Beyond Beliefs: Experimental Evidence on Follow-Through Frictions at School Transitions*

Carla Coccia† Eric V. Edmonds‡ Martina Jakob§ Mauricio Romero¶

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Abstract

A key puzzle in economics is why households underinvest in education despite high returns. This paper highlights the role of follow-through frictions using a school-level field experiment across 231 primary schools in Quiché, Guatemala. First, we document a large intention-action gap: 65% of students plan to enroll in lower secondary school, but 34% of planners fail to follow through, and 45% of all non-enrollment takes place among planners. We randomly assign schools to a control, an information treatment, and a mentoring treatment that delivers identical informational content along with individualized counseling and classroom-based socio-emotional skill-building. Information provision does not affect enrollment. Mentoring increases enrollment by 7.6 percentage points relative to information (an increase of 15% relative to a mean of 49.6 percent for our 13 year olds), with effects driven by students who already planned to enroll.
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†Paris School of Economics; E-mail: carla.coccia@unibe.ch

‡Dartmouth College, BREAD, IZA, NBER ; E-mail: eric.v.edmonds@dartmouth.edu

§University of Zurich; E-mail: martina.jakob@econ.uzh.ch

¶ITAM, BREAD, IZA; E-mail: mtromero@itam.mx

1 Introduction

In low- and middle-income countries, significant losses in school progression take place during transitions from primary to lower secondary and from lower to upper secondary, even when tuition is free and wage premiums are substantial (UNICEF, 2021; Adelman and Székely, 2017). A frequent explanation is that information frictions lead households to hold inaccurate beliefs about the returns, costs, or processes required for continued schooling, resulting in underinvestment. A growing body of experimental work provides information to correct these beliefs, with mixed results on enrollment (Avitabile and De Hoyos, 2018; Dinkelman and Martínez A, 2014; Dizon-Ross, 2019; Jensen, 2010; Nguyen, 2008).

Even when information is complete and correct, students and families may fail to act on their intentions. We define *follow-through frictions* as the set of reasons agents may fail to enroll despite their intent: inertia, present bias, limited rationality, self-control issues, administrative complexity, and insufficient social support (O'Donoghue and Rabin, 1999; Bhargava and Manoli, 2015; Alan et al., 2021). Despite their potential importance, the role of follow-through frictions in educational decision-making remains poorly understood, because supporting students in executing their plans typically requires sharing information, complicating the disentangling of these two channels.

We begin by establishing the prevalence of follow-through frictions in the department of Quiché, Guatemala, where approximately half of students fail to transition to lower secondary school. In our baseline survey collected in the first half of grade 6, the final year of primary, 65% of control subjects reported plans to enroll in lower secondary school the next academic year. In reality, half enroll. Among those with plans to enroll, 34% do not follow through, and 45% of all non-enrollment occurs among students who planned to enroll. We observe a similar pattern even two months before the start of lower-secondary, so this intention-action gap is unlikely to reflect errors owing to the length of the planning horizon. This gap is also unlikely to reflect information frictions, as students hold positive beliefs about returns to schooling at baseline, and 83% say they would enroll in lower secondary if it were solely up to them.

Using a school-level randomized controlled trial involving 231 primary schools and 3,273 sixth-grade students in Quiché we examined the role of information and follow-through frictions on educational enrollment. Schools are randomly assigned to one of three arms: (i) control, (ii) an information treatment that provides students and caregivers with sessions on schooling options, returns to education, and enrollment logistics, and (iii) a mentoring treatment that delivers the same informational content while additionally offering individualized support and building socio-emotional skills through in-school mentors. Compliance is high, with students in both treatment groups attending an average of 88 percent of sessions. We interpret the information–control difference as the effect of changing information and the mentoring–information difference as the effect of relaxing follow-through frictions. This interpretation relies on the assumption that the informational content delivered to and absorbed by the two treatment groups is the same on average, meaning the mentoring - information comparison reflect differences in follow-through support. We label this identification assumption *content parity*. Consistent with this assumption, in a series of survey questions, we observe that children in both treatment groups retain the same relevant information (see Section 4.2).

Information has no detectable effect on any educational outcome compared to the control group. The information–control contrast on school enrollment is -2.0 percentage points (SE 3.4), and the 95 percent confidence interval excludes gains 5 percentage points or larger (from a base of 52 percent). Our simple conceptual framework predicts that information should work on the intention margin and have no effect for students who already intend to enroll. In our case, we find no effect on the intention margin, a result we attribute to unexpectedly high beliefs about the value of education.

In contrast, adding mentoring to this informational content increases enrollment by 7.6 percentage points ($p < 0.05$) relative to information, a 15% increase from the information-arm base of 49.6% (control mean 51.6 percent). Follow-through support works on the implementation margin and should concentrate among those intending to attend school, especially given that information does not appear to change that intent. We prespecified

heterogeneity along student enrollment plans at baseline(Coccia et al., 2024). Among students with baseline plans to enroll, mentoring raises enrollment from 66.7% in the control group (66.4% in the information arm) to 73.7% while enrollment among non-planners is 21–23% in all three arms. The fact that mentoring effects are concentrated among planners implies that the mechanism of our mentoring effects is the resolution of follow-through frictions (in our conceptualization of the enrollment problem).

To evaluate potential channels behind this resolving follow-through frictions mechanisms, we follow our pre-analysis plan, which prespecified different potential ways our interventions may influence enrollment as well as how to measure these channels. Consistent with content parity, mentoring arm students are no more likely to recall the informational sessions or their content, understand relevant education laws, or update beliefs about schooling returns compared to information-only students, but mentoring students are significantly more likely to recall the socio-emotional topics taught only in the mentoring arm. In short, the arms differ in terms of student recall where they should and, as far as we can measure, only there. We also observe evidence that mentoring improved soft-skills as assessed by our enumerators. Mentoring arm students are more confident and successful in communicating with an unfamiliar adult, and these skills which improve in planners and non-planners might translate into planners being better able to advocate for fulfilling their enrollment plans. Of course, this channel for how mentoring might resolve follow through frictions is speculative and not identified by experimental design.

This paper makes four contributions. First, we document that a large share of failed school transitions occurs among students who intended to make them. The literature on school transitions in low- and middle-income countries has asked why students do not want, cannot afford, or cannot access further schooling, emphasizing direct costs, low learning levels, opportunity costs, gender-specific constraints, and information frictions (Case and Deaton, 1999; Duryea and Arends-Kuenning, 2003; Duflo et al., 2021). These explanations locate a failure at the moment the enrollment planning decision is made that propagates through to enrollment, and we show that in Quiché much of it occurs after: 45

percent of all non-enrollment is among students who reported plans to enroll, and a similar pattern holds when plans are measured two months before classes begin, so the gap is unlikely to reflect forecast error over a long planning horizon. Because we measure enrollment in the universe of public and private school records in Guatemala, these magnitudes are free of attrition and do not rely on self-reports. The closest analog is summer melt among college-intending high school graduates in the United States (Castleman et al., 2014). We show that the phenomenon extends to a compulsory-schooling-age, zero-tuition transition in a low-income setting, and that it accounts for nearly half of the aggregate enrollment shortfall. This finding has substantive value for future interventions aimed at promoting schooling. A single survey question on enrollment plans predicts a 41% spread in enrollment and predicts treatment heterogeneity perfectly. These findings raise the prospect of considerable efficiency gains in program delivery by targeting these planners.

Second, our findings help explain some recent findings in research providing information around schooling and mentoring. Existing evidence either varies information while holding support fixed (Jensen, 2010; Nguyen, 2008; Dinkelman and Martínez A, 2014; Avitabile and De Hoyos, 2018) or evaluates counseling, mentoring, and skill-building programs that bundle the two (Loyalka et al., 2013; Carrell and Sacerdote, 2017; Alan et al., 2019; Edmonds et al., 2023; Hayashi et al., 2025). In our design, we provide identical informational content in both treatment arms — which we verify via recall, knowledge, and belief measures, including a placebo check on content taught only in the mentoring arm — and show the mentoring component is key in our setting. Information alone has no detectable effect. The information–control contrast on enrollment is -2.0 percentage points (SE 3.4), excluding gains of 5 percentage points or more from a base of 52 percent. Follow-through support to the same content raises enrollment by 7.6 percentage points ($p < 0.05$), 15 percent over the information arm. This asymmetry suggests a reading of the mixed record of information interventions: information can only bind where beliefs are wrong, and in our setting biased beliefs do not seem to be discouraging schooling plans.

Third, our study design allows us to locate a binding friction for school enrollment in

our theater. The behavioral literature on incomplete take-up documents intention-action failures in repeated, low-stakes, or easily reversed decisions: retirement contributions (Madrian and Shea, 2001), tax and benefit claims (Bhargava and Manoli, 2015; Finkelstein and Notowidigdo, 2019), and fertilizer purchases (Duflo et al., 2011). A schooling transition is the opposite case (one-shot, high-stakes, and difficult to reverse), precisely the type of decision where deliberate choice should dominate and inertia should matter least. We nonetheless find the signature of an implementation friction rather than a friction in preferences or beliefs. Our framework predicts that follow-through support acts on the implementation margin, and therefore largely among students who already intend to enroll; information acts on the intention margin. Both predictions hold. Mentoring raises enrollment among baseline planners from 66.7 percent in the control group (66.4 percent in the information arm) to 73.7 percent, while enrollment among non-planners is 21 to 23 percent in all three arms, and no arm shifts stated intentions. The frictions that generate intention-action gaps in routine, low-stakes choices are thus also operative in the high-stakes decisions that determine human capital accumulation.

