CESifo WORKING PAPERS

10608
2023

August 2023

Occupational Aspirations and Investments in Education: Experimental Evidence from Cambodia

Esther Gehrke, Friederike Lenel, Claudia Schupp

CESifo

Impressum:

CESifo Working Papers
ISSN 2364-1428 (electronic version)
Publisher and distributor: Munich Society for the Promotion of Economic Research - CESifo GmbH
The international platform of Ludwigs-Maximilians University's Center for Economic Studies and the ifo Institute
Poschingerstr. 5, 81679 Munich, Germany
Telephone +49 (0)89 2180-2740, Telefax +49 (0)89 2180-17845, email office@cesifo.de
Editor: Clemens Fuest
https://www.cesifo.org/en/wp
An electronic version of the paper may be downloaded
· from the SSRN website: www.SSRN.com
· from the RePEc website: www.RePEc.org
· from the CESifo website: https://www.cesifo.org/en/wp

CESifo *Working Paper No. 10608*

Occupational Aspirations and Investments in Education: Experimental Evidence from Cambodia

Abstract

Students in low-income contexts often lack guidance in their career decisions which can lead to a misallocation of educational investments. We report on a randomized field experiment conducted with 1715 students in rural Cambodia and show that a half-day workshop designed to support adolescents in developing occupational aspirations increased educational investments. We document substantial heterogeneity in treatment effects by baseline student performance. While the workshop increased schooling efforts of high-performing students, treated low-performing students reduced their educational investments. We develop a simple model that explains why an information intervention can affect educational aspirations and investments in opposing directions.

JEL-Codes: C930, D830, D900, I210, O150.

Keywords: aspirations, career guidance, education, field experiment.

*Esther Gehrke**
Wageningen University and Research
Wageningen / The Netherlands
esther.gehrke@wur.nl

Friederike Lenel
University of Göttingen / Germany
friederike.lenel@uni-goettingen.de

Claudia Schupp
Technical University Munich / Germany
claudia.schupp@tum.de

*corresponding author

August 6, 2023
We thank Rith Sarakk (PEPY Empowering Youth), Som San and Khlok Yem (Child's Dream), as well as Jenny Aker, Alexandra Avdeenko, Jana Cahlikova, Patricio Dalton, Veronika Geyer, Thomas Ginn, Till Grüneberg, Renate Hartwig, Clément Imbert, Anett John, Julianna Nielson, Ingrid Hoem Sjursen, Amma Panin, Eva Raiber, Lisa Spantig and Susan Steiner for feedback on the design of the experiment and on the interpretation of findings. The paper also benefited from comments by conference and seminar participants at Wageningen University, the University of Göttingen, ENS Lyon, Radboud University, UNU-MERIT, University of Groningen, GDE Conference 2021, 2023, GlaD Conference 2021, EEA congress 2021, SEEDEC 2021, and the C4ED seminar. We thank Aiko Schmeisser, Chhern Sreyneang, Chhly Chaktokrong, Hong Sarith, Hong Sarou, Khann Rada, Khoeut Sochea, Kouth Sochampawatd, Phon Loem Bobon, Sin Chanita, Teung Seila, Touch Hean for excellent fieldwork assistance. This RCT obtained ethical approval by the Ethics Committee of the University of Göttingen (IRB approval date February 11, 2020, amendments approved on June 22, 2020) and was pre-registered with the AEA Trial Registry (Trial ID: AEARCTR-0005460). Funding by the German Research Foundation (RTG 1723) is gratefully acknowledged.

1 Introduction

While access to education in most low- and middle-income countries has improved substantially over the last decades, a large proportion of students still drop out of school prematurely, and few continue with higher education (UNESCO, 2020). Education-related decisions are not easy; they need to be taken when children are relatively young and require substantial guidance (Heckman and Mosso, 2014). In low-income contexts, such guidance is often hard to find. Parents typically have lower educational attainment than their children, and teachers lack the incentives and the resources to be in a position to guide their students individually. As a consequence, students from low-income contexts risk pursuing similar educational and career paths as their parents, thereby perpetuating cycles of poverty.

Against this background, a number of recent policy interventions have aimed to raise aspirations among adolescents, often by featuring role models.[1] In economic theory, aspirations are generally defined as long-term goals that act as reference-points in people's utility function (see e.g. Dalton et al., 2016; Genicot and Ray, 2017; La Ferrara, 2019). Interventions aimed at raising aspirations are based on the assumption that adolescents from low socio-economic backgrounds have inefficiently low aspirations with respect to the level of education they can achieve or the type of career they can pursue (as shown e.g. by Guyon and Huillery, 2020), which in turn deters their educational investments (Beaman et al., 2012; Rizzica, 2020). However, there is ample evidence to suggest that aspirations among adolescents can actually be very high in low- and middle-income countries, even among the poorest in those countries (Janzen et al., 2017; Ross, 2019). Aspirations that are too high could also deter educational investments, as they may lead to frustration once students realize that their goals are unattainable (Genicot and Ray, 2017). This raises the question of whether effective interventions should in fact aim at diversifying the aspirations window, *i.e.*, the set of aspirations that students perceive as attractive and attainable. Diversifying the aspirations window may be particularly relevant in the presence of heterogeneities in student abilities, as a constrained aspirations window implies that students develop aspirations that are insufficiently aligned with their abilities: too high for low performers and too low for high performers.[2]

In this paper, we study whether a half-day workshop designed to expand adolescents' aspirations window can support them in developing more diversified occupational aspirations and thereby influence their educational investments. During the workshop, students first work through an interest and career exploration tool — an app that helps them reflect on their personal interests and allows them explore (personalized) information about different careers that vary in their level of required schooling. Students then participate in an information session that presents paths both to higher education and vocational training, and discusses academic requirements for attending high school, educational costs, and financing options. This low-cost and easily-scalable intervention aims

[1]For recent reviews of various nudging interventions in education, see Damgaard and Nielsen (2018, 2020) and of role-model interventions specifically, see Serra (2022).

[2]The importance of aligning aspirations to individual abilities has been highlighted *inter alia* in the context of sport (Lockwood and Kunda, 1997; Berger and Pope, 2011). For the purpose of this study, we understand student ability to capture cognition, as well as educational investments accumulated earlier in life.

to equip students with the tools they need to develop occupational aspirations that match their abilities and interests, while providing the information necessary to take the next steps in their educational path, *i.e.*, transition to high school or into vocational training.

The workshop was conducted with Grade 9 students in their final year of compulsory schooling in rural Cambodia in early 2020, shortly before schools were closed due to the COVID-19 pandemic. Cambodia is a particularly interesting context in which to study educational investments. During the Khmer Rouge regime in the 1970s, the educational sector was systematically destroyed: schools and universities were closed and educated people fled the country or were persecuted (UNESCO, 2011). With the subsequent Third Indochina War and long period of internal conflict, the educational system was not reconstructed until the late 1990s. As a consequence, education levels among Cambodian adults are extremely low today, with severe repercussions on younger generations: Students lack information and guidance about career paths, and educational aspirations are often highly unrealistic (Eng et al., 2014). Dropout rates are high, and the transition rate to high school is low, especially in rural areas (Ministry of Education, Youth and Sport, 2017).

We evaluate the effect of the intervention by exploiting the randomized assignment of 37 schools to either treatment or control status, with 1,715 students participating in the study, of which 783 took part in the intervention. In terms of outcomes, we focus on schooling information from student-level administrative records collected throughout the last year of lower-secondary school and the first 1.5 years of high school, as well as on self-reported information regarding study-behavior, and educational and occupational aspirations from a phone survey that was conducted about four months after the intervention and during the first COVID-19 lockdown.

We find that attending the workshop had mixed results. We find no statistically significant treatment effects on students' educational or occupational aspirations. While students in treatment schools were also not more likely to study during the first lockdown period than their peers in control schools, they were 2.5 percentage points (pp) more likely to attend the final exam of Grade 9 (2.9% increase over the control group mean), performed better in the final exam conditional on participating (0.21SD) and were by about 5.9pp (7.9%) more likely to enroll in high school, 6.0pp (9.4%) more likely to progress to Grade 11 one year later, 5.2pp (8.5%) more likely to participate in, and 8.6pp (17.6%) more likely to pass the midterm exam of Grade 11, roughly two years after the intervention.

As the intervention provided tools to form aspirations that are more aligned with the students' abilities and interests, their resultant aspirations — and consequentially their educational investments — might differ depending on their ability. Using parametric and semi-parametric techniques, we examine treatment-effect heterogeneities along students' baseline academic performance — which we use as a proxy for students' abilities and which is by far the strongest predictor of subsequent schooling outcomes. We find that for low-performing students, the intervention had negative effects in the short-run (during Grade 9) on most outcomes and no effect in the medium-run (during high school), while the intervention benefited high-performing students in both the short-run and the medium-run. Treated students who performed in the bottom half of the grades distribution at baseline, studied less than their control group peers during the lockdown period, had (weakly) lower educational aspirations, and performed worse in the final exam. By contrast, students who ranked in the upper half of the grades distribution at baseline were more likely to study during the lockdown period, had (weakly) higher educational aspirations, and performed better in the final exam of Grade 9. About one year after the intervention, the negative treatment effect on low-performers disappears, as few of them transitioned to high school in either treatment or control schools. What persists into high school, however, is the positive treatment effect on high-performers: these students kept outperforming their peers from control schools into the midterm exam of Grade 11 and are driving the positive average treatment effect observed in the medium-run.

Average treatment effects and heterogeneities by baseline grades are robust to randomization inference, as well as to corrections for attrition. We also show that the treatment effect heterogeneities are not driven by parental characteristics nor by school characteristics.

Our results suggest that participation in the workshop made low-performing students aware of alternative career paths that do not include higher education and put them in a position to adjust their educational aspirations to levels that are better aligned with their abilities and preferences. The decline in educational investments observed among these students during Grade 9 is consistent with them adjusting their educational investments to a level that is just sufficient to graduate lower-secondary school. By contrast, the workshop seems to have raised aspirations among high-performing students, resulting in higher educational investments and better academic performance compared to their control peers.

To rationalize these findings, we develop a conceptual framework that defines aspirations as long-term goals, and in which students derive a milestone utility from achieving these goals. In particular we combine insights from the models presented in Dalton et al. (2016) and Genicot and Ray (2017). Our framework features a rational agent that endogenously chooses an effort-aspiration pair that is aligned with their individual abilities and assume that the agent formulates their *educational* aspirations to be consistent with their *occupational* aspirations. Aspirations (even though endogenously chosen) are, however, drawn from a distribution of outcomes: the aspirations window. This window may be too narrow if students lack knowledge about career possibilities or misperceive the level of education necessary for a certain occupation. A constrained aspirations window can then trigger students to define educational aspirations that are not sufficiently aligned with their innate ability. Our model can explain why an intervention that provides students with tools to re-assess their occupational aspirations and to re-define those aspirations in ways that better align with their preferences and abilities, can affect educational aspirations and investments in such heterogeneous ways.

Our study contributes to three strands of literature. *First*, we contribute to the literature on information interventions in educational contexts. Much of this literature focuses on high income countries and evaluates the effect of providing educational and career guidance to students from disadvantaged backgrounds (Bettinger et al., 2012; Hoest et al., 2013; Hoxby and Turner, 2015; Goux et al., 2017; Abbiati et al., 2018; Kerr et al., 2020; Carlana et al., 2022). Studies that focus on low- and middle-income countries have shown that providing information about the returns to education can increase school attendance, improve test scores, and change educational trajectories of students (Nguyen, 2008; Jensen, 2010; Avitabile and de Hoyos, 2018). With our study, we show that information on potential career paths and their educational requirements can similarly affect educational investments, indicating that the lack of information about career opportunities may lead students to sub-optimally invest in education. To the best of our knowledge, ours is the first study that evaluates a career-guidance intervention in a low-income context.

Second, we contribute to the literature that investigates the role of aspirations in inducing educational investments. Most of this literature is based on role-model interventions (Dinkelman and Martínez A., 2014; Bjorvatn et al., 2020; Bhan, 2020; Riley, 2022; Ahmed et al., 2022). By contrast, our intervention encourages students to explore their personal interests and provides them with personalized information about various possible career paths, thereby allowing them to develop more diversified occupational and educational aspirations. Importantly, our insights may rationalize why role-model interventions are not always successful (see e.g. Kipchumba et al., 2021; Leight et al., 2021): Without being presented with a variety of educational paths and career possibilities, students may not be able to formulate new aspirations that are within their reach and may fail to adjust educational investments.

Third, we contribute to the theoretical literature that seeks to understand the reasons for aspiration failures (Dalton et al., 2016; Genicot and Ray, 2017, 2020; La Ferrara, 2019).[3] Our conceptual framework combines insights from Dalton et al. (2016) with those from Genicot and Ray (2017), and features a rational agent that chooses optimal effort-aspiration pairs but whose set of possible choices may be constrained due to information frictions. This model serves to highlight a new type of aspiration failure: if students lack information about career paths, their perceived set of possible effort-aspiration pairs is overly constrained, which leads to a misalignment between educational aspirations and ability, and to the misallocation of educational investments.

The remainder of this paper proceeds as follows: In section 2 we describe the setting and design of the intervention; the implementation and collected data is described in section 3. Section 4 presents the empirical approach and results. Section 5 discusses the underlying mechanisms and presents a simple conceptual framework that helps rationalize the evidence, and section 6 concludes.

[3]The literature has so far identified two types of aspiration failures: *First*, Genicot and Ray (2017) consider a situation in which aspirations are exogenously drawn from a distribution of outcomes, the aspirations window. In such a setting, aspirations that are too low induce suboptimal effort, and aspirations that are too ambitious can lead to frustration because the goal becomes unachievable. *Second*, Dalton et al. (2016) consider a model in which aspirations are endogenously determined (and allowed to change over time), but individuals fail to internalize the feedback from effort to aspirations, and therefore choose suboptimally low aspirations.

2 Setting

2.1 Education in Cambodia

The educational sector in Cambodia was systematically destroyed by the regime of the Khmer Rouge in the 1970s during which the vast majority of teachers and academics fled the country or were killed (Chandler, 2007). The reconstruction of the educational sector did not start before the 1990s. The consequences are still visible today: most adults have not completed primary education. And while school completion rates have increased at primary and lower-secondary level, higher-secondary (high) school completion rates still lag behind (Huang et al., 2017).[4] Enrollment in lower-secondary schools is 56.5% and decreases to 28.1% in high schools. Furthermore, those students who manage to transition to high school are often not able to graduate with a diploma. During the school year of 2018-19, dropout rates in Grades 10, 11, and 12 were 14.1%, 7.2%, and 30.9%, respectively (Ministry of Education, Youth and Sport, 2019).

One of the reasons for these high dropout rates could be related to the fact that students often lack the necessary information and guidance from their parents and teachers to make informed educational decisions. Given their lack of education, parents can provide little support to their children in terms of homework or guidance in schooling decisions. Furthermore, parents often seem to underestimate the returns to education, most notably in low-income households (UNESCO, 2011). Overall, the involvement of parents or other family members in students' schooling is very rare (Benveniste et al., 2008), even though it has been shown that a healthy connection between the school and the family could prevent a substantial amount of dropouts (Edwards et al., 2014). Teachers, on the other side, are not sufficiently compensated for providing personalized support and lack adequate training.

These insights are corroborated by findings from a pilot study in 2019, for which we surveyed students about their educational aspirations and career goals, their knowledge on career paths, and their beliefs about the costs associated with higher education.[5] Our findings suggest that students have little knowledge of potential career paths, and that this lack in information may constrain them in their educational decision-making. Specifically, while all students were able to name a job they would like to do in the future, the range of different jobs mentioned is very limited. Over 85% of the students stated that they would like to become either a teacher, doctor, police officer or soldier.[6] At the same time, very few students demonstrated a clear understanding of how to reach their career goal, *i.e.*, what it requires in terms of schooling and where they would be able to pursue such studies. More than half of the students stated that they lack information about what they can do in the future. Talking to principals and teachers, it became apparent that future career options are *not* taught at school. Teachers admitted that they find it difficult to talk about career paths

[4] The education system in Cambodia consists of six years of primary, three years of lower-secondary, and three years of high school; the first nine years of schooling are compulsory.

[5] We conducted surveys with 200 students and focus group discussions with 32 students in Grade 8 and held interviews with teachers, parents, and education experts.

[6] The same is true in the group of students targeted by our intervention, see Table A.1. Figures A.1 to A.5 and Tables A.1 to A.20 are available in the online appendix A.

other than becoming a teacher as they have little knowledge about alternatives.

2.2 The Intervention

The intervention was designed as a half-day workshop tailored to Grade 9 students. The workshop consists of three main parts, the first of which is an interest exploration tool (IET), which allows students to reflect on their interests and preferences, and reveals the students' congruence with different personality types. The second part consists of a career exploration tool (CET), in which students are provided with detailed information about a number of different jobs that they might find interesting. The third part is an information session on high schools and vocational training. For the first two parts, students work individually on a tablet (with the support of a research assistant if needed); the third part is conducted in-person in small groups.

For the IET, students work through three personality tests that are programmed in an electronic application. These tests are based on the theory of vocational interest developed by Holland (1959, 1997), and more commonly know as RIASEC test.[7] The theory of vocational interest is well established in psychology and management science, and posits that individuals who display personality traits that fit with their occupation display higher job satisfaction.[8] In our intervention, the personality tests serve the purpose of encouraging students to engage with their preferences, while allowing us to personalize the display of career options in the CET. The tests have been adapted to the Cambodian context by the project team in collaboration with local experts. During the tests, students are presented with statements on activities they might like or interests they might have, and are asked to select the ones most applicable to them. Small pictures serve as further illustration. The three different tests differ in how statements are presented and how students can select them. After completion of all tests, the strongest personality types and a short description of what characterizes each type are revealed to each student based on their answers. It takes approximately 45 minutes to complete the tool. More details on the IET are presented in the online appendix B.1.

For the CET, students are shown a list of 18 occupations, 3 of which correspond to each personality type. For each personality type, the list contains one occupation that requires lower-secondary education plus some vocational training, one occupation that requires high school, and one occupation that requires a university degree. The list of jobs features occupations with which students are familiar, such as teacher, police officer, and doctor, as well as occupations that might not be known to the students but are relevant in the context, *i.e.*, agricultural technician, chef, or tour guide. The ordering of the occupations is personalized according to the students' strongest personality types. For each occupation, a detailed description (job content, societal value, and education requirements) is provided. Students can decide how much time they spend reading about each occupation.[9] For more details on the CET, see online appendix B.2.

[7] Holland 1997 proposes that there are six basic types of vocational interests, which he calls "RIASEC" (Realistic, Investigative, Artistic, Social, Enterprising, and Conventional) that describe how people interact with their work environments and shape their career choices.

[8] There is ample empirical literature that confirms this. For a review, see Nauta (2010).

[9] In total they have 17 minutes to read through the descriptions they are interested in, but can also log out earlier.

The last part, the information session, provides detailed information on high schools and vocational training centers in the area, about the requirements and costs related to attending either institution, as well as scholarship possibilities. The content of this session is adapted to the context of each lower-secondary school, and conducted in person and interactively. For more detail on the information session, see online appendix B.3. At the end of the intervention day, each student takes home with them a leaflet with all 18 occupations and their descriptions. Furthermore, teachers receive a poster on educational pathways that was discussed during the information session and are encouraged to place it on the wall of their class room.

3 Implementation and Data

3.1 Experimental Design and Timeline

For the implementation of our intervention, we collaborated with Child's Dream (CD), an international NGO that offers high school scholarships in Northwest Cambodia. Child's Dream partners with 51 lower secondary schools in 8 districts across 4 provinces (Battambang, Banteay Meanchey, Oddar Meanchey, and Siem Reap). For our study, we sampled all 39 schools that had a partnership with Child's Dream and a class size in Grade 9 above 30 students.[10] We expanded the sample by including 21 additional schools from the same provinces that are similar in characteristics to the Child's Dream partner schools. From these 60 schools, 30 were randomly assigned to receive the treatment (the half-day workshop); the remaining 30 served as control. For those schools that had more than one class in Grade 9, we randomized the class that would receive the treatment in case of treatment schools (or serve as control in case of control schools). The randomization was stratified by district. Figure 1a depicts the location of the initial sample of treatment and control schools.[11]

The implementation of the intervention started in mid February 2020. By the beginning of April, the intervention was supposed to have been implemented in all 30 treatment schools, allowing some time between the intervention and application deadlines for high school scholarships (including those awarded by Child's Dream). However, on March 16, 2020, as a measure to prevent the spread of COVID-19, the Cambodian government announced that all schools would be closed effective immediately. By that time, we had conducted the intervention in 18 schools across 8 districts. Our analysis therefore focuses on the 18 treatment schools, where the intervention had been implemented, and the 19 control schools that are located in the same districts (see Figure 1b for the geographical location of these schools). In the 18 treatment schools, 783 students out of the 862 invited students took part in the intervention.[12]

[10] In very few cases, the class size was below 30 but there was more than one class in Grade 9. In these cases, we combined two classes.

