### **ECONOMICS**

# The role of education interventions in improving economic rationality

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Schooling rewards people with labor market returns and nonpecuniary benefits in other realms of life. However, there is no experimental evidence showing that education interventions improve individual economic rationality. We examine this hypothesis by studying a randomized 1-year financial support program for education in Malawi that reduced absence and dropout rates and increased scores on a qualification exam of female secondary school students. We measure economic rationality 4 years after the intervention by using lab-in-the-field experiments to create scores of consistency with utility maximization that are derived from revealed preference theory. We find that students assigned to the intervention had higher scores of rationality. The results remain robust after controlling for changes in cognitive and noncognitive skills. Our results suggest that education enhances the quality of economic decision-making.

**R** ationality in human choices has been a cornerstone assumption in traditional economic analysis and yet one of the most controversial issues in social and behavioral sciences (1). Mounting evidence shows that people tend to make systematic errors in judgment and decision-making and that there is a high level of heterogeneity in the extent to which rationality is limited across decisions and individuals (2, 3). The welfare loss resulting from poor decisions can be substantial, which implies that policy-makers might want to rethink the role of public policy in response to the failure of rationality (4).

The behavioral science literature has accumulated evidence on ways of improving people's capabilities and quality of decision-making: changing incentives, restructuring choice architecture, and debiasing training (5–7). Most of these approaches target the reduction of decision biases in particular contexts of economic activities but do not address the improvement of general capabilities of decision-making that are transferrable across decision domains. It is often controversial to judge whether decision biases are driven by the failure of rationality or other factors such as anomalous preferences.

Schooling has been shown to influence a wide range of outcomes, including income, health, and crime (8, 9). One little-explored hypothesis is that education improves people's decision-making abilities and leads them to make better decisions across various choice environments. The impacts of education on decision-making can then be a potential mechanism underlying the pecuniary and nonpecuniary returns to education.

We examine this hypothesis by studying a nongovernmental organization-implemented randomized controlled trial of education support in Malawi, an environment where, among young females, only 21.4% have received some secondary education and 9.8% have completed secondary school education (10). The program randomly provided financial support for education in a sample of 2812 female 9th and 10th graders from 83 classrooms in 33 public schools between the third semester of the academic year 2011-2012 and the second semester of the academic year 2012-2013. The program was randomized at the classroom level and consisted of the payment of school tuition and fees for 1 year, as well as a monthly cash stipend. The total amount of support was ~\$70 per student as long as the student remained in school until the end of the program.

We conducted a short-term follow-up survey about 1 year later that measured short-term educational impacts. Four years after the intervention, we conducted a long-term follow-up survey that measured longer-term educational outcomes and implemented laboratory experiments of presenting subjects with a set of decision problems under risk and over time using a two-dimensional budget set. The risk-domain experiment consists of 20 decision problems representing a set of portfolio options associated with two equally probable unknown states. The time-domain experiment consists of two frames wherein the budget set represents a set of money allocations between two payment dates. The near time frame comprises 15 decisions of allocating money between tomorrow and 31 days from the time of the experiment. The distant time frame consists of 15 decisions of allocating money between 1 year and 1 year and 30 days from the time of the experiment.

This tool of laboratory experiments generates a rich set of individual choice data that are well suited to testing for consistency with utility maximization as the criterion for economic rationality (3, 11, 12). Classical revealed preference theory shows that choices from a finite collection of budget lines are consistent with maximizing a (well-behaved) utility function if and only if they satisfy the Generalized Axiom of Revealed Preference (GARP) (13). When the choice data do not satisfy GARP, we use Afriat's critical cost efficiency index (CCEI) to measure how closely they comply with the utility maximization hypothesis (14). We compute CCEI in each experimental domain to generate an index of the subject's level of economic rationality. The CCEI is bounded between 0 and 1. The closer the CCEI is to 1, the more closely the choice data are consistent with utility maximization. As the summary of economic rationality indices for the time domain, we use the minimum of two CCEIs at the near and distant time frames. The reason why consistency with utility maximization may be key to economic survival and thus serve as the basic criterion of economic rationality is offered by the classic money pump argument, which shows that inconsistent behavior can be exploited indefinitely by arbitrageurs. In addition, we consider two measures of compliance with stochastic dominance in the risk-domain experiment as an alternative criterion for economic rationality. Further details of the education intervention, laboratory experiment, and measurements are reported in the supplementary materials (15).

We present coefficients from regressions with baseline controls consisting of individual characteristics, parents' education and occupation, and school type. We cluster our standard errors at the classroom level. Because we deal with multiple outcomes of education and economic rationality, as well as the heterogeneous effects for 9th and 10th graders, we account for multiple hypothesis testing by following the approach in our preanalysis plan (*16*). We group all outcomes for the whole sample, 9th graders, and 10th graders in each realm of education or economic rationality and report standardized treatment effects with baseline controls as in (*17*), as well as family-wise adjusted P values.

First we evaluate the impacts of the intervention on various education outcomes: number of days absent during the past semester, school dropout rate, taking the Junior Certificate Examination (JCE) in 10th grade, passing the JCE, and total years of education. The information on the JCE comes from administrative data, whereas absence, dropout rate, and years of education are self-reported. Results for the whole sample, 9th graders, and 10th graders are presented in Table 1.

Students in the treated classrooms (i.e., those that were assigned the intervention) have better education outcomes compared with those in the control classrooms. Specifically, the treated students in the whole sample are 40% (1.6 days) less likely to be absent and 7% (5.5 percentage points) and 14% (8.6 percentage points) more likely to take and pass the JCE, respectively. The self-reported dropout rate decreases by 3.4 percentage points

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Table 1. Impacts of education support program on education outcomes. Coefficients are from linear regressions of each education outcome on the education intervention indicator. Standard errors (in parentheses) are clustered at the classroom level. FU, follow-up; N/A, not applicable.

Sources:	Short-term FU survey	Short-term FU survey	Administrative data	Administrative data	Long-term FU survey	Combined data				
Variables:	Absence, self-reported (2013)	Dropout, self-reported (2013)	Took JCE (2012–2013)	Passed JCE (2012–2013)	Total years of education (2015–2016)	Standardized treatment effect				
	1	2	3	4	5	6				
Overall sample										
Treated	-1.612***	-0.034	0.055**	0.086**	0.103	0.026***				
	(0.397)	(0.026)	(0.023)	(0.033)	(0.076)	(0.008)				
Family-wise adjusted P values	0.001	0.231	0.052	0.036	0.190	N/A				
Control group mean	4.01	0.112	0.789	0.597	11.5	0				
Number of observations	1851	1929	2808	2808	2420	11,816				
Baseline 9th graders										
Traatad	-1.498***	-0.083**	0.123***	0.141***	0.147	0.042***				
lleateu	(0.343)	(0.039)	(0.040)	(0.049)	(0.119)	(0.013)				
Family-wise adjusted P values	0.001	0.052	0.013	0.021	0.231	N/A				
Control group mean	3.53	0.135	0.624	0.509	11.3	0				
Number of observations	855	889	1220	1220	1051	5235				
		Ba	seline 10th graders							
Tracted	-1.761**	-0.004	-0.004	0.043	0.058	0.012				
Ireated	(0.703)	(0.033)	(0.021)	(0.040)	(0.098)	(0.009)				
Family-wise adjusted P values	0.052	0.945	0.945	0.320	0.740	N/A				
Control group mean	4.28	0.099	0.888	0.649	11.6	0				
Number of observations	996	1040	1588	1588	1369	6581				

\*\*P < 0.05; \*\*\*P < 0.01.

(30%), and the total years of education increases by ~0.1 of a year; however, neither change is statistically significant, and we expect a lot of measurement error in these self-reported measures. These treatment effects are heterogeneous between the two cohorts and come mainly from 9th graders: Those treated 9th graders are 42% (1.5 days) less likely to be absent per semester, 61% (8.3 percentage points) less likely to have dropped out, and 20% (12.3 percentage points) and 28% (14.1 percentage points) more likely to take and pass the JCE, respectively.

The standardized treatment effect shown in column 6 of Table 1 confirms that the intervention was successful in enhancing schooling, and this result is mainly driven by 9th graders. We interpret this heterogeneity because 9th graders are more vulnerable to dropping out of school and therefore could benefit more from the education intervention than 10th graders. Using data from our study, we estimate that the dropout rate was 26.4% in 9th grade compared with only 11.2% in 10th grade. This pattern is consistent with data from Malawian national statistics, as well as other settings (15).

