechanism (treatment affects attrition only in one direction, and girls would have attrited if in control but the girl does not attrit because of receiving the bicycle). We calculated the proportion of sample to trim with the following formula:

$$pTrim = (\frac{0.93 - 0.89}{0.93}) * 100 = 4.3$$

The tracking rate of the control group = 89.9, and the tracking rate of the pooled treatment = 93. The sample was trimmed such that the share of observed girls is equal for both groups (we found more girls in the treatment groups than the control group, therefore, we trim the treatment group). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.7: Lee Bounds: Empowerment Outcomes

Dependent variable:	Mobility & Safety			Aspirations			Control			Fertility		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Pooled Treatment	0.00	-0.08	0.10*	0.06	0.06	0.20***	0.16***	0.16***	0.26***	0.10	0.02	0.24***
	(0.06)	(0.06)	(0.06)	(0.05)	(0.05)	(0.04)	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)	(0.05)
Observations	1935	1896	1897	1919	1919	1881	2005	2005	1965	1945	1905	1906
Control group mean	0.33	0.33	0.33	1.44	1.44	1.44	0.50	0.50	0.50	0.95	0.95	0.95
Lee Bounding	No	Upper	Lower	No	Upper	Lower	No	Upper	Lower	No	Upper	Lower

NOTES: Standard errors in parentheses are clustered at the school level. All regressions include controls for demographics and the baseline value of the dependent variable (wherever available). All columns report coefficients from a linear model. The dependent variable in (1-3) is the index on mobility, in (4-6) is the index on aspirations, in (7-9) is the index on locus of control, and in (10-12) is the index on fertility and marriage. All indices have been variance-weighted using the methodology of Anderson (2008). Endline indices contain imputed values if less than 10% of the variables in the index had missing values for an observation. We follow Lee (2009) and the following two assumptions to do the Lee Bounding: (i) Random assignment of treatment, and (ii) Monotonicity assumption about selection mechanism (treatment affects attrition only in one direction, and girls would have attrited if in control but the girl does not attrit because of receiving the bicycle). We calculated the proportion of sample to trim with the following formula:

$$pTrim = (\frac{0.93 - 0.89}{0.93}) * 100 = 4.3$$

The tracking rate of the control group = 89.9, and the tracking rate of the pooled treatment = 93. The sample was trimmed such that the share of observed girls is equal for both groups (we found more girls in the treatment groups than the control group, therefore, we trim the treatment group). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.8: Lee Bounds: Behavioral Outcomes

Dependent variable:	Bargaining			Pro-Sociality			Self-Image		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Pooled Treatment	0.19***	0.19***	0.31***	0.14**	0.08	0.23***	0.11*	0.11*	0.20***
	(0.05)	(0.05)	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Observations	1988	1988	1948	1874	1841	1843	1889	1889	1851
Control group mean	0.40	0.40	0.40	0.77	0.77	0.77	0.68	0.68	0.68
Lee Bounding	No	Upper	Lower	No	Upper	Lower	No	Upper	Lower

NOTES: Standard errors in parentheses are clustered at the school level. All regressions include controls for demographics and the baseline value of the dependent variable (wherever available). All columns report coefficients from a linear model. The dependent variable in (1-3) is the index of bargaining (not including the variables on rebellion), in (4-6) is an index of pro-sociality, and in (7-9) an index of self-image. All indices have been variance-weighted using the methodology of Anderson (2008). Endline indices contain imputed values if less than 10% of the variables in the index had missing values for an observation. We follow Lee (2009) and the following two assumptions to do the Lee Bounding: (i) Random assignment of treatment, and (ii) Monotonicity assumption about selection mechanism (treatment affects attrition only in one direction, and girls would have attrited if in control but the girl does not attrit because of receiving the bicycle). We calculated the proportion of sample to trim with the following formula:

$$pTrim = (\frac{0.93 - 0.89}{0.93}) * 100 = 4.3$$

. The tracking rate of the control group = 89.9, and the tracking rate of the pooled treatment = 93. The sample was trimmed such that the share of observed girls is equal for both groups (we found more girls in the treatment groups than the control group, therefore, we trim the treatment group). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.9: Multiple Hypothesis Correction for First Stage Outcomes

Dependent variable:	Access	Time to school	Safety	Teased	Missed School
	(1)	(2)	(3)	(4)	(5)
Pooled Treatment	0.88***	-34.82***	0.11**	-0.08***	-0.06***
	(0.02)	(2.94)	(0.05)	(0.03)	(0.02)
Payment Arm	0.89***	-36.44***	0.13*	-0.08***	-0.06***
	(0.03)	(3.33)	(0.06)	(0.03)	(0.02)
No Payment Arm	0.88***	-33.17***	0.09	-0.08**	-0.06**
	(0.02)	(4.04)	(0.07)	(0.04)	(0.02)
W-Y p-value (Pooled Treatment)	0.00	0.00	0.04	0.00	0.00
B-H p-value (Pooled Treatment)	0.00	0.00	0.19	0.08	0.08
W-Y p-value (Payment Arm)	0.00	0.00	0.04	0.00	0.00
B-H p-value (Payment Arm)	0.00	0.00	0.05	0.02	0.01
W-Y p-value (No Payment Arm)	0.00	0.00	0.19	0.08	0.08
B-H p-value (No Payment Arm)	0.00	0.00	0.15	0.06	0.06
Observations	2001	1879	1938	1954	1953
Control group mean	0.02	103.77	0.31	0.37	0.18
Payment Arm = No Payment Arm (p-value)	0.92	0.45	0.65	0.90	0.80

NOTES: Standard errors clustered at the school level (in parentheses). All regressions include controls for demographics and the baseline value of the dependent variable (wherever available). The dependent variable in (Column 1) is a dummy indicating whether the girl has access to a bicycle, and in (2) is the time spent traveling to school (in minutes) each way, in (3) is the sub-index of perceived safety, in (4) is the probability of the girl being teased or whistled at on the way to school, and in (5) is the probability that a girl misses school or leaves early for a home for safety concerns. The details on the components of the index in column (3) are in the Appendix. The p-values adjusted for multiple hypotheses testing are corrected among the variables in this table. W-Y p-value gives the p-value adjusted for multiple hypothesis tests using the methodology of Westfall and Young (1993), and B-H p-value gives the p-value adjusted for multiple hypothesis tests using the methodology of Bonferroni-Holm (1979). All the columns use survey data. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.10: Multiple Hypothesis Correction for Education Outcomes

Dependent variable:	Days absent	Days late	Dropouts	Grade Transition	Mathematics	English
	(1)	(2)	(3)	(4)	(5)	
Pooled Treatment	-0.28***	-1.45***	-0.02	-0.03	0.11*	0.03
	(0.08)	(0.10)	(0.01)	(0.02)	(0.06)	(0.05)
Payment Arm	-0.27***	-1.39***	-0.03	-0.02	0.13*	0.06
	(0.09)	(0.11)	(0.02)	(0.02)	(0.07)	(0.06)
No Payment Arm	-0.27***	-1.53***	-0.01	-0.03	0.07	-0.02
	(0.10)	(0.10)	(0.02)	(0.02)	(0.06)	(0.07)
W-Y p-value (Pooled Treatment)	0.02	0.00	0.56	0.65	0.57	0.74
B-H p-value (Pooled Treatment)	0.02	0.00	0.84	0.36	0.87	0.87
W-Y p-value (Payment Arm)	0.02	0.00	0.56	0.65	0.57	0.74
B-H p-value (Payment Arm)	0.02	0.00	0.73	0.77	0.77	0.77
W-Y p-value (No Payment Arm)	0.02	0.00	0.84	0.36	0.87	0.87
B-H p-value (No Payment Arm)	0.03	0.00	1.00	0.42	1.00	1.00
Observations	1952	1952	2448	1931	2001	2001
Control group mean	1.01	2.19	0.06	0.94	0.00	0.00
Payment Arm = No Payment Arm (p-value)	0.98	0.13			0.44	0.30