[11] As we were interested in whether information take-up and processing differs when it is made self-relevant, we randomly allocate students into one of three treatment arms in treatment schools: main treatment arm (A1), placebo arm (A2), and information-only (A3). However, randomization within the treatment schools was not successful. We therefore refrain from any analyses comparing the different treatment arms, and instead investigate the impact of the intervention overall. More details are available in online appendix B.4.

[12] Students were informed about the workshop several days in advance, and they were told that they were free

Figure 2 depicts the timeline of the data collection and the sample composition. Administrative data collection began in November 2019. At that time, there were in total 862 students in the treatment and 853 students in the control schools in the selected Grade 9 classes. We collected administrative data for all students, in particular gender, age, village of residence, as well as grades and absences for the months before the intervention was conducted. Furthermore, we collected teacher and school characteristics. On the day of the intervention, we also collected some baseline characteristics from treated students.

In July to August 2020, we conducted a follow-up survey by phone. We reached 77% of the students (n= 1,327). At that time, schools were still closed in Cambodia due to COVID-19.[13] In the phone survey, we asked students about their daily activities, their expectations and aspirations in terms of education and future occupation. Participation in the phone survey was not random; in particular, female students and students who performed better in school at baseline are more likely to have participated in the phone survey. However, we find no evidence that attrition in the phone-survey is related to treatment status (see online appendix C for more details).

Schools reopened in September 2020 for ninth-graders, so that students could prepare for their final exam, which tool place at the end of November 2020. The final exam grade determines whether students are allowed to enroll in high school. In December, after the final exams were graded, the government unexpectedly announced that all students who had registered for the final exam would obtain their Grade 9 diploma and would be allowed to transition to high school, irrespective of their performance in the final exam. For that time period, we collected administrative information on participation in the final exam, which is our marker for whether a students dropped out during Grade 9, and final exam performance (actual grades given by the teacher before the government announcement). We also asked lower-secondary teachers if students requested the official transcripts necessary to enroll in high school. This gives us an important insight into who was intending to transition to high school (or to any other formal education). Furthermore, we collected data on scholarship applications from Child's Dream. This information can be analyzed for those schools that partner with the NGO (28 out of 37 schools).

The new school year started in January 2021, but all schools (including high schools) were closed again for a second lockdown at the end of February 2021 until the end of the school year in November 2021.[14] From the high schools, we collected data on high school transition (whether students started Grade 10), as well as students' performance when schools reopened after the second lockdown, *i.e.*, whether they started Grade 11, attended the midterm exam of Grade 11 and whether they passed that midterm exam. Passing the exam is our marker for dropout during Grade 11, as only students

to participate or not. Students that did not show up on the day of the workshop display overall lower academic performance and more days of absence in the months before the intervention. During the workshop, a total of five students left before the end. In the following analyses, we keep these five students as 'treated'; results do not change if these students are excluded.

[13]Throughout the school closure, students were encouraged to study on their own with grade-specific TV programs. Furthermore, teachers were responsible for providing students with additional content and assignments. In a separate study, we show that learning activities varied greatly across students (Gehrke et al., 2021).

[14]During this second lockdown, classes were held online; however, attendance was not tracked.

who pass the midterm exam are allowed to participate in the final exam and progress into Grade 12, the final year of high school. We were able to obtain high school information for 1,697 students (99% of the original sample). All in all, the data covers students' educational decisions two years into the intervention.

3.2 Sample Characteristics

The intervention targeted low-income students from rural areas. Table 1 presents student, school, and parental characteristics, as well as the balance between treatment and control groups in terms of these variables. This information is based on our two data sources: administrative data collected for all students in our sample before and after the intervention (n=1,715), and information from the phone survey that was conducted with students in treatment and control schools in summer 2020 (n=1,327).

A little over half of the students in our sample are female, and they were on average 15 years old at the time of the intervention. Students needed to travel on average 3.6 kilometers to their school, and lived approximately 11 kilometers from the district town. The high school that they would attend is almost as far away: about ten kilometers on average. In the phone survey, we asked students explicitly about their parents' education and occupation before the COVID-19 pandemic. From those students who knew their parents' education level, 81% (92%) reported that their father (mother) had completed primary education or less. At least one parent is a farmer for 69% of the students.

Baseline characteristics are overall well balanced between treatment and control schools: out of 19 variables for which we have baseline values, 4 display differences in means that are statistically significant at the 10% level. These variables are students' distance to school, students' grades (the sum of Math, English, and Khmer (the official language of Cambodia), averaged over the months December and January, and standardized across schools) and the teacher's age and number of years of work experience (which are strongly correlated because teachers rarely switch schools). The imbalance in the grade is potentially worrisome as past grades are a strong predictor of future educational outcomes. We show in Figure A.1a, that there is sufficient support between both groups over the entire grade distribution, and account for these imbalances in the empirical analysis.

We track student outcomes after the intervention through phone-survey answers, as well as administrative data obtained from lower-secondary and high schools. Summary statistics are presented in Table A.2. In terms of students' activities during school closure, 43% of the students strongly agreed with the statement "I kept studying during school closure", while only 25% reported that their main activity in the last 7 days was studying. Students' aspirations are quite high: 13.5 years of schooling on average. The vast majority (96%) reported to aspire to complete at least higher-secondary education and 44% aspired to a university degree. Similarly, a very large proportion aspired a career that requires higher secondary education (91%) and about one out of three students aspired a career that requires a university degree.[15]

[15]Interestingly, students aspirations (but not expectations) with regards to education and occupation remained

Of the surveyed students, 24% reported to have applied for some kind of high school related scholarship. This could be either the scheme operated by Child's Dream, which was available at 76% of the schools in our sample, a scholarship provided by the Cambodian government, or other scholarships from NGOs operating locally. From the administrative records of Child's Dream, we can infer that of all the students who had access to a CD scholarship (n=1,317), 17% applied for it, and 4% received it.

We use students' participation in the final exam as the main indicator for completion of the academic year. Of our targeted students, 13% did not attend the exam, and are therefore considered dropouts. Among those who participated in the exam, most students did surprisingly well. Students usually need 260 points to pass the exam and to be allowed to enroll in high school, and the vast majority falls above this threshold. However, when considering the distribution of the final exam grades (see Figure A.1b), it becomes apparent that there was likely considerable manipulation by the teachers, as a large proportion of students received just above 260 points. Note that this manipulation seems independent of treatment status.

Of all students, 81% requested their official transcripts for Grade 9, which are necessary in order to be able to enroll in high school.[16] A smaller share of students (75%) actually started high school as confirmed by high school teachers.[17] An even smaller share of students (64%) started Grade 11 a year later, and only 49% of all students that we were able to track passed the midterm exam of Grade 11.

4 Empirical Analysis

4.1 Empirical approach

In order to analyze whether our intervention affected schooling decisions, we analyze, *first*, whether students studied during the first school closure, and the students' aspirations during school closure in terms of education and career goals; in particular, the years of schooling students aspire to achieve, and whether the job they would like to do when they are 25 is outside the typical reference window (it is not teacher, doctor, police officer, or soldier). These outcomes are based on the students' answers in the phone survey. *Second*, using administrative data, we study whether students completed Grade 9, *i.e.*, they participated in the final exam, their performance in the final exam (standardized across schools), as well as whether they enroll in high school. The latter outcome is based on two pieces of information: the reports from lower secondary teachers who need to provide transcripts for the students who wish to enroll in high school, and high schools' confirmation that the student actually enrolled. *Third*, again using data from high schools, we investigate whether students started Grade 11 and whether they participated in, and passed the Grade 11 midterm exam. Except the

very stable throughout the first COVID-19 lockdown (see Figure A.2).

[16] This information is missing for four students, but it seems reasonable to assume that these students did not request their transcripts, as the teachers did not know their whereabouts.

[17] Note that this information is missing for 14 students, who had requested their transcripts but who we were unable to track.

information from Grade 11, all of these outcomes were pre-specified in the pre-analysis plan (Gehrke et al., 2020).

In order to identify the effect of the intervention, we estimate both intention to treat effects (ITT) as well as treatment on the treated effects (TOT). ITT estimates rely on the original treatment assignment, *i.e.*, whether a student is enrolled in a school and class in which the workshop was conducted. We estimate the following specification:

$$Y_{ijd} = \alpha + \beta T_j + \gamma' X_{ijd} + \xi_d + \epsilon_{ijd} \tag{1}$$

where Y_{ijd} is each of the outcomes of interest for student i in school j and district d. T_j is a dummy equal to one if the intervention was implemented in school j and zero otherwise. X_{ijd} is a vector of pre-specified student and school characteristics (student age, gender, pre-intervention grades and absence, class size, and Child's Dream partnership), and ξ_d are district fixed effects. ϵ_{ijd} is the idiosyncratic error term. Standard errors are corrected for clustering within schools.

The TOT estimates take into account that not all targeted students actually attended the workshop (in total 79 of the 862 students that were targeted did not show up on the day of the workshop). To address this, we estimate equations 2 in 2SLS, instrumenting $Treated_i$ with the original treatment assignment (T_j).

$$Treated_i = \eta + \theta T_j + \kappa' X_{ijd} + \phi_d + \nu_{ijd} \tag{2a}$$

$$Y_{ijd} = \alpha + \beta \widehat{Treated}_i + \gamma' X_{ijd} + \xi_d + \epsilon_{ijd}. \tag{2b}$$

Because a number of variables, including some that were not pre-specified as controls (such as teacher characteristics), are not well balanced between treatment and control, we also use the cross-fit partialing out lasso (Chernozhukov et al., 2018) to select the relevant covariates and estimate the coefficients of interest.[18] The cross-fit partialing out lasso (or double machine learning) method follows the partialing-out lasso, yet tunes the parameters of the lasso via cross-validation. It is generally considered the most suitable solution for lasso-based inference (Baiardi and Naghi, 2022; Cameron and Trivedi, 2022).[19]

4.2 Main Results

Results are presented in Tables 2, 3 and 4. We report intention to treat effects (Panel A) and treatment on the treated effects (Panel B). For each outcome, we first present the estimates that control for student and school characteristics as well as district fixed effects as pre-specified. We then present estimates derived using double machine learning (cross-fit partialing out lasso). We depict standard errors clustered at the school level in parentheses, and Anderson's (2008) sharpened q-values to account for the False Discovery Rate (FDR) in brackets.

[18] We implement the procedure in Stata using `xporegress` and `xpoivregress`.

[19] For the cross-fitting, we report results averaged over ten sample splits, for each using ten folds.

We find that the intervention had no effect on whether students self-report to have been studying during the lockdown period. We do, however, find a weakly positive effect on students' aspirations in terms of years of schooling, as well as a weakly negative effect on the diversification in students' occupational aspirations, which implies that treated students were less likely to name an occupation other than the main four as their stated career goal (Table 2). This result is strongly driven by the higher likelihood that treated students mentioned 'teacher' — featured as part of the career exploration tool — as their stated occupational aspiration in the phone survey. None of these effects are statistically significant after correcting for the FDR.

In terms of Grade 9 outcomes, we find that treated students were 2.5 percentage points more likely to return to school after the COVID-19 lockdown and attend the final exam (2.9% increase over the control group mean). Conditional on attending, treated students performed weakly better than control students in the final exam (0.21SD). These two effects are statistically significant after correcting for the FDR only in the lasso regression. In addition, we find a positive effect of 5.9pp (7.9%) on high school enrollment (Table 3). This effect is statistically significant at the 5% level after correcting for the FDR in both specifications, and slightly larger (9.6pp) when using OLS with pre-specified controls for estimation.

This positive effect on enrollment seems to be sustained throughout the first 1.5 years of high school (Table 4). Treated students were 6.0pp (9.4%) more likely to start Grade 11 and 5.2pp (8.5%) more likely to attend the midterm exam of Grade 11. The coefficient on passing the midterms exam suggests an increase in the likelihood of passing the midterm exam by 8.6pp (17.6%), and is statistically significant at the 1% level. Again the OLS estimates are slightly larger than the DML estimates, but less precisely estimated. All coefficients remain statistically significant after correcting for the FDR.

Taken together, these results suggest that participation in the workshop encouraged students to increase their educational investments. Given that the workshop was designed to help students formulate aspirations that are more aligned with their abilities and interests, the intervention's effect on aspirations and educational investment might differ by students' ability. We investigate this idea further by analyzing treatment effect heterogeneities along students' baseline academic performance.

4.3 Heterogeneities by Academic Performance

Figure 3 and 4 report semi-parametric estimates (local mean smoothing) of each outcome variable at all levels of students' performance prior to the intervention (standardized sum of grades in three main subjects), separately for students in treatment and control schools. Each of these estimates control linearly for student and school characteristics as well as district fixed effects. Parametric estimates that interact the treatment indicator with baseline students performance are reported in Tables A.3 to A.5 and reveal the same pattern.

We find strong evidence for heterogeneous treatment effects. Low-performing treated students were less likely to study during school closure than similarly performing students from control schools. By contrast, students that had been performing better than the median student before the intervention seem to have benefited from it; these students were significantly more likely to be studying at the time of the phone survey. In terms of aspirations, we find some evidence that low-performing students downward adjusted their educational aspirations, while high-performing students adjusted them upwards. By contrast, we find no effect on the diversification in career goals for low-performing students, yet a negative effect for high-performers. This effect is driven by high-performing students being more likely to state 'teacher' as their career goal. Treatment effects are also heterogeneous with respect to participation in the final exam, and more strongly so for students' performance in that exam; low-performing treated students were not less likely to participate in the final exam, but did perform worse than their control-school counterparts. High-performing treated students, in turn, were more likely to participate in the final exam and performed better than their counterparts from control schools.

The positive average effect on high school enrollment seems to be entirely driven by better-performing students, who have substantially higher high-school enrollment rates than similarly high-performing students from control schools. Among low-performing students, by contrast, high-school enrollment is not differentially affected. This can be explained by the fact that high-school enrollment rates are quite low among this group: while about 75% of the low-performing students participated in the final exam, only slightly more than 50% enrolled in high school. In other words, any negative effect on low-performing students disappears about nine months after the intervention, as most students in that part of the grade distribution would have dropped out by that time anyway. The positive average effect throughout Grade 11 continues to be driven by high-performing students, who were significantly more likely to start Grade 11, as well as to attend, and to pass the midterm exam of Grade 11 than high-performing students from control schools.

In summary, we find considerable heterogeneities in treatment effects by pre-intervention academic performance. Interestingly, the negative effect on low-performing students seems to vanish at the end of lower-secondary school, as low-performing students were not less likely to enroll in high school. This suggests that the intervention led low-performing students to decide against attending high school earlier, and that low-performing students adjusted their effort accordingly. Consistent with that interpretation, we find that low-performing treated students were less likely to report in the phone survey that their main activity in the last 7 days was studying, and to select educational mentoring (rather than phone credit) as a prize for participating in the phone-survey. Low-performing students were also less likely to apply for a scholarship as self-reported in phone survey, and as confirmed by administrative data from Child's Dream. However, they were *not* less likely to pass the final exam nor to obtain a Child's Dream scholarship (see Figure A.3 and Table A.6), suggesting that low-performing treated students invested just enough effort in education to be able to graduate lower-secondary school, while their control school counterparts studied more during Grade 9, but still did not start high school.

4.4 Robustness checks

We perform a number of robustness checks to corroborate the main findings, as well as the heterogeneities by student performance. *First*, we investigate if inference is sensitive to the small number of clusters in our sample or to how we correct for multiple hypothesis testing. The main estimates as well as the treatment effect heterogeneities remain statistically significant irrespective of whether we use randomization inference or control for the family-wise error rates using the Romano-Wolf stepdown procedure (see Table A.7). *Second*, we assess whether our estimates are affected by differential attrition. Because there is substantial non-response in the phone-survey, we test if our phone-survey results are sensitive to re-weighting the sample with the inverse of the probability of participating in the phone-survey. We predict phone-survey participation with student, teacher, and school characteristics obtained from administrative data as described in Appendix C. Our main findings, as well as the treatment effect heterogeneities by pre-intervention student performance, are unchanged (see Table A.8). Following the strategy outlined in Kling et al. (2007), we also calculate sensitivity bounds for our estimates by varying the assumptions about outcomes for those students that did not answer to individual survey questions in the phone-survey or that could not be tracked into high school. We start with very extreme assumptions, *i.e.*, setting the outcome values of attritors to the minimum value for students in the treatment arm and to the maximum value for students in the control arm, and then relax these assumptions step-by-step. For phone-survey outcomes, the interaction effects on studying during the lockdown and educational aspirations are significant throughout, but the negative average treatment effect on occupational aspirations declines somewhat. For high school related outcomes (enrollment in high school and in Grade 11, attendance and passing of midterm exam), the average treatment effects, as well as the treatment effect heterogeneities, are robust (see Tables A.9 and A.10).

Third, we study whether the observed treatment effect heterogeneities by performance are mere spurious correlations driven by confounding variables that both affect pre-intervention performance and educational investments. To this end, we analyze potential correlates of academic performance by regressing a number of relevant variables on academic performance while controlling for student's age, gender, and district fixed effects. For this we use administrative data and information from the phone survey. Results are shown in Table A.11. Neither remoteness of the student's village (in terms of distance to the school or next district town) nor occupation of the parents, smartphone availability in the household, or the extent to which parents were affected by COVID-19 are significantly correlated with academic performance. However, we find that students' pre-intervention grades are correlated with parental education, and with the probability that one of the parents lost their job during the COVID-19 crisis. We therefore add the interaction between parental education and treatment or parental job loss and treatment in our regressions. Note that we can only carry out this exercise for the students who participated in the phone survey and reported information on their parents' education or job loss; we consider these a selected sample. Results are reported in Tables A.12 to A.17. In the even columns, we report our main OLS specification with interaction effects for the selected sample, in the odd columns we include parental education or parental job loss interacted with treatment status. Overall, our results are unchanged, suggesting that it is indeed student academic performance that is driving the heterogeneities in treatment effects.

Fourth, we investigate if we find similar evidence when ranking students within their class rather than within the entire sample to address concerns that the effect heterogeneities we find are driven by underlying school rather than student characteristics. Results are reported in Figures A.4 to A.5, and again show the same pattern.

5 Underlying Mechanisms

5.1 A model of occupational aspirations as long-term goals

The evidence presented so far suggests that a half-day workshop designed to help students develop occupational aspirations increases educational aspirations and investments for high-performing students, while it decreases educational aspirations and investments (at least in the short-run) for low-performing students. To understand the mechanism underlying these empirical findings, consider a model of aspirations as long-term goals.

Similarly to Dalton et al. (2016), we focus on endogenously determined aspirations. Assuming that students set educational aspirations to match with their occupational aspirations, and setting educational attainment $e \in [0, 1]$ equal to effort, allows us to write the student's utility function as:

$$u = w_0(e) + \tau a w_1(max\{e - a, 0\}) - c(e, \mu), \tag{3}$$

with w_0 being direct utility, w_1 milestone utility, a the aspiration, and $c(e, \mu)$ the cost of effort, which is increasing in e and decreasing in innate ability μ. w_0 and w_1 are smooth, strictly increasing and concave in e. Two key assumptions are worth highlighting. *First*, we assume that milestone utility is multiplicative in a, *i.e.*, the utility reward from satisfying any aspiration is increasing in how ambitious that aspiration is. This assumption generates the necessary condition for individuals to define aspirations at the right level; not too low, as the utility reward would otherwise be low, but also not too high, as aspirations are otherwise unattainable. τ is a constant that determines how quickly milestone utility increases in a.[20] *Second*, the cost of effort c is strictly and convexly increasing in e, with $c(0, \mu) = 0$ and $\partial^2 c/\partial e \partial \mu \leq 0$. The negative cross-derivative implies that students with lower innate ability have a steeper cost of effort curve.

In this model, a rational student chooses the optimal effort-aspiration pair (e^*, a^*) that maximizes utility. An effort-aspiration pair is assumed to be consistent only if it is satisfied, that is if $e \geq a$. For any effort level $e > 0$, an individual will always choose a consistent effort-aspiration pair in optimum as long as $\tau > 0$ and $w_1(0) \geq 0$, that is, there is no utility discount of just satisfying

[20] Note, that this assumption deviates from the model in Dalton et al. (2016), who constrain feasible outcomes to effort-aspiration pairs in which the aspiration equals the final outcome, but essentially generates the same prediction: *ceteris paribus*, the utility reward from achieving any aspiration is increasing in the level of the aspiration.

the aspiration.[21]

Given the concavity in w_0 and w_1 and the convexity in c, the consistent solution is also a unique solution that satisfies: $w_0'(e) + \tau a w_1'(e-a) = c'(e,\mu)$. This solution is interior as long as $w_0'(1) + \tau a w_1'(1-a) \leq c'(1,\mu)$. Importantly, the interior solution is an increasing function of innate ability (due to the negative cross-derivative of cost of effort), with higher ability students choosing higher effort-aspiration pairs than low-ability students. Intuitively, each student sets their aspirations to be high enough to incentivize effort, but not too high (as they otherwise become unattainable and lead to frustration). A graphical representation of this optimization problem is depicted in Figure 5.