Next we study whether the educational intervention affected economic rationality. Columns 1 and 2 of Table 2 present the average treatment effects on the CCEIs in the risk and time domains with baseline controls. For the overall sample, we observe that girls who received the intervention display an increase in

CCEIs measured in the risk and time domain of 1.3 percentage points (1.6%) and 1.4 percentage points (1.7%), respectively, but only the time domain effect is significant at the 5% level. Turning to the alternative measure of economic rationality, both the relative frequency and the expected payoff ratio of complying with stochastic dominance exhibit similar patterns as shown in columns 3 and 4. The standardized treatment effect across all four measures indicates that the treatment is associated with a 0.02 standard deviation [standard error (SE) = 0.009] increase in economic rationality scores. Figure S2 shows that the intervention improves economic rationality throughout most of the range of CCEIs.

Table 2 confirms that the intervention had heterogeneous impacts. For 9th graders in the control group, the mean CCEIs measured by risk and time domain are 0.81 and 0.82, respectively. The CCEIs of the treatment group are 3.3 percentage points (4.0%) and 3.1 percentage points (3.7%) higher than those of the control group (columns 1 and 2, respectively). Those treated among 9th graders are more likely to make decisions in conformity with stochastic dominance than those in the control group (columns 3 and 4). The standardized treatment effect for 9th graders across all measures is 0.038 standard deviations (SE = 0.011) (column 5). We do not find any treatment effect for 10th graders, but the statistical significance of the treatment effects on economic rationality remain robust when using family-wise adjusted P values to account for multiple hypothesis testing.

A natural question that arises is whether our measures of economic rationality are proxies that are correlated with other primitives of decision-making that might also be affected by the intervention. To address this issue, we first examined the treatment effects of the intervention on time and risk preferences, cognitive abilities, and personality (15) and found that the intervention did not affect risk attitudes and time impatience but did enhance cognitive skills measured by the math test score and some aspects of personality traits (table S5). We then investigated the effects of the intervention on economic rationality, controlling for measures of risk and time preferences, cognitive skills, and personality. Our rationality scores are explained only partially by these control variables (table S6): For example, for 9th graders, the control variables reduce the impacts on rationality scores by about one-third.

There could be other explanations for our findings. First, the intervention might help subjects better understand the experiment instructions. However, when we drop the first three choices in the experiments, our results are robust, which suggests that differential learning during the experiment is not important (table S7). Second, beneficiaries might exert differential effort during the experimental games, despite

### Table 2. Impacts of education support program on economic rationality.

Coefficients are from linear regressions of each rationality measure on the education intervention indicator. Standard errors (in parentheses) are clustered at the classroom level. N/A, not applicable.

	CCEI, risk	CCEI, time	Complia stochastic	nce with dominance	Standardized treatment						
Variables	domain	domain	Freq.	Payoff	effect						
	1	2	3	4	5						
Overall sample											
Tree et e el	0.013	0.014**	0.012*	0.005*	0.020**						
Ireated	(0.008)	(0.006)	(0.007)	(0.003)	(0.009)						
Family-wise adjusted P value	0.084	0.018	0.070	0.080	N/A						
Control group mean	0.81	0.82	0.83	0.94	0.00						
Number of observations	2421	2416	2421	2421	9679						
Baseline 9th graders											
	0.033***	0.031***	0.018**	0.009**	0.038***						
Ireated	(0.010)	(0.008)	(0.008)	(0.004)	(0.011)						
Family-wise adjusted P value	< 0.001	<0.001	0.025	0.025	N/A						
Control group mean	0.83	0.83	0.84	0.94	0.00						
Number of observations	1051	1050	1051	1051	4203						
	В	aseline 10th į	graders								
Tracted	-0.003	0.003	0.006	0.002	0.005						
Ireated	(0.010)	(0.009)	(0.010)	(0.004)	(0.012)						
Family-wise adjusted P value	0.871	0.871	0.765	0.862	N/A						
Control group mean	0.80	0.82	0.82	0.93	0.00						
Number of observations	1370	1366	1370	1370	5476						
*P < 0.10 <sup>.</sup> **P < 0.05 <sup>.</sup>	***P < 0.01										

the fact that they are incentivized. We created several measures aimed at capturing effort during the survey, including indexes of missing and "do not know" responses and did not find differences between the treatment and control groups (table S8). Third, we cannot positively distinguish whether the intervention improved rationality only via its effect through increased education. For example, the monthly stipend that was part of the intervention could lead girls to think more rationally about how to spend their money.

Using a randomized controlled trial of education support and financially incentivized laboratory experiments, we established causal evidence that an education intervention increases not only educational outcomes but also economic rationality. The size of the treatment effects on CCEIs is economically meaningful and larger than the cross-sectional relationship between education and CCEIs in our control group and the study from the Netherlands (3), as well as recent work on this relationship using changes in compulsory schooling in England (18). The direct comparison of the results between our study and the two aforementioned studies (3, 18) is difficult for several reasons. First, measures of years of schooling coming from self-reported levels of education achievement are generally noisy (8), especially so in a developing country setting where drop-out and grade repetition are frequent (19). Second, there are differences in laboratory experimental design such as the num-

ber of choices per subject and the variations of budget sets. Third, our treatment effects are measured after 4 years, whereas the other studies (3, 18) measure outcomes during adulthood. If program effects fade out over time, they could help reconcile the different results in these three studies (20). Fourth, the English and Dutch samples differ from our sample along many dimensions of socioeconomic status, and therefore our findings might not apply to populations in developed countries. For example, people in developed countries may have more opportunities to learn to make more rational decisions outside of school. Finally, on a hopeful note, our relatively larger impacts of education interventions are consistent with the literature that shows larger returns to cognitive and noncognitive investments in resource constraint settings (21, 22). In our setting, the impact of the educational intervention is large, not just in terms of the effects on economic rationality but also on cognitive outcomes (15). More research is needed to determine the reproducibility and generalizability of our findings.

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#### SUPPLEMENTARY MATERIALS

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### The role of education interventions in improving economic rationality

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### Educating for economic rationality

The hypothesis that education enhances economic decision-making has been surprisingly underexplored. Kim *et al.* studied this question using a randomized control trial in a sample of 2812 girls in secondary schools in Malawi. Four years after providing financial support for a year's schooling, they presented the subjects with a set of decision problems (for example, allocating funds to immediate versus future expenses) that test economic rationality. The education intervention enhanced both educational outcomes and economic rationality as measured by consistency with utility maximization in the long run.

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# Supplementary Materials for

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### S1. Methods

### S1.1 Details of the randomized education intervention

Malawi is one of the least developed countries with GDP per capita in 2016 of US\$306 according to the World Bank (24). The education system consists of eight years of free primary education (Standard 1 through 8) and four years of secondary education (Form 1 through 4). In order to complete secondary education, students must pass two national exams: the Junior Certificate Examination (JCE) at the end of Form 2 (10th grade) and the Malawi School Certificate Examination (MSCE) at the end of Form 4 (12th grade) (25). Among 20–24 years old females, 21.4% have some secondary education and only 9.8% have completed secondary school education (10).

This study was started in 2012 in four districts near Lilongwe, Malawi. The NGO implementing the program initially targeted 3,997 female students in grades 9 to 11 from 33 public schools. A total of 3,397 female students were originally interviewed in the baseline survey. A short-term follow-up survey was implemented between January and June 2013 in order to understand whether the educational interventions were successful in increasing short-term educational outcomes. These results are reported in (26). The long-term follow-up survey was implemented in 2015-2016 and included long term educational outcomes, lab-in-the-field experiments to measure risk and time preferences as well as economic rationality, a personality test, a test to measure cognitive abilities and a math exam. For the long-term follow-up, only 9th and 10th graders at baseline were interviewed. 11th graders at baseline were excluded from the study sample because of the budget limitations of the NGO. As a result, the study sample includes 2,812 female 9th and 10th graders at baseline in 83 classrooms in 33 secondary schools. The number of study participants in the long term follow-up survey implemented four years after the baseline was 2,424 for a survey follow-up rate of 86.2%.