NOTES: Standard errors clustered at the school level (in parentheses). All regressions include controls for demographics and baseline value for Columns (1), (2), and (5). Columns (1), (2), and (5) report coefficients from a linear model, while columns (3) and (4) report marginal effects from a logit model. The dependent variable in (1) is the no. of days the girl missed school in the last week, in (2) is the no. of days in the last week the girl arrived late to school, in Column (3) is a dummy = 1 if the girl dropped out of school, in (4) is a dummy = 1 if the girl progressed to a higher grade, conditional on not dropping out, in (5) and (4) is the score of a Mathematics and English test. The p-values adjusted for multiple hypotheses testing are corrected with the outcome variables in Table 4b of the main outcomes in empowerment. W-Y p-value gives the p-value adjusted for multiple hypothesis tests using the methodology of Westfall and Young (1993), and B-H p-value gives the p-value adjusted for multiple hypothesis tests using the methodology of Bonferroni-Holm (1979). All the columns use survey data. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.11: Multiple Hypothesis Correction for Empowerment Outcomes

		Sub-index of						
Dependent variable:	Index of Empowerment	Mobility	Aspirations	Control	Fertility	Bargaining	Pro-sociality	Self-Image
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pooled Treatment	0.14**	-0.01	0.06	0.16***	0.10	0.19***	0.14**	0.11*
	(0.06)	(0.06)	(0.05)	(0.06)	(0.07)	(0.05)	(0.06)	(0.06)
Payment Arm	0.20**	-0.02	0.11**	0.18**	0.18**	0.18***	0.11*	0.13*
	(0.08)	(0.07)	(0.05)	(0.07)	(0.09)	(0.06)	(0.06)	(0.07)
No Payment Arm	0.06	-0.01	-0.00	0.13*	-0.01	0.18***	0.16*	0.09
	(0.07)	(0.07)	(0.08)	(0.07)	(0.08)	(0.07)	(0.08)	(0.07)
Observations	1863	1991	1919	2005	1945	1988	1874	1889
W-Y p-value	0.04	0.75	0.44	0.02	0.36	0.00	0.07	0.21
B-H p-value	0.12	0.81	0.50	0.04	0.39	0.01	0.12	0.21
W-Y p-value (Payment Arm)	0.01	0.71	0.11	0.09	0.11	0.01	0.17	0.12
B-H p-value (Payment Arm)	0.04	0.77	0.16	0.09	0.16	0.04	0.16	0.16
W-Y p-value (No Payment Arm)	0.86	1.00	1.00	0.39	1.00	0.16	0.40	0.72
B-H p-value (No Payment Arm)	1.00	1.00	1.00	0.36	1.00	0.10	0.36	1.00

NOTES: Standard errors clustered at the school level (in parentheses). All regressions include controls for demographics and the baseline value of the dependent variable (wherever available). All columns report coefficients from a linear model. The dependent variable in (1) is the index of empowerment, in (2) a subindex of mobility, in (3) is the subindex of aspirations, in (4) is the subindex of locus of control, and in (5) is the subindex on fertility and marriage, in (6) the subindex of bargaining (not including the variables on rebellion), in (7) is a subindex of pro-sociality and in (8) a subindex of self-image. The details on the components of each index are in the Appendix. All indices have been variance-weighted using the methodology of Anderson (2008). All the columns use survey data. Endline indices contain imputed values if less than 10% of the variables in the index had missing values for an observation. W-Y p-value gives the p-value adjusted for multiple hypothesis tests using the methodology of Westfall and Young (1993), and B-H p-value gives the p-value adjusted for multiple hypothesis tests using the methodology of Bonferroni-Holm (1979). All the columns use survey data. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.12: Multiple Hypothesis Correction for Primary Outcomes as described in the Pre-Analysis Plan

Dependent variable:	Index of Empowerment	Days absent	Days late	Dropouts	Grade Transition	Mathematics	English
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Pooled Treatment	0.14**	-0.28***	-1.45***	-0.02	-0.03	0.11*	0.03
	(0.06)	(0.08)	(0.10)	(0.01)	(0.02)	(0.06)	(0.05)
Payment Arm	0.20**	-0.27***	-1.39***	-0.03	-0.02	0.13*	0.06
	(0.08)	(0.09)	(0.11)	(0.02)	(0.02)	(0.07)	(0.06)
No Payment Arm	0.06	-0.27***	-1.53***	-0.01	-0.03	0.07	-0.02
	(0.07)	(0.10)	(0.10)	(0.02)	(0.02)	(0.06)	(0.07)
Observations	1863	1952	1952	2448	1931	2001	2001
W-Y p-value	0.06	0.00	0.00	0.35	0.25	0.48	0.98
B-H p-value	0.10	0.01	0.00	0.56	0.44	0.72	0.98
W-Y p-value (Payment Arm)	0.02	0.02	0.00	0.56	0.65	0.57	0.74
B-H p-value (Payment Arm)	0.02	0.02	0.00	0.73	0.77	0.77	0.77
W-Y p-value (No Payment Arm)	0.89	0.04	0.00	0.89	0.48	0.89	0.89
B-H p-value (No Payment Arm)	1.00	0.03	0.00	1.00	0.53	1.00	1.00
Control group mean	1.57	1.01	2.19	0.06	0.94	0.00	0.00

NOTES: Standard errors clustered at the school level (in parentheses). All regressions include controls for demographics and the baseline value of the dependent variable (wherever available). Columns (1), (2), (3), (6), and (7) report coefficients from a linear model, while columns (3) and (4) report marginal effects from a logit model. The dependent variable in (1) is the index of empowerment, in (2) is the no. of days the girl missed school in the last week, in (3) is the no. of days in the last week the girl arrived late to school, in Column (4) is a dummy = 1 if the girl dropped out of school, in (5) is a dummy = 1 if the girl progressed to a higher grade, conditional on not dropping out, in (6) and (7) is the score of a Mathematics and English test, respectively. W-Y p-value gives the p-value adjusted for multiple hypothesis tests using the methodology of Westfall and Young (1993), and B-H p-value gives the p-value adjusted for multiple hypothesis tests using the methodology of Bonferroni-Holm (1979). All the columns use survey data. The variables in this table were pre-specified as primary outcomes in the PAP. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.13: Multiple Hypothesis Correction for Primary Outcomes as described in the Pre-Analysis Plan w/o test scores

Dependent variable:	Index Empowerment	Days absent	Days late	Dropouts	Grade Transition
	(1)	(2)	(3)	(4)	(5)
Pooled Treatment	0.14**	-0.28***	-1.45***	-0.02	-0.03
	(0.06)	(0.08)	(0.10)	(0.01)	(0.02)
Payment Arm	0.20**	-0.27***	-1.39***	-0.03	-0.02
	(0.08)	(0.09)	(0.11)	(0.02)	(0.02)
No Payment Arm	0.06	-0.27***	-1.53***	-0.01	-0.03
	(0.07)	(0.10)	(0.10)	(0.02)	(0.02)
Observations	1863	1952	1952	2448	1931
W-Y p-value	0.04	0.00	0.00	0.15	0.15
B-H p-value	0.06	0.00	0.00	0.22	0.22
W-Y p-value (Payment Arm)	0.02	0.02	0.00	0.38	0.43
B-H p-value (Payment Arm)	0.01	0.01	0.00	0.37	0.39
W-Y p-value (No Payment Arm)	0.68	0.03	0.00	0.68	0.36
B-H p-value (No Payment Arm)	0.97	0.02	0.00	0.97	0.32
Control group mean	1.57	1.01	2.19	0.06	0.94

NOTES: Standard errors clustered at the school level (in parentheses). All regressions include controls for demographics and the baseline value of the dependent variable (wherever available). Columns (1), (2), and (3) report coefficients from a linear model, while Columns (4) and (5) report marginal effects from a logit model. The dependent variable in (1) is the index of empowerment, in (2) is the no. of days the girl missed school in the last week, in (3) is the number of days in the last week the girl arrived late to school, in Column (4) is a dummy = 1 if the girl dropped out of school, in (5) is a dummy = 1 if the girl progressed to a higher grade, conditional on not dropping out. W-Y p-value gives the p-value adjusted for multiple hypothesis tests using the methodology of Westfall and Young (1993), and B-H p-value gives the p-value adjusted for multiple hypothesis tests using the methodology of Bonferroni-Holm (1979). All the columns use survey data. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.14: Minimum Detectable Effects

Outcome Variables:	MDE	N	Mean	N	Mean	N	Mean	ICC
	Pooled	Control	Control	No Payment Arm	No Payment Arm	Payment Arm	Payment Arm	
# Days absent	0.162	1029	1.013	410	0.715	517	0.683	0.072
# Days Late	0.162	1029	2.188	410	0.566	517	0.704	0.072
Time to school	0.154	997	103.774	386	68.422	500	63.916	0.065
Drop-outs	0.124	1356	0.065	483	0.052	613	0.039	0.042
Grade Transition	0.203	1015	0.937	403	0.893	515	0.918	0.114
Access	0.169	1061	3.022	414	3.94	530	3.928	0.078
Index of Mobility	0.176	1023	0.327	404	0.376	514	0.347	0.085
Index of Aspiration	0.137	1014	1.442	403	1.431	506	1.591	0.051
Index of Locus of Control	0.161	1064	0.502	419	0.664	526	0.704	0.072
Index of Fertility	0.201	1039	0.954	420	0.981	518	1.173	0.113
Index of Bargaining	0.183	1050	0.399	419	0.619	523	0.611	0.093
Index of Pro-Sociality	0.16	987	0.772	395	0.945	496	0.909	0.07
Index of Self-image	0.158	997	0.68	397	0.79	499	0.832	0.069

NOTES: This table reports MDEs at $(1-\beta)=0.8$ and $\alpha=0.05$. Define μ_i as the mean outcome in arm $i \in \{1,2,c\}$, then the MDE relies on a t-test with $H_0: \frac{\mu_1+\mu_2}{2} - \mu_{control} = 0$, i.e. the program has an overall effect.

Table A.15: Impact on Index of Focus (d2 Test)

Dependent variable:	Index of Focus	Speed	Accuracy
	(1)	(2)	(3)
		Panel: A	
Pooled Treatment	0.07	6.11	9.70
	(0.08)	(9.04)	(9.08)
		Panel: B	
Payment Arm	0.15	14.55	18.45
	(0.11)	(11.95)	(12.37)
No Payment Arm	-0.05	-5.73	-2.49
	(0.09)	(9.47)	(9.19)
Observations	1932	1932	1932
Control group mean	0.55	459.06	393.45
Payment Arm = No Payment Arm (p-value)	0.12	0.12	0.12

NOTES: Standard errors in parentheses are clustered at the school level. All regressions include controls for demographics and the baseline value of the dependent variable (wherever available). All columns report coefficients from a linear model. The dependent variable in (1) is the index of focus (variance-weighted index of speed and accuracy), in (2) is a measure of speed, which is the total number of observations processed in the d2 test, in (3) is a measure of accuracy, which is the correct number of observations processed in the d2 test. All indices have been variance-weighted using the methodology of Anderson (2008). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix Tables with Controls: For Online Publication

Table B.1: First Stage Outcomes with Controls

Dependent variable:	Access	Time to school	Sub-Index of Safety	Teased	Missed School
	(1)	(2)	(3)	(4)	(5)
Pooled Treatment	0.88***	-34.82***	0.11**	-0.08***	-0.06***
	(0.02)	(2.94)	(0.05)	(0.03)	(0.02)
Payment Arm	0.89***	-36.44***	0.13*	-0.08***	-0.06***
	(0.03)	(3.33)	(0.06)	(0.03)	(0.02)
No Payment Arm	0.88***	-33.17***	0.09	-0.08**	-0.06**
	(0.02)	(4.04)	(0.07)	(0.04)	(0.02)
Observations	2001	1879	1938	1954	1953
Control group mean	0.02	103.77	0.31	0.37	0.18
Payment Arm = No Payment Arm (p-value)	0.92	0.45	0.65	0.90	0.80

NOTES: Standard errors clustered at the school level (in parentheses). All regressions include controls for demographics and the baseline value of the dependent variable (wherever available). All regressions include controls for demographics and the baseline value of the dependent variable (wherever available). The dependent variable in (Column 1) is a dummy indicating whether the girl has access to a bicycle, and in (2) is the time spent traveling to school (in minutes) each way, in (3) is the subindex of mobility, in (4) is the subindex of perceived safety, in (5) is the probability of the girl being teased or whistled at on the way to school, and in (6) is the probability that a girl misses school or leaves early for a home for safety concerns. The details on the components of the index in column (3) are in the Appendix. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B.2: Impact on Time Use with Controls

Dependent variable:	School Chores	ECA	Home Work	Household Chores	Income Generation	Friends
	(1)	(2)	(3)	(4)	(5)	(6)
				Panel: A		
Pooled Treatment	0.77	1.03	0.93	1.02	0.75**	1.04
	(0.14)	(0.13)	(0.11)	(0.12)	(0.10)	(0.11)
				Panel: B		
Payment Arm	0.91	1.15	1.02	1.01	0.78	1.06
	(0.20)	(0.17)	(0.15)	(0.16)	(0.13)	(0.15)
No Payment Arm	0.62**	0.89	0.81	1.02	0.73**	0.99
	(0.13)	(0.15)	(0.11)	(0.14)	(0.11)	(0.12)
Observations	1938	1925	1931	1997	2000	2005
Payment Arm = No Payment Arm (p-value)	0.15	0.17	0.14	0.95	0.72	0.66

NOTES: Standard errors in parentheses are clustered at the school level. All regressions include controls for demographics and the baseline value of the dependent variable (wherever available). All columns report the odds ratio from an ordered logit model. The dependent variable is the time spent by the girl doing various activities, and takes the following values: 0 - no time spent, 1 - less than 30 minutes, 2 - between 30 and 60 minutes, 3 - between 60 and 90 minutes, 4 - between 90 and 120 minutes, 5 - more than 120 minutes. The activities are: School chores like cleaning in column (1), Extra curricular activities (ECA) in Column (2), Studying and homework in Column (3), Household chores in Column (4), Engaging in income-generating activities in Column (5), and Spending time with friends in Column (6). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B.3: Impact on Measures of Empowerment with Controls

		Sub-index of						
Dependent variable:	Index of Empowerment	Mobility	Aspirations	Control	Fertility	Bargaining	Pro-sociality	Self-Image
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pooled Treatment	0.14**	-0.01	0.06	0.16***	0.10	0.19***	0.14**	0.11*
	(0.06)	(0.06)	(0.05)	(0.06)	(0.07)	(0.05)	(0.06)	(0.06)
Payment Arm	0.20**	-0.02	0.11**	0.18**	0.18**	0.18***	0.11*	0.13*
	(0.08)	(0.07)	(0.05)	(0.07)	(0.09)	(0.06)	(0.06)	(0.07)
No Payment Arm	0.06	-0.01	-0.00	0.13*	-0.01	0.18***	0.16*	0.09
	(0.07)	(0.07)	(0.08)	(0.07)	(0.08)	(0.07)	(0.08)	(0.07)
Observations	1863	1991	1919	2005	1945	1988	1874	1889
Control group mean	1.57	0.25	1.44	0.50	0.95	0.40	0.77	0.68
Payment Arm = No Payment Arm(p-value)	0.11	0.84	0.16	0.55	0.08	0.98	0.58	0.59

NOTES: Standard errors in parentheses are clustered at the school level. All regressions include controls for demographics and the baseline value of the dependent variable (wherever available). The dependent variable in Column (1) is the index on mobility, in Column (2) is the index on aspirations, in Column (3) is the index on locus of control, and in Column (4) is the index on fertility and marriage. All indices have been variance-weighted using the methodology of Anderson (2008). Endline indices contain imputed values if less than 10% of the variables in the index had missing values for an observation. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B.4: Impact on Educational Outcomes with Controls

Dependent variable:	Days absent	Days late	Dropouts	Grade Transition	Mathematics	English
	(1)	(2)	(3)	(4)	(5)	(6)
Pooled Treatment	-0.28***	-1.45***	-0.02	-0.03	0.11*	0.03
	(0.08)	(0.10)	(0.01)	(0.02)	(0.06)	(0.05)
Payment Arm	-0.27***	-1.39***	-0.03	-0.02	0.13*	0.06
	(0.09)	(0.11)	(0.02)	(0.02)	(0.07)	(0.06)
No Payment Arm	-0.27***	-1.53***	-0.01	-0.03	0.07	-0.02
	(0.10)	(0.10)	(0.02)	(0.02)	(0.06)	(0.07)
Observations	1952	1952	2448	1931	2001	2001
Control group mean	1.01	2.19	0.06	0.94	0.00	0.00
Payment Arm = No Payment Arm (p-value)	0.98	0.13			0.44	0.30

NOTES: Standard errors in parentheses are clustered at the school level. All regressions include controls for demographics and the baseline value of the dependent variable (wherever available). The dependent variable in Column (1) is the number of days the girl missed school in the last week, in Column (2) is the number of days the girl was late in the previous week, in Column (3) is a dummy = 1 if the girl dropped out of school, in (4) is a dummy = 1 if the girl progressed to a higher grade, conditional on not dropping out, in (5) and (6) is the standardized learning assessment score in Mathematics and English self administered test. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B.5: Heterogeneous Impact on Educational Outcomes by Baseline Time Taken to School with Controls

Dependent variable:	Bike Access	Days Absent	Time to school	Days Late	Mathematics	English
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.87***	-0.21**	-28.20***	-1.27***	0.12	0.01
	(0.03)	(0.11)	(4.21)	(0.14)	(0.08)	(0.08)
Treatment X Middle Tercile	0.02	-0.27*	-9.13*	-0.29*	0.01	0.09
	(0.03)	(0.14)	(5.38)	(0.16)	(0.08)	(0.08)
Treatment X Top Tercile	0.00	0.13	-15.37**	-0.35*	-0.08	-0.09
	(0.03)	(0.19)	(6.08)	(0.18)	(0.10)	(0.10)
Middle Tercile	-0.00	0.14	12.17**	0.14	-0.07	-0.13**
	(0.01)	(0.11)	(5.27)	(0.15)	(0.05)	(0.06)
Top Tercile	-0.00	-0.04	6.16	0.07	-0.00	-0.05
	(0.02)	(0.13)	(7.57)	(0.15)	(0.07)	(0.08)
Observations	2001	1952	1879	1952	2001	2001
Control group mean (Bottom Tercile)	0.02	0.96	94.00	2.12	0.06	0.07

NOTES: Standard errors in parentheses are clustered at the school level. All regressions include controls for demographics and the baseline value of the dependent variable (wherever available). The dependent variable in Column (1) is a dummy for whether the girl has access to a bicycle, in Column (2) is the number of days the girl missed school in the last week, in Column (3) is the time spent travelling to school (in minutes) each way, in Column (4) is the number of days in the last week the girl arrived late to school, in Column (5) is the standardised learning assessment score in Mathematics, and in Column (6) is the standardised learning assessment score in English. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B.6: Heterogeneous Impact on Educational Outcomes by Baseline Time Taken to School with controls

	Index of						
Dependent variable:	Mobility	Aspiration	Locus of Control	Fertility & Marriage	Bargaining	Pro-Sociality	Self-Image
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.01	0.09	0.19**	0.11	0.29***	0.08	0.14
	(0.09)	(0.08)	(0.09)	(0.09)	(0.08)	(0.06)	(0.09)
Treatment X Middle Tercile	0.04	-0.05	0.05	-0.07	-0.08	0.15	0.03
	(0.12)	(0.10)	(0.11)	(0.11)	(0.10)	(0.09)	(0.12)
Treatment X Top Tercile	0.01	-0.03	-0.20*	0.08	-0.29**	0.03	-0.16
	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.11)	(0.12)
Middle Tercile	-0.06	0.11	-0.02	-0.08	0.08	-0.06	-0.04
	(0.08)	(0.08)	(0.08)	(0.08)	(0.07)	(0.07)	(0.09)
Top Tercile	-0.01	0.18**	0.15*	-0.03	0.22**	0.02	0.14*
	(0.09)	(0.08)	(0.08)	(0.08)	(0.08)	(0.07)	(0.08)
Observations	1935	1919	2005	1945	1988	1874	1889
Control group mean (Bottom Tercile)	0.36	1.36	0.46	1.02	0.35	0.80	0.66

NOTES: Standard errors in parentheses are clustered at the school level. All regressions include controls for demographics and baseline value of the dependent variable (wherever available). The dependent variable in Column (1) is the index on mobility and safety, in Column (2) is the index on aspirations, in Column (3) is the index on locus of control, in Column (4) is the index on fertility and marriage, in Column (5) is the index of bargaining (not including the variables on rebellion), in Column (6) is the index of pro sociality, and in Column (7) is the index of self image. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix Figures

Figure A.1: Map of the Distribution of Schools in the Study Sample

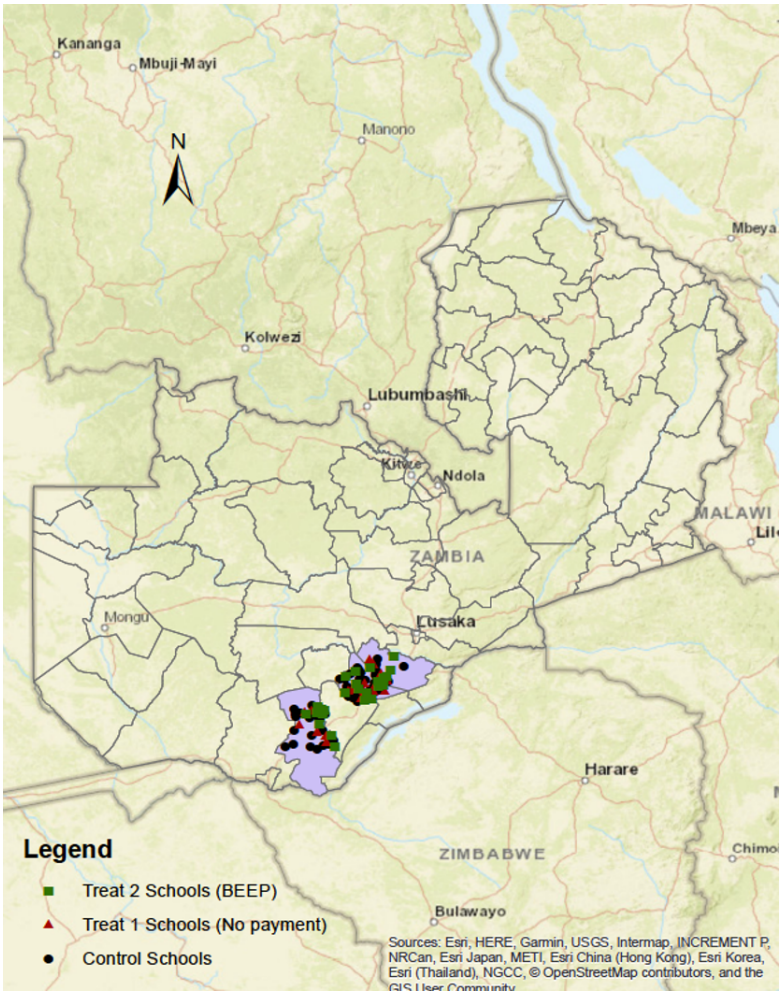


Figure A.2: Timeline

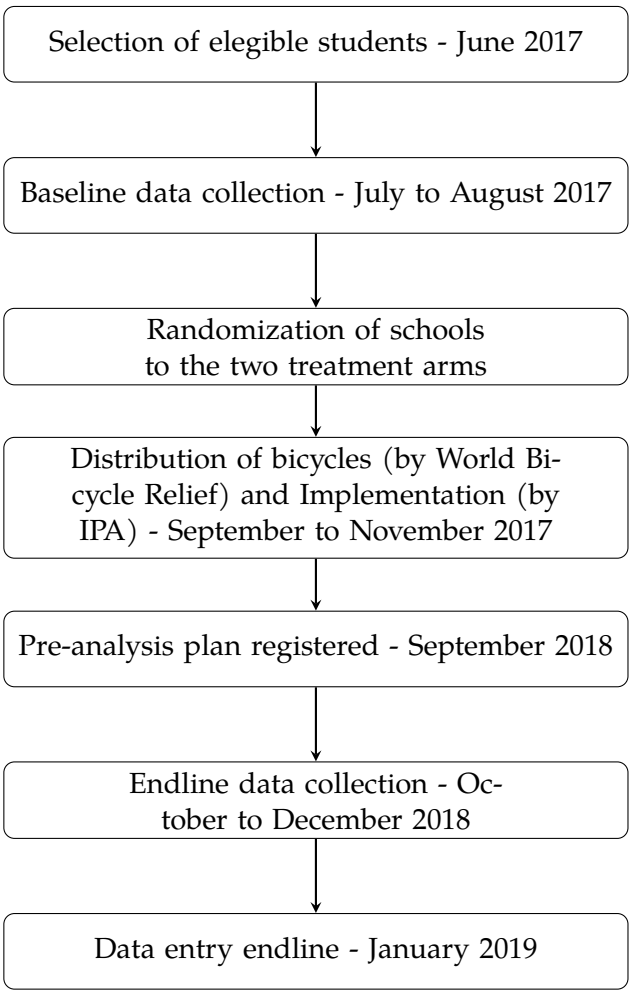
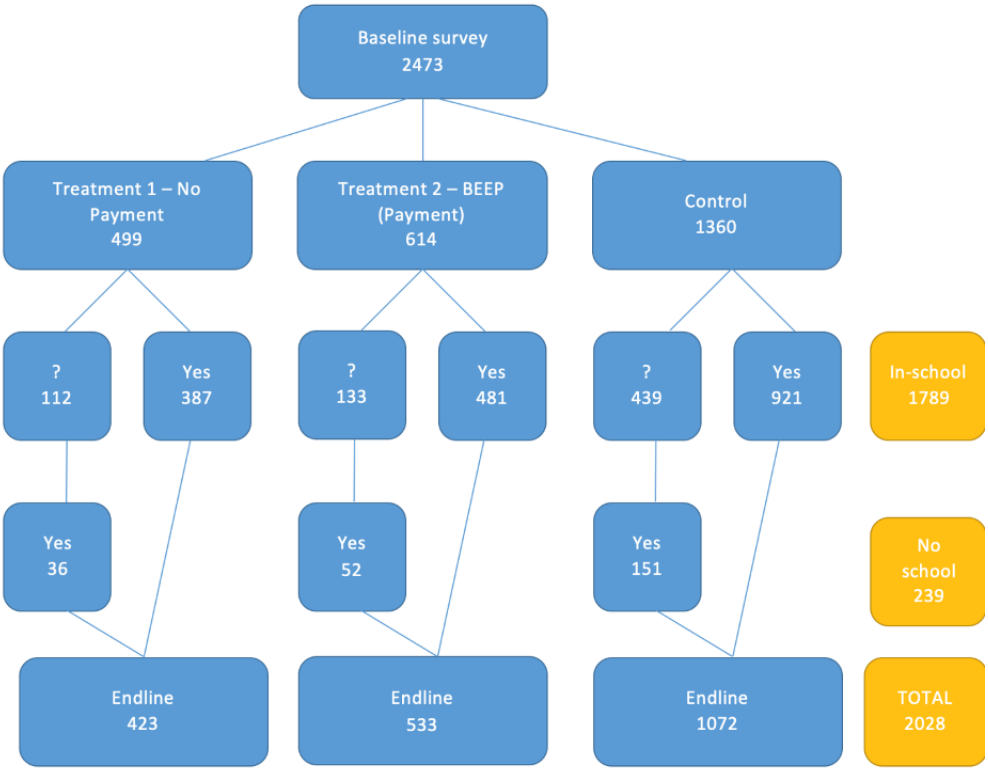


Figure A.3: Tracking and Attrition



A Construction of Outcome Variables

Outcomes	Variables
Primary:	
Mobility and Safety	- In the past week, how many times did you go outside the house alone to.. - Are you allowed to go alone when… - I feel safe when…
School Attendance	- Self-reported - Admin data from registers
Grade Transition	- Dropout - Grade Progression
Aspiration	- When you finish at school (either end of primary, secondary), what would you like to do? - How confident are you that you will be able to achieve this? - If for some reason you cannot (insert answer to previous question), what would you do? - In two years of time, how confident are you that you will be enrolled in school? - Do you think you will be working in a job or doing something that makes money in 10 years from now? - I am going to show you some drawings, could you tell me which ones you think a girl like you can become, if any? - And from these same drawings, which one would you like to become when you grow up, if any? - What does your role model do? (Occupation) - Do you want to do what he/she (Role model) does?

Outcomes	Variables
Locus of Control	- Let's say that one day when you are going to school you cannot find a path because of heavy rain or because a tree fell. This is a difficult situation because it is the only way to school and you are already late to school. In situations like this one or other ones similar to this one, you can usually find your way out? - There are many things that can happen to you in life. Some of them will be good and some will be not so good. For example: falling over and hurting my knee; forgetting to prepare for an exam or not doing well on an exam; your best friend is upset with you and not talking to you; you were not selected for a school team/club. Do you feel you can control what happens to you in life? - In general, would you say you are satisfied with your life? - I feel my life will improve in the future.
Fertility and Marriage	- The number of children to have in your whole life, how many would you like to have? - Of those children, how many girls and how many boys would you like to have? - Have you ever been pregnant? - Do you have any kids?
Secondary:	
Bicycle ownership and usage	- Do you have access to a bicycle that you can use? - Does this bicycle belong to you? - In a normal week, how many days do you use a bicycle to go to school? - Do you use a bicycle during the weekends? How often? - Do you have to ask permission to use the bicycle? - How much control do you think you have over the bicycle?

Outcomes	Variables
Time spent traveling to school	- Working for the school? (doing school chores like cleaning the classes, etc.) - Attending extra-curricular activities? (like sport, production unit, club, drama, board games, etc.) - Studying and doing homework outside of school? - Helping your family at home or doing other work for them? - Working to earn money by yourself? - Being with friends (chatting, playing, games, visiting them at home)?
Performance - Overall score and fraction in the lowest quartile	- Grade 7 end of year exam - English test - Mathematics test - D2 test of Focus
Bargaining	- Do you ever have small money of your own (K2 or K5) to use as you would like? This could be money you have earned or that you get from a family member. - Can you decide on what to spend it on your own? - Each year there are new fashions (e.g. hair pins) that come out. If you wanted to buy something new and had the money to do so, do you think your parents would allow you? - Do you own a pair of leggings? - Do you wear them on their own (if yes)? - If you don't like what is prepared for dinner, would you tell your mother/guardian you don't like the food or ask them if there is something else to eat? - When we talked about the activities you perform at home, like (insert activity here). Have you ever skipped doing household chores?

Outcomes	Variables
	- How often do you say something to your parents if you disagree with what they are saying? - Do you feel you can to talk to your parents about what you want to be when you grow up? - Do you think you can talk to your parents if you have problems with friends or at school? - Do you feel you can talk to your parents about when you wish to get married?
Self-image	- How would you rank yourself academically in your class? - Compared to your friends, how likely are you to succeed in life?
Identity	- Now let's play again with some drawings. Here you can see six drawings of roles girls usually take in society. Can you put them in order, starting from the one you that describes you better to the one that describes you the least? - How much do you think you can affect what other people think of your family?
Pro-sociality	- If you notice that one of your friends has a problem, would you help/participate/collaborate? - Could you tell us the name of your MP? - What is the name of the president of Zambia? - Are you a member of any club? - Think about the most active person in the club and the least active one. The most active would be a 10 and the least active would be a 0. How active are you in this club? - When you don't understand something in class, do you ask the teacher in front of everyone? (Not for out-of-school girls) - Do friends seek your opinion about important matters?

B Sampling Procedure

B.1 School Sampling

We decided to focus only on government schools, which are public schools and also the most common kind in Zambia. In addition, to be able to follow our sample over several years and observe a longer-term impact of the bicycles, all the schools selected are basic schools: Starting at Grade 1 or earlier and going beyond Grade 7 (end of primary) up to Grade 9 (last grade before secondary education).

All basic government schools of Monze and Mazabuka (our initial catchment districts) were asked to identify their pupils walking at least 3 kilometers to school and to generate a list with their names, gender, and grade. The research team had to find 100 schools with at least 25 eligible girls enrolled in grades 5, 6, and 7. Many of the schools which prepared the lists didn't have enough of such pupils. Hence, the research team had to extend the catchment area to a third district to find additional candidates for the sample. Kalomo, a third district of Southern Province, was chosen to have a good number of basic government schools, and not much prior work had been done there by World Bicycle Relief. In addition, some schools were also automatically excluded from the sample: (i) urban schools, where bicycles wouldn't be required by children to travel to school (existence of alternative public transportation), and (ii) a few very remote schools, which created logistical challenges in planning fieldwork. Limited by these constraints, the research team had a limited sample, from which 100 schools were selected.

B.2 Girls' Sampling

Prior to randomization, the research team had to identify a sample of 25 girls in each school to participate in the data collection activities to satisfy the power calculations. All these girls were required to be enrolled in grades 5, 6, or 7 (grades during which the girls are considered particularly vulnerable and likely to drop out of school).

Among the 100 sample schools, some of them had only 25 eligible girls (girls in grades 5, 6, or 7 and walking 3km or more to come to school), while other schools had 40, 50, or more of such girls. To build a representative sample, we generated two lists for each school. The first list, called list A, would always contain 25 names, balanced across our 3 sample grades (with a small priority given to grade 7 in which the girls are more likely to drop out of school soon - following as much as possible the pattern

8-8-9 for the grades 5-6-7). To generate the list, we used Stata and the only variable considered was the grade, as described above.

Then a second list was generated for each school, list B. This second list was containing additional girls, randomly selected among the remaining eligible girls (same methodology). The second list had between 0 and 25 girls, depending on how many girls in total were eligible in the school (in grades 5, 6, and 7). For example, a school with 33 eligible girls designated in grades 5, 6, and 7 might have a first list of 25 girls to be surveyed (list A), and a second list of 8 girls (list B). If the school had 70 eligible girls in grades 5, 6, and 7, then 25 of them would appear on list A, 25 others would appear on list B and 20 of them would not appear on any list, the selection being entirely random.

The purpose of these two lists was so that if everything ran smoothly, our field team of surveyors would be able to find the 25 girls of list A in the school and survey them on the day of their visit. If some of the girls were not able to be surveyed (absent, no consent, transferred, fake name, etc.) then the field team would be able to replace them with girls from list B. The result would be that the field team would be able to survey a sample of 25 girls in most cases. The names on the lists A and B are arranged in a random order, we introduce a limited bias when replacing the names. The only bias introduced was that the field team surveyed only those girls who were present. Fortunately, however, those present at the school were not representative of those present on a normal day, because prior to the visit of the team, the girls would have been encouraged by the school to attend on this special day.

C Field Protocols

Several tasks were assigned to the different members of the field team while visiting the schools. The protocol is detailed below:

(i) A few days before the team visits the school (surveyors and supervisors), the school is visited for the first time by a logistics supervisor. The logistics supervisor introduces Innovations for Poverty Action (IPA) and explains to the school staff the involvement of the school in the study. It's important to note that no formal communication from IPA was ever made to the schools prior to this visit. To illustrate the legitimacy of the procedure, the logistics supervisor carries two letters, one from the Ministry of General Education (MOGE), one from the District Education Board Secretary (DEBS) both showing the support of these institutions of the study. In a context where IPA or the Research team is absolutely unknown, much of the involvement from

the schools is obtained this way. The logistics supervisor explains that IPA is independent of WBR, although working in collaboration with them, and that participating, or not participating, in the study has no implication on the program that WBR is implementing. Once this is clear, the logistics supervisor provides the lists of girls to be surveyed and asks the school to collect the written informed consent of the parents to let their daughters participate in the study. The logistics supervisor does not speak to every parent whose daughter is asked to participate in the study. Instead, he leaves in the school a pile of consent forms (translated into the local language, Tonga) to be distributed by the school to the parents whose daughter appears on the list, which is also left at the school. A copy of the consent form is to be kept by the parents, another copy is to be signed and returned to the school for the research team. Between this day and the date of the visit of the team to the school, the Logistic supervisors are asked to follow up by phone or directly on-site to check if the consent forms will be ready.

(ii) A few days later (usually between 2 and 5 days), the field team finally arrives at the school. The supervisor meets with the Head-Teacher (or the acting Head) and collects the consent forms which have been signed. Then he/she checks and gathers the first 25 girls from the list, who have the consent of their parents and who are present at school.

(iii) Once the 25 girls are gathered, they are divided in groups of 4 to 5 pupils. Each group goes with one of the surveyors, who is in charge of explaining to them their role in the study. The girls who consent to participate sign an assent form.

(iv) The supervisor visits each of his/her surveyors to give him/her the IDs of the girls who are in his/her group. It's crucial that each surveyor gets the correct IDs because these will enable him/her to connect the face-to-face interview to the paper-based data.

(v) After this, the group activities begin, which are all paper-based: the attention test (10 minutes), the learning assessment (25 minutes), and the semi-self-administered survey. Each surveyor is supposed to explain and supervise these activities with her own group of 4 or 5 girls. Once the group activities are finished, the girls are released but asked not to go too far. A snack (biscuits and milk drink) is distributed to ease their wait.

(vi) Each surveyor then starts interviewing one of the girls in his/her group, with the other girls waiting some time to be interviewed (they might even go back to class if the surveyor is sure to find them easily again). The face-to-face interview (tablet based) usually lasts around 40 minutes. Once one is finished, the surveyor releases the girl and starts interviewing another girl,

until he has interviewed all the girls in his/her group. If the surveyor finishes his/her interviews way before one colleague, he/she can help him/her to finish their interviews.

(vii) Meanwhile, the supervisor conducts the school survey with the head teacher or the acting head teacher. Another teacher is welcome to participate if he/she can complete the knowledge of the head. This survey takes between 30 minutes to 1 hour depending on how organized the school is.

(viii) After finishing the school survey, the supervisor asks the school management to prepare the attendance registers to be photographed by the team (those for grades 5, 6, and 7 for the current and the past years), and all the attendance registers currently available in the school more generally. This is because the surveyors will have to collect in those the attendance of the siblings of the respondents (who are not necessarily enrolled in the same grades).

(ix) When the surveyors finish their interviews, they come to meet with their supervisor and add to their forms the attendance information they collect in the registers (for their respondents and for their siblings).

(x) It's only when all this work is finished, that the team can head back to town and meet with the RA to deliver the data collected and the outputs of the day.

(xi) A few days later, the school might be visited a last time, by one of the back-checkers. Only half of the schools will be back-checked. The back-checker, with no prior notice, will interview again of the girls again, with a short sub-survey (10-15 minutes). No additional consent or assent form needs to be signed.

D Timeline

The baseline data collection happened during the second term of the school year in 2017. It took place between the 5 July and the 10 August 2017, and the team (supervisors and surveyors) worked 21.5 days in the schools (20 days initially planned), over a period of 5 weeks (4 weeks initially planned).

The baseline survey was first launched in Monze and 9 days were necessary to visit all the schools of the district (44 schools). Then, the research team moved to Mazabuka and visited all the schools of the district (20 schools) in 4 days. Finally, the research team spent 8.5 days in the schools in Kalomo (36 schools).

The training for the supervisors, surveyors, and back-checkers lasted 5 days, including one day of field training. The supervisors had one extra day of training. The training happened in Monze between 26 June and 1 July 2017.

The logistics supervisors were trained in Lusaka before everyone (1-day training), and they started to visit the schools on 26 June 2017, earlier than the rest of the team, to start planning the visits. The back-checkers finished their work on the same day as most of the rest of the teams.

After one school year of using the bicycles, the endline survey was implemented, during the third term of the 2018 school year (September to November 2018).

E Steps for Index Construction

We create variance-weighted indices following the methodology proposed by Anderson (2008) for empowerment outcomes (also see Haushofer and Shapiro (2016); Dhar et al. (2018) for a recent application).

Anderson (2008) summarizes the index-creating process as the following. At the most basic level, an index created using this method is a weighted mean of several standardized variables. More weight is assigned to measures that are orthogonal (less similar or less correlated) to other measures. The weights are calculated to maximize the amount of information captured in the index. The index is computed using the following steps.

(i) For all variables switch signs where necessary so that the positive direction always indicates a "better" outcome.

(ii) Create standardized variables $(\tilde{y})$ by demeaning and then by dividing by standard deviation.

(iii) Compute covariance matrix $\widehat{\sum}$, which consist of elements:

$$\sum_{mn}^{\wedge} = \sum_{i=1}^{Nmn} \frac{(y_{im} - \bar{y}_m)}{\sigma_m^y} * \frac{(y_{in} - \bar{y}_n)}{\sigma_n^y}$$

where Nmn is the number of observations (total persons with non-missing data for variables m and n).

(iv) Next, we invert the covariance matrix, and define weight w_k for each variable k by summing the entries in the row of the inverted covariance matrix:

$$(\overset{\wedge}{\sum})^{-1} = \begin{bmatrix} c_{11}....c_{1K} \\ ..... \\ ..... \\ c_{K1}....c_{KK} \end{bmatrix}$$

$$w_k = \sum_{l=1}^{K} c_{kl}$$

(v) Finally create a new variable, $\hat{y}_i$, that is a weighted average of $\tilde{y}_{ik}$ for person i. When constructing $\hat{y}_i$, weight its inputs, standardized variables $\tilde{y}_{ik}$ by the inverse of the covariance matrix of the transformed variables. A simple way to do this is to set the weight on each outcome equal to the sum of its row entries in the inverted covariance matrix for the area. The index variable $\hat{y}_i$ is called because this transformation yields a generalized least squares estimator Anderson (2008).

$$\hat{y}_i = \Big(\sum_{k \in K} w_k \Big)^{-1} \sum_{k \in K_i} w_k * \frac{y_{ik} - \bar{y_k}}{\sigma_k^y}$$

F Learning Assessment

Figure F.4: Learning Assessment

(TEST INFO) SCHOOL NAME: _____ DATE: (DD)___/(MM)___/(YY)___ SURVEYOR ID: ____

(PUPIL INFO): FIRST NAME: _____ LAST NAME: _____ GRADE AND CLASS: _____

COMMENTS: _____

*For each question below, **four answers are given, but only one of the four is right**. Work out which is the best answer.*
*Then, **TICK** the answer of your choice. For example, if you had chosen answer B for a question, you would show it like this:* ☒ **B.**

1) Which animal is represented in the image?



- ☐ A. Donkey
- ☐ B. Snake
- ☐ C. Monkey
- ☐ D. Butterfly

2) 11 + 13 =

- ☐ A. 14
- ☐ B. 24
- ☐ C. 25
- ☐ D. 36

3) Which day comes before Friday and after Wednesday?

- ☐ A. Monday
- ☐ B. Tuesday
- ☐ C. Thursday
- ☐ D. Saturday

4) How many triangles are on the picture?



- ☐ A. 39
- ☐ B. 42
- ☐ C. 49
- ☐ D. 54

5) Did you see the man ..... stole the car?

- ☐ A. who
- ☐ B. which
- ☐ C. whom
- ☐ D. when

6) 18 ÷ 2 =

- ☐ A. 6
- ☐ B. 8
- ☐ C. 9
- ☐ D. 12

7) When he was young, Lawrence used to ..... a lot with his sisters?

- ☐ A. played
- ☐ B. plays
- ☐ C. play
- ☐ D. playing

8) 11 x 10 =

- ☐ A. 100
- ☐ B. 101
- ☐ C. 110
- ☐ D. 110

9) Choose the picture best describing the image:



- ☐ A. The lady is wearing a hat
- ☐ B. The lady has long hair
- ☐ C. The lady is raising her arm
- ☐ D. The lady is eating

10) 21 ☐ 3 = 7

- ☐ A. x
- ☐ B. ÷
- ☐ C. -
- ☐ D. >

11) Mercy is ..... than her sister

- ☐ A. Younger
- ☐ B. More young
- ☐ C. Youngest
- ☐ D. Most young

12) What is the denominator of the fraction $\frac{3}{6}$

- ☐ A. 9
- ☐ B. 3
- ☐ C. 6
- ☐ D. 18

13) Gift is a hairdresser. Which picture represents Gift?



- ☐ A. Picture 1
- ☐ B. Picture 2
- ☐ C. Picture 3
- ☐ D. Picture 4

14) Moses had 6 mangos and ate 2 of them. How many mangos remain?

- ☐ A. 2 mangos
- ☐ B. 4 mangos
- ☐ C. 5 mangos
- ☐ D. 8 mangos

15) Martin was born in 2003.

- ☐ A. He is 14 years old.
- ☐ B. He does 14 years.
- ☐ C. He has 14 years.
- ☐ D. He measures 14 years old.

16) She laughs because the joke is:

- ☐ A. Sad
- ☐ B. Funny
- ☐ C. Easy
- ☐ D. Wrong

17) 2,23 + 1,07 =

- ☐ A. 2,30
- ☐ B. 3,27
- ☐ C. 3,30
- ☐ D. 4,13

18) Which one is true?

- ☐ A. Goats eat lions
- ☐ B. Chickens eat humans
- ☐ C. Cats eat mice
- ☐ D. Birds eat crocodiles

19) How many K50 notes are in K500?

- ☐ A. 5
- ☐ B. 10
- ☐ C. 50
- ☐ D. 100

20) Anna is walking home with ..... brother.

- ☐ A. his
- ☐ B. her
- ☐ C. him
- ☐ D. theirs

21) 2410 – 1521 =

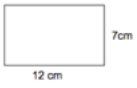
- ☐ A. 889
- ☐ B. 756
- ☐ C. 1029
- ☐ D. 999

22) Choose the sentence describing the image:



- ☐ A. The girl sweeps the floor
- ☐ B. The chicken lays an egg
- ☐ C. The girl collects the eggs
- ☐ D. The eggs hatch

23) Find the perimeter of the shape below:



- ☐ A. 20 cm
- ☐ B. 29 cm
- ☐ C. 38 cm
- ☐ D. 40 cm

24) Cheelo woke up late ..... he missed the bus.

- ☐ A. but
- ☐ B. so
- ☐ C. for
- ☐ D. because

25) Identify two lines which are parallel:



- ☐ A. Lines AB and BC
- ☐ B. Lines AC and DB
- ☐ C. Lines AD and BC
- ☐ D. Lines AD and CD

26) Which of the following is correct?

- ☐ A. There are five days in a week
- ☐ B. There are sixty seconds in a minute
- ☐ C. There are twenty days in a month
- ☐ D. There are fifteen months in a year

G Attention Test

Figure G.5: Learning Assessment



H Behavioral Games

In addition to the survey outcomes, we also used non-survey methods to elicit behavioral decisions. The objective of these activities was to capture real decisions and behavior of the girls in a controlled environment to measure an outcome that is hard to observe in real life and hard to measure with survey questions. The objective of using non-survey instruments was to observe directly the decision-making, which is often hard to see, and reduce reporting bias in those questions that are susceptible to either social desirability bias or internalized societal norms (Glennerster et al., 2018). Specifically, we planned to measure two outcomes using non-survey questions: *bargaining power at home* and *willingness to share an opinion*. The empirical analysis would be to estimate Equation 1 – comparing outcomes of girls' in the treatment and control schools. We had anticipated that girls in the treatment schools would have more bargaining power at home and more willingness to share an opinion.

H.1 Bargaining power in the household

This game aims to measure the bargaining power of the girl within the family. Since we only collected data at the school, this game had two parts, one at the school with the respondent and the second one at home with the parents. For the second part, the student facilitated the delivery of the form with the list of items to their parents and brought the form back to the school. In the first part of the game, the girl ranked a list of items from 1 to 10, the first one being the most preferred item. For the second part, the girl took home the same list of items (Figure H.6) and asked the parents to rank the items in the list according to their preferences (rank 1 to 10). Lastly, the student and their parents jointly ranked the items from 1 to 10, on a third list.

We then randomly selected one of two scenarios. In the first one, both the girl and her parents received one of the items they ranked in their respective independent lists. In the second scenario, they received an item from their joint list. The item received by the girl and the parents was selected using a formula of decreasing probability of being chosen for the least preferred goods.

We measured bargaining by comparing where the preferences of the girl lie on the joint list. A girl with more bargaining power would be able to place her preferred good towards the top of the joint list. This helped us to measure the extent to which the girl can affect decisions made jointly in the household.

H.2 Willingness to share an opinion

To measure the willingness to share an opinion in a group, we conducted an experiment inspired by Baldiga (2014). In this experiment, girls worked in groups of about five students. The girls were presented with a moral dilemma, for which there is no right or wrong answer; and for which the answer was independent of factors like school attendance, empowerment, etc., our main outcomes of interest.

The girls had to reach a group decision regarding the moral dilemma, where the decision was chosen based on the highest number of votes. The girls voted their opinion and submitted their votes in one of the two boxes. They could submit their vote in the "priority" box or in the "non-priority" box. If they chose to submit their vote in the "priority" box, the answer was always read publicly and was counted as a valid vote. If an agreement had not been reached with the votes in the "priority" box, then the "non-priority" box was opened. The votes in the "non-priority" box were only read if

the agreement was not reached with the votes in the "priority" box. To measure willingness to share an opinion, we created a variable indicating one if the girl answered in the "priority" box and zero if she answered in the "non-priority" box. This measured the extent to which the girl was willing to share her opinion in a group, as a proxy for future political participation.

H.3 Problems with implementation

During the implementation of the behavioral games, we encountered some challenges related to the cultural aspects of giving students goods for free. In this region of Zambia, receiving goods for free can be seen as an unfriendly act and some of the families were becoming suspicious. Thus, we decided to discontinue the games so as not to harm the rest of the data collection exercise. Before we discontinued implementation, we had covered ten schools in the district of Monze.

Figure H.6: Example of List of Goods