However, empirically, we find that educational aspirations are only modestly correlated with baseline academic performance in the control group. The model outlined above can also not explain why an intervention that supports students in developing occupational aspirations, while only providing relatively generic information about education, increases the correlation between educational aspirations and initial academic performance. To rationalize these findings, we have to accommodate the possibility that information frictions constrain students in choosing the right level of aspiration. In line with Genicot and Ray (2017), we therefore assume that aspirations, although endogenously defined, are drawn from a distribution of known outcomes, the aspirations window. This implies that

$$a^* = \Psi(\mu, e^*, F), \tag{4}$$

where F is the population-distribution of education-occupation combinations. An attenuation in the relationship between innate abilities and educational aspirations can then arise for two reasons. First, students lack information about career possibilities and are constrained by the overly narrow set of occupations they know. Second, students misperceive the educational requirements associated with their occupational aspiration and mistakenly set their educational aspirations to the wrong level. Both possibilities can lead to a constrained or biased aspirations window, and students define educational aspirations that do not match their abilities — too high (for low-performers) or too low (for high performers) — resulting in a relatively flat relationship between baseline academic performance and educational aspirations.

Importantly, if aspirations are insufficiently responsive to innate ability, students may choose levels of investment that are individually sub-optimal. Some students may keep investing in education, even though their abilities do not match with higher education and they could develop occupational aspirations that do not require higher education, while others underinvest in education not knowing the true educational requirements for their occupational aspiration or not being aware of sufficiently ambitious aspirations that would match their ability. Such a framework explains why providing students with the necessary tools to re-assess their career goals, while delivering

[21] To see this, consider an individual that derives no milestone utility. This individual will choose effort $\bar{e}$ such as to maximize $u = w_0(e) - c(e)$. Can the individual do better by endogenously setting aspirations? They can if and only if $\tau a w_1(\bar{e} - a) > 0$.

information about the educational requirements necessary to carry out these jobs, helps students re-calibrate their educational aspirations, and adjust educational investments accordingly.

In line with the predictions of our model, we observe a strong correlation between academic performance and types of occupations that students read about in the CET (see Table A.18). In particular, low-performing students spent more time reading about occupations that only require lower-secondary education. This seems to translate into a better match between occupational and educational aspirations: Low-performing treated students were less likely to aspire to a career that requires a university degree, and were also less likely to aspire to obtain a university degree. Concordantly, they were more confident in being able to reach their career goal. High-performing treated students, in contrast, were more likely to aspire to a career that requires a university degree and also to aspire to a university degree in terms of educational aspirations, while being similarly more confident than their control school counterparts that they would be able to achieve their occupational aspirations (Figure 6).

5.2 Alternative Explanations

There are at least two alternative explanations for the observed effects. *First*, the information on costs associated with high school attendance might have discouraged low-performing students. This effect could be particularly pronounced if low-performing students systematically underestimate the costs of higher education at baseline. Learning about the true costs of attending high school, low-performing students would then have needed to adjust their beliefs upward. As a consequence, attending high school would become less attractive to these students, in particular given the substantial amount of effort they would need to put into schooling. Indeed, we find that low-performing students were more likely to estimate lower costs of schooling at baseline (see Table A.19). However, there seems to have been no differential cost updating. Cost estimates in the follow-up do not differ by treatment status nor by academic performance, neither for the total cost nor for the cost of extra classes, which most students mentioned as most expensive part of attending high school (see Table A.20).

Second, the information on minimum requirements with respect to the grades students need to obtain in order to be able to enroll in high school, and the high standards applied to receive a scholarship, might have led low-performing students to realize that their academic performance does not match with a path of higher education. However, we find little evidence that low-performing treated students were less confident in reaching their educational or career aspirations (Figure 6), suggesting that this explanation is unlikely to be the main driver underlying our results.

6 Conclusions

This study provides experimental evidence that a half-day workshop designed to expand students' aspiration window — in terms of careers they can pursue — can improve students' educational outcomes. We also show that the average positive effects mask substantial heterogeneity by students' pre-intervention school performance. Treated low-performing students were less likely to study during school closure compared to low-performing students in control schools, had somewhat lower educational aspirations, and performed worse in the final exam of Grade 9, yet they were not less likely to transition to high school. By contrast, treated high-performing students were more likely to study during school closure than their control group peers, had somewhat higher educational aspirations, performed better in the final exam, and were more likely to transition to and progress in high school. It seems that our intervention made low-performing students aware of alternative career paths and led them to adjust their aspirations to more achievable levels, while it raised the aspirations and thereby the schooling effort of high-performing students.

Our findings suggest that the workshop, which was low-cost and is easily scalable, improved the quality of educational decision-making by helping students more closely align their aspirations to their potential. Given the high dropout rates observed during high school in our sample, and in the country more generally, this intervention has potentially caused substantial efficiency gains. This effect is akin to the productivity gains associated with improving workers' sorting along their comparative advantage as documented in previous work (Papageorgiou, 2014). While we cannot rule out that the intervention decreased human capital among some low-performing students as they oriented away from pursuing higher education, our findings suggest that they were *not* less likely to graduate lower-secondary school than their control school peers, and by adjusting their educational aspirations, these students were potentially less frustrated throughout Grade 9. For high-performing students, on the other hand, our intervention provided the incentives and information necessary to exert more effort in lower-secondary school, allowing them to start high school at higher rates, and apparently better prepared. The fact that the positive effect of the intervention persisted two years after the workshop had taken place suggests that this could indeed be a useful intervention to be implemented at scale.

It is important to emphasize that the average treatment effect of an intervention of this type will essentially depend on the underlying distribution of baseline academic performance in the student population. In settings in which many students are well prepared for higher education but still opt out, the average treatment effects would potentially be larger. On the other hand, in regions, in which few students are equipped with the skills that are necessary to attend high school, a similar intervention may actually have no or even a negative effect on schooling outcomes.

Our study comes with a number of caveats. *First*, the study was (unintentionally) conducted during a very specific time period. About midway into our intervention, schools were closed for half a year due to the COVID-19 pandemic and students had to study on their own with little external support. Although there were very few reported cases of COVID-19 infections in Cambodia in 2020, the global economic recession and travel disruptions had severe repercussions on the households. Many students reported that their parents lost income or even their job due to the crisis (Gehrke et al., 2021). Students were thus facing severe financial constraints. This might have likely undermined the positive effect of the intervention. *Second*, we were not able to track students once they left school. We therefore do not observe what students were doing after they dropped out, whether they started to work, what type of jobs they pursued, and whether they enrolled in vocational training. This is an avenue for future research.

Bibliography

Abbiati, G., G. Argentin, C. Barone, and A. Schizzerotto (2018). Information barriers and social stratification in higher education: evidence from a field experiment. *The British Journal of Sociology 69*(4), 1248–1270.

Ahmed, H., M. Mahmud, F. Said, and Z. S. Tirmazee (2022). Encouraging female graduates to enter the labor force: Evidence from a role model intervention in pakistan.

Aljojo, N. and H. Saifuddin (2017). A study of the reliability and validity of holland's riasec of vocational personalities in arabic. *American Journal of Information Systems 5*(1), 33–37.

Anderson, M. L. (2008). Multiple inference and gender differences in the effects of early intervention: A reevaluation of the abecedarian, perry preschool, and early training projects. *Journal of the American statistical Association 103*(484), 1481–1495.

Athanasou, J. A. (2000). *A brief, free and standardized assessment of interests for use in educational and vocational guidance: Version 3.1*. Sydney, Australia.

Athanasou, J. A. (2007). *Manual for the Career Interest Test (version 4.1)*. Sydney, Australia.

Avitabile, C. and R. de Hoyos (2018). The heterogeneous effect of information on student performance: Evidence from a randomized control trial in mexico. *Journal of Development Economics 135*, 318–348.

Baiardi, A. and A. A. Naghi (2022). The value added of machine learning to causal inference: Evidence from revisited studies. *The Econometrics Journal*.

Beaman, L., E. Duflo, R. Pande, and P. Topalova (2012). Female leadership raises aspirations and educational attainment for girls: a policy experiment in india. *Science (New York, N.Y.) 335*(6068), 582–586.

Benveniste, L., J. Marshall, and C. M. Araujo (2008). *Teaching in Cambodia*. The World Bank and Ministry of Education, Youth and Sport.

Berger, J. and D. Pope (2011). Can losing lead to winning? *Management Science 57*(5), 817–827.

Bettinger, E. P., B. T. Long, P. Oreopoulos, and L. Sanbonmatsu (2012). The role of application assistance and information in college decisions: Results from the h&r block fafsa experiment. *Journal of Economic Perspectives 127*(3), 1205–1242.

Bhan, P. C. (2020). Do role models increase student hope and effort? evidence from india. *University of Glasgow Working Paper*.

Bjorvatn, K., A. W. Cappelen, L. H. Sekei, E. Ø. Sørensen, and B. Tungodden (2020). Teaching through television: Experimental evidence on entrepreneurship education in tanzania. *Management Science 66*(6), 2308–2325.

Cameron, A. C. and P. K. Trivedi (2022). *Microeconometrics Using Stata, Volume 2*. Stata Press.

Carlana, M., E. La Ferrara, and P. Pinotti (2022). Goals and gaps: Educational careers of immigrant children. *Econometrica 90*(1), 1–29.

Chandler, D. (2007). *A History of Cambodia* (4 ed.). Boulder, Colorado: Westview Press.

Chernozhukov, V., D. Chetverikov, M. Demirer, E. Duflo, C. Hansen, W. Newey, and J. Robins (2018, 01). Double/debiased machine learning for treatment and structural parameters. *The Econometrics Journal 21*(1), C1–C68.

Clarke, D. (2021, July). RWOLF2: Stata module to calculate Romano-Wolf stepdown p-values for multiple hypothesis testing. Statistical Software Components, Boston College Department of Economics.

Dalton, P. S., S. Ghosal, and A. Mani (2016). Poverty and aspirations failure. *The Economic Journal* (126), 165–188.

Damgaard, M. T. and H. S. Nielsen (2018). Nudging in education. *Economics of Education Review 64*, 313–342.

Damgaard, M. T. and H. S. Nielsen (2020). Chapter 2 - behavioral economics and nudging in education: evidence from the field. In S. Bradley and C. Green (Eds.), *The Economics of Education (Second Edition)* (Second Edition ed.)., pp. 21–35. Academic Press.

Dinkelman, T. and C. Martínez A. (2014). Investing in schooling in chile: The role of information about financial aid for higher education. *Review of Economics and Statistics 96*(2), 244–257.

Edwards, D. B., T. Zimmermann, C. Sitha, J. H. Williams, and Y. Kitamura (2014). Student transition from primary to lower secondary school in cambodia: Narrative insights into complex systems. *Prospects 44*(3), 367–380.

Eng, S., W. Szmodis, and M. Mulsow (2014). Cambodian parental involvement. *The Elementary School Journal 114*(4), 573–594.

Gehrke, E., F. Lenel, and C. Schupp (2020). Trial registry for "Career goals and investments in education: Experimental evidence from Cambodia". AEARCTR-0005460.

Gehrke, E., F. Lenel, and C. Schupp (2021). Covid-19 crisis, economic hardships and schooling outcomes.

Genicot, G. and D. Ray (2017). Aspirations and inequality. *Econometrica 85*(2), 489–519.

Genicot, G. and D. Ray (2020). Aspirations and economic behavior. *Annual Review of Economics 12*(1), 715–746.

Goux, D., M. Gurgand, and E. Maurin (2017). Adjusting your dreams? high school plans and dropout behaviour. *The Economic Journal 127*(602), 1025–1046.

Guyon, N. and E. Huillery (2020, 06). Biased Aspirations and Social Inequality at School: Evidence from French Teenagers. *The Economic Journal 131*(634), 745–796.

Heckman, J. J. and S. Mosso (2014). The economics of human development and social mobility. *Annual Review of Economics 6*(1), 689–733.

Hess, S. (2017). Randomization inference with stata: A guide and software. *The Stata Journal 17*(3), 630–651.

Hoest, A., V. M. Jensen, and L. P. Nielsen (2013). Increasing the admission rate to upper secondary school: the case of lower secondary school student career guidance. *Education Economics 21*(3), 213–229.

Holland, J. L. (1959). A theory of vocational choice. *Journal of Counseling Psychology 6*(1), 35–45.

Holland, J. L. (1997). *Making vocational choices: A theory of vocational personalities and work environments / John L. Holland* (3rd ed. ed.). Odessa, Fla.: Psychological Assessment Resources.

Hoxby, C. M. and S. Turner (2015, May). What high-achieving low-income students know about college. *American Economic Review 105*(5), 514–17.

Huang, H., D. Filmer, and T. Fukao (2017). *Trends and Linkages in Schooling and Work among Cambodian Youth*. World Bank.

Janzen, S. A., N. Magnan, S. Sharma, and W. M. Thompson (2017). Aspirations failure and formation in rural nepal. *Journal of Economic Behavior and Organization 139*, 1–25.

Jensen, R. (2010). The (perceived) returns to education and the demand for schooling *. *Quarterly Journal of Economics 125*(2), 515–548.

Kerr, S. P., T. Pekkarinen, M. Sarvimäki, and R. Uusitalo (2020). Post-secondary education and information on labor market prospects: A randomized field experiment. *Labour Economics 66*, 101888.

Kipchumba, E. K., C. Porter, D. Serra, M. Sulaiman, et al. (2021). *Infuencing youths' aspirations and gender attitudes through role models: Evidence from Somali schools.*

Kling, J. R., J. B. Liebman, and L. F. Katz (2007). Experimental analysis of neighborhood effects. *Econometrica 75*(1), 83–119.

La Ferrara, E. (2019). Presidential address: Aspirations, social norms, and development. *Journal of the European Economic Association 17*(6), 1687–1722.

Leight, J., D. O. Gilligan, M. Mulford, A. S. Taffesse, and H. Tambet (2021). *Aspiring to more? New evidence on the effect of a light-touch aspirations intervention in rural Ethiopia*, Volume 2070. Intl Food Policy Res Inst.

Lockwood, P. and Z. Kunda (1997). Superstars and me: Predicting the impact of role models on the self. *Journal of Personality and Social Psychology 73*(1), 91–103.

Meireles, E. and R. Primi (2015). Validity and reliability evidence for assessing holland's career types. *Paidéia (Ribeirão Preto) 25*(62), 307–316.

Ministry of Education, Youth and Sport (2017). *Public Education Statistics & Indicators: 2016-2017.* Phnom Penh, Cambodia: Ministry of Education, Youth and Sport. Department of Education Management Information System.

Ministry of Education, Youth and Sport (2019). *Public Education Statistics & Indicators: 2018-2019.* Phnom Penh, Cambodia: Ministry of Education, Youth and Sport. Department of Education Management Information System.

Morgan, B. and G. P. de Bruin (2018). Structural validity of holland's circumplex model of vocational personality types in africa. *Journal of Career Assessment 26*(2), 275–290.

Nauta, M. M. (2010). The development, evolution, and status of holland's theory of vocational personalities: Reflections and future directions for counseling psychology. *Journal of counseling psychology 57*(1), 11.

Nguyen, T. (2008). Information, role models and perceived returns to education: Experimental evidence from madagascar. *Job Market Paper*.

Papageorgiou, T. (2014). Learning your comparative advantages. *The Review of Economic Studies 81*(3 (288)), 1263–1295.

Riley, E. (2022). Role models in movies: the impact of Queen of Katwe on students' educational attainment. *Review of Economics and Statistics*, 1–48.

Rizzica, L. (2020). Raising aspirations and higher education: Evidence from the united kingdom's widening participation policy. *Journal of Labor Economics 38*(1), 183–214.

Ross, P. H. (2019). Occupation aspirations, education investment, and cognitive outcomes: Evidence from indian adolescents. *World Development 123*, 104613.

Serra, D. (2022). Role models in developing countries. *Handbook of Experimental Development Economics*.

The Delaware Departement of Labor (2019). *Delaware Career Compass* (2019-2020 ed.).

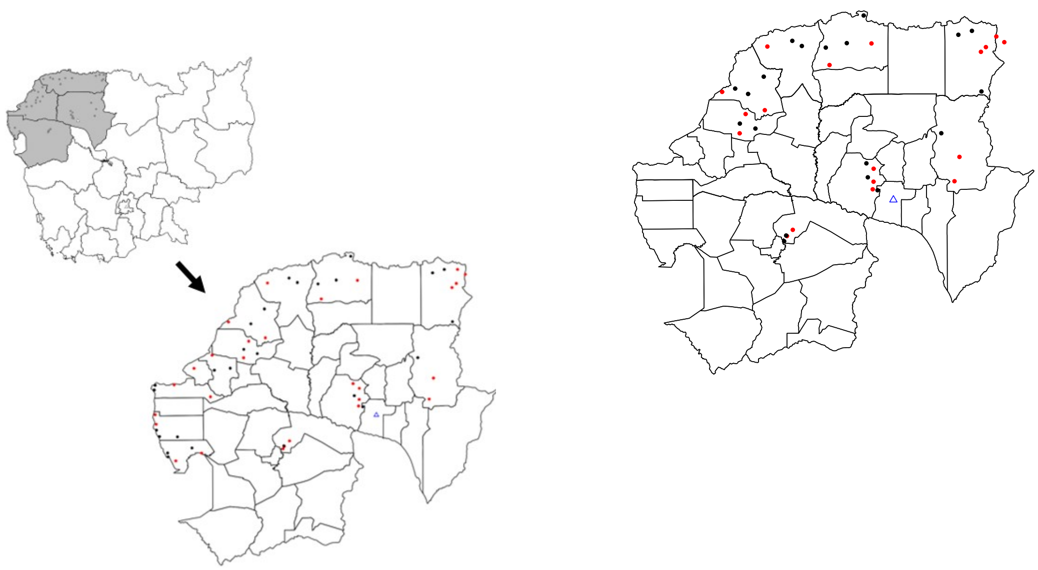
UNESCO (2011). *Education and Fragility in Cambodia.* Paris, France: International Institute for Educational Planning, Inter-Agency Network for Education in Emergencies.

UNESCO (2020). *Global education monitoring report, 2020.* Paris, France: United Nations Educational, Scientific and Cultural Organization.

Figures

Figure 1: Location of Treatment and Control Schools

(a) Selected Sample (b) Final Sample



Notes: Panel (a) shows the entire map of Cambodia in the upper left, highlighting the four provinces of interest in gray. The lower right map zooms into the four provinces, showing district borders and all initially selected treatment and control schools, marked in red and black respectively. Panel (b) highlights the location of the treatment and control schools in the final sample, again in red and black respectively.