Fourth, the intervention that closes the gap is not the standard remedy for high dropout rates at school transitions. Responses to incomplete take-up typically alter the choice environment: lowering the price (Duflo et al., 2021), or reducing hassle costs through simplification, reminders, and assistance with paperwork (Damgaard and Nielsen, 2018; Castleman et al., 2014). Mentoring leaves the enrollment process, prices, and household resources unchanged, and instead raises the student's capacity to act within an unchanged environment. Our experimental design does not identify the channels behind this resolution of follow-through frictions, but they are consistent with this interpretation of our findings. Mentored students are more confident and more successful in communicating with an unfamiliar adult. These gains appear among planners and non-planners alike, but plausibly change enrollment only for students who have a plan to advocate for (à la Ashraf et al. (2020)). This connects the take-up literature to evidence that socio-emotional skills matter for life outcomes (Heckman et al., 2006) and are malleable (Alan and Ertac, 2018; Alan

et al., 2019; Sarzosa and Urzúa, 2021), and suggests that such skills are a viable technology for closing the gap between educational intentions and actions, one that remains available where the price of schooling is already zero.

2 Research Design

2.1 Conceptual Framework and Identification

Educational underinvestment at key transition points is difficult to reconcile with the standard human capital model, which posits that individuals invest in schooling until the expected marginal benefit equals the marginal cost (Ben-Porath, 1967). We distinguish between two classes of frictions that can account for this pattern. First, students may face *information frictions*: they hold incomplete or inaccurate beliefs about the returns to schooling, its costs, or the feasibility of continuation (Jensen, 2010; Dominitz and Manski, 1994; Attanasio and Kaufmann, 2014). Second, students may face *follow-through frictions*: even with accurate beliefs, they may fail to act due to inertia, administrative complexity, or planning and self-control issues (O'Donoghue and Rabin, 1999; Madrian and Shea, 2001; Bhargava and Manoli, 2015).

We organize the design around a two-stage decision with a planning stage and an execution stage. At the planning stage during grade 6, student i (and their family) evaluates the option of continuing to lower secondary school. Let $V_i(\mathcal{I}_i)$ denote the perceived net value of continuing given information set $\mathcal{I}_i$. At the execution stage, enrollment requires completing a set of tasks within a fixed window: assembling documents, registering, arranging attendance, and managing family responsibilities in the new schooling context. Completing these tasks incurs an immediate effort cost e_i, drawn at the execution stage from a distribution $F(e \mid C_i)$ that depends on the student's follow-through capacity C_i, which includes her planning and self-regulation skills, confidence in approaching a new institution, and available adult support. Higher capacity shifts F toward lower costs.

A student intends to enroll when the planning-stage favors continuation, $V_i \geq \bar{e}_i(C_i)$,

where $\bar{e}_i$ is the anticipated execution cost. She enrolls when she completes the tasks at the execution stage, which occurs if $\beta V_i \geq e_i$, where $\beta \leq 1$ captures the wedge between how delayed benefits are weighed when planning and when acting (O'Donoghue and Rabin, 1999). Follow-through frictions arise from $\beta < 1$, higher-than-anticipated execution costs ($e_i > \bar{e}_i$), or both: a student whose plan is optimal at the planning stage fails to enroll when the realized, immediate cost of executing the transition exceeds the present-biased value she places on it at the moment of action. The intention-action gap among intenders is $\Pr(e_i > \beta V_i \mid V_i \geq \bar{e})$, which is zero in the neoclassical benchmark ($\beta = 1$ and execution costs known in advance).

The two treatment arms correspond to the two decision stages. We think of the information arm as shifting beliefs, and hence $V_i(\mathcal{I}_i)$, while leaving C_i unchanged; the mentoring arm delivers the same informational content and additionally raises C_i (and therefore the old $F(e|)$ is stochastically dominated by the new one). The mentoring–information contrast identifies the effect of relaxing follow-through frictions if the informational content delivered to and absorbed by the mentoring and information arms is the same, so that the two arms have the same $V_i(\mathcal{I}_i)$ in expectation and differ only in follow-through support. This is *content parity*, which has two components. *Delivery parity* is imposed by design: the same materials, topics, posters, leaflets, and comics are utilized in both arms, and mentors were trained not to extend beyond the common curriculum. *Absorption parity* is not guaranteed by design because the mentoring arm's longer, more interactive format could increase retention of the common content. Absorption parity is testable, and Section 4.2 reports direct tests using recall, knowledge, and belief measures, including a placebo check on content taught only in the mentoring arm. If mentoring raised absorption of the common content, part of the mentoring–information contrast would reflect information rather than follow-through support; while we find no evidence of this, the mentoring–information contrast should be read jointly with the information parity evidence.

This two-stage framework has implications for who is the marginal child influenced by each treatment. Information moves students whose perceived value V_i is near the intention

threshold. Its predicted effect on enrollment is limited on two counts. These students have lower V_i than current planners, and therefore complete the execution stage at below-average rates. For students who already intend to enroll, information matters only to the extent that it changes beliefs; if baseline beliefs about returns are already favorable and broadly accurate, as we document in Sections 3 and 4.2, there is little updating, resulting in a small average information effect, including among planners.

Mentoring raises C_i and operates at the execution stage. Its direct effect is among students with existing intentions to enroll, converting them into enrollees by lowering realized execution costs. Students without intentions to enroll may also experience a second effect through lower anticipated execution costs at the planning stage. A testable implication is that the mentoring effect should be higher among students who plan to enroll at baseline, and within the planner group, the mentoring effect should hold against the control group, not just against the information arm. