Occupational Aspirations and Investments in Education: Experimental Evidence from Cambodia *

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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 1,715 students in rural Cambodia and show that a half-day career-guidance 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 this treatment effect heterogeneity by baseline performance.

Keywords: Aspirations; Career guidance; Education; Field Experiment
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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 lack the knowledge and experience to properly support children in their educational decisions, and teachers lack the incentives and the resources to guide their students individually. Without adequate guidance, students from low-income contexts may not only have insufficient knowledge about the available educational pathways, but also fail to develop aspirations that enable them to pursue different educational and career paths than their parents, and escape the self-perpetuating cycles of poverty (Mani and Riley, 2021; van der Weide et al., 2024).

Against this background, a number of recent policy interventions have aimed to raise aspirations among adolescents, often by featuring role models.[1] Such interventions 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 deter 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). This is problematic as too high aspirations can cause frustration once students realize that their goals are unattainable which in turn can lead to insufficient educational investments (Genicot and Ray, 2020). This raises the question of whether interventions should in fact aim at expanding the aspirations window, *i.e.*, the set of outcomes that are known and from which aspirations can be drawn (Genicot and Ray, 2017). This could lead to more diversity in students' aspirations, and may be particularly important in the presence of heterogeneities in student abilities, as a constrained aspirations window can force students to 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 career-guidance 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

[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). For a recent review of various role-model interventions in education, see Serra (2022).

[2] The importance of aligning aspirations to individual abilities has been highlighted *inter alia* in the context of sports (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.

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 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 career-guidance workshop was conducted with students in Grade 9 —the 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 a 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 immensely 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 subsequent three 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 career-guidance workshop had positive effects on educational investments, which started to materialize about eight months after the intervention took place and persist until high school completion. At the time of the phone survey, we find no statistically significant treatment effects on students' educational or occupational aspirations, nor on their confidence in reaching these aspirations. Students in treatment schools were also not more likely to study during the first lockdown period than their peers in control schools. However, treated students 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 (0.21 SD) and were by about 5.8pp (7.7%) more likely to enroll in high school, 6.1pp (9.5%) more likely to progress to Grade 11 one year later, 5.6pp (9.2%) more likely to attend the midterm exam of Grade 11, and 3.9pp (7.0%) more likely to complete high school roughly 3.5 years after the intervention took place.

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 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 and occupational aspirations, but were no less confident in reaching their aspirations. Treated low-performers were also not less likely to attend the final exam of Grade 9. However, they performed worse in the exam conditional on attending. 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. 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 higher educational and occupational aspirations, and were (weakly) more confident in reaching their aspired education and occupation. They were more likely to attend the final exam of Grade 9 and performed better in that exam. High-performing treated students also kept outperforming their peers from control schools throughout high school and are driving the positive average treatment effects observed in the medium run.

The small number of schools in our sample, as well as imbalances in baseline grades are potential concerns to identification. We therefore perform a wealth of robustness checks. First, we show that average treatment effects and heterogeneities by baseline grades are robust to randomization inference, as well as to corrections for attrition. Second, we trim outlier schools (below 10th and above 90th percentile) in average baseline grades from the sample. This procedure achieves balance in key variables, and if anything increases the significance of treatment effects and of the heterogeneities by grades. Finally, we show that the treatment effect heterogeneities are not driven by parental characteristics nor by school characteristics, and that the differential association between baseline grades and subsequent outcomes is non-existent before the intervention took place.

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 (and occupational) 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 who endogenously chooses an effort-aspiration pair that is aligned with their individual abilities and assume that the agent formulates their *edu-*

cational aspirations to be consistent with their *occupational* aspirations. Aspirations (even though endogenously chosen) are, however, drawn from a known 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 truncated 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, affects educational aspirations and investments in such heterogeneous ways.

Our study contributes to three strands of literature. *First*, we contribute to the literature on information or mentoring 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; Renée, 2023). 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).[3] 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.

Second, we contribute to the literature that investigates the role of students' 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).[4] 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

[3] A notable exception that studies career counseling in a middle-income country is Loyalka et al. (2013).

[4] 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.

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.

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 (United Nations Development Programme, 2023); 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).[5] 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 might 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). 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 and occupational aspirations, their knowledge on career paths, and their beliefs about the costs associated with higher education.[6] Our findings suggest that students have little knowledge about potential career paths, and that this lack of information may constrain them in their educational decisions. Specifically, while all students were able to name a job they

[5] 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.

[6] We conducted surveys with 200 students and focus group discussions with 32 students who were at the end of Grade 8 and held interviews with teachers, parents, and local and international education experts.

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, general practitioner (doctor), police officer, or soldier.[7] 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 other than becoming a teacher as they have little knowledge about alternatives.

While there is a similar concentration in educational aspirations in other countries, and career and educational expectations are often misaligned, this is less pronounced than in our context. Among adolescents participating in PISA in 2018, for example, 53% (47%) of girls (boys) expect to work in one of the ten most commonly cited jobs. Also in neighboring Thailand, where educational attainment is substantially higher than in Cambodia, aspirations seems to be less concentrated, with 61% (64%) of girls (boys) mentioning one of the ten most commonly cited jobs (Mann et al., 2020).

2.2 The Career-Guidance Workshop

The intervention was designed as a half-day career-guidance 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 commonly known as the RIASEC model.[8] The theory of vocational interest is well established in psychology and management sciences, and posits that individuals who display personality traits that fit with their occupation display higher job satisfaction.[9] 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,

[7]These findings are replicated for the group of students targeted by our intervention, see Table A.1. Figures A.1 to A.6 and Tables A.1 to A.21 are available in online appendix A.

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

[9]There is ample empirical literature that confirms this. For recent reviews, see van Iddekinge et al. (2011) and Hoff et al. (2020).

and are asked to select the ones most applicable to them. Small pictures serve as 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, three 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 general practitioner, 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. We ensured that all presented jobs existed in the local labor market and that there was sufficient demand for them. The ordering of the occupations is personalized according to the students' strongest personality types. For each occupation, students can read more detail on the job content and main responsibilities, as well as the school subjects related to the occupation and the educational requirements.[10] Students can decide how much time they spend reading about each occupation.[11] For more details on the CET, see online appendix B.2.

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 details on the information session, see online appendix B.3. At the end of the intervention day, each student takes home 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. CD partners with 51 lower-secondary schools in 8 districts across 4 provinces (Battambang, Banteay Meanchey, Oddar Meanchey, and Siem Reap). The activities of CD in these schools are limited to handing out scholarship application materials to school principals to be distributed among interested students. For

[10] We did not include information on earnings prospects for two main reasons. First and foremost, we did not want students to anchor their reading choices on the earning potential, but rather consider the job content and how it matches their interests. Second, even if we would have wanted to include that information, reported earnings in Cambodia's Socio-Economic survey are scarce and exhibit large variance within occupation. This would have likely created false expectations.

[11] They have a total of 17 minutes to read the descriptions that interest them, but can also log out earlier.

our study, we sampled all 39 schools that had a partnership with CD and a class size in Grade 9 above 30 students.[12] We expanded the sample by including 21 additional schools from the same provinces that are similar in characteristics to the CD partner schools. Of these 60 schools, 30 were randomly assigned to receive the treatment (the half-day workshop); the remaining 30 served as control. For 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). Randomization was stratified by district. Figure 1a depicts the location of the initial sample of treatment and control schools.[13]

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 the application deadlines for high school scholarships (including those awarded by CD). 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 these 18 treatment schools 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.[14]

Figure 2 shows the timeline of the data collection and the sample composition. The collection of administrative data 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. In addition, we collected teacher and school characteristics. On the day of the intervention, we also collected 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.[15] In the phone survey, we asked students about their daily activities, their expectations, and aspirations

[12]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.

[13]As we were interested in whether information take-up and processing differs when it is made self-relevant, we randomly allocated students into one of three treatment arms in treatment schools: A1, A2, and A3 with respective chances 2:2:1. Students in A1 participated in all three parts of the workshop, students in A2 participated only in the career exploration tool (not personalized) and the information session, while students in A3 participated in the information session only. As such 80% of the treated students participated both in the CET as well as in the information session, which we consider to form the core of the intervention. More details on the within-school randomization are available in online appendix B.4.

[14]Students were informed about the workshop several days in advance, and they were told that they were free to participate or not. Students who 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 remainder of this article, these five students are considered 'treated'; results do not change if these students are excluded.

[15]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., 2023).

in terms of education and future occupation. As a token of appreciation, students who finished the survey were eligible to participate in a lottery, and could choose between one of two potential prizes: phone credit or educational mentoring.

Schools reopened in September 2020 for ninth-graders, so that students could prepare for their final exam, which took 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 student 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 CD. This information is analyzed for those schools that partner with the NGO (28 out of 37 schools).

The new school year for tenth-graders started in January 2021, but all schools were closed again for a second lockdown at the end of February 2021 until the end of the school year in November 2021.[16] From the high schools, we collected data on whether students started Grade 10 (*i.e.*, transitioned to high school in January of 2021), whether they continued with Grade 11 in January of 2022 after the second lock-down, whether they attended and passed the midterm exam of Grade 11 (two years after our intervention) as well as data on high school completion in Nov 2023. Importantly, high school completion is contingent on participation in the final exam, but not the exam results.[17] The main selection thus seems to take place in the lead-up to the final exam. All in all, the data covers students' educational decisions more than three and a half years after the intervention.

3.2 Attrition

Of the 1,715 students included in the study, we reached 1,317 through the phone survey (77% of the original sample) and were able to track between 1,672 and 1,697 (97-99% of the original sample) with administrative data.[18] Participation in the phone survey was not random; in particular, female

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

[17] Those students who fail that exam (in our sample less than 0.5% of the students) need to take extra classes if they would like to continue with university.

[18] Attrition in the administrative data is due to a number of reasons. For very few students, teachers did not know whether they requested the official transcripts to enroll in high school. For others, we could not determine which school they had enrolled in (if any). And finally, during high school, some students transferred to other schools, and we were not able to verify where. To minimize attrition in the administrative data, we cross-checked all missing students with official records of all students who participated in the grade 12 graduation exam available at province level. For a student not to appear on those lists, they would have had to drop out of school, to transfer to a school in a different province, or to repeat the school year. As repetition is quite low, and we observe that only very few students switch schools in the first two years of high school, the most likely outcome is that these students dropped out.

students and students who performed better in school at baseline are more likely to have participated in the phone survey. However, we do not find that attrition is systematically related to treatment status in either the survey or the administrative data (see Table A.2).

In the robustness checks, we address attrition in two ways. First, we compute Kling-Liebmann sensitivity bounds to account for missing values. Second, we test the sensitivity of the estimated effects on the survey-based outcomes by re-weighting the phone-survey sample with the inverse probability of participating in the phone survey.

3.3 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, and information from the phone survey that was conducted with students in treatment and control schools in summer 2020.

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. Nearly all students lived in rural areas, on average 11 kilometers away from the district town. To reach their lower-secondary school, students needed to travel on average 3.6 kilometers and about ten kilometers to reach the next high school. 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 reasonably 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 the most worrisome as past grades are strong predictors of future educational outcomes. Nevertheless, there is sufficient support between both groups over the entire grade distribution (as shown in Figure A.1). We address the imbalances through standard regression-based corrections and data-driven approaches (double machine learning), as well as by excluding the schools in the tails of the grades distribution (class-averaged) from the sample as additional robustness check.

We track student outcomes after the intervention through phone-survey answers, as well as administrative data obtained from lower-secondary and from high schools (as summarized in Table A.3). 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 aimed for a career that requires higher secondary education (91%) and about one out of three students aspired a career that requires a university degree.[19] As a prize for participating in the phone survey, the great majority of students (80%) chose educational mentoring over phone credit.

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 CD, 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 of Grade 9 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 performed surprisingly well. Students usually need 260 points to pass the exam and to be allowed to enroll in high school, and the vast majority exceeded 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 is 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. A smaller share of students (75%) actually started high school as confirmed by high school teachers. An even smaller share of students (64%) started Grade 11 a year later, and only 61% of all students who we were able to track attended the midterm exam of Grade 11, which 49% passed in the first attempt. Finally, about 54% of students were able to complete high school about three years and nine months after our intervention took place.

4 Empirical Analysis

4.1 Empirical approach

In order to analyze whether our intervention affected schooling decisions, we focus on six outcomes that are self-reported and based on answers collected in the phone survey and on six outcomes constructed from administrative data. With the phone-survey data, we analyze whether students strongly agreed with the statement that they kept studying during the first school closure, as well as students' aspirations, in particular, the years of schooling students aspire to achieve, whether the job they would like to do when they are 25 is outside the typical reference window (*i.e.*, it is not teacher, general practitioner, police officer, or soldier), and the years of schooling that the aspired occupation requires. In addition, we consider whether students rate the probability that they will achieve their

[19] Interestingly, students aspirations (but not expectations) with regards to education and occupation remained very stable throughout the first COVID-19 lockdown (see Figure A.2).

stated educational (and occupational) aspiration as high.[20] 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. To study longer-term effects, we investigate whether students started Grade 11, whether they attended the Grade 11 midterm exam, and whether they completed high school. Of each group of outcomes, the first three were pre-specified in the pre-analysis plan (Gehrke et al., 2020), while the latter three serve to provide deeper insights into the mechanisms at play or to understand the persistence of the estimated effects.

Because not all students were present during 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. The specification takes the form:

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

where Y_{ijd} is any 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 CD 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).

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

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

In addition, we use a data-driven approach, namely double machine learning (DML, Chernozhukov et al., 2018), to select the relevant covariates and estimate the coefficients of interest.[21] The 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).

[20] These two variables are coded as dummies that take the value one if the stated confidence on a scale from zero to 10 is greater than 5. We use the dummy rather than the continuous variable because of substantial bunching in the data around the middle value.

[21] We use a lasso-based procedure, sometimes referred to as cross-fit partialing-out lasso. We implement the procedure in Stata using `xporegress` and `xpoivregress`. For the cross-fitting, we report results averaged over ten sample splits, for each using ten folds.

4.2 Main Results

Results are presented in Tables 2 and 3. We report ITT effects (Panel A) and TOT effects (Panel B). For each outcome, we first show the estimates that control for student and school characteristics as well as district fixed effects as pre-specified. We then present the double machine learning estimates. 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.

We find that the intervention had no effect on average on whether students self-report to have been studying during the lockdown period (Table 2). We do, however, find a weakly positive effect on students' educational aspirations in years of schooling (0.24 additional years), as well as a weakly negative effect on the diversification in students' occupational aspirations (0.05 pp), which implies that treated students were less likely to name an occupation other than the main four as their stated career goal. This result is strongly driven by the higher likelihood that treated students mentioned 'teacher' as their occupational aspiration, which had been featured as part of the career exploration tool. When studying occupational aspirations in terms of the years of schooling required to carry out the aspired occupation, we find a positive effect that is comparable in magnitude to the effect observed on educational aspirations. Overall, treated students were slightly more confident in being able to attain the aspired level of education or the aspired occupation. However, none of the effects observed in the phone survey are statistically significant after correcting for the FDR.[22]

In terms of administrative outcomes (Table 3), we find that treated students were 2.5 percentage points more likely to return to school and attend the Grade 9 final exam after the COVID-19 lockdown (2.9% increase over the control group mean). Conditional on attending, treated students also performed better than control students in the final exam (0.21 SD). These two effects are statistically significant after correcting for the FDR only in the DML specification. The magnitude of the effect on final exam performance is striking given that the intervention also led to higher attendance in that exam, and the marginal students to attend the exam were likely among the students with lower school performance.[23] In addition, we find a positive effect of 5.8pp (7.7%) on high school enrollment. This effect is statistically significant at the 10% 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 is sustained throughout high school. Treated students were 6.1pp (9.5%) more likely to start Grade 11 and 5.6pp (9.2%) more likely to attend the midterm exam of Grade 11. 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. Finally, treated students were 3.9pp (7.0%) more likely to complete high school, although this effect

[22]The number of observations vary by survey outcomes as students could refuse to answer individual survey items. We test the sensitivity of our results to these missing values using bound analyses (see Section 4.4).

[23]This raises suspicion that teachers may have become more lenient due to our intervention. However, we do not find a statistically significant effect on the probability of passing the final exam, the threshold at which most manipulation seems to have taken place (see Figure A.1). This attenuates concerns that the observed effect on grades may be driven by teachers rather than students.

is not statistically significant at conventional levels before correcting for the FDR.

Taken together, these results imply that participation in the workshop encouraged students to increase their educational investments. While it is suggestive at this point, the evidence so far also indicates that these effects may be due to students adjusting the levels of their aspirations (both educational and occupational) as well as their confidence in reaching these aspirations.

4.3 Heterogeneities by Academic Performance

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.

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 (corresponding to the odd columns in Tables 2 and 3). Parametric estimates that interact the treatment indicator with students' baseline performance and account both for pre-specificed controls as well as Lasso-selected controls are reported in Tables A.4 and 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 who 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 educational aspirations, we find evidence that low-performing students downward adjusted their educational aspirations, while high-performing students adjusted them upwards. A similar pattern —albeit somewhat less pronounced among low-performing students— can be observed for occupational aspirations when expressed in years of schooling.[24] 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 treated students being more likely to state 'teacher' as their occupational aspiration, an occupation that was also featured in the intervention. We find no clear pattern by baseline academic performance with respect to student's beliefs about their ability to reach these aspirations. In terms of beliefs that students will reach the aspired level of education, the intervention seems to have had marginal but positive effects over the entire distribution of initial grades. With respect to beliefs about reaching the aspired occupation, the effects are again positive throughout, and most pronounced among the lowest-performing students and those that perform somewhat better than average.

Treatment effects are also heterogeneous with respect to participation in the final exam, and

[24] The interaction effect is statistically significant for this outcome in the DML specification but not in OLS (see Table A.4). This attenuation may be due to the fact that we manually coded years of schooling for all listed occupations, which may not always be correct.

more strongly so for students' performance in that exam; low-performing treated students were marginally more likely to participate in the final exam, but did perform worse than their control-school counterparts conditional on participating. High-performing treated students, in turn, were (up to 10pp) more likely to participate in the final exam and also performed better than their counterparts from control schools.[25]

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 relatively low among this group: while more than 75% of the low-performing students participated in the final exam, only about 60% enrolled in high school. In other words, any negative effect on low-performing students disappears about nine months after the intervention, as a large fraction of students performing in the lower-end of the grade distribution would have dropped out by that time anyway. The positive average effect throughout high school continues to be driven by high-performing students, who were significantly more likely to start Grade 11, to attend the midterm exam of Grade 11, as well as to complete high school 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 sooner against attending high school, and that low-performing students adjusted their effort accordingly. Consistent with that interpretation, we find that low-performing treated students (especially those with a standardized grade below minus one) 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 (for these and the following results see Figure A.3 and Table A.6). 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 CD. However, they were *not* less likely to pass the final exam of Grade 9 nor to obtain a CD scholarship, suggesting that low-performing treated students invested just enough effort in education to be able to complete 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.

[25]Note that this positive effect on high performers is unlikely to be driven by selection on performance, as we are flexibly conditioning on baseline academic performance. It may still be the case however that our intervention motivated a very specific group of students within the high performers to participate in the exam (s.a. students with higher socio-emotional skills), which may drive the positive effect on final exam grades.

First, we investigate whether inference is sensitive to alternative assignments of treatment status in our sample or to how we correct for multiple hypothesis testing. The p-values on the main effects (on high school enrollment, grade 11 enrollment and performance, and high school completion) as well as the treatment effect heterogeneities (on study behavior, on educational and occupational aspirations, and on schooling outcomes) remain similar 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 evaluate the robustness of our findings to selective attrition. For the survey-based outcomes, we re-weight 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 throughout 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, while the interaction effect on occupational aspirations (in years of schooling) declines somewhat in magnitude, which can likely be attributed to the larger number of missings in this variable. For high school related outcomes (enrollment in high school and in Grade 11, attendance of midterm exam, high school completion), the average treatment effects, as well as the treatment effect heterogeneities are largely robust. Only the average effect on high school completion is insignificant in the specifications in which more extreme assumptions are taken. However, it is worth keeping in mind, that most of the students we could not track probably have dropped out of high school (see Tables A.9 and A.10).

Third, we address remaining concerns regarding the large imbalance in baseline grades, and the possibility that it could be driving the observed effects, given that baseline grades are strong determinants of later schooling outcomes. We first identify which schools drive these imbalances. We find that treatment schools are substantially over-represented among the schools with the lowest average grades and under-represented among the schools with the highest grades.[26] If we restrict the sample to the schools above the 10th and below the 90th percentiles in average grades, our sample drops to 1,328 students from 14 treatment and 14 controls schools. As shown in Table A.11 baseline characteristics are largely balanced in this subsample, except for teacher age, the distance the teacher lives from the school, and whether the school partners with CD. We then estimate the main treatment effects as well as treatment effect heterogeneities by baseline grades in this

[26]We also investigate if these imbalances are driven by particular districts, and find that they materialize in almost all of the sample districts.

subsample, and find that our results are virtually unchanged (see Tables A.12 and A.13).[27]

Fourth, we study whether the observed treatment effect heterogeneities by performance are mere spurious correlations driven by confounding variables that affect pre-intervention performance and educational investments. To identify potential confounders, we employ two strategies. We first analyze potential correlates of academic performance by regressing an extensive set of household-level and school-level 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. At the household level, neither remoteness of the student's village (in terms of distance to the school or next district town) nor occupation of the parents in agriculture 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 or income during the COVID-19 crisis (c.f. Table A.14).[28] At the school level, we find that teacher age and experience at school are correlated with baseline performance, but none of the other characteristics: class size, gender of teacher, teacher distance to school, whether a high school is attached to the lower-secondary school and whether the school partners with CD (c.f. Table A.15). We then split our sample into four quartiles of student baseline academic performance and investigate which student, school, or parental characteristics are ever imbalanced in each of these quartiles.[29] For each of the potential confounders (that are either significantly correlated with academic performance or that ever show up as imbalanced), we then add the interaction of the confounder with treatment status to the main regression. For parental characteristics, 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. The analysis is therefore split in two. We first interact treatment with parental characteristics (in Tables A.16 and A.17).[30] Overall, our results are unchanged. In a separate exercise, we add interactions of any other individual- or school-level characteristic identified as potential confounders with treatment (in Tables A.18 and A.19). Again our results are unchanged, suggesting that none of these student, parental, teacher, or school characteristics are driving the observed heterogeneities in treatment effects.

Fifth, we investigate if the heterogeneities we find are merely driven by differences between treatment and controls schools in how baseline grades predict subsequent outcomes.[31] This seems not to be the case, however, as the semi-parametric relationship between February grades (which

[27]In each of these regressions, we control for district fixed effects, as well as student age, gender, and baseline grades, in addition to the imbalanced variables mentioned above.

[28]In previous work (Gehrke et al., 2023), we show that the economic repercussions of the COVID-19 crisis affected students' academic performance mostly through parental income losses.

[29]The variables that show up as imbalanced at the 10% level at least once are: student gender, distance to school, high school, and to district town, teacher age, experience, and distance to school (log), and school partnership with CD.

[30]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, parental job loss, and parental income loss interacted with treatment status.

[31]A reason may be, for example, the extent to which teachers concentrate their efforts towards high-performing students, or conversely, those who are falling behind.

we were able to collect from 17 treatment schools and 14 control schools) and the main running variable (grades averaged over December and January) are virtually identical (as shown in Figure A.4).

5 Underlying Mechanisms

The evidence presented so far suggests that a half-day workshop designed to help students develop occupational aspirations that match their interests and abilities increases educational and occupational aspirations as well as educational investments for high-performing students, while it decreases educational and occupational aspirations as well as educational investments (at least in the short-run) for low-performing students. In the following, we present a model of endogenous aspiration formation under information constraints to rationalize these findings, and subsequently explore a range of alternative explanations and discuss their empirical relevance in this context.

5.1 Endogenous Aspirations and Information Constraints

Consider a model of endogenously determined aspirations as in Dalton et al. (2016), and assume that students choose educational aspirations and occupational aspirations that are aligned with each other. For example, a student may want to become a medical practitioner and knows that they need to get a university degree (in medicine) to achieve this. Imposing that occupational and educational aspirations are aligned with each other implies that students' aspirations can be formulated over educational outcomes, even if students do not derive utility from achieving any given level of education *per se* but rather from the occupational opportunities that arise from this achievement. The utility function of a student can then be described by:

$$u = b(\theta) + v(\frac{\theta - a}{\theta}) - c(e), \tag{3}$$

with θ being the final level of education, $a \in R_+$ being the aspired level of education, and $e \in [0, 1]$ being effort. We follow Dalton et al. (2016) in our assumptions about the functional forms of the three additive components of u. We assume that direct utility, $b(\theta)$, is concave and twice differentiable with $b(0) = 0$, and the coefficient of relative risk aversion $r(x) = -\theta b''(\theta)/b'(\theta) < 1$. Milestone utility, $v[(\theta - a)/\theta]$, then, is continuously differentiable with $v'(0) > 0$ and $[v'(x) - v''(x)(1 - x)] \geq 0$ for all feasible values of x, with one possible formulation being an S-shaped value function with diminishing sensitivity as in Kahneman and Tversky (1979). The functional form of $v(x)$ ensures that the utility associated with satisfying any aspiration increases in the level of a. The cost of effort, $c(e)$, finally, is continuous, twice differentiable and convex in e, with $c(0) = 0$.

When choosing educational aspirations, students take into account their own preferences, while being constrained by their academic ability, *i.e.*, the rate at which educational investments transform into educational outcomes. Similarly to Dalton et al. (2016), we incorporate these external constraints by imposing that skills $\mu \in [0, \infty]$ and effort e are complements in the production of education θ. For simplicity, assume:

$$\theta = f(\mu, e) = (1 + \mu)e. \tag{4}$$

A rational student chooses the optimal effort-aspiration pair (e^*, a^*) that maximizes utility. As in Dalton et al. (2016), we constrain possible solutions to consistent effort-aspiration pairs, *i.e.*, pairs in which the aspiration equals the final outcome: $a = f(\mu, e) = (1 + \mu)e$.[32] This assumption, together with eq. (4) and the functional form assumptions concerning u are sufficient to ensure a unique solution level of aspiration and effort, which is increasing in skills. The association between aspirations and skills in optimum is weakly positive throughout and strictly positive for interior solutions. 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).

However, in our context, we find that educational and occupational aspirations are only modestly correlated with baseline academic performance in the control group. To rationalize why our intervention increases the correlation between educational aspirations and initial academic performance, we have to accommodate the possibility that information frictions constrain students in choosing the right level of aspirations. 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

$$a^* = \Psi(\mu, e^*, F), \tag{5}$$

where F is the known 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 truncated aspirations window, and cause students to 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 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 occupational aspirations, while delivering information about the educational requirements neces-

[32]See eq. (2) in their paper.

sary to carry out these jobs, helps students re-calibrate their 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.20). In particular, low-performing students spent more time reading about occupations that only require lower-secondary education.

5.2 Alternative Explanations

There are a number of alternative explanations for the observed effects. In the following, we discuss each of these mechanisms and the extent to which the empirical evidence supports them.

Expectations. Rather than broadening the aspirations window, the intervention may have forced students to adjust their expectations in heterogeneous ways. For example, 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. In contrast, for high-performing students, this same information may have helped them understand how well they are qualified for higher education and given them the necessary push to pursue this trajectory. However, we find no evidence that low-performing treated students were less confident in reaching their educational or occupational aspirations at the time of the phone survey (Figure 3). Likewise, educational expectations do not adjust in exactly the same way as educational aspirations: while we do find that low-performing students downward adjusted their expectations, high-performing students (in particular those with standardized grade above 1) were not expecting more years of schooling than those in control schools (see Figure A.5), while their aspirations and investments clearly diverged. Together these findings suggest that adjustments in expectations are unlikely to be the main driver underlying our results.

Differential information acquisition. An alternative explanation may be that different aspects of the information provided during the intervention were acquired and processed by low- vs. high-performers. For instance, low-performing students may only have absorbed and processed the information about high school costs, while high performers processed all information accurately, including costs, scholarship opportunities, etc. However, we find no evidence of differential cost updating for low- vs. high-performers among treated students. In fact, all students seem to update in the right direction (as shown in Figure A.6).[33]

Discouragement. Another alternative explanation may be that the intervention initially led all students to adjust their aspirations upwards, but that low-performing students quickly realized after the intervention that they are not able to attain the goods grades required to enroll in high school, and became frustrated, therewith adjusting their aspirations to new, even lower, levels (as in McKenzie et al., 2022). However, we do find a strong gradient in educational aspirations with

[33] In addition, 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.21).

baseline grades already immediately in the aftermath of the intervention in treatment schools (see Figure A.5), which suggests that discouragement effects are unlikely to explain our findings.[34]

Broadening of occupational interests. A final possibility could be that our intervention simply broadened the set of occupations that students are potentially interested, similar to the mechanism described in Belot et al. (2018). While this is consistent with increased educational investments among the high-performers, this argument is less well suited in explaining why low-performing students would react to the intervention by studying less during the lockdown, and perform worse during the final exam (conditional on attending).

6 Conclusions

This study provides experimental evidence that a half-day career-guidance workshop designed to expand students' aspirations window —in terms of the careers they can pursue— improves 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 and occupational 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 and occupational aspirations, performed better in the final exam, and were more likely to transition to, progress in, and complete 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 is low-cost and easily scalable, improves 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 the potential to generate 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 complete 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 more than

[34]Note that aspirations were collected in a slightly different way at endline on the intervention day. We also cannot systematically compare treatment and control schools in this exercise, as this information was collected in treatment schools only.

three and a half 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., 2023). 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.

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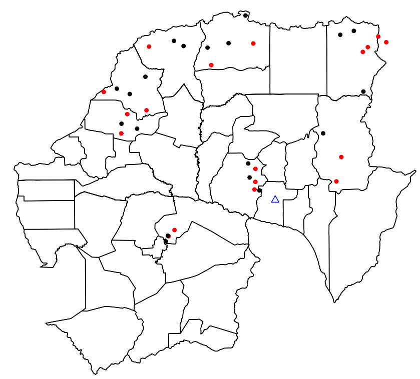
Figures

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

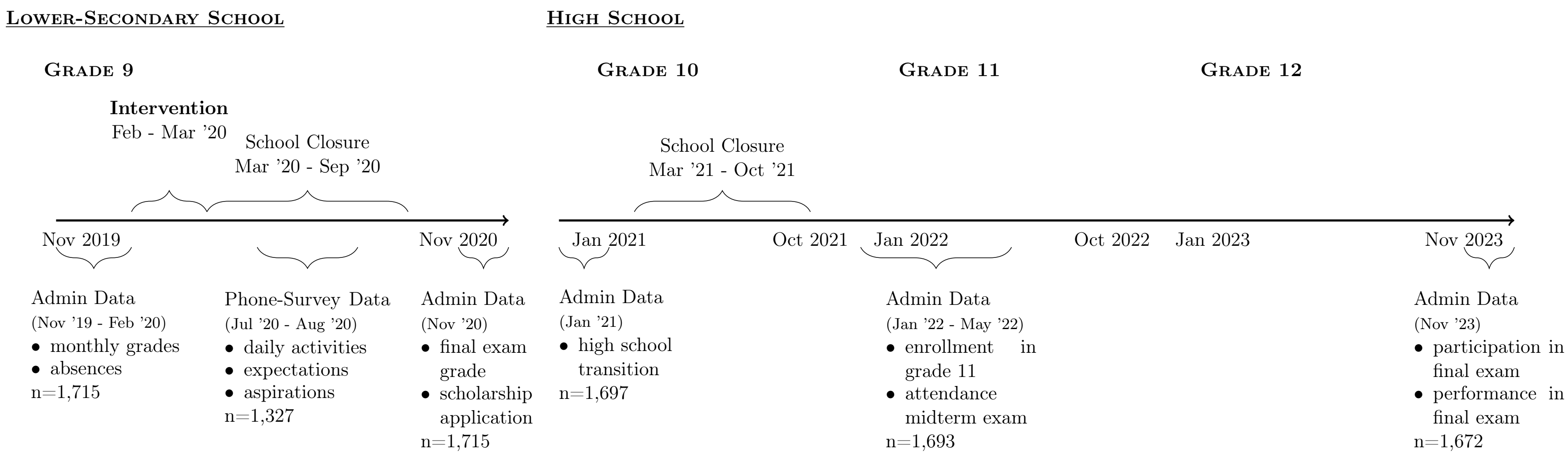
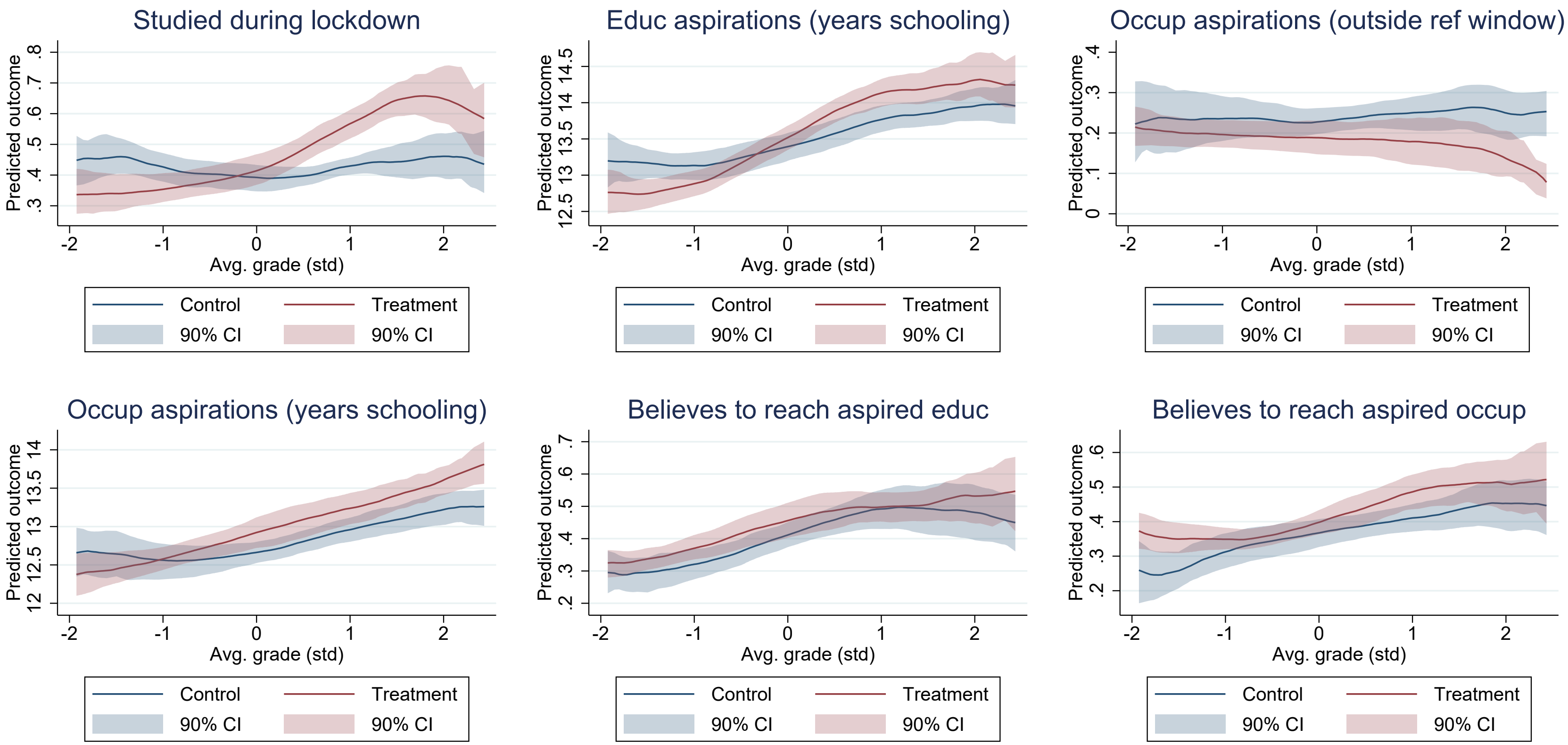
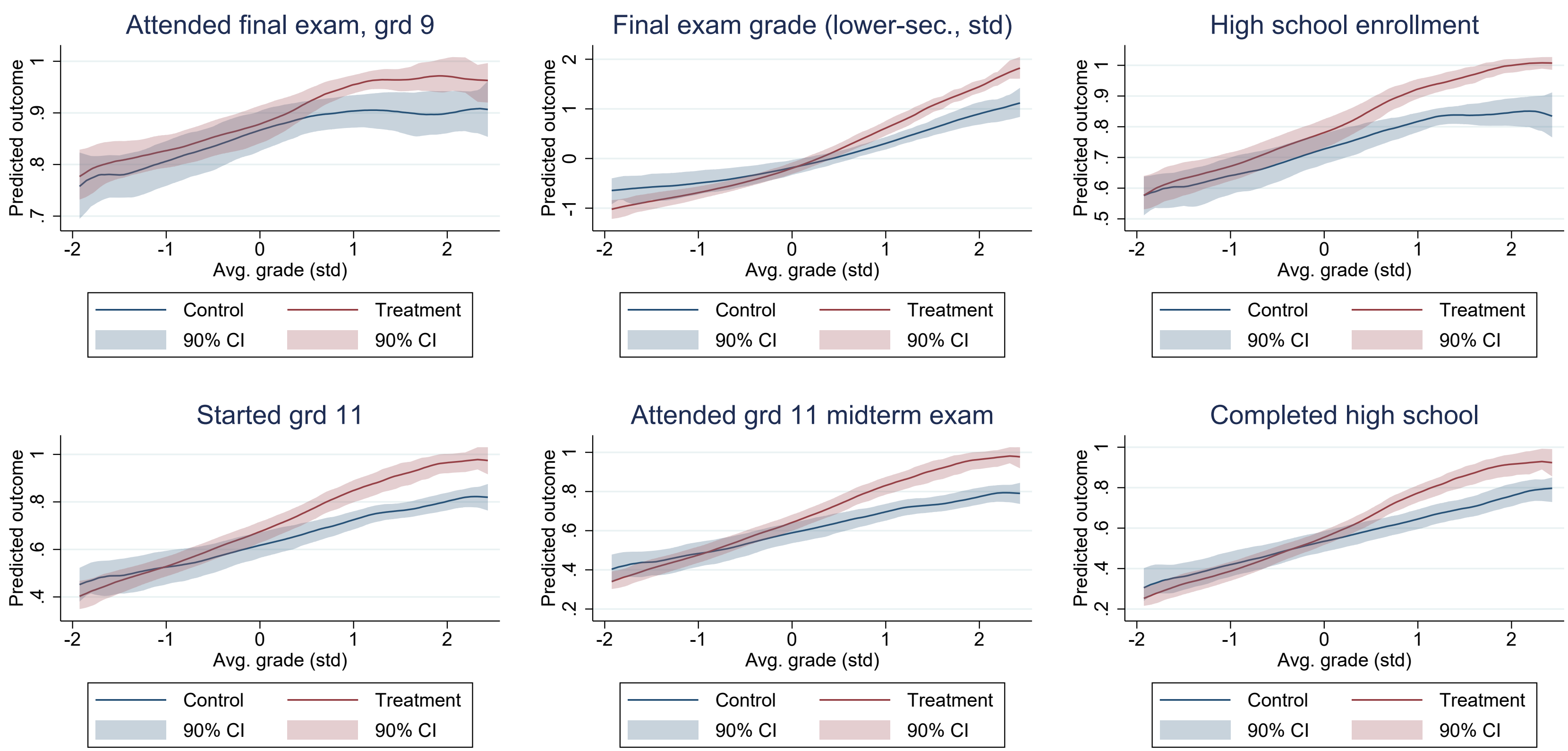


Figure 3: Treatment Effect Heterogeneity by Pre-intervention Grades (1)



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).

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
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)		Occup aspirations (years schooling)		Believes to reach aspired educ		Believes to reach aspired occup	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Intention to Treat												
Treatment Assigned	0.049	0.009	0.127	0.225	-0.064	-0.043	0.252	0.218	0.050	0.012	0.064	0.034
	(0.041)	(0.029)	(0.133)	(0.124)*	(0.029)**	(0.025)*	(0.135)*	(0.107)**	(0.035)	(0.029)	(0.031)**	(0.028)
	[0.169]	[0.480]	[0.209]	[0.200]	[0.161]	[0.200]	[0.161]	[0.200]	[0.161]	[0.480]	[0.161]	[0.200]
Panel B: Treatment on the Treated												
Treated	0.053	0.008	0.138	0.245	-0.068	-0.046	0.271	0.231	0.054	0.011	0.069	0.036
	(0.043)	(0.031)	(0.141)	(0.131)*	(0.031)**	(0.027)*	(0.142)*	(0.115)**	(0.037)	(0.032)	(0.033)**	(0.030)
	[0.155]	[0.608]	[0.197]	[0.208]	[0.127]	[0.208]	[0.127]	[0.208]	[0.127]	[0.608]	[0.127]	[0.234]
OLS w pre-specified controls	✓		✓		✓		✓		✓		✓	
Cross-fit partialing out Lasso		✓		✓		✓		✓		✓		✓
Control Mean	0.42		13.45		0.23		12.80		0.43		0.37	
Observations	1,296		1,317		1,291		1,291		1,291		1,289	

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 the uneven columns, 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 the even columns, 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		Started grd 11		Attended grd 11 midterm exam		Graduated high school	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Intention to Treat												
Treatment Assigned	0.040	0.022	0.156	0.200	0.088	0.053	0.086	0.056	0.088	0.051	0.064	0.036
	(0.028)	(0.018)	(0.151)	(0.052)***	(0.036)**	(0.023)**	(0.043)*	(0.025)**	(0.041)**	(0.025)**	(0.041)	(0.026)
	[0.117]	[0.072]	[0.183]	[0.001]	[0.117]	[0.046]	[0.117]	[0.046]	[0.117]	[0.055]	[0.117]	[0.072]
Panel B: Treatment on the Treated												
Treated	0.044	0.025	0.167	0.213	0.096	0.058	0.094	0.061	0.096	0.056	0.070	0.039
	(0.031)	(0.020)	(0.159)	(0.057)***	(0.039)**	(0.025)**	(0.046)**	(0.028)**	(0.044)**	(0.027)**	(0.044)	(0.029)
	[0.102]	[0.076]	[0.171]	[0.001]	[0.085]	[0.042]	[0.085]	[0.042]	[0.085]	[0.051]	[0.093]	[0.076]
OLS w pre-specified controls	✓		✓		✓		✓		✓		✓	
Cross-fit partialing out Lasso		✓		✓		✓		✓		✓		✓
Control Mean	0.87		0.04		0.75		0.64		0.61		0.56	
Observations	1,715		1,485		1,697		1,693		1,692		1,672	

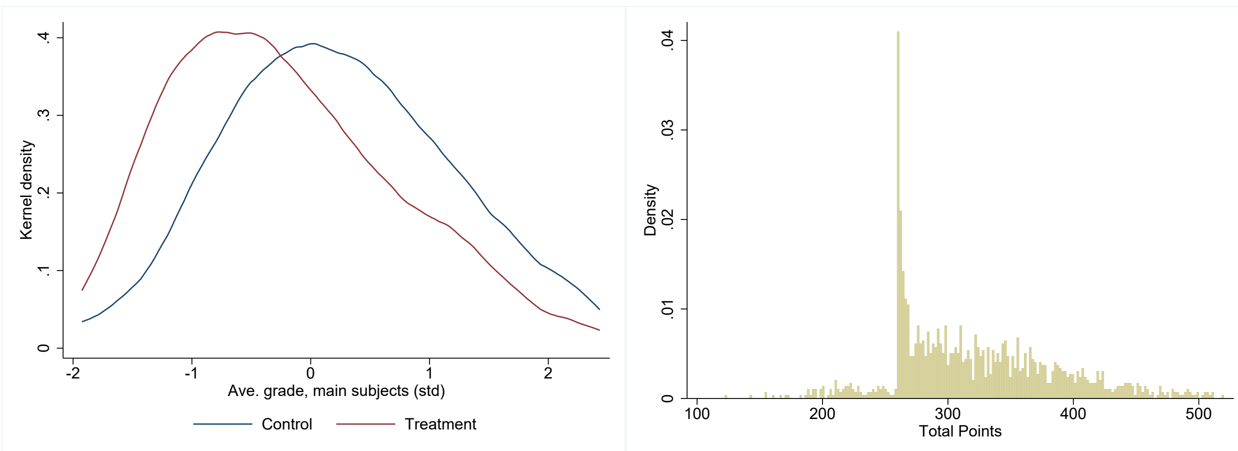
Notes: Panel A: OLS estimates. Treatment Assigned = The student's school received the intervention. Panel B: 2SLS estimates. Treatment (participation in the intervention) instrumented with assigned treatment status. Standard errors (in parentheses) are clustered at the school level. In brackets, Anderson's sharpened q-values to account for the False Discovery Rate (Anderson, 2008). In the uneven columns, 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 the even columns, we report results from cross-fit partialing out lasso using 10 folds and 10 re-samples.

A Online Appendix: Additional Figures and Tables

A.1 Figures

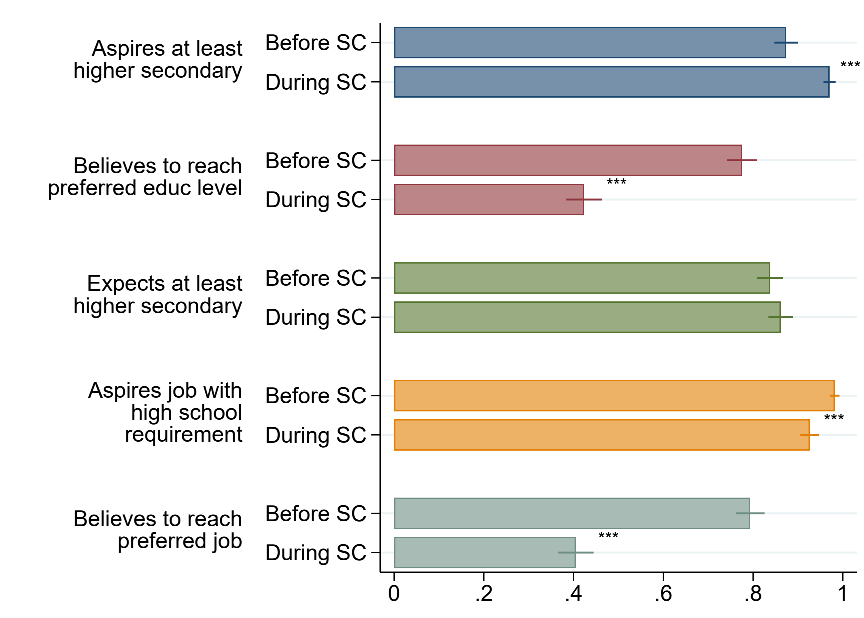
Figure A.1: Distribution of Grades

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



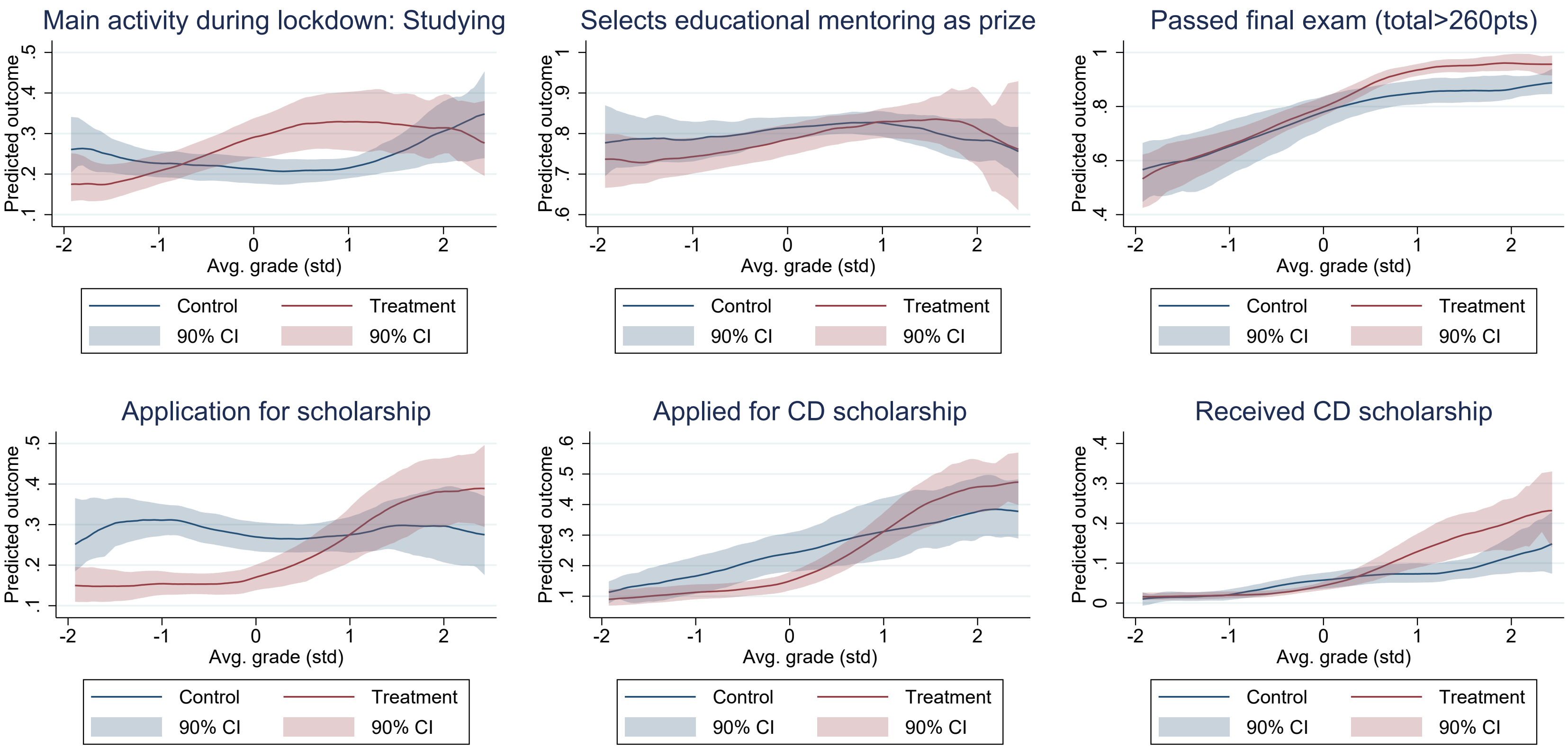
Notes: Panel (a) shows the distribution of pre-intervention grades for treatment and control. Panel (b) shows the distribution of the total points students obtained in the final exam.

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



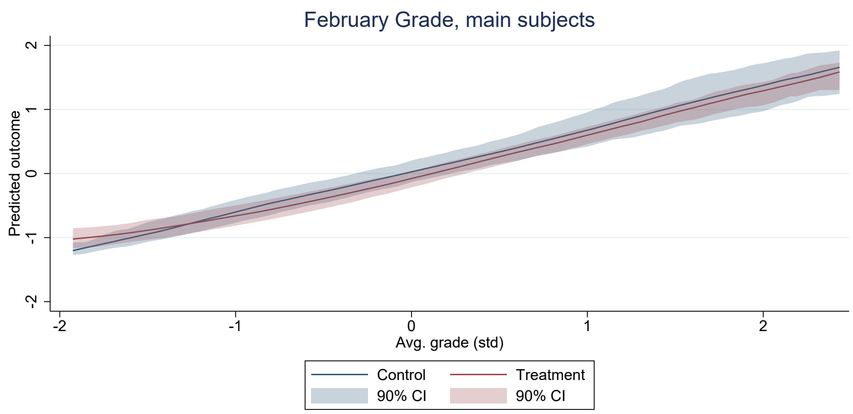
Notes: For treated students who also participated in the phone survey. All outcomes are dummies, SC denotes school closure. */**/*** denote significance levels at 10/5/1 percent respectively.

Figure A.3: Treatment Effect Heterogeneity by Pre-intervention Grades – Educ. Investment during Grade 9



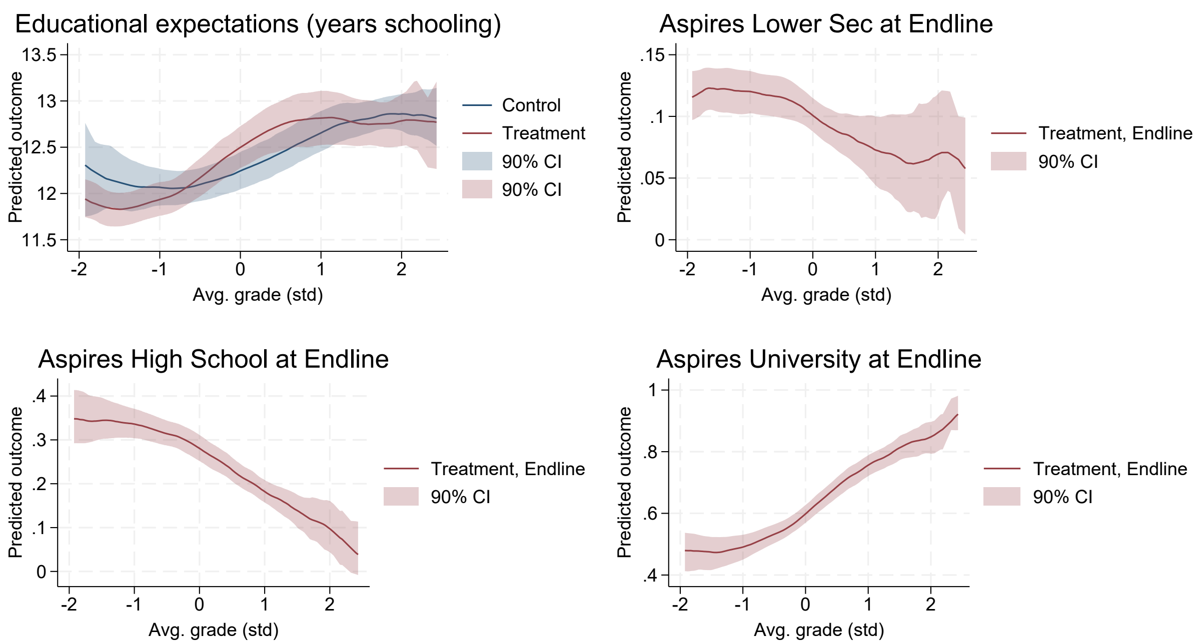
Notes: These figures display the weighted moving-average (bandwith = 0.5, Epanechnikov kernel) and bootstrap confidence intervals (clustered at the level of the school) of the dependent variable (in the figure header) over the baseline grade (sum of Math, Khmer and English grades, averaged over the months 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: February Grades by Pre-intervention Grades



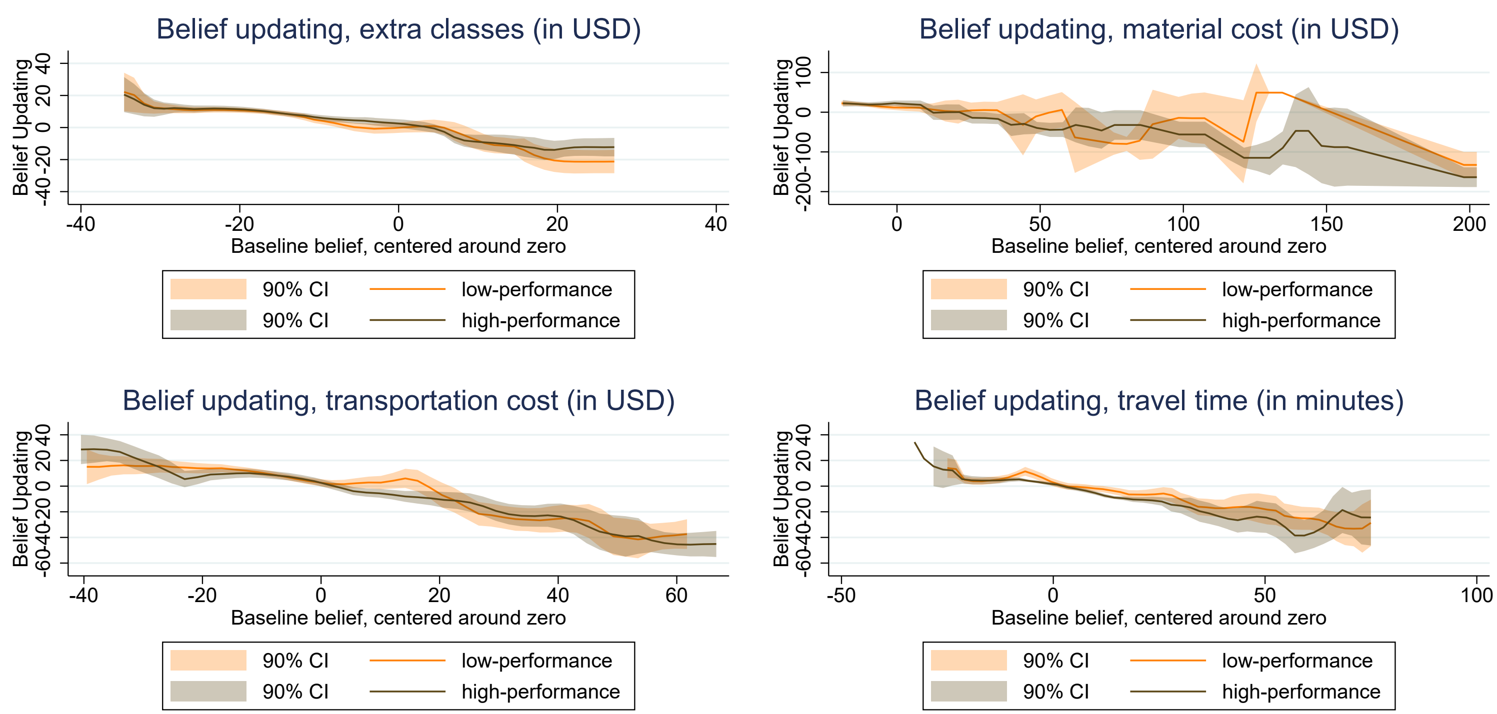
Notes: This figure displays the weighted moving-average (bandwith = 0.5, Epanechnikov kernel) and bootstrap confidence intervals (clustered at the level of the school) of the dependent variable (in the figure header) over the baseline grade (sum of Math, Khmer and English grades, averaged over the months 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.5: Educational Expectations and Aspirations by Pre-intervention Grades



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 in the first panel, and for treatment schools only in the remaining panels. 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.6: Belief Updating on High School Costs



Notes: Sample are treated students. Belief updating is the endline belief minus the baseline belief. Sample is split at the median of the baselina grade distribution.

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: Attrition Analysis

	Not reached in phone survey		No high school enrollment data		No high school graduation data	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Assigned	0.002	-0.031	0.003	0.001	0.001	0.000
	(0.017)	(0.020)	(0.005)	(0.006)	(0.010)	(0.010)
Female		-0.091		0.006		0.002
		(0.022)***		(0.004)		(0.008)
Age		0.005		-0.003		-0.003
		(0.008)		(0.002)		(0.003)
Pre-grade (std)		-0.069		-0.001		0.004
		(0.013)***		(0.003)		(0.005)
Avg. Absence		0.005		0.002		0.002
		(0.006)		(0.001)		(0.002)
Class Size		0.001		-0.000		0.000
		(0.001)		(0.000)		(0.000)
Partnership with Child's Dream		-0.005		0.009		0.008
		(0.030)		(0.011)		(0.013)
District fixed effects	✓	✓	✓	✓	✓	✓
Mean	0.23		0.01		0.03	
Observations	1,715		1,715		1,715	

Notes: OLS estimates. Treatment Assigned = The student's school received the intervention. Standard errors (in parentheses) are clustered at the school level. */**/*** denote significance levels at 10/5/1 percent respectively.

Table A.3: 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
Believes to reach aspired educ	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
Occup aspirations (years schooling)	12.86	12.00	1.84	9.00	15.00	1291
Believes to reach aspired occup	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, grd 9	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 (grd 9)	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	1693
Attended grd 11 midterm exam	0.61	1.00	0.49	0.00	1.00	1692
Passed grd 11 midterm exam	0.49	0.00	0.50	0.00	1.00	1664
Completed high school	0.54	1.00	0.50	0.00	1.00	1672
Final exam grade (grd 12)	60.08	55.00	10.59	45.00	95.00	885

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 of Grade 9 excludes students who scored 0 or did not write the exam.

Table A.4: Heterogeneity by Performance - Survey Data

	Studied during lock down		Educ aspirations (years schooling)		Occup aspirations (outside ref window)		Occup aspirations (years schooling)		Believes to reach aspired educ		Believes to reach aspired occup	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Intention to Treat												
Treatment Assigned	0.038	0.021	0.102	0.143	-0.060	-0.032	0.236	0.174	0.050	0.029	0.064	0.055
	(0.040)	(0.030)	(0.124)	(0.119)	(0.029)**	(0.025)	(0.131)*	(0.110)	(0.034)	(0.030)	(0.030)**	(0.028)**
Pre-grade (std)	0.004	0.029	0.300	0.298	0.014	0.044	0.252	0.140	0.069	0.081	0.063	0.076
	(0.024)	(0.025)	(0.096)***	(0.112)***	(0.021)	(0.022)**	(0.085)***	(0.097)	(0.024)***	(0.025)***	(0.019)***	(0.024)***
Treat. Ass. x Pre-grade	0.099	0.101	0.264	0.260	-0.039	-0.027	0.163	0.202	0.005	0.011	-0.004	-0.004
	(0.032)***	(0.029)***	(0.125)**	(0.122)**	(0.027)	(0.024)	(0.104)	(0.108)*	(0.028)	(0.028)	(0.029)	(0.029)
Panel B: Treatment on the Treated												
Treated	0.036	0.020	0.096	0.147	-0.062	-0.035	0.247	0.190	0.054	0.033	0.070	0.061
	(0.044)	(0.032)	(0.134)	(0.129)	(0.031)**	(0.027)	(0.140)*	(0.120)	(0.035)	(0.032)	(0.032)**	(0.030)**
Pre-grade (std)	0.004	0.029	0.299	0.299	0.014	0.043	0.252	0.141	0.069	0.081	0.063	0.076
	(0.024)	(0.025)	(0.094)***	(0.112)***	(0.021)	(0.022)**	(0.084)***	(0.097)	(0.023)***	(0.025)***	(0.019)***	(0.024)***
Treated x Pre-grade	0.106	0.109	0.285	0.282	-0.040	-0.028	0.167	0.214	0.003	0.011	-0.007	-0.007
	(0.035)***	(0.032)***	(0.135)**	(0.135)**	(0.029)	(0.026)	(0.111)	(0.119)*	(0.030)	(0.031)	(0.031)	(0.031)
OLS w pre-specified controls	✓		✓		✓		✓		✓		✓	
Cross-fit partialing out Lasso		✓		✓		✓		✓		✓		✓
Control Mean	0.42		13.45		0.23		12.80		0.43		0.37	
Observations	1,296		1,317		1,291		1,291		1,291		1,289	

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 uneven columns, 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 even columns, 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 - Administrative Data

	Attended final exam		Final exam grade (std.)		High school enrollment		Started grd 11		Attended grd 11 midterm exam		Completed high school	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Intention to Treat												
Treatment Assigned	0.039	0.022	0.131	0.116	0.087	0.059	0.085	0.066	0.086	0.062	0.063	0.036
	(0.028)	(0.017)	(0.145)	(0.059)**	(0.034)**	(0.023)**	(0.040)**	(0.026)**	(0.037)**	(0.025)**	(0.037)	(0.027)
Pre-grade (std)	0.046	0.040	0.557	0.470	0.088	0.085	0.113	0.102	0.119	0.097	0.140	0.104
	(0.017)**	(0.015)***	(0.100)***	(0.053)***	(0.024)***	(0.020)***	(0.024)***	(0.023)***	(0.022)***	(0.022)***	(0.023)***	(0.023)***
Treat. Ass. x Pre-grade	0.023	0.020	0.260	0.250	0.055	0.056	0.073	0.079	0.088	0.093	0.080	0.082
	(0.020)	(0.017)	(0.099)**	(0.053)***	(0.027)*	(0.021)***	(0.028)**	(0.024)***	(0.026)***	(0.022)***	(0.028)***	(0.023)***
Panel B: Treatment on the Treated												
Treated	0.042	0.023	0.128	0.115	0.092	0.063	0.089	0.070	0.089	0.063	0.064	0.035
	(0.031)	(0.019)	(0.155)	(0.064)*	(0.037)**	(0.026)**	(0.043)**	(0.029)**	(0.040)**	(0.028)**	(0.040)	(0.029)
Pre-grade (std)	0.046	0.040	0.558	0.470	0.088	0.085	0.113	0.102	0.119	0.097	0.140	0.104
	(0.017)***	(0.015)***	(0.099)***	(0.053)***	(0.023)***	(0.020)***	(0.023)***	(0.023)***	(0.021)***	(0.022)***	(0.022)***	(0.023)***
Treated x Pre-grade	0.023	0.021	0.275	0.267	0.056	0.059	0.076	0.085	0.093	0.101	0.085	0.090
	(0.023)	(0.019)	(0.108)**	(0.059)***	(0.031)*	(0.024)**	(0.031)**	(0.027)***	(0.028)***	(0.025)***	(0.031)***	(0.026)***
OLS w pre-specified controls	✓		✓		✓		✓		✓		✓	
Cross-fit partialing out Lasso		✓		✓		✓		✓		✓		✓
Control Mean	0.87		0.04		0.75		0.64		0.61		0.56	
Observations	1,715		1,485		1,697		1,693		1,692		1,672	

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 uneven columns 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 even columns, 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)***
Treat. Ass. x Pre-grade		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		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)
Studied during lockdown	0.240	0.232	0.415	0.004	0.014	0.008
Educ aspirations (years schooling)	0.345	0.384	0.415	0.041	0.066	0.032
Occup aspirations (outside ref window)	0.037	0.101	0.131	0.151	0.171	0.105
Occup aspirations (years schooling)	0.069	0.120	0.166	0.126	0.150	0.183
Believes to reach aspired educ	0.157	0.265	0.193	0.866	0.877	0.973
Believes to reach aspired occup	0.049	0.078	0.166	0.881	0.909	0.973
Attended final exam, grd 9	0.171	0.191	0.328	0.270	0.315	0.157
Final exam grade (std)	0.309	0.300	0.328	0.013	0.004	0.009
High school enrollment	0.019	0.033	0.098	0.052	0.047	0.031
Started grd 11	0.052	0.078	0.067	0.012	0.016	0.009
Attended grd 11 midterm exam	0.039	0.067	0.053	0.002	0.002	0.001
Completed high school	0.129	0.157	0.113	0.007	0.019	0.008

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)		Occup aspirations (years schooling)		Believes to reach aspired educ		Believes to reach aspired occup	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Intention to Treat												
Treatment Assigned	0.035	0.033	0.119	0.115	-0.067	-0.066	0.248	0.245	0.050	0.050	0.068	0.068
	(0.041)	(0.040)	(0.135)	(0.122)	(0.030)**	(0.029)**	(0.136)*	(0.131)*	(0.034)	(0.034)	(0.031)**	(0.031)**
Pre-grade (std)	0.057	0.005	0.435	0.290	-0.009	0.010	0.342	0.257	0.071	0.067	0.063	0.067
	(0.020)***	(0.024)	(0.081)***	(0.096)***	(0.014)	(0.022)	(0.061)***	(0.086)***	(0.015)***	(0.024)***	(0.015)***	(0.020)***
Treat. Ass. x Pre-grade		0.099		0.277		-0.036		0.165		0.008		-0.009
		(0.032)***		(0.124)**		(0.028)		(0.109)		(0.029)		(0.030)
Panel B: Treatment on the Treated												
Treated	0.038	0.030	0.129	0.109	-0.072	-0.070	0.267	0.257	0.054	0.054	0.074	0.074
	(0.043)	(0.043)	(0.143)	(0.132)	(0.032)**	(0.031)**	(0.143)*	(0.139)*	(0.036)	(0.036)	(0.033)**	(0.033)**
Pre-grade (std)	0.056	0.005	0.432	0.290	-0.008	0.010	0.338	0.257	0.070	0.067	0.061	0.067
	(0.020)***	(0.024)	(0.079)***	(0.094)***	(0.013)	(0.021)	(0.059)***	(0.085)***	(0.015)***	(0.023)***	(0.015)***	(0.019)***
Treated x Pre-grade		0.107		0.299		-0.037		0.170		0.006		-0.012
		(0.034)***		(0.134)**		(0.030)		(0.117)		(0.030)		(0.032)
OLS w pre-specified controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	0.42		13.45		0.23		12.80		0.43		0.37	
Observations	1,296		1,317		1,291		1,291		1,291		1,289	

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) $x_{a\|T} = min(x_T)$ $x_{a\|C} = max(x_C)$	(2) $x_{a\|T} = \bar{x}_T - .5\sigma_{xT}$ $x_{a\|C} = \bar{x}_C + .5\sigma_{xC}$	(3) $x_{a\|T} = \bar{x}_T - .25\sigma_{xT}$ $x_{a\|C} = \bar{x}_C + .25\sigma_{xC}$	(4) $x_{a\|T} = \bar{x}_T$ $x_{a\|C} = \bar{x}_C$	(5) $x_{a\|T} = \bar{x}_T + .25\sigma_{xT}$ $x_{a\|C} = \bar{x}_C - .25\sigma_{xC}$	(6) $x_{a\|T} = \bar{x}_T + .5\sigma_{xT}$ $x_{a\|C} = \bar{x}_C - .5\sigma_{xC}$	(7) $x_{a\|T} = max(x_T)$ $x_{a\|C} = min(x_C)$
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)*
Occup aspirations (years schooling)							
Treatment Assigned	0.103	0.209	0.232	0.255	0.278	0.301	0.404
	(0.129)	(0.130)	(0.131)*	(0.132)*	(0.133)**	(0.134)**	(0.141)***
Treatment Assigned	0.082	0.192	0.216	0.240	0.264	0.288	0.394
	(0.125)	(0.127)	(0.128)*	(0.129)*	(0.130)**	(0.131)**	(0.139)***
Treatment Assigned x Pre-grade (std)	0.227	0.178	0.169	0.159	0.150	0.141	0.106
	(0.109)**	(0.102)*	(0.102)	(0.101)	(0.101)	(0.101)	(0.103)
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) $x_{a\|T} = min(x_T)$ $x_{a\|C} = max(x_C)$	(2) $x_{a\|T} = \bar{x}_T - .5\sigma_{xT}$ $x_{a\|C} = \bar{x}_C + .5\sigma_{xC}$	(3) $x_{a\|T} = \bar{x}_T - .25\sigma_{xT}$ $x_{a\|C} = \bar{x}_C + .25\sigma_{xC}$	(4) $x_{a\|T} = \bar{x}_T$ $x_{a\|C} = \bar{x}_C$	(5) $x_{a\|T} = \bar{x}_T + .25\sigma_{xT}$ $x_{a\|C} = \bar{x}_C - .25\sigma_{xC}$	(6) $x_{a\|T} = \bar{x}_T + .5\sigma_{xT}$ $x_{a\|C} = \bar{x}_C - .5\sigma_{xC}$	(7) $x_{a\|T} = max(x_T)$ $x_{a\|C} = min(x_C)$
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	0.071	0.078	0.081	0.084	0.087	0.090	0.096
	(0.043)	(0.043)*	(0.043)*	(0.043)*	(0.043)**	(0.042)**	(0.042)**
Treatment Assigned	0.070	0.077	0.080	0.083	0.086	0.089	0.096
	(0.040)*	(0.040)*	(0.040)*	(0.040)**	(0.040)**	(0.039)**	(0.039)**
Treatment Assigned x Pre-grade (std)	0.073	0.073	0.072	0.072	0.072	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	0.079	0.082	0.085	0.088	0.092	0.098
	(0.042)*	(0.041)*	(0.041)*	(0.041)**	(0.041)**	(0.041)**	(0.040)**
Treatment Assigned	0.071	0.078	0.081	0.084	0.087	0.090	0.097
	(0.038)*	(0.037)**	(0.037)**	(0.037)**	(0.037)**	(0.037)**	(0.036)**
Treatment Assigned x Pre-grade (std)	0.088	0.087	0.087	0.087	0.086	0.086	0.085
	(0.026)***	(0.026)***	(0.026)***	(0.026)***	(0.026)***	(0.026)***	(0.026)***
COMPLETED HIGH SCHOOL							
Treatment Assigned	0.038	0.049	0.055	0.061	0.067	0.073	0.085
	(0.041)	(0.041)	(0.041)	(0.041)	(0.040)	(0.040)*	(0.040)**
Treatment Assigned	0.037	0.048	0.054	0.060	0.066	0.072	0.084
	(0.038)	(0.037)	(0.037)	(0.037)	(0.036)*	(0.036)*	(0.036)**
Treatment Assigned x Pre-grade (std)	0.078	0.079	0.079	0.080	0.081	0.081	0.083
	(0.028)***	(0.028)***	(0.028)***	(0.028)***	(0.027)***	(0.027)***	(0.027)***
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: Balance Table in Restricted Sample

		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	1328	0.54	0.50	691	0.56	0.50	637	0.52	0.50	0.04	0.15	0.08
Age	1328	15.03	1.33	691	15.08	1.36	637	14.98	1.30	0.10	0.60	0.08
Distance to school (km)	1328	3.70	3.81	691	4.09	4.04	637	3.27	3.49	0.83	0.25	0.22
Distance to district town (km)	1328	12.39	7.55	691	10.48	6.62	637	14.46	7.96	-3.98	0.12	-0.54
Distance to high school (km)	1328	10.01	6.83	691	9.37	6.34	637	10.71	7.26	-1.33	0.56	-0.20
Final Exam Grade 8	999	31.89	5.96	561	32.08	6.15	438	31.65	5.71	0.43	0.69	0.07
Pre-grade, main subjects (standardized)	1328	-0.03	0.93	691	-0.15	0.94	637	0.10	0.90	-0.25	0.13	-0.27
Avg absence (Dec&Jan)	1328	1.55	2.05	691	1.66	1.99	637	1.44	2.10	0.21	0.54	0.11
SCHOOL CHARACTERISTICS - ADMIN. DATA												
Class Size	1328	46.84	10.74	691	46.52	12.00	637	47.20	9.18	-0.68	0.88	-0.06
Teacher: Female	1328	0.30	0.46	691	0.26	0.44	637	0.34	0.48	-0.08	0.64	-0.17
Teacher: Age	1328	32.57	5.43	691	30.65	5.57	637	34.65	4.41	-4.00	0.06	-0.80
Teacher: Years of Experience	1328	9.14	5.14	691	7.85	5.43	637	10.55	4.40	-2.70	0.19	-0.55
Teacher: Has University Degree	1328	0.52	0.50	691	0.54	0.50	637	0.51	0.50	0.03	0.88	0.06
Teacher: Log Distance to School (km)	1328	1.80	1.04	691	2.12	0.97	637	1.45	0.99	0.67	0.09	0.68
High school attached	1328	0.15	0.36	691	0.06	0.24	637	0.25	0.44	-0.19	0.18	-0.54
Partnership with Child's Dream	1328	0.73	0.45	691	0.87	0.33	637	0.57	0.50	0.30	0.07	0.71
PARENTAL CHARACTERISTICS - PHONE SURVEY												
Father completed $\leq$ primary educ.	906	0.81	0.39	465	0.83	0.38	441	0.79	0.41	0.03	0.50	0.10
Mother completed $\leq$ primary educ.	968	0.94	0.24	502	0.93	0.25	466	0.94	0.23	-0.01	0.70	-0.04
Any parents is farmer	1031	0.70	0.46	536	0.70	0.46	495	0.70	0.46	0.00	0.96	0.00

Notes: Treatment - Control difference, and p-values are obtained by regressing variable of interest on a treatment dummy 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 A.12: Treatment Effects & Heterogeneities in Restricted Sample (1)

	Studied during lock down		Educ aspirations (years schooling)		Occup aspirations (outside ref window)		Occup aspirations (years schooling)		Believes to reach aspired educ		Believes to reach aspired occup	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Intention to Treat												
Treatment Assigned	-0.017	-0.028	0.214	0.190	-0.051	-0.047	0.208	0.202	0.026	0.027	0.011	0.010
	(0.037)	(0.032)	(0.163)	(0.150)	(0.046)	(0.045)	(0.182)	(0.184)	(0.050)	(0.049)	(0.035)	(0.034)
Pre-grade (std)	0.076	0.008	0.529	0.366	-0.012	0.012	0.417	0.381	0.078	0.085	0.056	0.053
	(0.023)***	(0.029)	(0.085)***	(0.119)***	(0.016)	(0.025)	(0.064)***	(0.109)***	(0.016)***	(0.026)***	(0.017)***	(0.022)**
Treat. Ass. x Pre-grade		0.116		0.279		-0.041		0.063		-0.012		0.006
		(0.038)***		(0.148)*		(0.033)		(0.142)		(0.029)		(0.029)
Panel B: Treatment on the Treated												
Treated	-0.019	-0.035	0.230	0.192	-0.054	-0.049	0.221	0.213	0.027	0.029	0.011	0.011
	(0.039)	(0.035)	(0.170)	(0.159)	(0.048)	(0.048)	(0.188)	(0.192)	(0.052)	(0.051)	(0.036)	(0.035)
Pre-grade (std)	0.077	0.009	0.523	0.367	-0.011	0.012	0.413	0.381	0.077	0.085	0.056	0.053
	(0.022)***	(0.028)	(0.082)***	(0.115)***	(0.015)	(0.024)	(0.061)***	(0.106)***	(0.015)***	(0.025)***	(0.016)***	(0.021)**
Treated x Pre-grade		0.125		0.292		-0.042		0.060		-0.014		0.006
		(0.040)***		(0.157)*		(0.035)		(0.149)		(0.030)		(0.030)
Control Mean	0.43		13.38		0.20		12.84		0.43		0.37	
Observations	1,009		1,024		1,002		1,002		1,006		1,002	

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.)) and school controls (teacher age, teacher distance from school (log), Child's Dream partnership), as well as district fixed effects are included in all specifications. */**/*** denote significance levels at 10/5/1 percent respectively.

Table A.13: Treatment Effects & Heterogeneities in Restricted Sample (2)

	Attended final exam		Final exam grade (std.)		High school enrollment		Started grd 11		Attended grd 11 midterm exam		Completed high school	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Intention to Treat												
Treatment Assigned	0.082	0.082	0.264	0.249	0.135	0.135	0.129	0.128	0.121	0.119	0.106	0.104
	(0.025)***	(0.025)***	(0.146)*	(0.145)*	(0.037)***	(0.036)***	(0.039)***	(0.037)***	(0.042)***	(0.039)***	(0.041)**	(0.040)**
Pre-grade (std)	0.073	0.078	0.813	0.725	0.146	0.122	0.187	0.152	0.199	0.158	0.215	0.181
	(0.013)***	(0.023)***	(0.050)***	(0.069)***	(0.018)***	(0.034)***	(0.016)***	(0.029)***	(0.015)***	(0.029)***	(0.017)***	(0.029)***
Treat. Ass. x Pre-grade		-0.008		0.152		0.042		0.062		0.072		0.059
		(0.025)		(0.098)		(0.038)		(0.034)*		(0.035)*		(0.037)
Panel B: Treatment on the Treated												
Treated	0.088	0.089	0.280	0.260	0.145	0.143	0.138	0.135	0.130	0.125	0.114	0.110
	(0.027)***	(0.028)***	(0.151)*	(0.151)*	(0.039)***	(0.039)***	(0.041)***	(0.039)***	(0.044)***	(0.041)***	(0.043)***	(0.042)***
Pre-grade (std)	0.070	0.078	0.808	0.725	0.142	0.122	0.184	0.153	0.195	0.158	0.212	0.182
	(0.013)***	(0.022)***	(0.049)***	(0.067)***	(0.017)***	(0.033)***	(0.016)***	(0.028)***	(0.015)***	(0.028)***	(0.016)***	(0.028)***
Treated x Pre-grade		-0.014		0.152		0.038		0.060		0.071		0.059
		(0.028)		(0.100)		(0.040)		(0.036)*		(0.037)*		(0.039)
Control Mean	0.87		-0.06		0.74		0.63		0.61		0.56	
Observations	1,328		1,157		1,313		1,309		1,308		1,294	

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.)) and school controls (teacher age, teacher distance from school (log), Child's Dream partnership), as well as district fixed effects are included in all specifications. */**/*** denote significance levels at 10/5/1 percent respectively.

Table A.14: Household-level Correlates of Academic 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.15: School-level Correlates of Academic Performance

	Class size	Teacher is female	Teacher age	Teacher experience	Teacher-school distance (log)	High school attached	Partnership w Child's Dream
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Pre-grade (std)	0.162	0.001	0.899	0.970	-0.041	-0.033	0.002
	(0.689)	(0.036)	(0.529)*	(0.471)**	(0.068)	(0.029)	(0.029)
Female	-0.356	0.015	-0.067	-0.150	0.002	0.019	-0.020
	(0.468)	(0.021)	(0.273)	(0.261)	(0.042)	(0.018)	(0.015)
Age	-0.452	0.003	-0.042	-0.021	0.008	0.009	-0.008
	(0.298)	(0.011)	(0.205)	(0.190)	(0.024)	(0.008)	(0.007)
Mean	45.71	0.33	32.42	9.30	1.57	0.17	0.77
Observations	1,715	1,715	1,715	1,715	1,715	1,715	1,715

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.16: Treatment Effect Heterogeneities by School Performance vs. Parental Characteristics (1)

	Studied during lock down		Educ aspirations (years schooling)		Occup aspirations (outside ref window)		Occup aspirations (years schooling)		Believes to reach aspired educ		Believes to reach aspired occup	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Intention to Treat												
Treatment Assigned	0.033	-0.001	0.113	0.420	-0.060	-0.066	0.203	0.306	0.062	0.061	0.060	0.016
	(0.043)	(0.087)	(0.120)	(0.311)	(0.029)**	(0.061)	(0.135)	(0.274)	(0.032)*	(0.057)	(0.031)*	(0.066)
Pre-grade (std)	0.004	0.006	0.298	0.302	0.019	0.020	0.222	0.223	0.074	0.072	0.062	0.062
	(0.024)	(0.025)	(0.098)***	(0.096)***	(0.021)	(0.022)	(0.090)**	(0.091)**	(0.025)***	(0.024)***	(0.020)***	(0.019)***
Treat. Ass. x Pre-grade	0.100	0.101	0.297	0.281	-0.042	-0.043	0.187	0.181	0.001	0.000	-0.007	-0.007
	(0.032)***	(0.034)***	(0.129)**	(0.126)**	(0.028)	(0.028)	(0.107)*	(0.106)*	(0.029)	(0.028)	(0.032)	(0.030)
Panel B: Treatment on the Treated												
Treated	0.030	-0.010	0.107	0.434	-0.063	-0.069	0.210	0.318	0.067	0.067	0.065	0.017
	(0.047)	(0.092)	(0.129)	(0.325)	(0.031)**	(0.065)	(0.144)	(0.295)	(0.034)**	(0.059)	(0.032)**	(0.070)
Pre-grade (std)	0.004	0.006	0.297	0.300	0.019	0.020	0.222	0.222	0.074	0.072	0.063	0.063
	(0.023)	(0.025)	(0.096)***	(0.094)***	(0.020)	(0.021)	(0.089)**	(0.089)**	(0.025)***	(0.024)***	(0.020)***	(0.019)***
Treated x Pre-grade	0.107	0.109	0.319	0.301	-0.043	-0.045	0.196	0.189	-0.002	-0.002	-0.010	-0.011
	(0.035)***	(0.036)***	(0.137)**	(0.136)**	(0.029)	(0.029)	(0.115)*	(0.113)*	(0.031)	(0.029)	(0.033)	(0.032)
Pre-specified controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Treat. x parent chars.		✓		✓		✓		✓		✓		✓
Control Mean	0.43		13.48		0.23		12.81		0.43		0.37	
Observations	1,245		1,265		1,244		1,244		1,240		1,243	

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 in all columns as pre-specified. Even columns additionally control for parental education, job loss during COVID-19 and income loss during COVID-19, all interacted with treatment status. */**/*** denote significance levels at 10/5/1 percent respectively.

Table A.17: Treatment Effect Heterogeneities by School Performance vs. Parental Characteristics (2)

	Attended final exam		Final exam grade (std.)		High school enrollment		Started grd 11		Attended grd 11 midterm exam		Completed high school	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Intention to Treat												
Treatment Assigned	0.011	-0.044	0.087	0.166	0.025	-0.037	0.027	-0.069	0.033	-0.052	0.032	-0.059
	(0.025)	(0.042)	(0.147)	(0.153)	(0.035)	(0.058)	(0.040)	(0.055)	(0.038)	(0.058)	(0.036)	(0.056)
Pre-grade (std)	0.040	0.040	0.533	0.532	0.067	0.062	0.095	0.088	0.108	0.101	0.139	0.131
	(0.014)***	(0.013)***	(0.105)***	(0.105)***	(0.021)***	(0.020)***	(0.023)***	(0.022)***	(0.022)***	(0.021)***	(0.022)***	(0.020)***
Treat. Ass. x Pre-grade	-0.010	-0.011	0.313	0.312	0.052	0.055	0.066	0.071	0.080	0.084	0.057	0.063
	(0.018)	(0.018)	(0.104)***	(0.103)***	(0.026)*	(0.026)**	(0.029)**	(0.028)**	(0.028)***	(0.028)***	(0.029)*	(0.028)**
Panel B: Treatment on the Treated												
Treated	0.013	-0.046	0.083	0.151	0.024	-0.043	0.026	-0.080	0.032	-0.062	0.032	-0.068
	(0.027)	(0.045)	(0.155)	(0.161)	(0.037)	(0.060)	(0.042)	(0.056)	(0.041)	(0.059)	(0.038)	(0.058)
Pre-grade (std)	0.040	0.040	0.533	0.532	0.067	0.062	0.095	0.088	0.108	0.101	0.138	0.131
	(0.013)***	(0.013)***	(0.103)***	(0.103)***	(0.020)***	(0.020)***	(0.023)***	(0.022)***	(0.022)***	(0.020)***	(0.021)***	(0.020)***
Treated x Pre-grade	-0.011	-0.012	0.331	0.329	0.056	0.060	0.071	0.077	0.086	0.091	0.061	0.068
	(0.020)	(0.020)	(0.110)***	(0.109)***	(0.028)**	(0.027)**	(0.031)**	(0.030)**	(0.030)***	(0.029)***	(0.031)*	(0.030)**
Pre-specified controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Treat. x parent chars.		✓		✓		✓		✓		✓		✓
Control Mean	0.92		0.12		0.82		0.72		0.68		0.62	
Observations	1,275		1,167		1,261		1,257		1,257		1,240	

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 in all columns as pre-specified. Even columns additionally control for parental education, job loss during COVID-19 and income loss during COVID-19, all interacted with treatment status. */**/*** denote significance levels at 10/5/1 percent respectively.

Table A.18: Treatment Effect Heterogeneities by Academic Performance vs. Individual, Teacher and School Characteristics (1)

	Studied during lock down		Educ aspirations (years schooling)		Occup aspirations (outside ref window)		Occup aspirations (years schooling)		Believes to reach aspired educ		Believes to reach aspired occup	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Intention to Treat												
Treatment Assigned	0.038	-0.485	0.102	1.192	-0.060	0.386	0.236	0.419	0.050	-0.223	0.064	-0.170
	(0.040)	(0.412)	(0.124)	(1.041)	(0.029)**	(0.232)	(0.131)*	(1.499)	(0.034)	(0.279)	(0.030)**	(0.215)
Pre-grade (std)	0.004	-0.011	0.300	0.307	0.014	0.019	0.252	0.217	0.069	0.070	0.063	0.051
	(0.024)	(0.027)	(0.096)***	(0.102)***	(0.021)	(0.019)	(0.085)***	(0.080)**	(0.024)***	(0.023)***	(0.019)***	(0.019)**
Treat. Ass. x Pre-grade	0.099	0.126	0.264	0.301	-0.039	-0.051	0.163	0.272	0.005	-0.005	-0.004	0.008
	(0.032)***	(0.035)***	(0.125)**	(0.143)**	(0.027)	(0.026)*	(0.104)	(0.112)**	(0.028)	(0.029)	(0.029)	(0.029)
Panel B: Treatment on the Treated												
Treated	0.036	-0.575	0.096	1.357	-0.062	0.408	0.247	0.508	0.054	-0.264	0.070	-0.213
	(0.044)	(0.455)	(0.134)	(1.125)	(0.031)**	(0.243)*	(0.140)*	(1.574)	(0.035)	(0.307)	(0.032)**	(0.236)
Pre-grade (std)	0.004	-0.011	0.299	0.307	0.014	0.020	0.252	0.216	0.069	0.071	0.063	0.051
	(0.024)	(0.026)	(0.094)***	(0.100)***	(0.021)	(0.019)	(0.084)***	(0.078)***	(0.023)***	(0.023)***	(0.019)***	(0.019)***
Treated x Pre-grade	0.106	0.134	0.285	0.316	-0.040	-0.054	0.167	0.280	0.003	-0.006	-0.007	0.007
	(0.035)***	(0.037)***	(0.135)**	(0.151)**	(0.029)	(0.027)**	(0.111)	(0.119)**	(0.030)	(0.031)	(0.031)	(0.031)
Pre-specified controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Treat. x other chars.		✓		✓		✓		✓		✓		✓
Control Mean	0.42		13.45		0.23		12.80		0.43		0.37	
Observations	1,296		1,317		1,291		1,291		1,291		1,289	

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 in all columns as pre-specified. Even columns additionally control for student gender, distance to school and distance to district town, teacher age, experience, distance to school (log), and school partnership with CD, all interacted with treatment status. */**/*** denote significance levels at 10/5/1 percent respectively.

Table A.19: Treatment Effect Heterogeneities by Academic Performance vs. Individual, Teacher and School Characteristics (2)

	Attended final exam		Final exam grade (std.)		High school enrollment		Started grd 11		Attended grd 11 midterm exam		Completed high school	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Intention to Treat												
Treatment Assigned	0.039	-0.027	0.131	-0.702	0.087	0.094	0.085	0.217	0.086	0.314	0.063	0.189
	(0.028)	(0.227)	(0.145)	(0.786)	(0.034)**	(0.261)	(0.040)**	(0.279)	(0.037)**	(0.291)	(0.037)	(0.353)
Pre-grade (std)	0.046	0.044	0.557	0.602	0.088	0.096	0.113	0.126	0.119	0.129	0.140	0.146
	(0.017)**	(0.018)**	(0.100)***	(0.095)***	(0.024)***	(0.023)***	(0.024)***	(0.020)***	(0.022)***	(0.020)***	(0.023)***	(0.019)***
Treat. Ass. x Pre-grade	0.023	0.030	0.260	0.218	0.055	0.054	0.073	0.064	0.088	0.080	0.080	0.075
	(0.020)	(0.020)	(0.099)**	(0.107)**	(0.027)*	(0.027)*	(0.028)**	(0.025)**	(0.026)***	(0.024)***	(0.028)***	(0.025)***
Panel B: Treatment on the Treated												
Treated	0.042	0.387	0.128	-0.381	0.092	0.600	0.089	0.664	0.089	0.668	0.064	0.526
	(0.031)	(0.239)	(0.155)	(0.923)	(0.037)**	(0.261)**	(0.043)**	(0.278)**	(0.040)**	(0.295)**	(0.040)	(0.357)
Pre-grade (std)	0.046	0.047	0.558	0.612	0.088	0.100	0.113	0.129	0.119	0.132	0.140	0.149
	(0.017)***	(0.018)***	(0.099)***	(0.092)***	(0.023)***	(0.022)***	(0.023)***	(0.019)***	(0.021)***	(0.019)***	(0.022)***	(0.018)***
Treated x Pre-grade	0.023	0.034	0.275	0.216	0.056	0.056	0.076	0.071	0.093	0.087	0.085	0.083
	(0.023)	(0.023)	(0.108)**	(0.111)*	(0.031)*	(0.029)*	(0.031)**	(0.026)***	(0.028)***	(0.026)***	(0.031)***	(0.027)***
Pre-specified controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Treat. x other chars.		✓		✓		✓		✓		✓		✓
Control Mean	0.87		0.04		0.75		0.64		0.61		0.56	
Observations	1,715		1,485		1,697		1,693		1,692		1,672	

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 in all columns as pre-specified. Even columns additionally control for student gender, distance to school and distance to district town, teacher age, experience, distance to school (log), and school partnership with CD, all interacted with treatment status. */**/*** denote significance levels at 10/5/1 percent respectively.

Table A.20: Information Acquisition 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	-0.035	0.004	0.006	0.029	0.030
	(0.013)***	(0.013)***	(0.008)	(0.008)	(0.013)**	(0.013)**
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.21: 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)
Treat. Ass. x Pre-grade		-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		-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

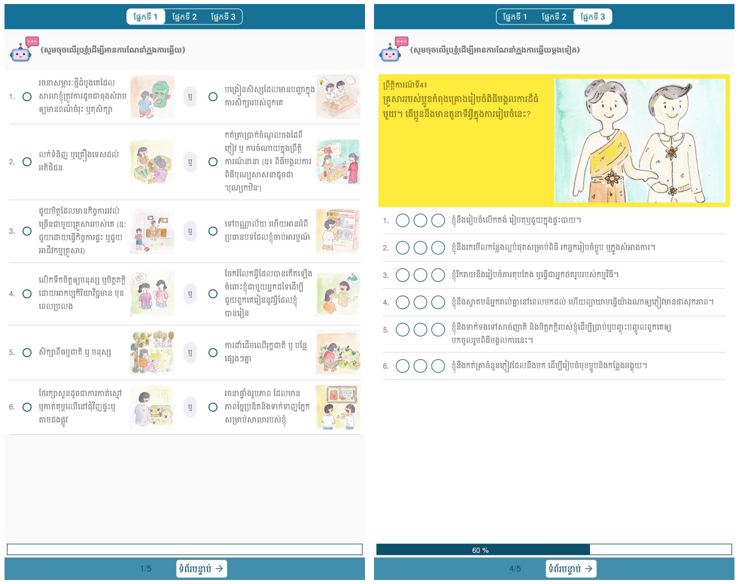
The interest exploration tool (IET) builds on Holland's theory of vocational interest (Holland, 1997), and is designed to help students reflect on their personal interests and to reveal to students which personality types they display. In his hexagonal model, Holland (1997) identifies six personality 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 (see van Iddekinge et al., 2011, for a meta-anlaysis).

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.

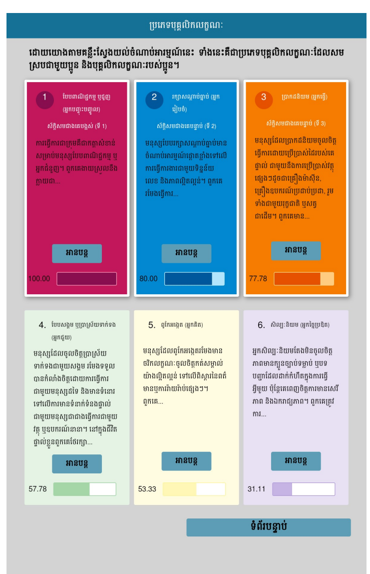
After the tests are completed, the app reveals the ordering of personality types corresponding to a student's answers in the tests, with the strongest personality type being shown first. In addition to the ordering, students are revealed a personalized score per personality type representing the relative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

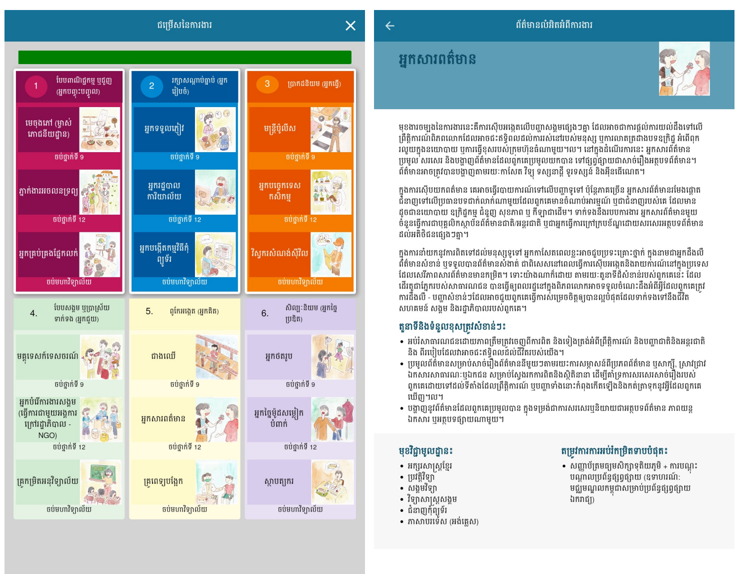


B.2 Career Exploration Tool

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

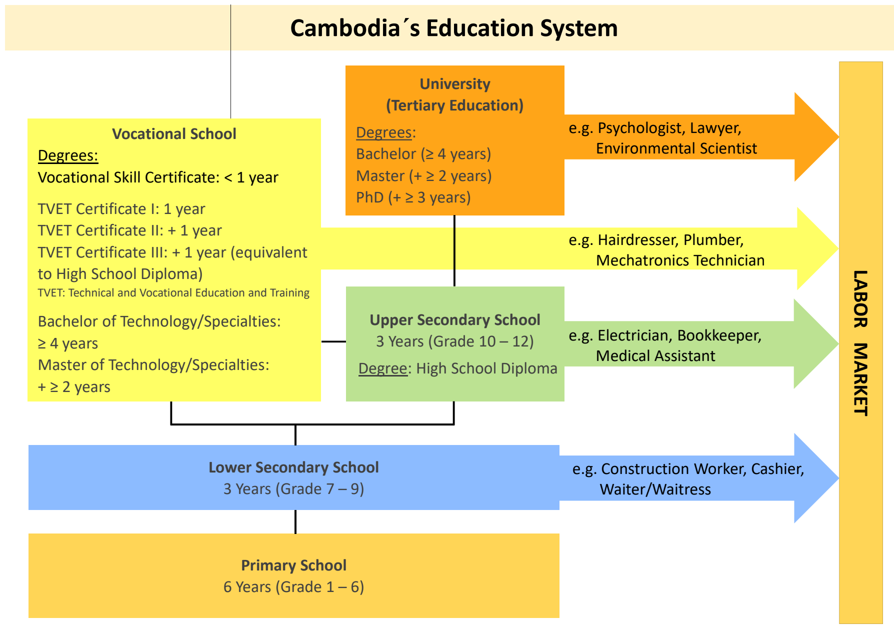
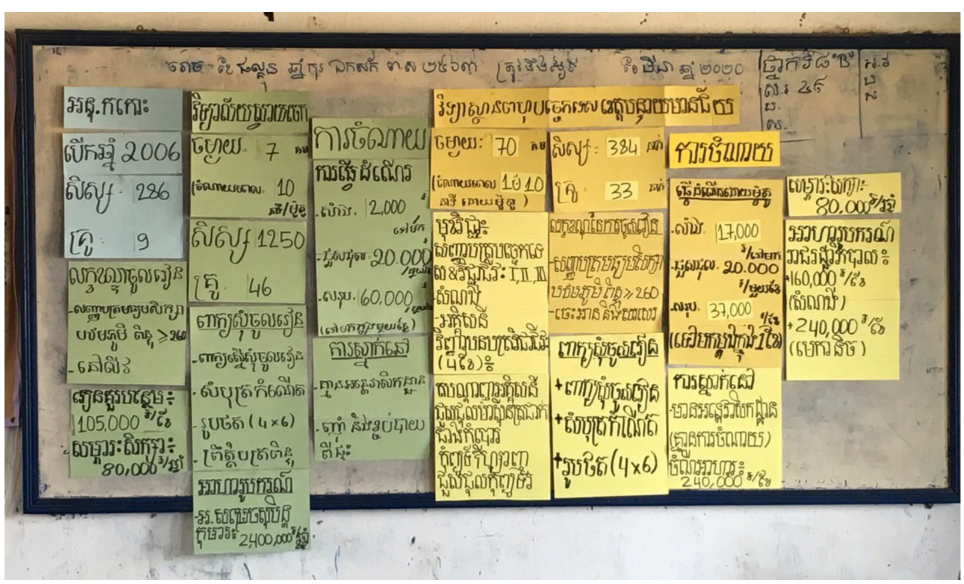


Figure B.5: Example of Display of Information about High and Vocational School



B.4 Randomization within schools

In treatment schools, we randomly allocated students into one of three treatment arms: the main treatment arm (A1), placebo arm (A2), and information-only (A3), with the respective chances of 2:2:1. Randomization was done on the day of the intervention, by havings students draw numbered badges from a box. While students in A1 participated in all three parts of the intervention, students in A2 only received the job information and attended the school information session, and students in A3 participated 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 outdoors
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 outdoors
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 A3, *i.e.*, students that participated in the information session only, are more likely to be female, were overall performing better in school 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. Treatment status was determined based on a number that students had blindly drawn from a box at the beginning of the workshop, the number was directly recorded and could not be changed during the workshop. Most likely, randomization was unsuccessful by pure chance; potentially a result of the reduced sample size.

Table B.3: Balance Table Experiment

Variable	(1) Mean A1	(2) Mean A2	(3) Mean A3	(4) A1 - A2	(5) A1 - A3	(6) A2 - A3
Female	0.53	0.54	0.66	-0.01	-0.13***	-0.12**
	(0.50)	(0.50)	(0.48)	(0.86)	(0.01)	(0.01)
Age	15.11	15.05	15.03	0.06	0.08	0.02
	(1.32)	(1.32)	(1.36)	(0.57)	(0.54)	(0.87)
Distance to school (km)	3.98	3.99	4.21	-0.01	-0.22	-0.22
	(3.86)	(4.02)	(4.28)	(0.99)	(0.58)	(0.60)
Distance to district town (km)	9.96	9.74	9.74	0.22	0.22	0.00
	(6.47)	(6.45)	(6.44)	(0.68)	(0.73)	(1.00)
Distance to high school (km)	9.33	9.27	9.17	0.06	0.17	0.10
	(6.59)	(6.37)	(6.40)	(0.90)	(0.79)	(0.87)
Pre-grade, main subjects (standardized)	-0.31	-0.15	0.05	-0.16**	-0.36***	-0.20**
	(0.90)	(0.96)	(1.02)	(0.03)	(0.00)	(0.04)
Avg absence (Dec&Jan)	1.63	1.59	1.28	0.05	0.36**	0.31*
	(1.88)	(1.96)	(1.45)	(0.76)	(0.02)	(0.05)
Observations	315	311	151	626	466	462

Notes: This analysis omits 6 students out of the 783 who came late to the intervention, and were allowed to participate in the activities of the control group, but not randomly allocated to any of the treatment arms. (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

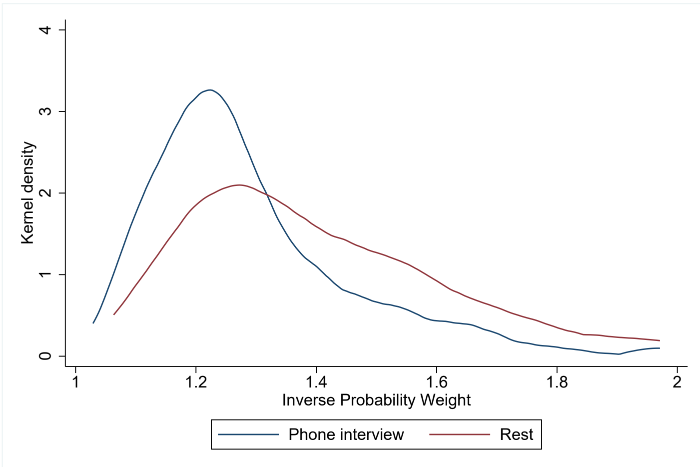
As discussed in Section 3.2, the phone-survey sample is positively selected on female and better performing students. To make the sample of interviewed students within the phone survey comparable to the full sample, we therefore construct survey weights. The weights are estimated by a logistic regression which includes treatment status, student, school and teacher characteristics, and interactions thereof. The regression output is shown in Table C.1. 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.1: 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.