Figure 2: Timeline of the Data Collection

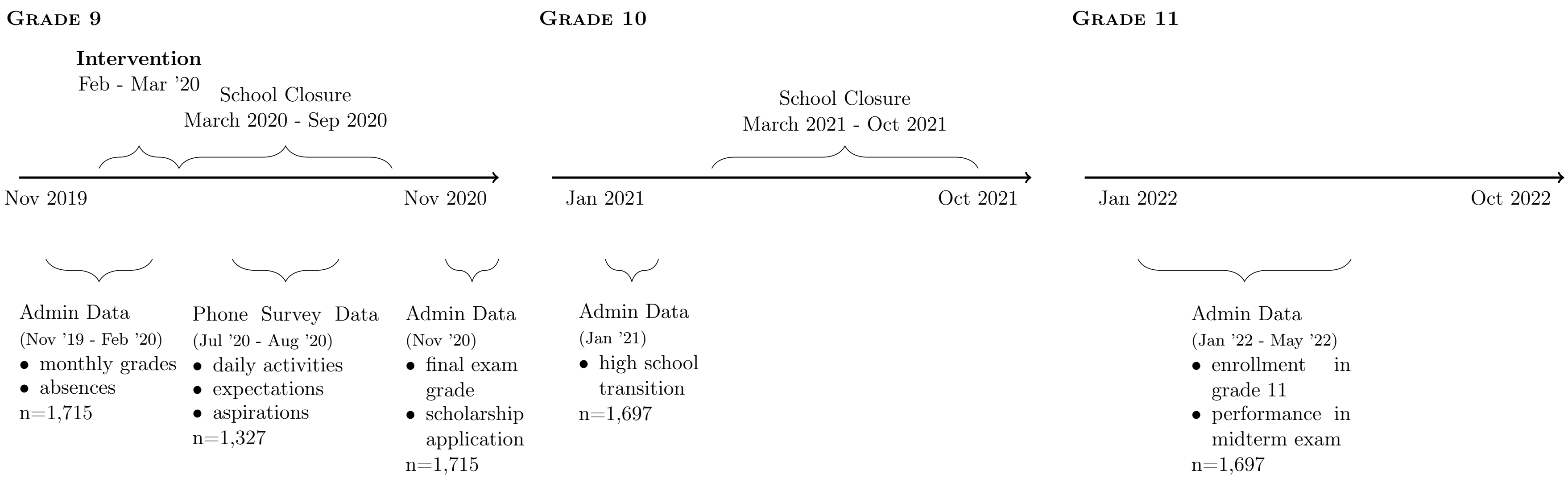
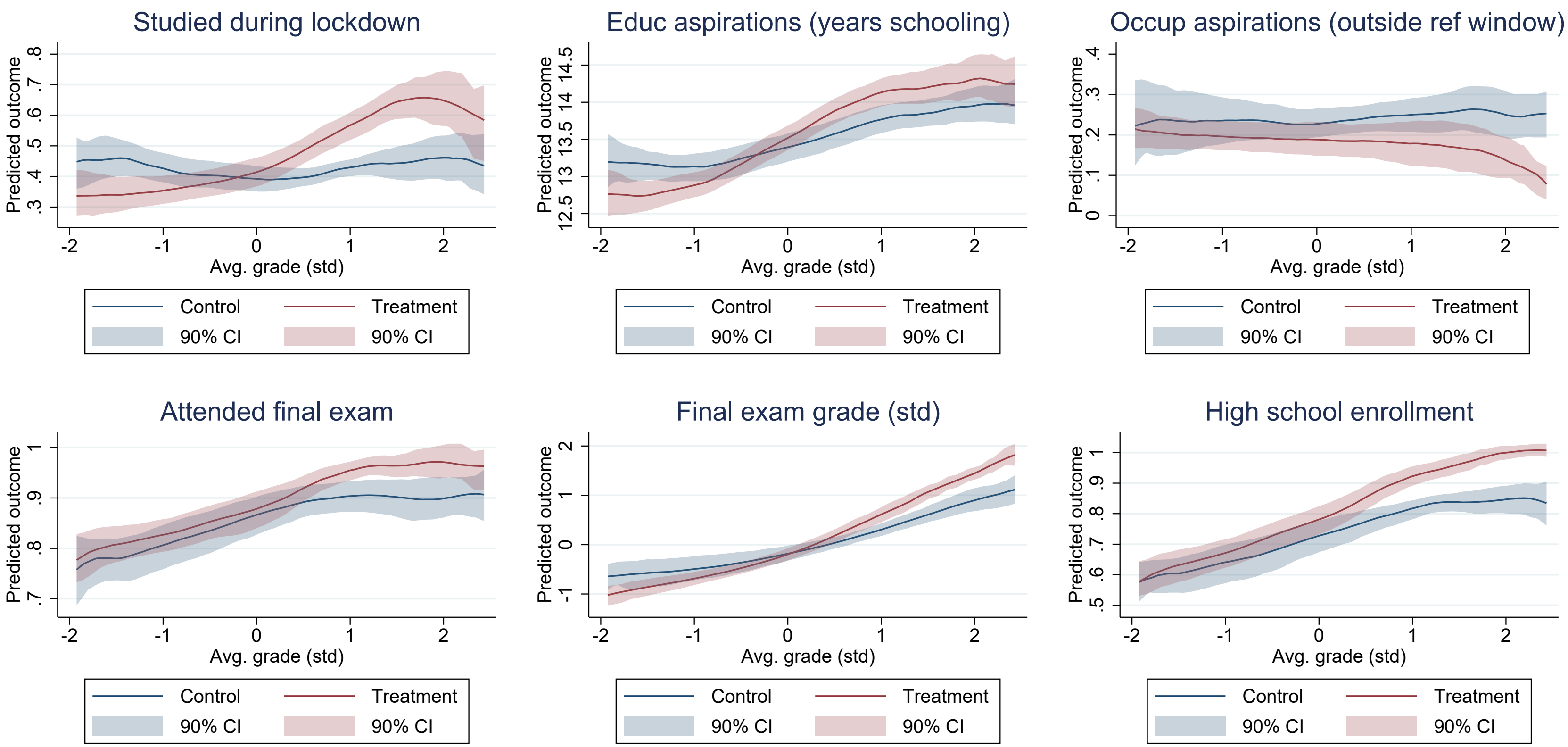
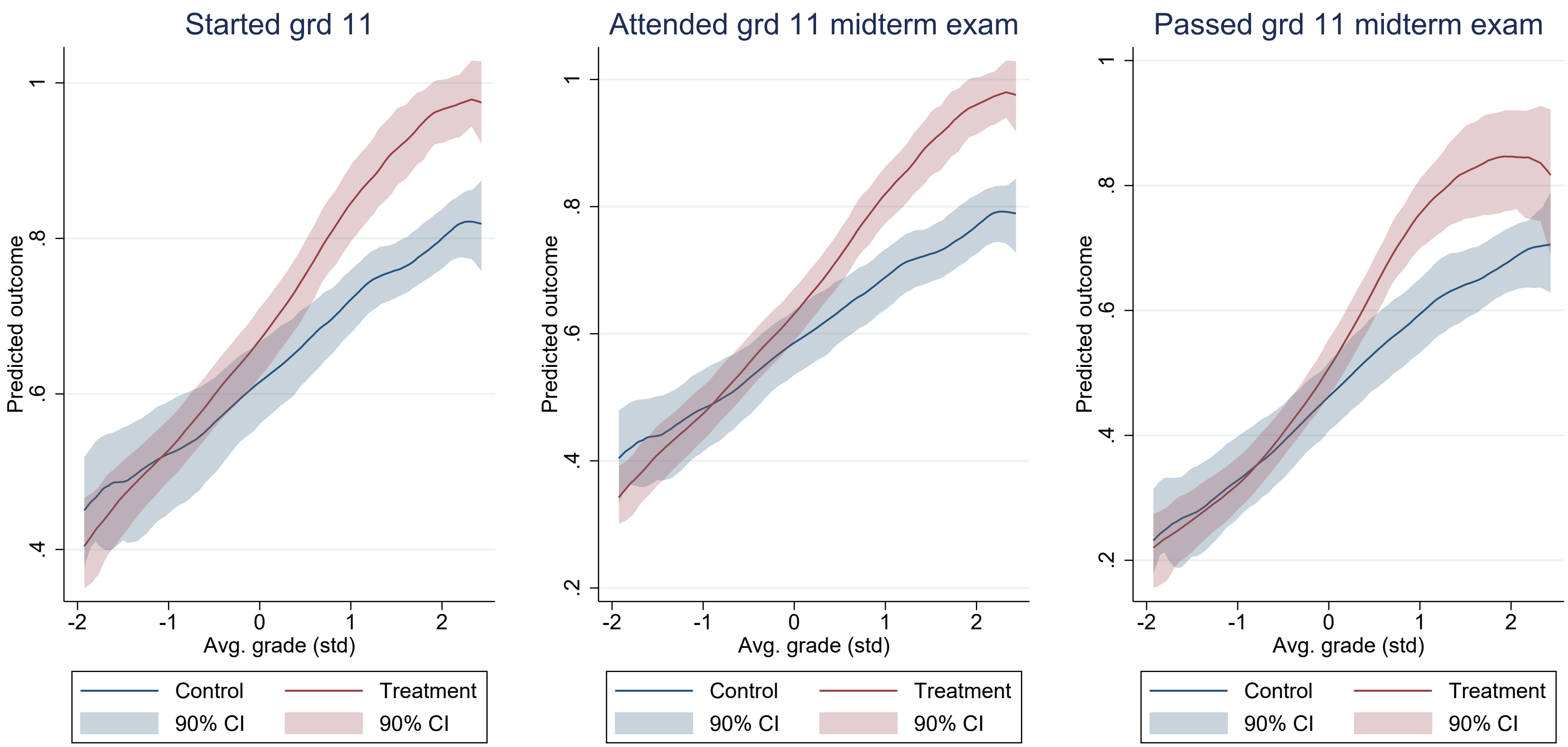


Figure 3: Treatment effect heterogeneity by pre-intervention grades (1)



Notes: These figures display the weighted moving-average (bandwith = 0.5, Epanechnikov kernel) and bootstrap confidence intervals (clustered at the level of the school) of the dependent variable (in the figure header) over the baseline grade (sum of Math, Khmer and English grades, averaged over the months December and January, and standardized over all schools), separately for treatment and control schools. To produce semi-parametric estimates, the dependent variable is first partialled out from District fixed effects, as well as student controls (gender, age, average absence) and school controls (class size, Child's Dream partnership).

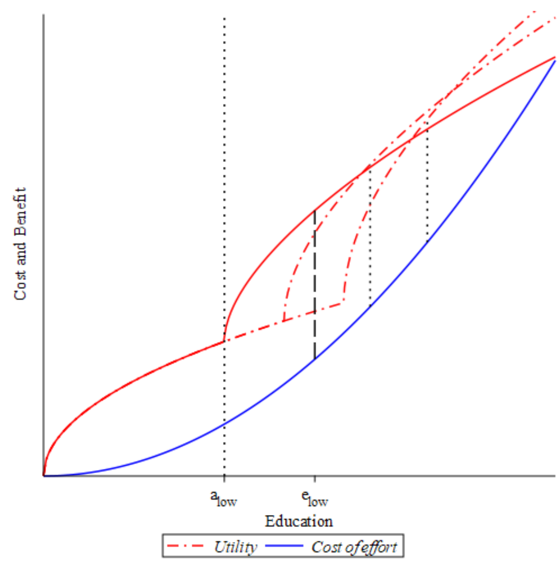
Figure 4: Treatment effect heterogeneity by pre-intervention grades (2)



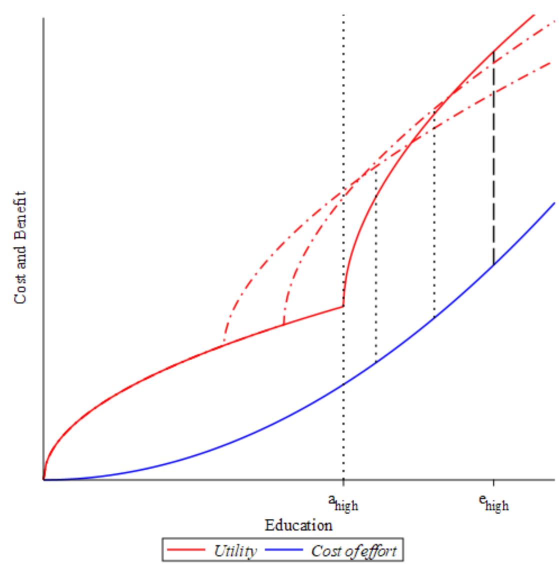
Notes: These figures display the weighted moving-average (bandwith = 0.5, Epanechnikov kernel) and bootstrap confidence intervals (clustered at the level of the school) of the dependent variable (in the figure header) over the baseline grade (sum of Math, Khmer and English grades, averaged over the months December and January, and standardized over all schools), separately for treatment and control schools. To produce semi-parametric estimates, the dependent variable is first partialled out from District fixed effects, as well as student controls (gender, age, average absence) and school controls (class size, Child's Dream partnership).

Figure 5: The choice of optimal effort-aspiration pairs at different levels of innate ability

(a) Low-ability student

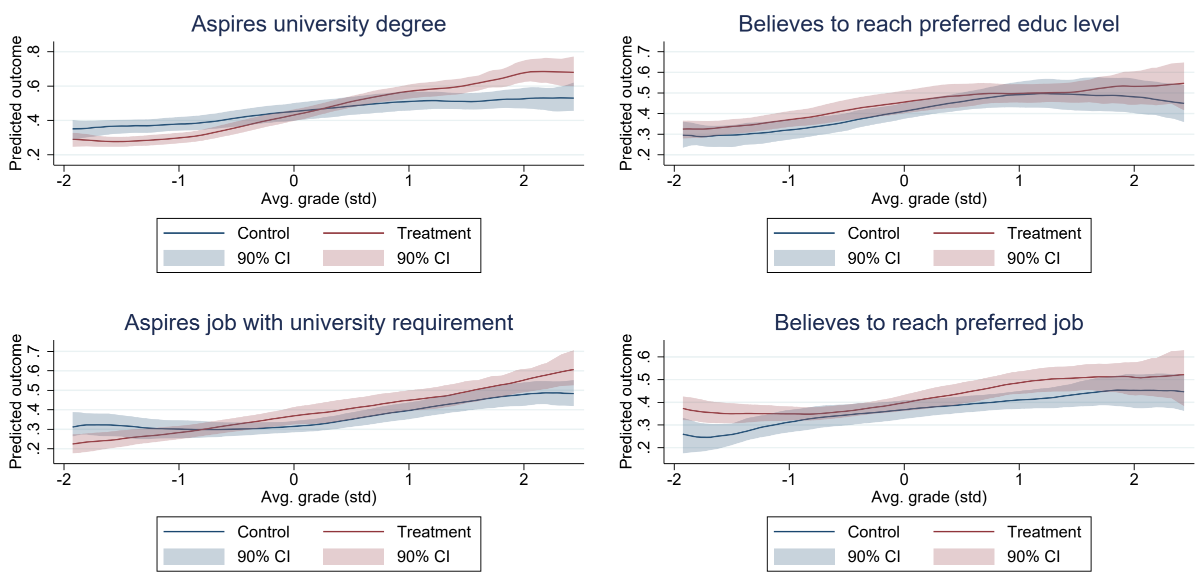


(b) High-ability student



Notes: Panel (a) shows the optimal effort-aspiration pair for a low-ability student. Panel (b) shows the optimal effort-aspiration pair for a high-ability student.

Figure 6: Treatment effect heterogeneity by pre-intervention grades - beliefs



Notes: These figures display the weighted moving-average (bandwith = 0.5, Epanechnikov kernel) and bootstrap confidence intervals (clustered at the level of the school) of the dependent variable (in the figure header) over the baseline grade (sum of Math, Khmer and English grades, averaged over the months December and January, and standardized over all schools), separately for treatment and control schools. To produce semi-parametric estimates, the dependent variable is first partialled out from District fixed effects, as well as student controls (gender, age, average absence) and school controls (class size, Child's Dream partnership)

Tables

Table 1: Balance Table: Pre-Intervention Characteristics in Treatment and Control Schools

	All			Treatment			Control			Treat. - Contr.		Norm.
Variable	N	Mean	SD	N	Mean	SD	N	Mean	SD	Diff.	p-val	Diff.
STUDENT CHARACTERISTICS - ADMIN. DATA												
Female	1715	0.54	0.50	862	0.54	0.50	853	0.53	0.50	0.01	0.60	0.02
Age	1715	15.05	1.32	862	15.11	1.36	853	15.00	1.28	0.00	0.97	0.08
Distance to school (km)	1715	3.55	3.78	862	4.01	3.95	853	3.09	3.54	1.15	0.03	0.25
Distance to district town (km)	1715	11.33	7.44	862	9.80	6.40	853	12.88	8.08	-2.84	0.13	-0.42
Distance to high school (km)	1715	9.68	6.80	862	9.24	6.43	853	10.13	7.12	-1.10	0.49	-0.13
Final Exam Grade 8	1200	31.71	6.05	665	31.60	6.37	535	31.84	5.64	-0.23	0.78	-0.04
Pre-grade, main subjects (standardized)	1715	0.01	0.98	862	-0.22	0.96	853	0.24	0.95	-0.44	0.00	-0.48
Avg absence (Dec&Jan)	1715	1.52	2.02	862	1.63	1.92	853	1.41	2.10	0.30	0.20	0.11
SCHOOL CHARACTERISTICS - ADMIN. DATA												
Class Size	1715	45.71	11.04	862	46.15	11.52	853	45.26	10.52	1.90	0.49	0.08
Teacher: Female	1715	0.33	0.47	862	0.29	0.45	853	0.36	0.48	-0.05	0.68	-0.15
Teacher: Age	1715	32.42	6.53	862	29.86	5.34	853	35.00	6.61	-4.37	0.02	-0.86
Teacher: Years of Experience	1715	9.30	6.10	862	7.19	5.26	853	11.44	6.16	-3.59	0.05	-0.74
Teacher: Has University Degree	1715	0.51	0.50	862	0.55	0.50	853	0.47	0.50	0.04	0.77	0.16
Teacher: Log Distance to School (km)	1715	1.57	1.22	862	1.80	1.17	853	1.34	1.23	0.47	0.12	0.38
High school attached	1715	0.17	0.37	862	0.14	0.35	853	0.19	0.39	-0.02	0.89	-0.14
Partnership with Child's Dream	1715	0.77	0.42	862	0.86	0.35	853	0.68	0.47	0.14	0.18	0.44
PARENTAL CHARACTERISTICS - PHONE SURVEY												
Father completed $\leq$ primary educ.	1170	0.81	0.39	581	0.84	0.37	589	0.79	0.41	0.04	0.31	0.13
Mother completed $\leq$ primary educ.	1246	0.92	0.27	622	0.93	0.26	624	0.92	0.27	0.01	0.53	0.04
Any parents is farmer	1327	0.69	0.46	666	0.70	0.46	661	0.68	0.47	0.00	0.99	0.04

Notes: Treatment - Control difference, and p-values are obtained by regressing variable of interest on a treatment dummy and district fixed effects with standard errors clustered at the school level. The pre-grade in the main subjects is the sum of Math, English and Khmer, averaged over the months December and January, and standardized across schools. The highest achievable points in Math, Khmer, and English are 100, 100 and 50, respectively. Absences are absent days per month. One school did not report absences, for this school the sample mean is imputed.

Table 2: Main Results – Phone-survey Data

	Studied during lock down		Educ aspirations (years schooling)		Occup aspirations (outside ref window)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Intention to Treat						
Treatment Assigned	0.049	0.008	0.127	0.223	-0.064	-0.045
	(0.041)	(0.030)	(0.133)	(0.122)*	(0.029)**	(0.025)*
	[0.299]	[0.346]	[0.299]	[0.129]	[0.125]	[0.129]
Panel B: Treatment on the Treated						
Treated	0.053	0.008	0.138	0.247	-0.068	-0.046
	(0.043)	(0.031)	(0.141)	(0.132)*	(0.031)**	(0.026)*
	[0.280]	[0.333]	[0.280]	[0.161]	[0.089]	[0.161]
OLS w pre-specified controls	✓		✓		✓	
Cross-fit partialing out Lasso		✓		✓		✓
Control Mean	0.42		13.45		0.23	
Observations	1,296		1,317		1,291	

Notes: Panel A: OLS estimates. Treatment Assigned = The student's school received the intervention. Panel B: 2SLS estimates. Treatment (participation in the intervention) instrumented with assigned treatment status. Standard errors (in parentheses) are clustered at the school level. In brackets, Anderson's sharpened q-values to account for the False Discovery Rate (Anderson, 2008). In columns (1), (3) and (5) we report results from OLS with pre-specified controls including student age, gender, pre-intervention grades and absence, class size, Child's Dream partnership, and district fixed effects. In columns (2), (4) and (6) we report results from cross-fit partialing out lasso using 10 folds and 10 re-samples. */**/*** denote significance levels at 10/5/1 percent respectively.

Table 3: Main Results - Administrative Data

	Attended final exam		Final exam grade (std.)		High school enrollment	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Intention to Treat						
Treatment Assigned	0.040	0.022	0.156	0.201	0.088	0.053
	(0.028)	(0.018)	(0.151)	(0.053)***	(0.036)**	(0.023)**
	[0.207]	[0.076]	[0.260]	[0.001]	[0.061]	[0.020]
Panel B: Treatment on the Treated						
Treated	0.044	0.025	0.167	0.214	0.096	0.059
	(0.031)	(0.020)	(0.159)	(0.056)***	(0.039)**	(0.025)**
	[0.183]	[0.071]	[0.242]	[0.001]	[0.041]	[0.020]
OLS w pre-specified controls	✓		✓		✓	
Cross-fit partialing out Lasso		✓		✓		✓
Control Mean	0.87		0.04		0.75	
Observations	1,715		1,485		1,697	

Notes: Panel A: OLS estimates. Treatment Assigned = The student's school received the intervention. Panel B: 2SLS estimates. Treatment (participation in the intervention) instrumented with assigned treatment status. Standard errors (in parentheses) are clustered at the school level. In brackets, Anderson's sharpened q-values to account for the False Discovery Rate (Anderson, 2008). In columns (1), (3) and (5) we report results from OLS with pre-specified controls including student age, gender, pre-intervention grades and absence, class size, Child's Dream partnership, and district fixed effects. In columns (2), (4) and (6) we report results from cross-fit partialing out lasso using 10 folds and 10 re-samples.