The education intervention was implemented in public secondary schools near Lilongwe. The Africa Future Foundation (AFF), a non-governmental organization (NGO), randomly provided one-year of financial support for education for female 9<sup>th</sup> and 10<sup>th</sup> grade students. In order to receive the educational intervention, treated classrooms were randomly selected after stratification by grade. 83 classes of 9th and 10th graders at 33 schools were randomly assigned into the treatment and control groups. The unit of randomization was a classroom. 1,461 and 1,351 female students in

39 and 44 classrooms were included in the treatment and control group, respectively. Most schools (28 out of 33) have one class per grade and there are limited cross-grade school activities.

The educational intervention consisted of a payment for one year of school tuition and fees (3,500 Malawi kwacha (about US \$21) for each of the three semesters) and a monthly cash stipend of 300 - 500 Malawi kwacha (about US \$2-3). The school tuition and fees were directly deposited in the school accounts at the beginning of each semester, while the monthly cash stipends were distributed directly to treated students. The intervention was discontinued for transfer and dropout students.

The educational support program started in the third semester of the 2011-2012 academic year (April, 2012) and continued through the first and second semesters of the 2012-2013 academic year. The monthly stipend was increased from 300 Kwacha to 500 Kwacha in the second semester of academic year 2012-13 due to the depreciation of the Malawian Kwacha. In total, a treated student in the scholarship program received about \$70 (13,900 Kwacha = 3,500 Kwacha × 3 semesters + 300 Kwacha × 6 times + 500 Kwacha × 3 times), if it remained at the baseline school for the duration of the intervention.

The baseline characteristics are shown in Table S1. Study participants are about 15.3 years old at the time of baseline survey. They have a relatively higher socioeconomic status compared to average Malawian students. For example, about 20.4% of students have a father who attended tertiary education and 26.6% of fathers are white-collar workers. Table S1 also confirms that the treatment and control groups are well balanced: none of the 12 baseline characteristics are statistically different at the 10% level between the treatment and control group in the overall sample. The attrition rates between groups are reported in Table S2 and they confirm that there is no systematic attrition.

### **S1.2 Estimating equation**

We measure the impacts of the educational intervention using the following ordinary least-squares (OLS) specification:

$$Y_{ijg} = \beta_0 + \beta_1 \operatorname{Treat}_{jg} + X_{ijg} + \Psi_g + \mu_{ijg}$$

where  $Y_{ijg}$  is the outcomes (e.g., a measure of educational achievement or economic rationality) for student *i* in classroom *j*, and grade g. Treat<sub>*j*</sub> = 1 if the individual was in the education intervention classroom.  $\beta_1$  is our primary interest, which represents the

impact of being assigned to the educational intervention on outcomes. X is a control vector.  $\Psi$  is a grade fixed effect. The vector of baseline controls includes sociodemographic characteristics including age (years), orphan status (=1 when both parents died), parents' tertiary education (= 1 when they graduated from a college or a university), parents' white-collar job (= 1 when one of the parents have a professional or government job), household assets, and school type (conventional vs. selective secondary school). We report the main estimation results without the baseline controls in Table S3 and Table S4 as a robustness check. In addition, we implement heterogeneity analysis by grade because the program had different effects across grades. Standard errors are clustered at the classroom level. The estimation and selection of the baseline controls strictly follows the pre-analysis plan at AEA RCT Registry AEARCTR – 0001243 (*16*).

### S1.3 Technical details of multiple hypotheses testing

In order to account for multiple hypotheses testing, we follow (27). The first approach is to group outcome measures into a domain and to take an average standardized treatment effect in each domain, as suggested in (17). For Table 1, we group five education outcomes (absence, drop-out, JCE take, JCE pass, and total years of education) into the first-stage education domain. Self-reported absence and self-reported drop-out variables are reversed in sign in order to make five education variables go in the same direction within the domain. For Table 2, we group four experimental outcomes (CCEI in the risk domain, the minimum of CCEIs of two time frames in the time domain, and two measures of compliance with stochastic dominance in the risk domain experiment) into the economic rationality domain. Then, we stack the data for the individual outcomes within each domain and estimate a single regression equation while clustering standard errors both at classroom level and at individual level in order to compute the average standardized treatment effect.

Next, we use the free step-down resampling method for multiple hypotheses testing to adjust the family-wise error rate (28). Specifically, the Stata command 'wyoung' is used developed for (29). For Table 1, the 15 hypotheses (five education outcomes for three panels (overall / 9th grade / 10th grade)) arise from examining both multiple outcomes and multiple subgroups in the education domain. For Table 2, the 12 hypotheses (four experimental outcomes for three panels (overall / 9th grade / 10th grade)) are tested. We conducted 10,000 simulations for each multiple hypotheses testing

with and without baseline covariates, respectively. In order to make our multiple hypotheses testing results replicable, we use the seed number of 20.

### S1.4 Heterogeneous treatment effects by grade

In this section we provide explanations for why the education response to the intervention was different for 9th and 10th graders. As a reminder, our results are statistically significant when we measure the impact of the intervention on education and economic rationality using the whole sample in terms of standardized treatment effects (last column in Tables 1 and 2), but these results seem to be entirely driven by changes among 9th graders. We also note that given the heterogeneous response to the intervention for these two cohorts in terms of education outcomes, the similar responses in terms of effects on rationality measures are in fact expected if the effect of the intervention on rationality is driven by changes in education.

On a general level, researchers in the field of education have documented in the context of the US that 9th grade plays a critical role in shaping students' long-term outcomes (30-34). These studies document that compared to all the other high-school cohorts, the 9th grade cohort displays the lowest GPA, most absences from school and the highest probability of misbehavior and failing grades. The main explanations for these behaviors are related to changes in parental supervision and peer structure and the difficulty that students face when they transition from middle school to the high school (31). In addition, (35) document that difficulty during grade 9th is an important predictor of high school graduation as well as a mediating variable between social disadvantage (such as poverty) and high school graduation. While a causal interpretation of these relationships is difficult to make, these patterns nevertheless suggest that 9th graders might be more vulnerable to dropping out of school than those in upper grades and would therefore benefit more from the financial support that our intervention has provided.

Turning to our specific Malawian setting, we have an additional reason to believe that the impacts on education outcomes could be greater among 9th graders than 10th graders. Unlike in many higher income settings, the transition from primary school (grade 8th) to secondary school (9th grade) also comes with an increase in costs in terms of higher tuition payments. Primary school education is free while annual tuition in secondary schools in our sample is about \$60-\$100 per year.

Our best evidence that 9th graders are more vulnerable to dropping out of school and therefore could benefit more from the education intervention comes from the survey and administrative data that we have collected for our sample. The probability of taking the national JCE exam, which is administered at the end of  $10^{\text{th}}$  grade provides the most reliable information on education achievement because it comes from administrative sources and passing this exam is required in order to be able to continue into 11th grade of high school. For our control group, the probability of eventually taking the JCE exam is 62.4% for the students that we enrolled at the start of 9th grade and 88.8% for those who we enrolled at the start of  $10^{\text{th}}$  grade. Assuming that not taking the JCE means dropping out of school by the end of 10th grade, these probabilities imply that the dropout rate during  $10^{\text{th}}$  grade of our 10th grade cohort is 11.2%. If this probability can be applied to the  $9^{\text{th}}$  grade cohort, this implies that 26.4% (=37.6%-11.2%) dropped out in  $9^{\text{th}}$  grade. A similar picture emerges from the self-reported drop-out rate of 9th and 10th graders, measured during the first follow-up after one year, which is 13.5% and 9.9%, respectively. While these self-reported outcomes are likely to suffer from measurement error, these numbers imply that the self-reported drop-out rate of 9th graders is about 36% (=(13.5-9.9)/9.9) higher than that of 10th graders.

In addition, we note that the patterns of drop-out by grade that we observe in our sample can be observed more generally in Malawi. While data from education information systems are known to be subject to reporting issues, Education Management Information System data allows us to calculate average drop-out rates in Malawi for the period 2009-2014 (*36*). The drop-out rate of female Form 1 students (9th grade) during this period is 8.4% compared to 6.4% for those in Form 2 (10th grade). Moreover, there is also a difference in dropout due to inability to pay school fees (3% versus 2%) during the same period.

### S1.5 Further details of the risk-domain and time-domain experiments

Study subjects were presented with a set of decision problems under risk and a set of inter-temporal choice problems in the experiments. A decision problem in each task was graphically presented as a choice from a two-dimensional budget set. The budget set in decision problems under risk represents a set of portfolio options associated with two equally probable unknown states. In inter-temporal choice problems this represents a set of money allocations between two payment dates. This experimental method has been used in the literature over different decision domains: decision making under uncertainty (3, 11, 37) and social decisions (12, 38, 39). In order to facilitate the implementation of the experiments in the field, we presented subjects with a paper version of budget sets. The detail of the experiment can be found in the experimental instructions included in Section S4.

For decision making under risk, subjects were presented with 20 decision problems that share a common format but have varying slopes and levels of the budget lines. In each decision problem a subject was asked to choose one option out of 11 options that are presented along a budget line. An option (x, y) indicates the amount of money x that the subject would earn if the x-axis is chosen and the amount of money y that the subject would earn if the y-axis is chosen. Once the subject made a decision, the subject moved to a next problem. After the experiment and the survey were over, one problem out of 20 decision problems was randomly chosen. In the selected decision problem, the x-axis or the y-axis was randomly chosen with equal probability. Subjects obtained earnings according to their choices and coin tossing in the selected decision problem before they left.

The inter-temporal choice experiment contains two parts. The first part comprises 15 decisions of allocating money between tomorrow and 31 days later from the time of the experiment. The second part consists of 15 decisions of allocating money between 1 year and 1 year and 30 days from the time of the experiment. In each decision problem, the subjects were asked to choose one out of 11 options that are again presented along a budget line. Each option (x, y) indicates the amount of money x that the subject will receive at the earlier payment date and the amount of money y that the subject will receive at the later payment date. The set of decision problems in the first part have varying slopes and levels of the budget lines and were used again in the second part in a different order. One out of every 100 subjects was randomly chosen to be paid in the inter-temporal choice experiment. For each of the subjects who were selected for earnings, she received money at two payment dates according to her choice in one out of the 30 inter-temporal choices, which was randomly chosen. Payments at promised dates from the time-domain experiment were paid through the AFF local office. Because the operation of AFF has been well known to local people, we believe there is little concern of credibility of money delivery on promised dates. All payments were made as promised.

The order of the risk-domain experiment and the time-domain experiment was randomized at the individual level.

### S1.6 Measuring economic rationality

Our lab-in-the-field experiments allow us to measure, as the criterion of economic rationality, consistency with utility maximization in the risk domain and in the time domain. In addition we consider compliance with stochastic dominance as an alternative criterion.

**Consistency with utility maximization hypothesis.** Classical revealed preference theory (*13, 40*) allows us to test for economic rationality, *i.e.*, whether individual behavior is consistent with the utility maximization model: Choices from a finite collection of budget lines are consistent with maximizing a (well-behaved) utility function if and only if they satisfy the Generalized Axiom of Revealed Preference (GARP).

Each budget line in the experiments can be represented by  $p_1x_1 + p_2x_2 = 1$ . In the experiment of decision making under risk, for  $i = 1,2, x_i$  denotes the demand for the security that pays off if state *i* is realized and  $p_i$  denotes its price. In the intertemporal choice experiment,  $x_i$  denotes the allocation of money at a payment date i and  $p_i$  denotes its price. We normalize the individual's income to 1. Let  $\{(p^t, x^t)\}_{t=1}^T$  be the data generated by an individual's choices from one of the experiments, where  $p^{t}$ denotes the tth observation of the price vector and  $x^t$  the associated choice vector. A utility function  $u(x^t)$  is said to *rationalize* the data  $(p^t, x^t)$  for t = 1, ..., T if for all t,  $u(x^t) \ge u(x)$  for all x such that  $p^t \cdot x^t \ge p^t \cdot x$ . A choice vector  $x^t$  is directly revealed preferred to a choice vector  $x^s$ , denoted  $x^t R^D x^s$ , if  $p^t \cdot x^t \ge p^t \cdot x^s$ . A choice vector  $x^{t}$  is (indirectly) revealed preferred to a choice vector  $x^{s}$ , denoted  $x^{t}Rx^{s}$ , if there exists a sequence of choice vectors  $\{x^k\}_{k=1}^K$  with  $x^1 = x^t$  and  $x^K = x^s$ , such that  $x^k R^D x^{k+1}$ for every k = 1, ..., K - 1. The GARP requires that if  $x^t R^D x^s$  then  $p^s \cdot x^s \le p^s \cdot x^t$ . Afriat (13) shows that the following two are equivalent: (i) a finite data set generated by an individual's choices in budget lines satisfies GARP; and (ii) there exists a wellbehaved utility function that can rationalize the choice data. Therefore, we check GARP using individual choice data from the choice under risk experiment and each part of the inter-temporal choice experiment separately. Because we select a set of extensively intersecting budget lines for each experiment, our choice data yields a stringent test for utility-maximizing behavior.

Because GARP offers an exact test-either the data satisfy GARP or not-, it is desirable to measure the extent of GARP violations. We report Afriat's (14) critical cost efficiency index (CCEI), which measures the fraction of the income by which each budget constraint must be shifted in order to remove all violations of GARP. Formally,

For any number  $0 \le e \le 1$ , define the direct revealed preference relation  $R^{D}(e)$  as  $x^{t}R^{D}(e)x^{s}$  if  $ep^{t} \cdot x^{t} \ge p^{t} \cdot x^{s}$ , and define R(e) to be the transitive closure of  $R^{D}(e)$ . Let  $e^{*}$  be the largest value of e such that the relation R(e) satisfies GARP. For any data set  $\{(p^{t}, x^{t})\}_{t=1}^{T}$ , we define Afriat's CCEI as the value of  $e^{*}$  associated with that. By definition, the CCEI is bounded between 0 and 1. The closer the CCEI is to one, the smaller the fraction of the budget constraints required to remove all violations and thus the closer the data are to satisfying GARP. We compute CCEI in each experimental domain for an index of the subject's level of economic rationality.

In addition, we report an alternative measure of economic rationality in the literature, proposed by (41), to refine Afriat's CCEI. The Varian measure reflects the minimum adjustment required to remove the violations of GARP related to each observation  $(p^t, x^t)$ . Let  $e^t$  denote the largest value of e such that R(e) has no violations of GARP within the set of choice vectors  $x^s$  such that  $x^tR(e)x^s$ . The value  $e^t$  measures the extent to which GARP is violated when the choice data are compared to the choice vector  $x^t$ . In this way, we can compute the set  $\{e^t\}_{t=1}^T$  that allow us to know where the inefficiency caused by the GARP violation is greatest or smallest. While there are several aggregation rules of summarizing the set to a single index, we take the minimum of  $\{e^t\}_{t=1}^T$  as Varian's index (41).

In the pre-analysis plan at AEA RCT Registry AEARCTR–0001243 (16), we suggested an alternative way of grouping outcomes in the realm of economic rationality for multiple hypothesis testing: CCEI in the risk domain, the minimum of two CCEIs in the time domain, Varian in the risk domain, and the minimum of two Varian measures in the time domain. We believe that this grouping method is inferior to the grouping method we used in Table 2 because the Varian index measures the same outcome of the extent of GARP violation as CCEI. In any way, we pursued this alternative way of grouping. The results (available upon request) are similar to the results of Table 2.

**Stochastic dominance**. In our risk-domain experiment where the two states are equally probable, we can check whether choices are also consistent with first-order stochastic dominance. The discussion of compliance with stochastic dominance in the experiment is based on (3). It requires that a portfolio of two securities should be preferred to another, regardless of participants' risk attitudes, if the former yields unambiguously more money than the latter in a stochastic sense. The principle of first-order stochastic dominance is compelling and widely accepted in economic modelling.

A simple violation of stochastic dominance is illustrated in Figure S1, which is borrowed from Online Appendix IV of (3). The budget line is defined by the straight

line  $\overline{AE}$  where the price of the security associated with state 1,  $p_1$ , is higher than that of the security with state 2,  $p_2$ . The point *B*, which lies on the 45 degree line, corresponds to an allocation with a certain outcome. The individual chooses allocation *x* (an allocation along the line segment  $\overline{AB}$ ). We can find an allocation *y* inside the budget set by taking the mirror image of allocation *x* along the 45 degree line. Because the two states are equally probable, the resulting payoff distributions of allocations *x* and *y*,  $F_x$ and  $F_y$ , are basically the same. Any allocation *x'* along the line segment  $\overline{CD}$  would then yield unambiguously more money than the original allocation *x* in the sense that  $F_{x'} \leq F_x$ . Therefore, allocation *x* violates first-order stochastic dominance. Notice that any decision along  $\overline{AB}$  violates stochastic dominance but do not need to involve a violation of GARP, whereas any decision along  $\overline{BE}$  never violates dominance.

In the analysis of the paper, we report two measures of individual choices complying with stochastic dominance. Firstly, we have the relative frequency of decision problems complying with stochastic dominance out of the 20 decisions in the experiment. If all choices are consistent with the dominance principle, this index would then be equal to one. Otherwise, it would be less than one. Secondly, we use expected payoff calculations to assess how closely individual choices comply with dominance. We illustrate the second measure using Figure S1. The extent to which allocation x in Figure S1 violates dominance can be measured by its expected return as a fraction of the maximal expected return that could be achieved by choosing an allocation x'. The point D corresponds to the allocation x' with the highest expected return. We thus have a measure of compliance with stochastic dominance in this decision problem as the ratio of the expected return of allocation x to that of allocation  $x':(\alpha + \beta)/(\gamma + \beta)$ . For each observation  $(p^t, x^t)$ , this measure takes a value of one if no feasible allocation dominates the chosen allocation. Otherwise, it has a value less than one. Because there are in total 20 decision problems, we average this violation index across the 20 decisions for each individual.

### S1.7 Measuring economic preferences

Our experimental design allows us to measure both rationality and economic preferences–risk attitudes from the risk-domain experiment and impatience from the time-domain experiment–from a single realm of decision-making.

In the risk-domain experiment, we measure an individual's attitudes toward risk with the average fraction of money that she allocated to the cheaper security. The less risk averse the individual is, the larger the fraction of money she will allocate to the cheaper security. Hence, this index of risk attitudes amounts to one if the individual exhibits risk neutral or risk seeking behavior–putting all money to the cheaper security– and equals one half if her behavior is consistent with infinite risk aversion–allocating money equally between the two securities. It has the merit of measuring attitudes toward risk without making assumptions about the parametric form of the underlying utility function. It has been also used as a summary statistic of risk attitudes in (*3*).

Analogously, in each time frame of the time-domain experiment, we summarize an individual's degree of impatience with the average fraction of money that she allocated to the sooner payment date. The more impatient the individual is, the larger fraction of money she will allocate to the sooner payment date. In the case where the price of money at the sooner payment date is higher than that at the later payment date as in our experiment, this index of impatience is equal to one if the individual is extremely impatient–allocating all money to the sooner payment date–and corresponds to 0 if she is extremely patient–allocating all money to the later payment date. Again, an advantage of this measure is nonparametric in the sense that we do not need to make assumptions about the parametric form of time discounting and the underlying utility function.

Table S5 Columns (1) ~ (3) report the results using measures of economic preferences with the choice data of the risk-domain and the time-domain experiments. Overall, we find no difference in economic preferences between subjects in the treatment group compared to the control group in terms of nonparametric measures of time impatience and risk attitudes.

Our results of no impact of education on economic preferences are somewhat different from the findings of two studies in the literature (42, 43), which reported positive impacts of education on patience. We point out the important methodological differences between these studies and ours by noting that our evidence is based on a randomized controlled trial of an education intervention and financially-incentivized experiments.

### **S1.8 Measuring personality**

We measure an individual's personality types using a 10-item scale that assesses the respondent's characteristics based on traits commonly known as the Big 5 personality traits (extroversion, agreeableness, conscientiousness, emotional stability, openness to experience) (44). The literature has established that personality is associated with long-term economic outcomes such as employment and income (45-48). Table S5 Columns (4) ~ (8) report the effect of the education support program on personality. We observe small but positive treatment effects on extroversion, agreeableness, conscientiousness, and emotional stability of personality traits. The treatment group students have 0.051 percentage points (1.3 percent), 0.087 percentage points (1.4 percent), 0.131 percentage points (2.2 percent), and 0.107 percentage points (1.9 percent) higher on extroversion (Column (4)), agreeableness (Column (5)), conscientiousness (Column (6)), and emotional stability (Column (7)), respectively than the control group students. These impacts on personality are mainly driven by 9<sup>th</sup> graders (Panel B).

Our findings are in line with those reported in the literature of the malleability of personality. There is evidence that personality changes over time. For example, (49) showed that at age 15, individuals have on average a 40% probability to change their personality type, but by age 36 their type stabilizes based on Household Income and Labour Dynamics in Australia (HILDA). Using the same dataset, (50) showed several life events such as marriage, family members detained in jail, leaving the workforce and long-term health problems are associated with subsequent changes in personality. Psychology studies on personality trait stability also report that intra-individual stability increases up to age 30 and thereafter stabilizes (51, 52).

More importantly, there are several studies to show that education changes personality traits. For example, (53) showed that shortening the length of high school in Germany caused students on average to be more extroverted and less emotionally stable. (54) also argue that exposure to university may change students to be more extroverted and agreeable. (55) provide evidence from a compulsory schooling reform in China that schooling makes individuals more conscientious, open, and extroverted.

### **S1.9** Measuring cognitive skills

We measure cognitive ability using the Raven's Progressive Matrices test (56) and a simple math test in the long term follow-up survey. According to the literature on intelligence (e.g., (57)), there are at least two distinct types of IQ: crystallized intelligence and fluid intelligence. The former relates to the individual's store of knowledge about the nature of the world and learned operations such as arithmetic calculations. Our math test intends to capture the aspect of crystallized intelligence. The latter is the ability to solve novel problems that depend relatively little on stored knowledge as well as the ability to learn. Raven's Progressive Matrices test is often

considered the best available measure for fluid intelligence (57). Since Raven test is independent of language skills, it is very easy to conduct in any setting including developing countries where the mother tongue is not English. We use 12 Raven test questions.

Cognitive skills are also measured through a math test on addition, subtraction, multiplication, and division. For the 12 math questions, three minutes were given to the study participants to complete this test. The grading scheme was carefully explained to study participants: The total score is  $1 \times$  the number of correct answers –  $0.25 \times$  the number of wrong answers. Both the Raven and math scores are standardized using a sample of students who were 9<sup>th</sup> and 10<sup>th</sup> graders at baseline.

Table S5 Columns (9) ~ (10) show that the education support program enhances cognitive skills, in particular, the math test score. The treated students perform 0.089 standard deviations for the Raven test score and 0.196 standard deviations for the math test score better than the control group students, although the impact on the Raven test score is imprecisely estimated. These impacts on cognitive skills are larger for 9<sup>th</sup> graders (0.161 SD for the Raven score and 0.244 SD for the math test score) than 10<sup>th</sup> graders (0.023 SD for the Raven score and 0.164 SD for the math test score). The impact of the educational intervention on our measures of cognitive outcomes is large. As reported earlier, our intervention increases schooling by 0.1 years and the performance on the math score by 0.196 standard deviations. This result, when scaled in terms of the years of schooling induced by the intervention is much larger than the results from existing studies in Western settings (*58, 59*).

# S1.10 Accounting for cognitive and non-cognitive skills and economic preferences

In order to investigate whether the impacts of the education intervention on economic rationality are mediated through the changes in cognitive and non-cognitive skills as well as changes in economic preferences, we report in Table S6 the impacts of education support program on economic rationality with and without these controls.

Columns (1) and (2) of Table S6 present the main results with and without the baseline controls. In Columns (3) to (7), we additionally control for risk preferences, time preferences, cognitive ability (math score and raven test result), and personality (extroversion, agreeableness, conscientiousness, emotional stability, openness to experience) measured at the long-term follow-up survey.

We find that our rationality scores are explained only partially by these variables. For example, for 9<sup>th</sup> graders, the additional controls (Column (7)) reduce the impacts on rationality scores by 33% (=(0.033-0.022)/0.033, in CCEI from risk domain). Among additional controls, cognitive abilities and measures of personality explain the variation in rationality scores more than the measures of time and risk preference. For example, cognitive abilities (Column (5)) reduce by 30% (=(0.033-0.023)/0.033) and 15% (=(0.027-0.023)/0.027) the CCEI scores from the risk and time domain for 9<sup>th</sup> graders, respectively while risk preferences (Column (3)) and time preferences (Column (4)) have no impact.

While we tried to encompass distinct aspects of cognitive and non-cognitive skills and control their mediating role in the causal effect of education on economic rationality, it is likely that our measures cannot fully capture all aspects of cognitive and non-cognitive skills that could potentially be related to economic rationality. Therefore, the results in Table S6 need to be interpreted with care, because our experimental educational intervention can have a complex and multidimensional impact on a wide range of outcomes such as preferences, constraints, information, or beliefs, which could be measured with error by our survey and experiment instruments.

### S1.11 Imperfect understanding

A possible concern is that some respondents did not fully understand the experiments. Since most of the previous literature with these experiments used samples from developed countries, adolescents in a low-income country setting may be unlikely to understand these exercises to the full extent. If treated individuals become better at understanding the instructions and the experiments, it may spuriously lead to improved measure of decision quality. In order to dissipate this concern, we begin by comparing the cumulative distribution functions of CCEIs for the control group and the treatment group in each of the risk and time domains. The CDFs in Figure S2 show that the education intervention improves individual economic rationality by shifting the distribution of CCEIs to the right almost over the whole range of CCEI except for the lowest values of CCEI. That is, the education intervention does not appear to change the left-most part of the distributions of the control group and the treatment group. Therefore, we conclude that the treatment impact on the shift of the distribution is difficult to justify by an imperfect understanding of the tasks.

We also address this imperfect understanding concern within the experiment. It is possible that respondents who don't understand the exercise at first may learn it as the task progresses. Thus, we conduct the analysis of what happens after throwing out the first few choices. We throw out the first three choices for all subjects in the risk domain experiment and in each frame of the time domain experiment. As shown in Table S7, the treatment effects on rationality scores do not change much. Given the assumption that the speed of learning in the experiment is on average similar between the control group and the treatment group, our exercise of eliminating the first three choices confirms that the education intervention improves economic rationality even when we confine attention to choices after learning without feedback reasonably occurs.

Finally, we would like to highlight that we were also aware of this potential concern from the beginning of the project. We conducted a pilot experiment with those who at baseline were in 11th grade (those are not in our long term follow-up sample) and found that subjects in the pilot understood well the nature of decision problems but they were not accustomed to using the mouse in the computerized experiment as in (3). Therefore we resorted to the pen-and-paper design that was implemented in the field.

### S1.12 Differential efforts

One alternative explanation of our main results in Table 2 mentioned in the main text is that program beneficiaries might put in more effort during the follow-up survey, perhaps because they are grateful for having received the financial support or because they trusted the implementing NGO for making the payments that were part of the experimental intervention. This differential effort during the experiments could affect the measures of their decision quality. We have implemented two indirect tests that are possible with our data that help mitigate concerns related to this alternative explanation.

First, we note that participation in the follow-up survey is balanced between the treatment and control groups (Table S2) and one might argue that this is a first order indicator of effort on the part of the subjects enrolled in the study. Second, we created other indirect effort indicators such as number of missing and "do not know" responses. Columns (1) ~ (2) of Table S8 present the number of missing responses in the experiment and Columns (3) ~ (4) presents results for the number of "do not know" responses in the survey. We do not find evidence that the treatment group put more efforts on the experiment and survey at least when using these measures. Finally, we note that our paper surveys did not record the time spent on different tasks and the survey in general and as a result we could not estimate the impact of the intervention on time responding to our follow-up survey.

# S2. Supplementary Figures





# Figure S2. The cumulative distribution functions of rationality indices

A. CDFs of CCEI in the risk domain



# B. CDFs of CCEI in the time domain



## **S3.** Supplementary Tables

## **Table S1: Randomization Check**

	(	Overall sample	Bas	eline 9th graders	8	Baseline 10th graders			
	Mean			Mean			Mean		
	control	Treated	Ν	control	Treated	Ν	control	Treated	Ν
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Height (cm)	156	0.146	2,800	155	0.804*	1,211	157	-0.104	1,589
	[5.78]	(0.314)		[5.80]	(0.437)		[5.68]	(0.373)	
Weight (kg)	51.3	0.116	2,803	50.0	0.212	1,213	52.1	0.499	1,590
	[7.32]	(0.481)		[7.61]	(0.764)		[7.03]	(0.406)	
Age (years)	15.3	-0.007	2,812	14.8	-0.010	1,220	15.6	0.187	1,592
	[1.51]	(0.186)		[1.50]	(0.222)		[1.43]	(0.207)	
Orphan	0.047	-0.008	2,808	0.040	0.004	1,220	0.052	-0.017	1,588
	[0.213]	(0.008)		[0.195]	(0.010)		[0.222]	(0.011)	
Majority Ethnicity Groups	0.769	-0.011	2,812	0.778	-0.024	1,220	0.764	-0.001	1,592
	[0.422]	(0.031)		[0.416]	(0.046)		[0.425]	(0.043)	
Muslim	0.068	-0.004	2,810	0.065	0.000	1,219	0.070	-0.007	1,591
	[0.252]	(0.012)		[0.248]	(0.018)		[0.255]	(0.017)	
Father's Tertiary Education	0.204	0.01	2,812	0.224	-0.015	1,220	0.193	0.027	1,592
	[0.403]	(0.029)		[0.417]	(0.042)		[0.395]	(0.040)	
Mother's Tertiary Education	0.098	-0.001	2,812	0.111	-0.012	1,220	0.091	0.004	1,592
	[0.298]	(0.018)		[0.314]	(0.028)		[0.288]	(0.023)	
Father's White Collar Job	0.266	-0.001	2,812	0.271	0.006	1,220	0.262	-0.009	1,592
	[0.442]	(0.028)		[0.445]	(0.045)		[0.440]	(0.034)	
Mother's White Collar Job	0.111	-0.007	2,812	0.131	-0.031	1,220	0.099	0.009	1,592
	[0.314]	(0.018)		[0.337]	(0.030)		[0.299]	(0.023)	
Household Assets (0 - 16)	7.72	-0.194	2,812	7.89	-0.496	1,220	7.63	0.037	1,592
	[3.499]	(0.487)		[3.544]	(0.748)		[3.470]	(0.644)	
School Type	0.176	0.107	2,812	0.110	0.189	1,220	0.216	0.052	1,592
	[0.381]	(0.101)		[0.312]	(0.121)		[0.412]	(0.156)	

*Notes*: Standard deviations are in square brackets, standard errors clustered at classroom level are in parentheses. Column (2), (5), and (8) display the difference in means between the treatment group and the control group. Column 3 displays the *p*-value of the *F*-test that the difference in means between the treatment group and the control group is zero. Orphan equals one when both parents died. Majority ethnicity groups equals one when students' ethnicity is Chewa, Ngoni, or Tumbukas. Parent's tertiary education equals one when they graduate from a 2-year college or a 4-year university. Parent's white-collar job equals one when they have a professional or government job. Household Assets are defined the total number of assets they own from 16 asset questions. School type equals one when students attend a conventional secondary school. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

	Overall sample				Baseline 9th graders				Baseline 10th graders			
Variables:	= 1 if in the short up s	surveyed t-term follow- survey	= 1 if in the long- su	surveyed term follow-up ırvey	= 1 if s in the short- up s	surveyed -term follow- urvey	= 1 if in the long- su	surveyed term follow-up urvey	= 1 if s in the short up s	surveyed -term follow- survey	= 1 if in the long- su	surveyed term follow-up urvey
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment	0.048*	0.044	-0.002	-0.003	0.043	0.020	0.014	0.001	0.052*	0.057**	-0.014	-0.007
	(0.029)	(0.029)	(0.017)	(0.015)	(0.058)	(0.061)	(0.026)	(0.023)	(0.028)	(0.026)	(0.022)	(0.021)
Age (years)		-0.021***		-0.024***		-0.016		-0.029***		-0.026**		-0.021**
		(0.008)		(0.006)		(0.011)		(0.007)		(0.011)		(0.008)
Orphan		-0.063		-0.005		-0.013		-0.046		-0.097		0.027
		(0.051)		(0.039)		(0.062)		(0.055)		(0.077)		(0.052)
Father's		0.033		-0.013		0.018		0.029		0.044		-0.040
Tertiary Education		(0.030)		(0.019)		(0.048)		(0.028)		(0.041)		(0.025)
Mother's		-0.085**		0.007		-0.074		-0.037		-0.090*		0.040
Tertiary Education		(0.037)		(0.022)		(0.056)		(0.033)		(0.051)		(0.029)
Father's		-0.038*		0.021		-0.047		-0.020		-0.027		0.049**
White Collar Job		(0.021)		(0.016)		(0.034)		(0.024)		(0.028)		(0.019)
Mother's		-0.038		0.006		-0.022		0.013		-0.051		0.003
White Collar Job		(0.035)		(0.024)		(0.056)		(0.035)		(0.046)		(0.032)
Household Assets		-0.001		-0.008***		0.000		-0.009***		-0.003		-0.008***
(0 - 16)		(0.005)		(0.002)		(0.008)		(0.003)		(0.005)		(0.002)
School Type		0.061**		0.028		0.120**		0.044		0.027		0.015
(Conventional School)		(0.030)		(0.020)		(0.059)		(0.028)		(0.034)		(0.028)
Grade Fixed Effect	-0.036	-0.019	0.001	0.023								
(10th grade)	(0.032)	(0.032)	(0.017)	(0.018)								
Observations	2,812	2,808	2,812	2,808	1,220	1,220	1,220	1,220	1,592	1,588	1,592	1,588

## Table S2: Attrition Balance

Sources:	Short-term FU Survey	Short-term FU Survey	Administrative Data	Administrative Data	Long-term FU Survey	Combined Data
	Absence	Dropout	Took	Pass	Total years	Standardized
Variables:	Self-reported	Self-reported	JCE	JCE	of Education	Treatment
	(2013)	(2013)	(2012-2013)	(2012-2013)	(2015-2016)	Effect
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Overall sample						
Treated	-1.703***	-0.036	0.059**	0.081*	0.092	0.026**
	(0.441)	(0.035)	(0.024)	(0.041)	(0.131)	(0.011)
Family-wise adjusted p-values	0.001	0.282	0.038	0.038	0.524	
Control group mean	4.01	0.112	0.789	0.597	11.5	0
Number of observations	1,852	1,930	2,812	2,812	2,423	11,829
Panel B: Baseline 9th graders						
Treated	-1.491***	-0.101**	0.144***	0.151**	0.158	0.047***
	(0.365)	(0.049)	(0.042)	(0.060)	(0.204)	(0.017)
Family-wise adjusted p-values	0.001	0.038	0.005	0.038	0.453	
Control group mean	3.53	0.135	0.624	0.509	11.3	0
Number of observations	855	889	1,220	1,220	1,051	5,235
Panel C: Baseline 10th graders						
Treated	-1.861**	0.012	-0.004	0.028	0.042	0.010
	(0.720)	(0.047)	(0.023)	(0.056)	(0.171)	(0.014)
Family-wise adjusted p-values	0.038	0.947	0.947	0.751	0.947	
Control group mean	4.28	0.099	0.888	0.649	11.6	0
Number of observations	997	1,041	1,592	1,592	1,372	6,594

## Table S3: Impacts of Education Support Program on Education Outcomes (without baseline controls)

V	CCEI	CCEI	Compliance with s	tochastic dominance	Standardized
variables:	Risk domain	Time domain	Freq.	Payoff	Treatment Effect
	(1)	(2)	(3)	(4)	(5)
Panel A: Overall sample					
Treated	0.012	0.013*	0.011	0.005	0.019*
	(0.008)	(0.007)	(0.008)	(0.004)	(0.011)
Family-wise adjusted p-value	0.108	0.058	0.120	0.120	
Control group mean	0.81	0.82	0.83	0.94	0.00
Number of observations	2,424	2,419	2,424	2,424	9,691
Panel B: Baseline 9th graders					
Treated	0.033***	0.027**	0.020*	0.010**	0.039***
	(0.011)	(0.011)	(0.010)	(0.005)	(0.015)
Family-wise adjusted p-value	0.001	0.003	0.033	0.014	
Control group mean	0.83	0.83	0.84	0.94	0.00
Number of observations	1,051	1,050	1,051	1,051	4,203
Panel C: Baseline 10th graders					
Treated	-0.003	0.002	0.004	0.001	0.003
	(0.012)	(0.010)	(0.011)	(0.005)	(0.015)
Family-wise adjusted p-value	0.953	0.953	0.908	0.953	
Control group mean	0.80	0.82	0.82	0.93	0.00
Number of observations	1,373	1,369	1,373	1,373	5,488

# Table S4: Impacts of Education Support Program on Economic Rationality (without baseline controls)

Economic Preference			Personality						Cognitive skills	
Variables:	Time impatience near frame	Time impatience distant frame	Risk tolerance	Extroversion	Agreeableness	Conscien- tiousness	Emotional stability	Openness to experience	Raven test	Math score
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Overall sample										
Differences adjusted	-0.004	-0.002	0.001	0.051*	0.087*	0.131**	0.107*	-0.055	0.089	0.196***
for baseline controls	(0.010)	(0.010)	(0.004)	(0.030)	(0.049)	(0.054)	(0.057)	(0.062)	(0.056)	(0.050)
Number of observations	2,416	2,416	2,421	2,421	2,421	2,421	2,421	2,421	2,421	2,421
Control group mean	0.398	0.400	0.625	3.867	6.211	6.004	5.605	3.840	0	0
Panel B: Baseline 9th gra	ders									
Differences adjusted	-0.015	-0.009	0.002	0.056	0.159**	0.202**	0.203**	0.033	0.161*	0.244***
for baseline controls	(0.014)	(0.014)	(0.006)	(0.045)	(0.078)	(0.083)	(0.095)	(0.102)	(0.081)	(0.066)
Number of observations	1,050	1,050	1,051	1,051	1,051	1,051	1,051	1,051	1,051	1,051
Control group mean	0.411	0.408	0.632	3.842	6.201	5.992	5.623	3.811	0.053	0.014
Panel C: Baseline 10th gr	aders									
Differences adjusted	0.004	0.004	-0.001	0.049	0.031	0.078	0.030	-0.109	0.023	0.164**
for baseline controls	(0.014)	(0.014)	(0.005)	(0.040)	(0.059)	(0.071)	(0.065)	(0.075)	(0.069)	(0.070)
Number of observations	1,366	1,366	1,370	1,370	1,370	1,370	1,370	1,370	1,370	1,370
Control group mean	0.390	0.396	0.621	8.882	6.218	6.012	5.595	3.856	-0.031	-0.008

# Table S5: Impacts of education support program on economic preferences, personality, and cognitive skills

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. CCEI from risk domain							
Panel A1. Overall sample							
Difference	0.012	0.013	0.012	0.014*	0.006	0.009	0.005
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)
family-wise adjusted p-value	0.086	0.071	0.086	0.053	0.509	0.250	0.587
Panel A2. Baseline 9th graders							
Difference	0.033***	0.033***	0.032***	0.035***	0.023**	0.026***	0.022**
	(0.011)	(0.010)	(0.011)	(0.011)	(0.009)	(0.009)	(0.010)
family-wise adjusted p-value	0.001	0.000	0.001	0.000	0.009	0.002	0.023
Panel A3 Baseline 10th graders							
Difference	-0.003	-0.003	-0.003	-0.003	-0.009	-0.005	-0.009
	(0.012)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.009)
family-wise adjusted p-value	0.910	0.871	0.865	0.865	0.496	0.760	0.451
Panel B. CCEI from time domain							
Panel B1. Overall sample							
Difference	0.013*	0.014**	0.014**	0.015**	0.009	0.012*	0.008
	(0.007)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)
family-wise adjusted p-value	0.049	0.010	0.010	0.010	0.164	0.035	0.183
Panel B2. Baseline 9th graders							
Difference	0.027**	0.031***	0.030***	0.032***	0.023***	0.026***	0.022***
	(0.011)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
family-wise adjusted p-value	0.002	0.000	0.000	0.000	0.002	0.001	0.003
Panel B3 Baseline 10th graders							
Difference	0.002	0.003	0.003	0.002	-0.002	0.002	-0.001
	(0.010)	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)	(0.008)
family-wise adjusted p-value	0.910	0.871	0.865	0.865	0.801	0.760	0.813
Panel C. Standardized Treatment	Effect						
Panel C1. Overall sample							
Difference	0.035*	0.039**	0.037**	0.041**	0.020	0.029*	0.019
	(0.020)	(0.018)	(0.018)	(0.018)	(0.017)	(0.017)	(0.016)
Panel C2. Baseline 9th graders							
Difference	0 083***	0 088***	0.086***	0 00/***	0.065***	0.073***	0.062***
	(0.029)	(0.021)	(0.023)	(0.023)	(0.021)	(0.019)	(0.021)
Panel C3 Baseline 10th graders	(0.027)	(010-1)	(01020)	(01020)	(0.02-2)	(0.000)	(010-1)
Difference	0.001	0.000	0.001	0.001	0.012	0.002	0.010
Difference	-0.001	(0.000)	(0.001)	-0.001 (0.024)	-0.013	-0.002	-0.013
Baseline controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Risk preference	No	No	Vec	No	No	No	Vec
	NU.	N-	1 C5	NU V	N-	N-	V
Time preference	INO	NO	NO	r es	INO V	NO	res
Cognitive abilities	No	No	No	No	Yes	No	Yes
Personality	No	No	No	No	No	Yes	Yes

### **Table S6: Impacts on economic rationality with further controls**

V	CCEI		C	CCEI		Compliance with stochastic dominance				
variables:	Risk o	domain	Time	domain	Fr	eq.	Pay	/off		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
Panel A: Overall sample										
Treated	0.011	0.012	0.014**	0.016**	0.010	0.011*	0.005	0.005		
	(0.008)	(0.007)	(0.007)	(0.006)	(0.007)	(0.006)	(0.003)	(0.003)		
Baseline control	Ν	Y	Ν	Y	Ν	Y	Ν	Y		
Control group mean	0.83		0	0.85		83	0.94			
Number of observations	2,424	2,421	2,419	2,416	2,424	2,421	2,424	2,421		
Panel B: Baseline 9th graders										
Treated	0.029**	0.029***	0.023**	0.027***	0.019*	0.016*	0.009*	0.007*		
	(0.011)	(0.009)	(0.011)	(0.008)	(0.010)	(0.008)	(0.005)	(0.004)		
Baseline control	Ν	Y	Ν	Y	Ν	Y	Ν	Y		
Control group mean	0	.85	0	.86	0.	0.85		94		
Number of observations	1,051	1,051	1,050	1,050	1,051	1,051	1,051	1,051		
Panel C: Baseline 10th g	raders									
Treated	-0.002	-0.002	0.007	0.008	0.004	0.005	0.001	0.002		
	(0.010)	(0.009)	(0.009)	(0.008)	(0.010)	(0.009)	(0.005)	(0.004)		
Baseline control	Ν	Y	Ν	Y	Ν	Y	Ν	Y		
Control group mean	0	.82	0	0.85		82	0.93			
Number of observations	1 373	1 370	1 360	1 366	1 373	1 370	1 373	1 370		

Table S7: Impacts on economic rationality by eliminating the first 3 choices

Notes: Coefficients are from linear regressions of each outcome on the education intervention indicator.

Standard errors (in parentheses) are clustered at the classroom level. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Variables:	Missing res Exper	ponses from iments	"Don't Know" responses		
	(1)	(2)	(3)	(4)	
Panel A: Overall sample					
Treated	-0.020	-0.026	-0.057	-0.076	
	(0.030)	(0.028)	(0.079)	(0.072)	
Baseline control	Ν	Y	Ν	Y	
Control group mean	0.2	222	0.918		
Number of observation	2,424	2,421	2,424	2,421	
Panel B: Baseline 9th graders					
Treated	0.033	0.006	-0.099	-0.093	
	(0.047)	(0.040)	(0.125)	(0.110)	
Baseline control	Ν	Y	Ν	Y	
Control group mean	0.1	155	1.0	81	
Number of observation	1,051	1,051	1,051	1,051	
Panel C: Baseline 10th graders					
Treated	-0.059	-0.056	-0.026	-0.034	
	(0.037)	(0.037)	(0.102)	(0.090)	
Baseline control	Ν	Y	Ν	Y	
Control group mean	0.2	261	0.822		
Number of observation	1,373	1,370	1,373	1,370	

# Table S8: Probability of missing and "do not know" responses

## **S4. Survey Instruments**

### Instructions for the risk-domain experiment

Direction: Please mark in a circle you would like to choose. You will earn real money, depending on your decisions. Please make careful decisions. In this experiment, you will participate in 20 independent decision problems that share a common format. In each decision problem, you will be asked to choose <u>one option out of 11 options</u> that are presented along a line.

An option [X, Y] indicates the amount of money you will earn if the X-axis (the horizontal axis) is chosen and the amount of money you will earn if the Y-axis (the vertical axis) is chosen. For instance, in the sample picture below, the third option from left, [45, 120], indicates that you will earn 45 MK if the X-axis is chosen and 120 MK if the Y-axis is chosen. The X-axis and Y-axis will be equally likely to be chosen. (First number in each box is the amount of money, and second number is the number of Y tokens.)



In each decision problem, you are encouraged to examine all 11 options along the line and

should choose only one option you like most (please mark N inside the circle of the option you would like choose, as in the sample picture.)



In this example, if you make a decision to choose point such as **B**, you will be given either 90 MK or 90 MK with equal probability. It means you will always get 90 MK for sure. We call this point as the safest point. On the other hand, if you choose point such as **A**, you will be given either 0 MK or 150 MK with equal probability. And if you choose point such as **C**, you will be given either 225 MK or 0 MK. We call this kind of points as the riskiest points, when you can have either nothing or lots of money.



BUT! When you decide to choose a corner point, such as **A or C**, please examine thoroughly because it is possible one of the points would be clearly better than the other. Please think thoroughly before you make a decision. **Again, you have 11 options in total. Please choose one point out of 11 points.** 

At the end of today, one question out of total 20 questions will be randomly chosen. In the chosen question, the x-axis or the y-axis will be randomly chosen with equal probability as well. You will receive your earnings corresponding to your decision in the chosen question. For instance, if you have chosen point B in the example above and the x-axis is randomly chosen, you will receive 90 MK.

# Instructions for the time-domain experiment

Direction: Please mark in a circle you choose. You will earn real money, depending on your decisions. Please make careful decisions.

In this experiment, you will participate in 15 decisions that share a common form. In each decision problem, you will be asked to choose one option out of 11 options that are presented along a line. Each option [ ##, &&] indicates the amount of money (##) you will receive at a sooner date and the amount of money (&&) you will receive at a later date.

In the first part, the sooner payment date is **TOMORROW** and the later payment date is **31 DAYS LATER**. The sample picture below presents a decision problem presenting 11 options of allocating money between the two payment dates. For instance, the third option from left indicates that you will receive 3000 MK tomorrow and 18000 MK 31 days later.



In each decision problem, you are encouraged to examine all 11 options along the line and should choose only one option you like most (please mark  $\sqrt{}$  inside the circle of the option you would like choose, as in the sample picture.)



At the end of today, one question out of total 30 questions from this experiment will be randomly chosen by picking one card from the set of cards with numbers from 1 to 30. According to your choice in the chosen question, you will receive your earnings in future dates.



As you see the sample picture above, you receive more money 31 days later, as you go to the left-upper corner. And you receive more money tomorrow, as you go to the right-lower corner. If you want to receive more money faster, you may go to right-down corner. If you want to receive more money later, you may go to left-up corner. But!! You still have 11 options. You choose one point out of 11 options along the line.

Direction: Please mark in a circle you choose. You will earn real money, depending on your decisions. Please make careful decisions.



This experiment is similar to the previous part of the experiment. The only difference is that the sooner payment date is **365 DAYS LATER** and the later payment date is **395 DAYS LATER**.

The sample picture below presents a decision problem presenting 11 options of allocating money between 365 days later and 395 days later from today.

In each decision problem, you are encouraged to examine all 11 options along the line and should choose only one option you like most (please mark $\sqrt{}$  inside the circle of the option you would like choose, as in the sample picture.)

As you see the sample graph above, you receive more money 395 days later, as you go to the left-upper corner. And you receive more money 365 days later, as you go to the right-lower corner. If you want to receive more money faster, you may go to right-down corner. If you want to receive more money later, you may go to left-up corner.

REMEMBER!! You have 11 options. You choose one point out of 11 points along the line.

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