Table 4: High-school Results - Grade 11

	Started grd 11		Attended grd 11 midterm exam		Passed grd 11 midterm exam	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Intention to Treat						
Treatment Assigned	0.086	0.055	0.084	0.047	0.093	0.077
	(0.043)*	(0.025)**	(0.041)**	(0.025)*	(0.046)*	(0.026)***
	[0.056]	[0.027]	[0.056]	[0.041]	[0.056]	[0.010]
Panel B: Treatment on the Treated						
Treated	0.094	0.060	0.092	0.052	0.101	0.086
	(0.046)**	(0.028)**	(0.044)**	(0.027)*	(0.049)**	(0.028)***
	[0.043]	[0.030]	[0.043]	[0.045]	[0.043]	[0.010]
OLS w pre-specified controls	✓		✓		✓	
Cross-fit partialing out Lasso		✓		✓		✓
Control Mean	0.64		0.61		0.49	
Observations	1,697		1,697		1,697	

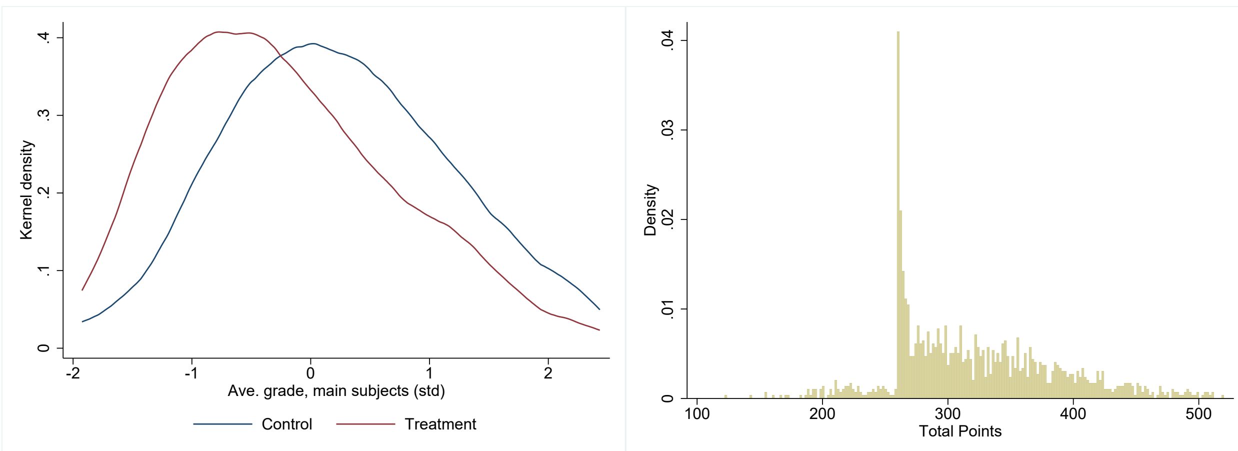
Notes: Panel A: OLS estimates. Treatment Assigned = The student's school received the intervention. Panel B: 2SLS estimates. Treatment (participation in the intervention) instrumented with assigned treatment status. Standard errors (in parentheses) are clustered at the school level. In brackets, Anderson's sharpened q-values to account for the False Discovery Rate (Anderson, 2008). In columns (1), (3) and (5) we report results from OLS with pre-specified controls including student age, gender, pre-intervention grades and absence, class size, Child's Dream partnership, and district fixed effects. In columns (2), (4) and (6) we report results from cross-fit partialing out lasso using 10 folds and 10 re-samples.

A Online Appendix: Additional Tables and Figures

A.1 Figures

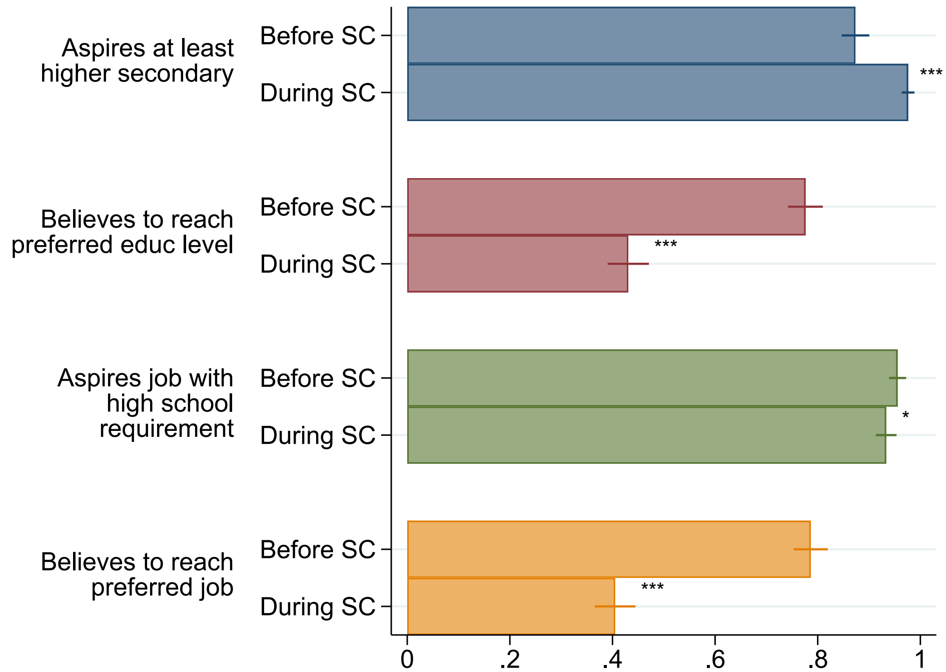
Figure A.1: Distribution of Grades

(a) Pre-intervention Grades (b) Final Exam Grades



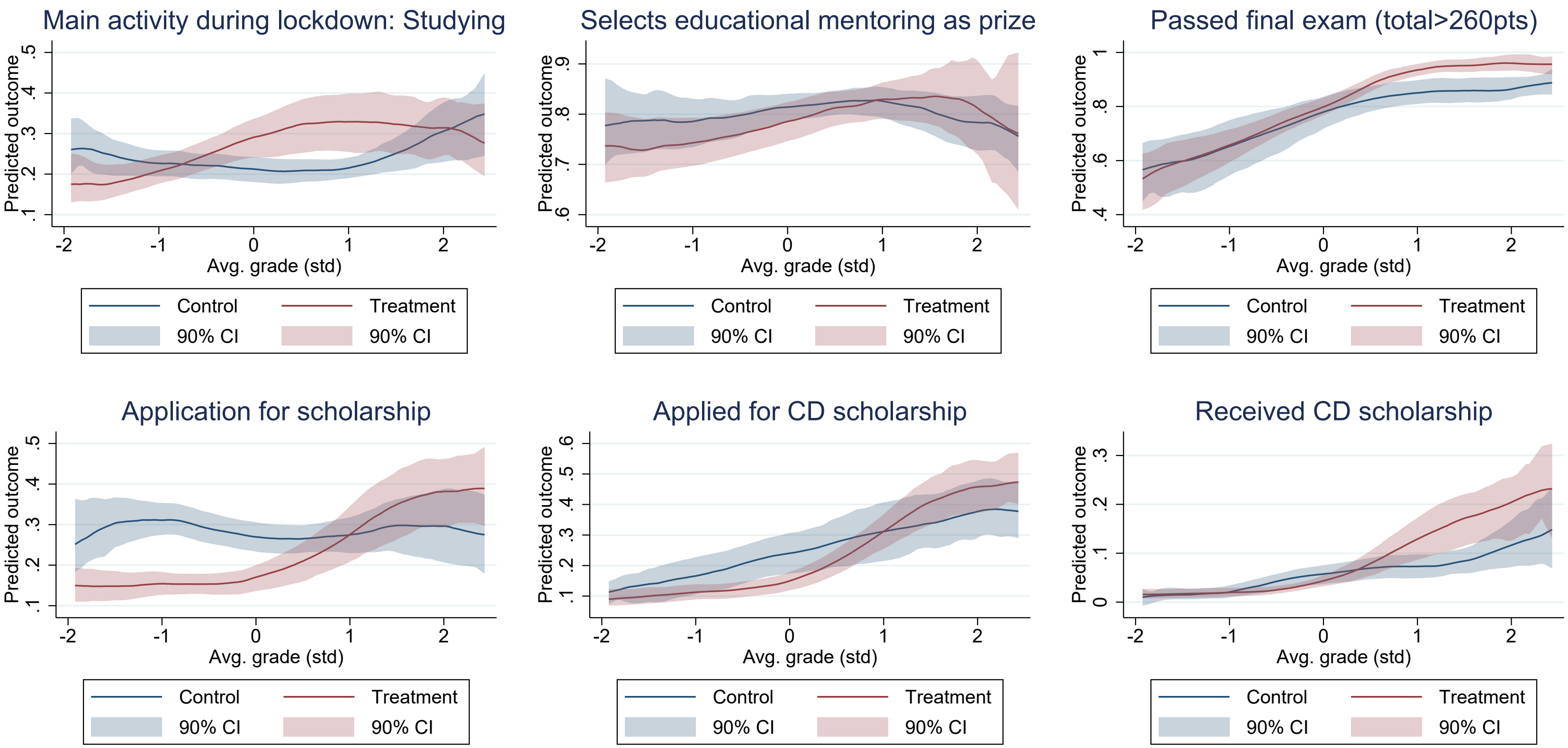
Notes: Panel (a) shows the distribution of pre-intervention grades for treatment and control. Panel (b) shows the distribution of the total points students obtained in the final exam.

Figure A.2: Aspirations and Expectations Before and During COVID-19 Lockdown



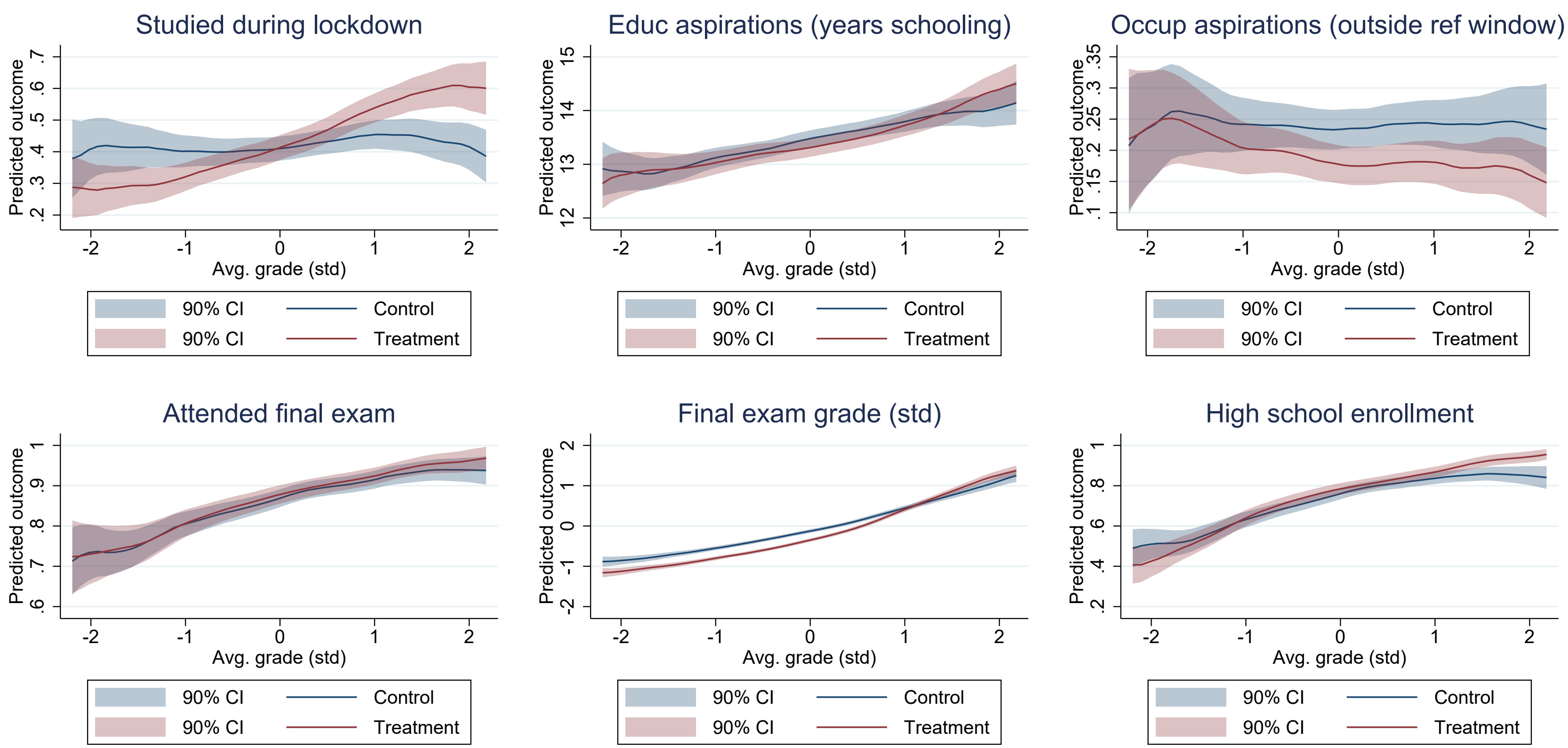
Notes: For treated students who also participated in the phone survey. All outcomes are dummies, SC denotes school closure. */**/*** denote significance levels at 10/5/1 percent respectively.

Figure A.3: Treatment effect heterogeneity by pre-intervention grades - educ. investment during grade 9



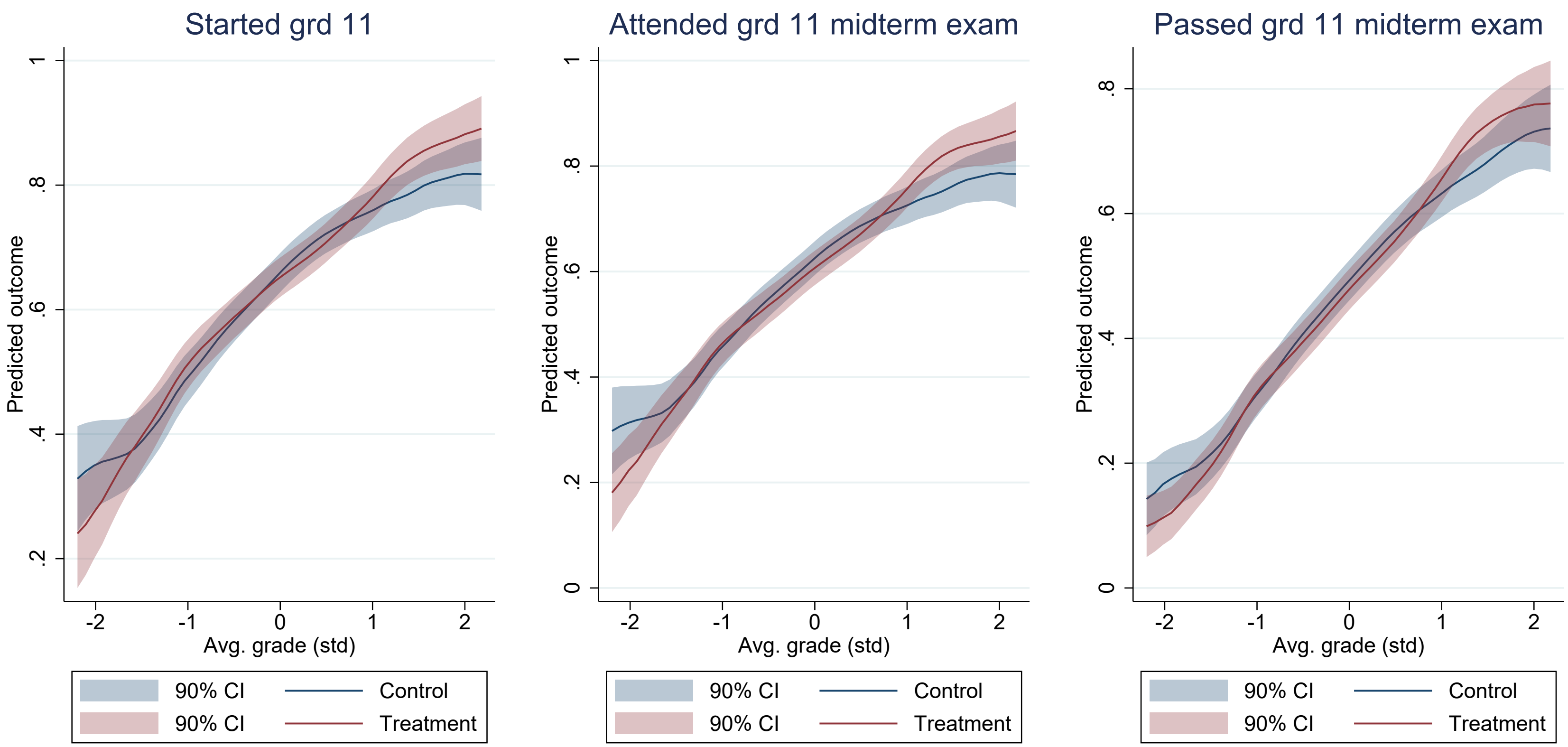
Notes: These figures display the weighted moving-average (bandwith = 0.5, Epanechnikov kernel) and bootstrap confidence intervals (clustered at the level of the school) of the dependent variable (in the figure header) over the baseline grade (sum of Math, Khmer and English grades, averaged over the months December and January, and standardized over all schools), separately for treatment and control schools. To produce semi-parametric estimates, the dependent variable is first partialled out from District fixed effects, as well as student controls (gender, age, average absence) and school controls (class size, Child's Dream partnership).

Figure A.4: Treatment effect heterogeneity by pre-intervention grades - standardized within class (1)



Notes: These figures display the weighted moving-average (bandwith = 0.5, Epanechnikov kernel) and confidence intervals of the dependent variable (in the figure header) over the pre-intervention grade (total main subjects, averaged over the months December and January, standardized within each class), separately for treatment and control schools. To produce semi-parametric estimates, the dependent variable is first partialled out from District fixed effects, as well as student controls (gender, age, average absence) and school controls (class size, Child's Dream partnership).

Figure A.5: Treatment effect heterogeneity by pre-intervention grades - standardized within class (2)



Notes: These figures display the weighted moving-average (bandwith = 0.5, Epanechnikov kernel) and confidence intervals of the dependent variable (in the figure header) over the average pre-intervention grade (total main subjects, averaged over the months December and January, standardized within each class), separately for treatment and control schools. To produce semi-parametric estimates, the dependent variable is first partialled out from District fixed effects, as well asstudent controls (gender, age, average absence) and school controls (class size, Child's Dream partnership).

A.2 Tables

Table A.1: Eight most frequently mentioned jobs

Job	Freq.	Percent	Cum Percent
Teacher	58	38.16	38.16
Doctor	24	15.79	53.95
Police Officer	24	15.79	69.74
Soldier	23	15.13	84.87
Engineer	4	2.63	87.50
Banker	3	1.97	89.47
Tailor	3	1.97	91.45
Dancer	2	1.32	92.76
Other	11	7.24	100.00
Total	152	100.00	

Notes: Students are asked in an open-ended question what job they would like to be doing when they are about 25 years old. Answers were categorized by the researchers.

Table A.2: Summary Statistics

Variable	(1) Mean	(2) Median	(3) SD	(4) Min	(5) Max	(6) Obs.
Follow-up Characteristics - Phone Survey						
Studied during lockdown	0.43	0.00	0.50	0.00	1.00	1296
Main activity during lockdown: Studying	0.25	0.00	0.43	0.00	1.00	1323
Educ aspirations (years schooling)	13.48	12.00	2.15	9.00	20.00	1317
Aspires at least higher secondary	0.96	1.00	0.19	0.00	1.00	1317
Aspires university degree	0.44	0.00	0.50	0.00	1.00	1317
Believes to reach preferred educ level	0.43	0.00	0.49	0.00	1.00	1291
Occup aspirations (outside ref window)	0.21	0.00	0.41	0.00	1.00	1291
Aspires job with high school requirement	0.91	1.00	0.28	0.00	1.00	1291
Aspires job with university requirement	0.36	0.00	0.48	0.00	1.00	1327
Believes to reach preferred job	0.39	0.00	0.49	0.00	1.00	1289
Application for scholarship	0.24	0.00	0.43	0.00	1.00	1296
Selects educational mentoring as prize	0.80	1.00	0.40	0.00	1.00	1247
Admin Characteristics (post) - Complete Sample						
Applied for CD scholarship	0.17	0.00	0.37	0.00	1.00	1317
Received CD scholarship	0.04	0.00	0.20	0.00	1.00	1317
Attended final exam	0.87	1.00	0.34	0.00	1.00	1715
Final total grade	321.06	310.00	64.56	122.00	520.00	1485
Passed final exam (total>260pts)	0.77	1.00	0.42	0.00	1.00	1715
Grade 9 transcripts requested	0.81	1.00	0.39	0.00	1.00	1711
High school enrollment	0.75	1.00	0.43	0.00	1.00	1697
Started grd 11	0.64	1.00	0.48	0.00	1.00	1697
Attended grd 11 midterm exam	0.60	1.00	0.49	0.00	1.00	1697
Passed grd 11 midterm exam	0.48	0.00	0.50	0.00	1.00	1697

Notes: Population means, median, standard deviation, minimum and maximum, as well as the number of observations are provided for each characteristic. The final total grade excludes students who scored 0 or did not write the exam.

Table A.3: Heterogeneity by Performance - Survey Data

	Studied during lock down		Educ aspirations (years schooling)		Occup aspirations (outside ref window)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Intention to Treat						
Treatment Assigned	0.038	0.020	0.102	0.137	-0.060	-0.030
	(0.040)	(0.029)	(0.124)	(0.117)	(0.029)**	(0.025)
Pre-grade (std)	0.004	0.029	0.300	0.293	0.014	0.044
	(0.024)	(0.025)	(0.096)***	(0.112)***	(0.021)	(0.022)**
Treatment Assigned x Pre-grade (std)	0.099	0.101	0.264	0.265	-0.039	-0.027
	(0.032)***	(0.029)***	(0.125)**	(0.122)**	(0.027)	(0.024)
Panel B: Treatment on the Treated						
Treated	0.036	0.019	0.096	0.149	-0.062	-0.034
	(0.044)	(0.032)	(0.134)	(0.131)	(0.031)**	(0.027)
Pre-grade (std)	0.004	0.029	0.299	0.301	0.014	0.043
	(0.024)	(0.025)	(0.094)***	(0.113)***	(0.021)	(0.022)*
Treated x Pre-grade (std)	0.106	0.109	0.285	0.277	-0.040	-0.028
	(0.035)***	(0.032)***	(0.135)**	(0.135)**	(0.029)	(0.026)
OLS w pre-specified controls	✓		✓		✓	
Cross-fit partialing out Lasso		✓		✓		✓
Control Mean	0.42		13.45		0.23	
Observations	1,296		1,317		1,291	

Notes: Panel A: OLS estimates. Treatment Assigned = The student's school received the intervention. Panel B: 2SLS estimates. Treatment (participation in the intervention) instrumented with assigned treatment status. Standard errors (in parentheses) are clustered at the school level. In brackets, Anderson's sharpened q-values to account for the False Discovery Rate (Anderson, 2008). In columns (1), (3) and (5), we report results from OLS with pre-specified controls including student age, gender, pre-intervention grades and absence, class size, Child's Dream partnership, and district fixed effects. In columns (2), (4) and (6), we report results from cross-fit partialing out lasso using 10 folds and 10 re-samples. */**/*** denote significance levels at 10/5/1 percent respectively.

Table A.4: Heterogeneity by Performance - Administrative Data

	Attended final exam		Final exam grade (std.)		High school enrollment	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Intention to Treat						
Treatment Assigned	0.039	0.022	0.131	0.116	0.087	0.061
	(0.028)	(0.017)	(0.145)	(0.059)*	(0.034)**	(0.023)***
Pre-grade (std)	0.046	0.040	0.557	0.465	0.088	0.085
	(0.017)**	(0.016)**	(0.100)***	(0.053)***	(0.024)***	(0.021)***
Treatment Assigned x Pre-grade (std)	0.023	0.019	0.260	0.252	0.055	0.056
	(0.020)	(0.017)	(0.099)**	(0.054)***	(0.027)*	(0.021)***
Panel B: Treatment on the Treated						
Treated	0.042	0.023	0.128	0.119	0.092	0.066
	(0.031)	(0.019)	(0.155)	(0.064)*	(0.037)**	(0.025)***
Pre-grade (std)	0.046	0.040	0.558	0.472	0.088	0.086
	(0.017)***	(0.016)***	(0.099)***	(0.054)***	(0.023)***	(0.021)***
Treated x Pre-grade (std)	0.023	0.020	0.275	0.272	0.056	0.057
	(0.023)	(0.020)	(0.108)**	(0.059)***	(0.031)*	(0.024)**
OLS w pre-specified controls	✓		✓		✓	
Cross-fit partialing out Lasso		✓		✓		✓
Control Mean	0.87		0.04		0.75	
Observations	1,715		1,485		1,697	

Notes: Panel A: OLS estimates. Treatment Assigned = The student's school received the intervention. Panel B: 2SLS estimates. Treatment (participation in the intervention) instrumented with assigned treatment status. Standard errors (in parentheses) are clustered at the school level. In brackets, Anderson's sharpened q-values to account for the False Discovery Rate (Anderson, 2008). In columns (1), (3) and (5), we report results from OLS with pre-specified controls including student age, gender, pre-intervention grades and absence, class size, Child's Dream partnership, and district fixed effects. In columns (2), (4) and (6), we report results from cross-fit partialing out lasso using 10 folds and 10 re-samples. */**/*** denote significance levels at 10/5/1 percent respectively.

Table A.5: Heterogeneity by Performance - Grade 11

	Started grd 11		Attended grd 11 midterm exam		Passed grd 11 midterm exam	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Intention to Treat						
Treatment Assigned	0.085	0.068	0.083	0.058	0.092	0.068
	(0.040)**	(0.026)***	(0.037)**	(0.025)**	(0.043)**	(0.026)***
Pre-grade (std)	0.113	0.102	0.117	0.094	0.148	0.116
	(0.024)***	(0.023)***	(0.022)***	(0.022)***	(0.026)***	(0.024)***
Treatment Assigned x Pre-grade (std)	0.072	0.079	0.086	0.091	0.076	0.082
	(0.028)**	(0.023)***	(0.026)***	(0.022)***	(0.031)**	(0.024)***
Panel B: Treatment on the Treated						
Treated	0.089	0.069	0.085	0.059	0.096	0.070
	(0.043)**	(0.029)**	(0.040)**	(0.028)**	(0.046)**	(0.028)**
Pre-grade (std)	0.113	0.102	0.117	0.094	0.148	0.116
	(0.023)***	(0.023)***	(0.022)***	(0.023)***	(0.025)***	(0.023)***
Treated x Pre-grade (std)	0.075	0.085	0.090	0.100	0.079	0.087
	(0.031)**	(0.026)***	(0.028)***	(0.025)***	(0.034)**	(0.026)***
OLS w pre-specified controls	✓		✓		✓	
Cross-fit partialing out Lasso		✓		✓		✓
Control Mean	0.64		0.61		0.49	
Observations	1,697		1,697		1,697	

Notes: Panel A: OLS estimates. Treatment Assigned = The student's school received the intervention. Panel B: 2SLS estimates. Treatment (participation in the intervention) instrumented with assigned treatment status. Standard errors (in parentheses) are clustered at the school level. In brackets, Anderson's sharpened q-values to account for the False Discovery Rate (Anderson, 2008). In columns (1), (3) and (5), we report results from OLS with pre-specified controls including student age, gender, pre-intervention grades and absence, class size, Child's Dream partnership, and district fixed effects. In columns (2), (4) and (6), we report results from cross-fit partialing out lasso using 10 folds and 10 re-samples. */**/*** denote significance levels at 10/5/1 percent respectively.

Table A.6: Scholarship Application

	Applied for any scholarship (reported)		Applied for CD scholarship (admin data)		Received CD scholarship (admin data)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Intention to Treat						
Treatment Assigned	-0.075	-0.083	-0.036	-0.044	0.023	0.017
	(0.033)**	(0.028)***	(0.045)	(0.042)	(0.016)	(0.013)
Pre-grade (std)	0.036	-0.005	0.102	0.078	0.051	0.035
	(0.017)**	(0.025)	(0.018)***	(0.022)***	(0.010)***	(0.011)***
Treatment Assigned x Pre-grade (std)		0.076		0.044		0.031
		(0.033)**		(0.030)		(0.017)*
Panel B: Treatment on the Treated						
Treated	-0.081	-0.095	-0.039	-0.049	0.024	0.018
	(0.035)**	(0.028)***	(0.048)	(0.044)	(0.017)	(0.013)
Pre-grade (std)	0.038	-0.005	0.103	0.078	0.051	0.035
	(0.017)**	(0.024)	(0.018)***	(0.022)***	(0.009)***	(0.010)***
Treated x Pre-grade (std)		0.086		0.050		0.032
		(0.035)**		(0.031)		(0.018)*
OLS w pre-specified controls	✓	✓	✓	✓	✓	✓
Control Mean	0.30		0.21		0.04	
Observations	1,296		1,317		1,317	

Notes: Panel A: OLS estimates. Treatment Assigned = The student's school received the intervention. Panel B: 2SLS estimates. Treatment (participation in the intervention) instrumented with assigned treatment status. Standard errors (in parentheses) are clustered at the school level. Student (gender, age, pre-intervention grades in main subjects (total, std.), pre-intervention absences) and school controls (class size, Child's Dream partnership) included as pre-specified. */**/*** denote significance levels at 10/5/1 percent respectively.

Table A.7: Randomization Inference and Familywise Error Rate Correction

	Treatment Assigned			Treatment Assigned x Pr-grade (std)		
Variable	Model p-value	RI test p-value	rwolf2 p-value	Model p-value	RI test p-value	rwolf2 p-value
	(1)	(2)	(3)	(4)	(5)	(6)
Studying during lockdown	0.240	0.237	0.414	0.004	0.014	0.005
Educational Aspirations (years schooling)	0.345	0.390	0.414	0.041	0.064	0.039
Occupational aspirations (outside ref. window)	0.037	0.086	0.132	0.151	0.176	0.112
Participated in final exam, grd 9	0.171	0.202	0.336	0.270	0.327	0.135
Final Exam grade (std)	0.309	0.329	0.336	0.013	0.006	0.010
High school enrollment	0.019	0.038	0.105	0.052	0.045	0.018
Started grade 11	0.053	0.080	0.095	0.013	0.011	0.014
Attended grade 11 midterm	0.049	0.076	0.095	0.002	0.003	0.003
Passed grade 11 midterm	0.053	0.070	0.095	0.019	0.023	0.014

Notes: Includes pre-specified controls and with standard errors clustered at the school level. Columns (1) and (4) report p-values of the OLS specification with pre-specified controls. Columns (2) and (5) report p-values from the Fisher's permutation-based randomization inference (RI) test with 1000 replications implemented by `ritest` (Hess, 2017). Columns (3) and (6) report Romano-Wolf stepdown adjusted p-values to control familywise error rates implemented by `rwolf2` (Clarke, 2021).

Table A.8: Main Results – Weighted Phone-survey Data

	Studied during lock down		Educ aspirations (years schooling)		Occup aspirations (outside ref window)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Intention to Treat						
Treatment Assigned	0.035	0.033	0.119	0.115	-0.067	-0.066
	(0.041)	(0.040)	(0.135)	(0.122)	(0.030)**	(0.029)**
Pre-grade (std)	0.057	0.005	0.435	0.290	-0.009	0.010
	(0.020)***	(0.024)	(0.081)***	(0.096)***	(0.014)	(0.022)
Treatment Assigned x Pre-grade (std)		0.099		0.277		-0.036
		(0.032)***		(0.124)**		(0.028)
Panel B: Treatment on the Treated						
Treated	0.038	0.030	0.129	0.109	-0.072	-0.070
	(0.043)	(0.043)	(0.143)	(0.132)	(0.032)**	(0.031)**
Pre-grade (std)	0.056	0.005	0.432	0.290	-0.008	0.010
	(0.020)***	(0.024)	(0.079)***	(0.094)***	(0.013)	(0.021)
Treated x Pre-grade (std)		0.107		0.299		-0.037
		(0.034)***		(0.134)**		(0.030)
OLS w pre-specified controls	✓	✓	✓	✓	✓	✓
Control Mean	0.42		13.45		0.23	
Observations	1,296		1,317		1,291	

Notes: Panel A: OLS estimates. Treatment Assigned = The student's school received the intervention. Panel B: 2SLS estimates. Treatment (participation in the intervention) instrumented with assigned treatment status. Observations are weighted by the inverse of the probability of participating in the phone-survey. Standard errors (in parentheses) are clustered at the school level. Student (gender, age, pre-intervention grades in main subjects (total, std.), pre-intervention absences) and school controls (class size, Child's Dream partnership) included as pre-specified. */**/*** denote significance levels at 10/5/1 percent respectively.

Table A.9: Kling-Liebmann Sensitivity Bounds for Missing Values – Survey Data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	$x_{a\|T} = min(x_T)$	$x_{a\|T} = \bar{x}_T - .5\sigma_{xT}$	$x_{a\|T} = \bar{x}_T - .25\sigma_{xT}$	$x_{a\|T} = \bar{x}_T$	$x_{a\|T} = \bar{x}_T + .25\sigma_{xT}$	$x_{a\|T} = \bar{x}_T + .5\sigma_{xT}$	$x_{a\|T} = max(x_T)$
	$x_{a\|C} = max(x_C)$	$x_{a\|C} = \bar{x}_C + .5\sigma_{xC}$	$x_{a\|C} = \bar{x}_C + .25\sigma_{xC}$	$x_{a\|C} = \bar{x}_C$	$x_{a\|C} = \bar{x}_C - .25\sigma_{xC}$	$x_{a\|C} = \bar{x}_C - .5\sigma_{xC}$	$x_{a\|C} = min(x_C)$
STUDIED DURING LOCKDOWN							
Treatment Assigned	0.022	0.035	0.041	0.047	0.053	0.059	0.069
	(0.039)	(0.039)	(0.040)	(0.040)	(0.040)	(0.041)	(0.042)
Treatment Assigned	0.012	0.026	0.032	0.038	0.044	0.050	0.061
	(0.039)	(0.039)	(0.039)	(0.040)	(0.040)	(0.040)	(0.041)
Treatment Assigned x Pre-grade (std)	0.110	0.104	0.102	0.099	0.097	0.094	0.090
	(0.033)***	(0.032)***	(0.032)***	(0.032)***	(0.032)***	(0.032)***	(0.032)***
EDUC ASPIRATIONS (YEARS SCHOOLING)							
Treatment Assigned	0.027	0.110	0.118	0.127	0.135	0.144	0.207
	(0.125)	(0.130)	(0.131)	(0.132)	(0.133)	(0.133)	(0.140)
Treatment Assigned	0.000	0.084	0.093	0.102	0.110	0.119	0.183
	(0.116)	(0.121)	(0.122)	(0.122)	(0.123)	(0.124)	(0.130)
Treatment Assigned x Pre-grade (std)	0.285	0.270	0.268	0.267	0.265	0.264	0.255
	(0.128)**	(0.125)**	(0.124)**	(0.124)**	(0.124)**	(0.124)**	(0.124)**
OCCUP ASPIRATIONS (OUTSIDE REF WINDOW)							
Treatment Assigned	-0.088	-0.073	-0.068	-0.063	-0.058	-0.053	-0.038
	(0.029)***	(0.029)**	(0.029)**	(0.029)**	(0.029)*	(0.029)*	(0.028)
Treatment Assigned	-0.085	-0.070	-0.065	-0.059	-0.054	-0.049	-0.033
	(0.028)***	(0.028)**	(0.028)**	(0.028)**	(0.028)*	(0.028)*	(0.028)
Treatment Assigned x Pre-grade (std)	-0.031	-0.035	-0.037	-0.039	-0.041	-0.043	-0.052
	(0.027)	(0.027)	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)*
OLS w pre-specified controls	✓	✓	✓	✓	✓	✓	✓

Notes: Analysis of treatment effects by varying the outcome values of students with missing values (due to item non-response) as follows: Column (1) - set to the minimum value for students in the treatment arm and to the maximum value for students in the control arm; column (2) - set to the average value minus half a standard deviation for students in the treatment arm and to the average value plus half a standard deviation for students in the control arm; column (3) - set to the average value minus .25 standard deviation for students in the treatment arm and to the average value plus .25 standard deviation for students in the control arm; column (4) - set to the respective average in the treatment and control group; column (6) - set to the average value plus .25 standard deviation for students in the treatment arm and to the average value minus .25 standard deviation for students in the control arm; column (7) - set to the average value plus half a standard deviation for students in the treatment arm and to the average value minus half a standard deviation for students in the control arm; column (8) - set to the maximum value for students in the treatment arm and to the minimum value for students in the control arm. Intention to treat estimates with pre-specified controls reported throughout. Standard errors clustered at the school level in parentheses. */**/*** denote significance levels at 10/5/1 percent respectively.

Table A.10: Kling-Liebmann Sensitivity Bounds for Missing Values – High School Data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	$x_{a\|T} = min(x_T)$ $x_{a\|C} = max(x_C)$	$x_{a\|T} = \bar{x}_T - .5\sigma_{xT}$ $x_{a\|C} = \bar{x}_C + .5\sigma_{xC}$	$x_{a\|T} = \bar{x}_T - .25\sigma_{xT}$ $x_{a\|C} = \bar{x}_C + .25\sigma_{xC}$	$x_{a\|T} = \bar{x}_T$ $x_{a\|C} = \bar{x}_C$	$x_{a\|T} = \bar{x}_T + .25\sigma_{xT}$ $x_{a\|C} = \bar{x}_C - .25\sigma_{xC}$	$x_{a\|T} = \bar{x}_T + .5\sigma_{xT}$ $x_{a\|C} = \bar{x}_C - .5\sigma_{xC}$	$x_{a\|T} = max(x_T)$ $x_{a\|C} = min(x_C)$
HIGH SCHOOL ENROLLMENT							
Treatment Assigned	0.076	0.082	0.084	0.087	0.089	0.091	0.096
	(0.036)**	(0.036)**	(0.036)**	(0.036)**	(0.036)**	(0.036)**	(0.035)***
Treatment Assigned	0.076	0.082	0.084	0.086	0.088	0.090	0.096
	(0.034)**	(0.034)**	(0.034)**	(0.034)**	(0.034)**	(0.034)**	(0.034)***
Treatment Assigned x Pre-grade (std)	0.055	0.054	0.054	0.054	0.054	0.054	0.054
	(0.027)*	(0.027)*	(0.027)*	(0.027)*	(0.027)*	(0.027)*	(0.027)*
STARTED GRD 11							
Treatment Assigned	0.075	0.080	0.082	0.085	0.087	0.090	0.095
	(0.043)*	(0.043)*	(0.043)*	(0.043)*	(0.043)**	(0.043)**	(0.043)**
Treatment Assigned	0.074	0.079	0.081	0.084	0.086	0.089	0.094
	(0.040)*	(0.040)*	(0.040)**	(0.040)**	(0.040)**	(0.040)**	(0.040)**
Treatment Assigned x Pre-grade (std)	0.071	0.071	0.071	0.071	0.071	0.071	0.070
	(0.028)**	(0.028)**	(0.028)**	(0.028)**	(0.028)**	(0.028)**	(0.028)**
ATTENDED GRD 11 MIDTERM EXAM							
Treatment Assigned	0.072	0.077	0.080	0.082	0.085	0.087	0.092
	(0.041)*	(0.041)*	(0.041)*	(0.041)*	(0.041)**	(0.041)**	(0.041)**
Treatment Assigned	0.071	0.076	0.079	0.081	0.084	0.086	0.091
	(0.037)*	(0.037)**	(0.037)**	(0.037)**	(0.037)**	(0.037)**	(0.037)**
Treatment Assigned x Pre-grade (std)	0.085	0.084	0.084	0.084	0.084	0.084	0.084
	(0.026)***	(0.026)***	(0.026)***	(0.026)***	(0.026)***	(0.026)***	(0.026)***
PASSED GRD 11 MIDTERM EXAM							
Treatment Assigned	0.081	0.085	0.088	0.090	0.093	0.096	0.101
	(0.046)*	(0.046)*	(0.046)*	(0.046)*	(0.046)**	(0.046)**	(0.046)**
Treatment Assigned	0.080	0.084	0.087	0.090	0.092	0.095	0.100
	(0.043)*	(0.042)*	(0.042)**	(0.042)**	(0.042)**	(0.042)**	(0.042)**
Treatment Assigned x Pre-grade (std)	0.074	0.074	0.074	0.074	0.074	0.074	0.074
	(0.031)**	(0.031)**	(0.031)**	(0.031)**	(0.031)**	(0.031)**	(0.031)**
OLS w pre-specified controls	✓	✓	✓	✓	✓	✓	✓

Notes: Analysis of treatment effects by varying the outcome values of students with missing values (because students could not be tracked) as follows: Column (1) - set to the minimum value for students in the treatment arm and to the maximum value for students in the control arm; column (2) - set to the average value minus half a standard deviation for students in the treatment arm and to the average value plus half a standard deviation for students in the control arm; column (3) - set to the average value minus .25 standard deviation for students in the treatment arm and to the average value plus .25 standard deviation for students in the control arm; column (4) - set to the respective average in the treatment and control group; column (6) - set to the average value plus .25 standard deviation for students in the treatment arm and to the average value minus .25 standard deviation for students in the control arm; column (7) - set to the average value plus half a standard deviation for students in the treatment arm and to the average value minus half a standard deviation for students in the control arm; column (8) - set to the maximum value for students in the treatment arm and to the minimum value for students in the control arm. Intention to treat estimates with including pre-specified controls reported throughout. Standard errors clustered at the school level in parentheses. */**/*** denote significance levels at 10/5/1 percent respectively.

Table A.11: Correlates of School Performance

	Distance to to school	Distance to to district town	Both parents' educ $\leq$ primary	Parents are farmers	Parent lost income due to COVID-19	Parent lost job due to COVID-19
	(1)	(2)	(3)	(4)	(5)	(6)
Pre-grade (std)	-0.072	0.328	-0.025	0.011	-0.025	-0.024
	(0.161)	(0.403)	(0.014)*	(0.019)	(0.014)*	(0.013)*
Female	-0.110	0.392	0.062	-0.009	0.049	0.034
	(0.189)	(0.395)	(0.024)**	(0.025)	(0.025)*	(0.018)*
Age	0.146	0.348	0.049	0.012	-0.007	0.012
	(0.107)	(0.170)**	(0.011)***	(0.011)	(0.010)	(0.008)
Mean	3.55	11.33	0.70	0.69	0.70	0.15
Observations	1,715	1,715	1,275	1,327	1,327	1,327

Notes: OLS estimates. Standard errors are depicted in parentheses and clustered at the school level. */**/*** denote significance levels at 10/5/1 percent respectively.

Table A.12: Impact of the Intervention by School Performance vs. Parental Education (1)

	Studied during lock down		Educ aspirations (years schooling)		Occup aspirations (outside ref window)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Intention to Treat						
Treatment Assigned	0.033	-0.009	0.113	0.112	-0.060	-0.091
	(0.043)	(0.068)	(0.120)	(0.213)	(0.029)**	(0.052)*
Pre-grade (std)	0.004	0.004	0.298	0.295	0.019	0.018
	(0.024)	(0.024)	(0.098)***	(0.098)***	(0.021)	(0.021)
Treatment Assigned x Pre-grade (std)	0.100	0.103	0.297	0.288	-0.042	-0.041
	(0.032)***	(0.033)***	(0.129)**	(0.129)**	(0.028)	(0.027)
Parents low educ		0.017		-0.284		-0.050
		(0.066)		(0.167)*		(0.040)
Treatment Assigned x Parents low educ		0.058		0.015		0.046
		(0.079)		(0.223)		(0.060)
Panel B: Treatment on the Treated						
Treated	0.030	-0.017	0.107	0.100	-0.063	-0.096
	(0.047)	(0.073)	(0.129)	(0.229)	(0.031)**	(0.056)*
Pre-grade (std)	0.004	0.004	0.297	0.295	0.019	0.018
	(0.023)	(0.024)	(0.096)***	(0.096)***	(0.020)	(0.021)
Treated x Pre-grade (std)	0.107	0.111	0.319	0.310	-0.043	-0.042
	(0.035)***	(0.035)***	(0.137)**	(0.139)**	(0.029)	(0.029)
Parents low educ		0.017		-0.283		-0.050
		(0.064)		(0.164)*		(0.039)
Treated x Parents low educ		0.065		0.024		0.049
		(0.084)		(0.238)		(0.064)
OLS w pre-specified controls	✓	✓	✓	✓	✓	✓
Control Mean	0.43		13.48		0.23	
Observations	1,245		1,265		1,244	

Notes: Panel A: OLS estimates. Treatment Assigned = The student's school received the intervention. Panel B: 2SLS estimates. Treatment (participation in the intervention) instrumented with assigned treatment status. Standard errors (in parentheses) are clustered at the school level. Student (gender, age, pre-intervention grades in main subjects (total, std.), pre-intervention absences) and school controls (class size, Child's Dream partnership) included as pre-specified. */**/*** denote significance levels at 10/5/1 percent respectively.

Table A.13: Impact of the Intervention by School Performance vs. Parental Education (2)

	Attended final exam		Final exam grade (std.)		High school enrollment	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Intention to Treat						
Treatment Assigned	0.011	0.043	0.087	0.058	0.025	0.004
	(0.025)	(0.035)	(0.147)	(0.152)	(0.035)	(0.043)
Pre-grade (std)	0.040	0.041	0.533	0.532	0.067	0.066
	(0.014)***	(0.014)***	(0.105)***	(0.105)***	(0.021)***	(0.020)***
Treatment Assigned x Pre-grade (std)	-0.010	-0.011	0.313	0.313	0.052	0.052
	(0.018)	(0.018)	(0.104)***	(0.103)***	(0.026)*	(0.026)**
Parents low educ		0.024		-0.070		-0.073
		(0.030)		(0.074)		(0.038)*
Treatment Assigned x Parents low educ		-0.045		0.044		0.032
		(0.036)		(0.093)		(0.050)
Panel B: Treatment on the Treated						
Treated	0.013	0.047	0.083	0.044	0.024	0.002
	(0.027)	(0.038)	(0.155)	(0.163)	(0.037)	(0.045)
Pre-grade (std)	0.040	0.041	0.533	0.532	0.067	0.066
	(0.013)***	(0.013)***	(0.103)***	(0.103)***	(0.020)***	(0.020)***
Treated x Pre-grade (std)	-0.011	-0.013	0.331	0.332	0.056	0.056
	(0.020)	(0.020)	(0.110)***	(0.109)***	(0.028)**	(0.028)**
Parents low educ		0.024		-0.070		-0.073
		(0.030)		(0.072)		(0.037)*
Treated x Parents low educ		-0.049		0.057		0.035
		(0.038)		(0.095)		(0.052)
OLS w pre-specified controls	✓	✓	✓	✓	✓	✓
Control Mean	0.92		0.12		0.82	
Observations	1,275		1,167		1,261	

Notes: Panel A: OLS estimates. Treatment Assigned = The student's school received the intervention. Panel B: 2SLS estimates. Treatment (participation in the intervention) instrumented with assigned treatment status. Standard errors (in parentheses) are clustered at the school level. Student (gender, age, pre-intervention grades in main subjects (total, std.), pre-intervention absences) and school controls (class size, Child's Dream partnership) included as pre-specified. */**/*** denote significance levels at 10/5/1 percent respectively.

Table A.14: Impact of the Intervention by School Performance vs. Parental Education (3)

	Started grade 11		Attended grd 11 midterm exam		Passed grd 11 midterm exam	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Intention to Treat						
Treatment Assigned	0.027	-0.005	0.028	0.004	0.051	0.050
	(0.040)	(0.050)	(0.038)	(0.057)	(0.042)	(0.057)
Pre-grade (std)	0.095	0.093	0.107	0.105	0.152	0.152
	(0.023)***	(0.023)***	(0.022)***	(0.021)***	(0.024)***	(0.024)***
Treatment Assigned x Pre-grade (std)	0.065	0.065	0.077	0.077	0.055	0.054
	(0.029)**	(0.028)**	(0.028)***	(0.028)***	(0.029)*	(0.029)*
Parents low educ		-0.086		-0.084		-0.037
		(0.048)*		(0.046)*		(0.050)
Treatment Assigned x Parents low educ		0.050		0.038		0.004
		(0.057)		(0.065)		(0.056)
Panel B: Treatment on the Treated						
Treated	0.027	-0.009	0.026	-0.001	0.053	0.050
	(0.042)	(0.053)	(0.041)	(0.059)	(0.045)	(0.060)
Pre-grade (std)	0.094	0.093	0.106	0.105	0.152	0.151
	(0.023)***	(0.022)***	(0.022)***	(0.021)***	(0.024)***	(0.024)***
Treated x Pre-grade (std)	0.070	0.070	0.083	0.083	0.058	0.057
	(0.031)**	(0.030)**	(0.030)***	(0.030)***	(0.031)*	(0.031)*
Parents low educ		-0.086		-0.084		-0.037
		(0.047)*		(0.045)*		(0.049)
Treated x Parents low educ		0.055		0.042		0.005
		(0.059)		(0.068)		(0.058)
OLS w pre-specified controls	✓	✓	✓	✓	✓	✓
Control Mean	0.72		0.68		0.55	
Observations	1,261		1,261		1,261	

Notes: Panel A: OLS estimates. Treatment Assigned = The student's school received the intervention. Panel B: 2SLS estimates. Treatment (participation in the intervention) instrumented with assigned treatment status. Standard errors (in parentheses) are clustered at the school level. Student (gender, age, pre-intervention grades in main subjects (total, std.), pre-intervention absences) and school controls (class size, Child's Dream partnership) included as pre-specified. */**/*** denote significance levels at 10/5/1 percent respectively.

Table A.15: Impact of the Intervention by School Performance vs. Parental Job Loss (1)

	Studied during lock down		Educ aspirations (years schooling)		Occup aspirations (outside ref window)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Intention to Treat						
Treatment Assigned	0.038	0.040	0.102	0.072	-0.060	-0.048
	(0.040)	(0.046)	(0.124)	(0.140)	(0.029)**	(0.030)
Pre-grade (std)	0.004	0.006	0.300	0.299	0.014	0.016
	(0.024)	(0.025)	(0.096)***	(0.096)***	(0.021)	(0.021)
Treatment Assigned x Pre-grade (std)	0.099	0.098	0.264	0.264	-0.039	-0.042
	(0.032)***	(0.033)***	(0.125)**	(0.124)**	(0.027)	(0.027)
Parents lost job (COVID-19)		0.046		-0.022		0.068
		(0.060)		(0.187)		(0.051)
Treatment Assigned x Parents lost job		-0.011		0.179		-0.074
		(0.093)		(0.270)		(0.071)
Panel B: Treatment on the Treated						
Treated	0.036	0.037	0.096	0.064	-0.062	-0.050
	(0.044)	(0.050)	(0.134)	(0.151)	(0.031)**	(0.032)
Pre-grade (std)	0.004	0.006	0.299	0.299	0.014	0.017
	(0.024)	(0.025)	(0.094)***	(0.094)***	(0.021)	(0.021)
Treated x Pre-grade (std)	0.106	0.105	0.285	0.285	-0.040	-0.043
	(0.035)***	(0.035)***	(0.135)**	(0.134)**	(0.029)	(0.029)
Parents lost job (COVID-19)		0.046		-0.021		0.067
		(0.059)		(0.183)		(0.050)
Treated x Parents lost job		-0.012		0.193		-0.079
		(0.098)		(0.288)		(0.074)
OLS w pre-specified controls	✓	✓	✓	✓	✓	✓
Control Mean	0.42		13.45		0.23	
Observations	1,296		1,317		1,291	

Notes: Panel A: OLS estimates. Treatment Assigned = The student's school received the intervention. Panel B: 2SLS estimates. Treatment (participation in the intervention) instrumented with assigned treatment status. Standard errors (in parentheses) are clustered at the school level. Student (gender, age, pre-intervention grades in main subjects (total, std.), pre-intervention absences) and school controls (class size, Child's Dream partnership) included as pre-specified. */**/*** denote significance levels at 10/5/1 percent respectively.

Table A.16: Impact of the Intervention by School Performance vs. Parental Job Loss (2)

	Attended final exam		Final exam grade (std.)		High school enrollment	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Intention to Treat						
Treatment Assigned	0.012	0.003	0.100	0.093	0.030	0.020
	(0.025)	(0.024)	(0.147)	(0.155)	(0.034)	(0.036)
Pre-grade (std)	0.038	0.039	0.536	0.536	0.073	0.070
	(0.014)***	(0.014)***	(0.104)***	(0.106)***	(0.022)***	(0.023)***
Treatment Assigned x Pre-grade (std)	-0.004	-0.004	0.300	0.301	0.048	0.050
	(0.017)	(0.017)	(0.104)***	(0.105)***	(0.026)*	(0.026)*
Parents lost job (COVID-19)		0.000		-0.011		-0.070
		(0.028)		(0.098)		(0.046)
Treatment Assigned x Parents lost job		0.050		0.040		0.065
		(0.034)		(0.126)		(0.065)
Panel B: Treatment on the Treated						
Treated	0.013	0.004	0.094	0.088	0.031	0.020
	(0.027)	(0.027)	(0.156)	(0.164)	(0.037)	(0.039)
Pre-grade (std)	0.038	0.038	0.536	0.536	0.073	0.070
	(0.013)***	(0.013)***	(0.103)***	(0.104)***	(0.022)***	(0.022)***
Treated x Pre-grade (std)	-0.005	-0.005	0.318	0.319	0.051	0.053
	(0.019)	(0.019)	(0.110)***	(0.111)***	(0.028)*	(0.028)*
Parents lost job (COVID-19)		0.000		-0.011		-0.070
		(0.028)		(0.096)		(0.046)
Treated x Parents lost job		0.054		0.038		0.070
		(0.036)		(0.133)		(0.068)
OLS w pre-specified controls	✓	✓	✓	✓	✓	✓
Control Mean	0.92		0.11		0.81	
Observations	1,327		1,212		1,313	

Notes: Panel A: OLS estimates. Treatment Assigned = The student's school received the intervention. Panel B: 2SLS estimates. Treatment (participation in the intervention) instrumented with assigned treatment status. Standard errors (in parentheses) are clustered at the school level. Student (gender, age, pre-intervention grades in main subjects (total, std.), pre-intervention absences) and school controls (class size, Child's Dream partnership) included as pre-specified. */**/*** denote significance levels at 10/5/1 percent respectively.

Table A.17: Impact of the Intervention by School Performance vs. Parental Job Loss (3)

	Started grd 11		Attended grd 11 midterm exam		Passed grd 11 midterm exam	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Intention to Treat						
Treatment Assigned	0.029	0.036	0.032	0.042	0.058	0.068
	(0.040)	(0.047)	(0.039)	(0.054)	(0.042)	(0.050)
Pre-grade (std)	0.098	0.090	0.106	0.102	0.150	0.149
	(0.023)***	(0.023)***	(0.022)***	(0.022)***	(0.025)***	(0.026)***
Treatment Assigned x Pre-grade (std)	0.063	0.068	0.078	0.080	0.058	0.057
	(0.028)**	(0.028)**	(0.027)***	(0.028)***	(0.030)*	(0.030)*
Parents lost job (COVID-19)		-0.105		-0.116		-0.069
		(0.051)**		(0.048)**		(0.056)
Treatment Assigned x Parents lost job		0.110		0.119		0.039
		(0.071)		(0.065)*		(0.082)
Panel B: Treatment on the Treated						
Treated	0.028	0.011	0.031	0.012	0.060	0.055
	(0.043)	(0.045)	(0.041)	(0.044)	(0.045)	(0.049)
Pre-grade (std)	0.098	0.094	0.106	0.101	0.150	0.147
	(0.023)***	(0.023)***	(0.022)***	(0.021)***	(0.025)***	(0.026)***
Treated x Pre-grade (std)	0.068	0.072	0.084	0.089	0.060	0.063
	(0.030)**	(0.030)**	(0.029)***	(0.029)***	(0.032)*	(0.033)*
Parents lost job (COVID-19)		-0.098		-0.107		-0.062
		(0.050)*		(0.048)**		(0.056)
Treated x Parents lost job		0.108		0.116		0.038
		(0.076)		(0.070)*		(0.089)
OLS w pre-specified controls	✓	✓	✓	✓	✓	✓
Control Mean	0.71		0.67		0.54	
Observations	1,313		1,313		1,313	

*Notes:*Panel A: OLS estimates. Treatment Assigned = The student's school received the intervention. Panel B: 2SLS estimates. Treatment (participation in the intervention) instrumented with assigned treatment status. Standard errors (in parentheses) are clustered at the school level. Student (gender, age, pre-intervention grades in main subjects (total, std.), pre-intervention absences) and school controls (class size, Child's Dream partnership) included as pre-specified. */**/*** denote significance levels at 10/5/1 percent respectively.

Table A.18: Information Seeking in CET

	Reading time of jobs requiring at least					
	lower secondary		high school		university	
	(1)	(2)	(3)	(4)	(5)	(6)
Pre-grade (std)	-0.034	-0.044	0.004	-0.000	0.029	0.044
	(0.013)***	(0.014)***	(0.008)	(0.009)	(0.013)**	(0.015)***
Observations	601	601	601	601	601	601
Mean	0.3921	0.3921	0.1906	0.1906	0.4173	0.4173

Notes: OLS estimates. Robust standard errors are depicted in parentheses. Reading time measured proportional to total reading time. Each column controls for students' assignment to treatment arms. Even columns additionally control for students' gender, age, distance to high school, and school fixed effects. */**/*** denote significance levels at 10/5/1 percent respectively.

Table A.19: Estimated Monthly Costs of Attending High School, at Baseline

	Total costs		Extra classes		Transportation		Material	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre-grade (std)	83.944	98.743	2.689	3.525	3.837	2.626	3.843	2.984
	(12.862)***	(14.743)***	(0.528)***	(0.577)***	(1.936)**	(2.339)	(0.825)***	(0.914)***
Observations	777		777		777		777	
Mean	284.47		15.57		34.74		22.14	

Notes: OLS Estimates. Robust standard errors are depicted in parentheses. Cost estimates in US-$, winsorized at the 95^{th} percentile, and centered around the true value. Each regression controls for students' assignment to treatment arms. Even columns additionally control for gender, age, distance to high school, and school fixed effects. */**/*** denote significance levels at 10/5/1 percent respectively.

Table A.20: Estimated Monthly Costs of Attending High School, at Follow-up

	Total Costs				Extra classes			
	estimated		*(estimated − true)*		*estimated*		*(estimated − true)*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Intention to Treat								
Treatment Assigned	-1.742	-1.448	0.099	0.551	0.439	0.404	0.589	0.538
	(2.501)	(2.355)	(3.346)	(3.271)	(1.021)	(0.991)	(0.986)	(0.965)
Pre-grade (std)	-0.706	0.594	-0.770	1.236	0.220	0.138	0.215	0.092
	(1.321)	(2.137)	(1.411)	(2.344)	(0.621)	(0.939)	(0.620)	(0.928)
Treatment Assigned x Pre-grade (std)		-2.392		-3.689		0.140		0.208
		(2.326)		(2.529)		(1.007)		(0.993)
Panel B: Treatment on the Treated								
Treated	-1.882	-1.433	0.107	0.820	0.474	0.433	0.637	0.574
	(2.651)	(2.497)	(3.545)	(3.529)	(1.079)	(1.062)	(1.042)	(1.036)
Pre-grade (std)	-0.665	0.594	-0.773	1.236	0.208	0.137	0.199	0.091
	(1.300)	(2.096)	(1.358)	(2.298)	(0.604)	(0.915)	(0.603)	(0.905)
Treated x Pre-grade (std)		-2.547		-4.058		0.135		0.203
		(2.474)		(2.739)		(1.081)		(1.067)
OLS w pre-specified controls	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	69.74		20.12		25.29		-2.85	
Observations	1,178		1,177		729		729	

Notes: Panel A: OLS estimates. Treatment Assigned = The student's school received the intervention. Panel B: 2SLS estimates. Treatment (participation in the intervention) instrumented with assigned treatment status. Standard errors (in parentheses) are clustered at the school level. Student (gender, age, pre-intervention grades in main subjects (total, std.), pre-intervention absences) and school controls (class size, Child's Dream partnership) included as pre-specified. */**/*** denote significance levels at 10/5/1 percent respectively.

B Online Appendix: Details on the Intervention

B.1 Interest Exploration Tool

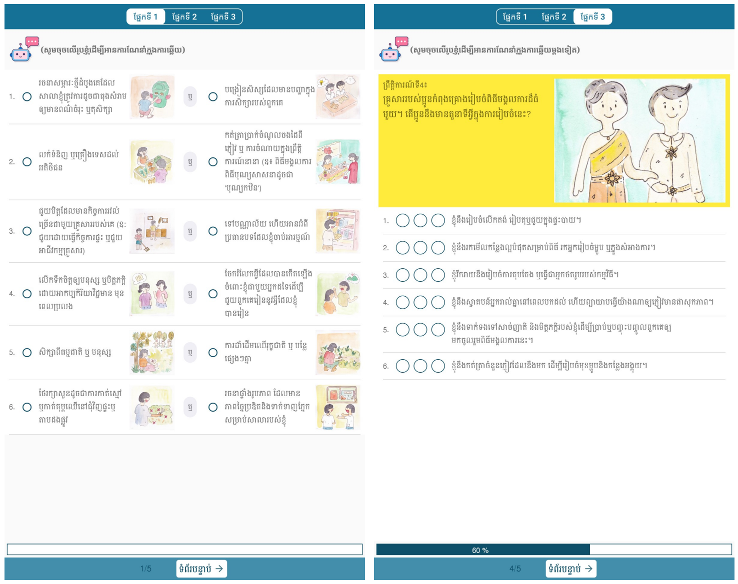
The interest exploration tool (IET) builds on Holland's theory of vocational interest (Holland, 1997), and is designed to help students reflect on their personal interests and to reveal to students which personality types they display. In his hexagonal model, Holland (1997) identifies six personality types, namely realistic, investigative, artistic, social, enterprising, and conventional (RIASEC), of which he expects up to three to be most strongly pronounced within an individual. The theory of vocational interest posits that individuals working in professions that match their personality type(s) are more satisfied with their work. Ample empirical evidence suggests that this is indeed the case (CITE).

The implementation of the personality test in our experiment was done by combining answers across three different tests. The format of the first test is based on Athanasou (2000, 2007), and is designed such that students are presented with opposing statements of which they are expected to pick one, each statement representing one personality type. A total of 30 opposing statements are included (two for each combination of personality types). The second test follows the most widely used, and internationally validated (Morgan and de Bruin, 2018; Aljojo and Saifuddin, 2017; Meireles and Primi, 2015, see e.g.), implementation of Holland's personality test and consists of a list of 42 statements (seven per personality type) to which students can agree or not . This test is retrieved online from a cooperation between Hawaii Department of Education and the Occupation Information Network (O*NET). The third test was created by the researchers. It consists of descriptions of five different situations, in each of which students are asked to select their preferred activity concerning that situation. For example, one of such situations is a wedding, and students are asked whether they would rather choose to help organizing the guest list, or prefer preparing a short performance, and so forth. Figure B.1 provides examples for the design of the first and third test. All statements in tests one and three are adapted to our target population, meaning that they depict specific activities to which adolescents in rural Cambodia are used to or have access to. Tests one and three are also complemented by small pictures drawn by a local artist that are intended to contribute to the understanding of the statements.

The testing format varies across the three tests to ensure that the outcome of the tests does not depend on a specific testing format. Research assistants guided students through all three tests, but students worked independently once they understood what to do. We implemented breaks between the three tests such that all students were able to follow instructions to each of the tests before getting started. If questions arose, students could ask them directly or select a pop-up window with written information about the testing method.

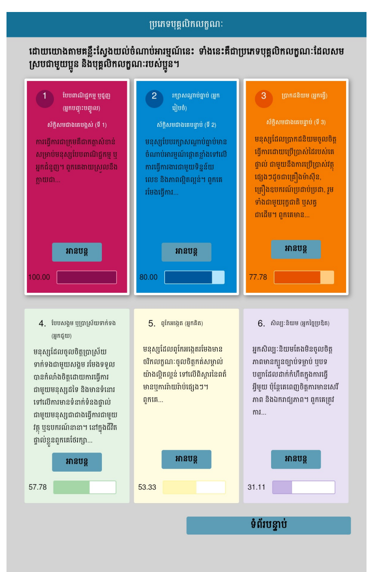
After the tests are completed, the app reveals the ordering of personality types corresponding to a student's answers in the tests, with the strongest personality type being shown first. In addition to the ordering, students are revealed a personalized score per personality type representing the relative match with each type. The score of the strongest personality type is scaled to 100, and

Figure B.1: Examples of Tests 1 and 3



all other scores for the remaining types are expressed relative to the main type, and depicted in bar format to visualize the degree of match between the student and each of the six personality types. The three personality types with the highest score are highlighted in the first row while the remaining three types are shown in the second row in less vivid colors (see Figure B.2 for an example of a personalized result). It was possible for students to click on each personality type and read a brief descriptions about the main personality traits and interests associated with a specific personality type. The description of these types was adapted from The Delaware Departement of Labor (2019).

Figure B.2: Component 2: Result of the Personality Tests

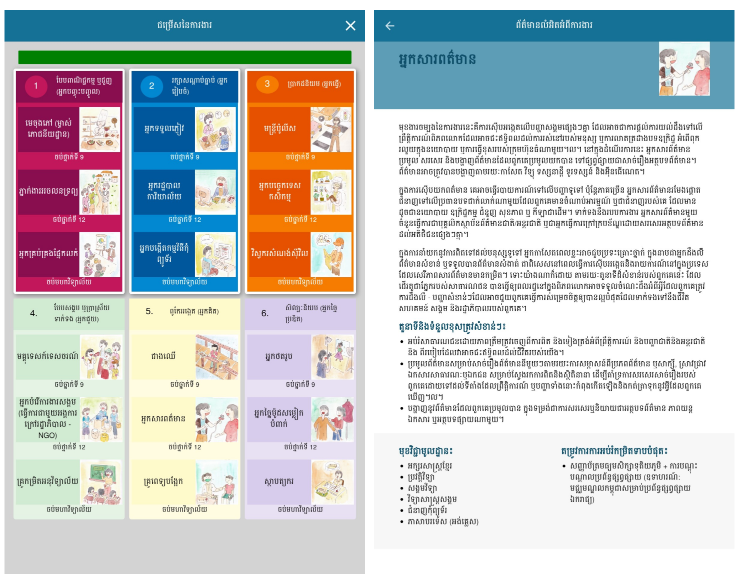


B.2 Career Exploration Tool

In the Career Exploration Tool (CET), then, students are presented with a list of 18 possible occupations, that were chosen because of their relevance in the context. Each personality type is linked to three of the occupations listed in the app. The linking is based on the O*NET list (see above).

The display of occupations is similar to the display of personality types in the IET: the first row reveals occupations that correspond to the three strongest personality types and the second row shows all remaining occupations in less vivid colors. All occupations are accompanied by pictures drawn by a local artist. Students can click on the icon of any job to access more information about each of these occupations. In particular, the app provides a detailed description of the main tasks and responsibilities associated with each occupation, its societal value, and the required educational level. Students are given 17 minutes in total to read all descriptions they want to, but they can also log out sooner. Figure B.3 shows one example of the ordered display of all 18 jobs plus of one job description. At the end of the intervention day, each students receives a leaflet with all 18 occupations and their descriptions, that they can take home.

Figure B.3: Component 4: Overview of Job List (Left) and an Individual Example of a Job Description (Right)



To provide students with a balanced picture of career opportunities, the three occupations per personality type are chosen so that each require a different level of formal education, out of the three levels that these students might achieve (unless they drop out during grade 9): grade 9 diploma, grade 12 diploma, or a university degree. Table B.1 gives an overview of all 18 jobs listed in the app, and their respective allocation to personality types and minimum educational requirement.

Table B.1: Job Categorization in the CET

	Required educational degree		
Type	grade 9	grade 12	university
Realistic	police officer	agricultural technician	civil engineer
Investigative	carpenter	journalist	general practitioner
Artistic	photographer	clothes designer	architect
Social	tour guide	social worker	sec.-level teacher
Enterprising	chef	real-estate agent	sales manager
Conventional	receptionist	office administrator	software developer

Notes: Each occupation is assigned to one of the six personality types and to one of three educational degrees. The former categorization relies on the classification by the National Employment Agency of Cambodia, the latter is categorized by the research team.

B.3 Information Session

The information session is organized in a small group setting, with two research assistants typically interacting with 15-30 students per session. This information session in conducted in the students' classrooms, , and covers (i) important facts about the Cambodian education system in general, (ii) detailed information about high schools and vocational schools that are located close to the school and to which students can transition after completing grade 9, and (iii) scholarships to which students could apply. Students can ask questions at any time during the presentation and research assistants are encouraged to engage students in discussions about the session's content.

Each group starts off with a set of easy-to-answer questions about their own school (name of the school, inauguration, number of students and teachers). This introductory round is followed by a discussion of a poster which gives an overview of the complete Cambodian education system (see Figure B.4) from primary school up to university and distinguishes between two paths after lower secondary school: either vocational school or upper secondary school (=high school). The poster also highlights which kind of professions one can pursue depending on the educational degree.

The focus is then set on high school and vocational school and they are presented subsequently. Both parts include information on the number of students, distance to the closest school and its associated time and travel costs, information about admission, living costs and school expenses, and available scholarships. The overall structure of the information stays the same across schools but is tailored to the location of the school. Figure B.5 provides an example of how information is displayed at schools. Information in green refers to high school and yellow to vocational school (cards in blue are related to the questions about the students' own lower secondary school). Teachers also receive two posters with a summary of the information tailored to each school and they are asked to put it up somewhere visible for the students.

Figure B.4: Poster Demonstrating Cambodian Education System

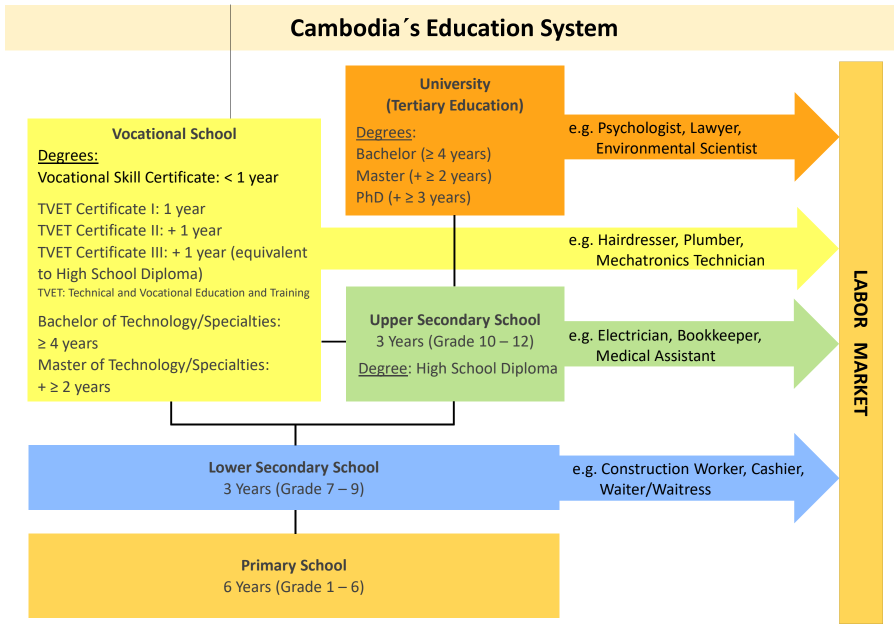
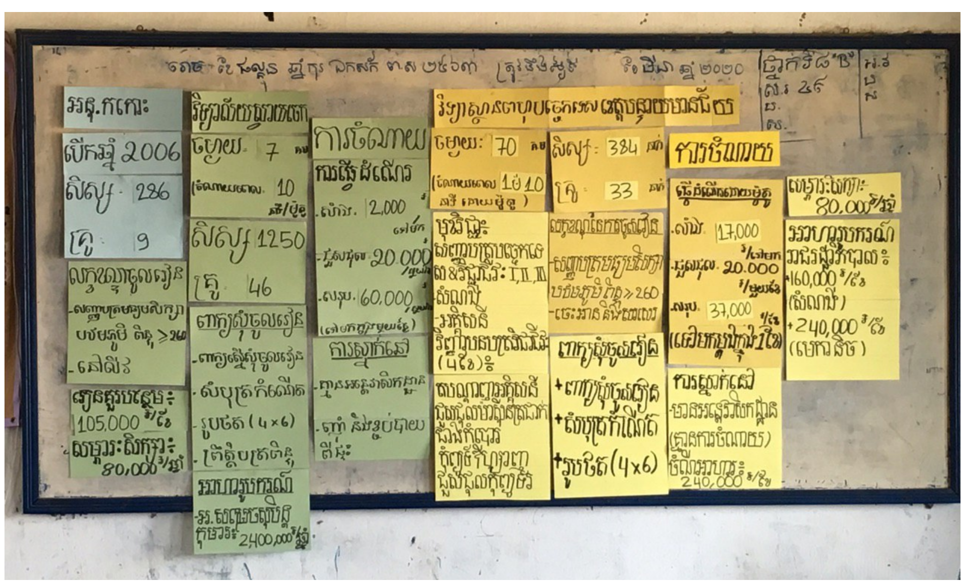


Figure B.5: Example of Display of Information about High and Vocational School



B.4 Randomization within schools

In treatment schools, we randomly allocate students into one of three treatment arms in treatment schools: the main treatment arm (A1), placebo arm (A2), and information-only (A3), with the respective chances of 2:2:1. While students in A1 participate in all three parts of the intervention, students in A2 only receive the job information and attend the school information session, and students in A3 participate in the information session only. The outline of the intervention for each of these groups is described in Table B.2.

Table B.2: Outline of Intervention in Treatment Schools

	A1	A2	A3
Baseline survey	Background information on student('s family); beliefs about costs of attending high school		
IET	TREATMENT (a) three tests on personal interests and preferences (b) personality types	PLACEBO (a) three tests on gender attitudes and climate change (b) —	NO TOOL game outside
CET	(a) list of 18 jobs; students indicate most interesting ones(s) (b) list of 18 jobs (ordered by personality types), students can click on each job to read more detail	(a) list of 18 jobs; students indicate most interesting one(s) (b) list of 18 jobs (ordered randomly), students can click on each job to read more detail	game outside
Midline survey	Perceived constraints of attending high school; quizz: interpreting graph with costs of education		
SCHOOL INFORMATION SESSION	Detailed information on high schools and vocational training, including costs involved and available scholarships		
Endline survey	Questions capturing information retention; aspirations and expectations on education and career path		

Within the treatment schools, randomization into the different treatment arms was unfortunately not successful (see Table B.3). Students in the treatment arm A3, *i.e.*, students that participated in the information session only, are more likely to be female, were performing overall better and were less likely to be absent prior to the intervention as compared to students in the treatment arms A2 or A1. It is not clear why randomization was unsuccessful. Neither students nor research assistants were able to manipulate students' treatment status. Participants had blindly drawn from a box the participant badge with a number that determined treatment status, the number was directly recorded and could not be changed during the workshop. Likely it was just bad luck. In the following, we will therefore refrain from any analyses comparing the different treatment arms, and instead investigate the impact of the intervention overall.

Table B.3: Balance Table Experiment

Variable	(1) Mean A1	(2) Mean A2	(3) Mean A3	(4) A2 vs. A1	(5) A3 vs. A1	(6) A2 vs. A3
Female	0.53	0.54	0.66	-0.01	-0.13***	0.12***
	(0.50)	(0.50)	(0.48)	(0.90)	(0.01)	(0.01)
Age	15.11	15.05	15.04	0.06	0.07	-0.01
	(1.32)	(1.31)	(1.36)	(0.55)	(0.58)	(0.93)
Distance to school (km)	3.98	3.99	4.20	-0.01	-0.21	0.20
	(3.86)	(4.02)	(4.29)	(0.97)	(0.60)	(0.63)
Distance to district town (km)	9.96	9.74	9.73	0.21	0.23	-0.01
	(6.47)	(6.44)	(6.47)	(0.68)	(0.72)	(0.98)
Distance to high school (km)	9.33	9.27	9.15	0.06	0.18	-0.12
	(6.59)	(6.36)	(6.42)	(0.91)	(0.78)	(0.85)
Pre-grade, main subjects (standardized)	-0.31	-0.15	0.06	-0.16**	-0.37***	0.21**
	(0.90)	(0.96)	(1.02)	(0.03)	(0.00)	(0.04)
Avg absence (Dec&Jan)	1.63	1.58	1.28	0.05	0.35**	-0.30*
	(1.88)	(1.96)	(1.45)	(0.75)	(0.03)	(0.06)
Observations	315	312	150	627	465	462

Notes: (1)-(3): standard errors in parentheses (clustered at the school level); (4) & (5): p-values in parentheses. */**/*** denote significance levels at 10/5/1 percent respectively.

C Online appendix: Survey Weighting

Although we managed to reach a considerable share of the students calling them during school closure, Table C.1 reveals that female and better performing students were easier to reach via phone. We therefore construct survey weights to make the sample of interviewed students within the phone survey more comparable to the full sample.

Table C.1: Balance before Weighting

Variable	(1) Mean Interviewed	(2) Mean All	(3) Difference
Female	0.57	0.54	0.04**
	(0.49)	(0.50)	(0.04)
Age	15.03	15.05	-0.03
	(1.28)	(1.32)	(0.57)
Distance to school (km)	3.53	3.55	-0.02
	(3.74)	(3.78)	(0.88)
Distance to district town (km)	11.38	11.33	0.05
	(7.51)	(7.44)	(0.86)
Distance to high school (km)	9.78	9.68	0.09
	(6.84)	(6.80)	(0.71)
Pre-grade, main subjects (standardized)	0.09	0.01	0.08**
	(0.99)	(0.98)	(0.03)
Avg absence (Dec&Jan)	1.45	1.52	-0.07
	(2.00)	(2.02)	(0.37)
Teacher: Age	32.36	32.42	-0.06
	(6.43)	(6.53)	(0.79)
Teacher: Years of Experience	9.28	9.30	-0.02
	(6.01)	(6.10)	(0.92)
Teacher: Female	0.32	0.33	-0.00
	(0.47)	(0.47)	(0.90)
Teacher: Has University Degree	0.51	0.51	0.00
	(0.50)	(0.50)	(0.88)
Teacher: Log Distance to School (km)	1.58	1.57	0.01
	(1.22)	(1.22)	(0.82)
Observations	1,327	1,715	3,042

Notes: (1) and (2): standard errors in parentheses (clustered at the school level); (3): p-values in parentheses. */**/*** denote significance levels at 10/5/1 percent respectively.

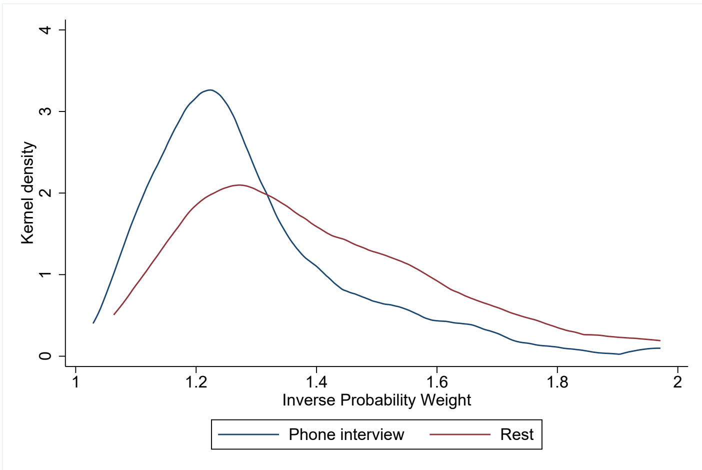
The weights are estimated by a logistic regression which includes student, school and teacher characteristics. The regression output is shown in Table C.2. The distribution of the resulting weights as inverse of its predicted values can be seen in Figure C.1 for both phone survey participants and remaining students.

Table C.2: Determinants of Participation in Phone Survey (Logit)

	(1)	
Female=1	0.624***	(0.182)
Age	-0.090	(0.069)
Distance to school (km)	-0.018	(0.026)
Distance to district town (km)	0.006	(0.020)
Distance to high school (km)	0.037	(0.024)
Pre-grade, main subjects (standardized)	0.501***	(0.134)
Pre-grade, main subjects (standardized) × Pre-grade, main subjects (standardized)	-0.022	(0.083)
Female=1 × Pre-grade, main subjects (standardized)	-0.350*	(0.199)
Treated students	-0.622	(1.425)
Female=1 × Treated students=1	-0.156	(0.271)
Treated students=1 × Age	0.091	(0.093)
Treated students=1 × Distance to school (km)	0.013	(0.035)
Treated students=1 × Distance to district town (km)	-0.009	(0.036)
Treated students=1 × Distance to high school (km)	-0.051	(0.040)
Treated students=1 × Pre-grade, main subjects (standardized)	0.166	(0.213)
Treated students=1 × Pre-grade, main subjects (standardized) × Pre-grade, main subjects (standardized)	0.128	(0.129)
Female=1 × Treated students=1 × Pre-grade, main subjects (standardized)	0.207	(0.291)
Teacher: Female	0.072	(0.174)
Teacher: Age	-0.030	(0.032)
Teacher: Years of Experience	0.007	(0.031)
Teacher: Has University Degree	0.080	(0.167)
Teacher: Log Distance to School (km)	-0.192**	(0.094)
group(SchoolDistrict)=1	0.000	(.)
group(SchoolDistrict)=2	0.086	(0.397)
group(SchoolDistrict)=3	0.036	(0.278)
group(SchoolDistrict)=4	0.662	(0.428)
group(SchoolDistrict)=5	0.640*	(0.386)
group(SchoolDistrict)=6	-0.255	(0.373)
group(SchoolDistrict)=7	-0.010	(0.283)
group(SchoolDistrict)=8	0.424	(0.366)
Partnership with Child's Dream=1	-0.084	(0.260)
Observations	1715	
Pseudo R^2	0.053	

Notes: Standard errors in parentheses (clustered at the school level). */**/*** denote significance levels at 10/5/1 percent respectively.

Figure C.1: Distribution of Inverse Probability Weights



Notes: The graph shows density of the calculated inverse probability weights for both students participating in the phone interview and non-participants.

Table C.3 reports the student characteristics after survey weights are applied. There are no more significant differences between the sample of interviewed students via phone and the full sample.

Table C.3: Balance after Weighting

Variable	(1) Mean Interviewed	(2) Mean All	(3) Difference
Female	0.54	0.54	0.00
	(0.50)	(0.50)	(1.00)
Age	15.05	15.05	0.00
	(1.29)	(1.32)	(0.98)
Distance to school (km)	3.56	3.55	0.00
	(3.76)	(3.78)	(0.99)
Distance to district town (km)	11.35	11.33	0.02
	(7.46)	(7.44)	(0.95)
Distance to high school (km)	9.71	9.68	0.03
	(6.77)	(6.80)	(0.91)
Pre-grade, main subjects (standardized)	0.01	0.01	0.00
	(0.98)	(0.98)	(0.98)
Avg absence (Dec&Jan)	1.50	1.52	-0.02
	(2.04)	(2.02)	(0.83)
Teacher: Age	32.41	32.42	-0.01
	(6.44)	(6.53)	(0.96)
Teacher: Years of Experience	9.30	9.30	-0.00
	(6.03)	(6.10)	(0.99)
Teacher: Female	0.32	0.33	-0.00
	(0.47)	(0.47)	(0.97)
Teacher: Has University Degree	0.51	0.51	0.00
	(0.50)	(0.50)	(0.97)
Teacher: Log Distance to School (km)	1.57	1.57	0.00
	(1.22)	(1.22)	(0.96)
Observations	1,327	1,715	3,042

Notes: (1) and (2): standard errors in parentheses (clustered at the school level); (3): p-values in parentheses. */**/*** denote significance levels at 10/5/1 percent respectively.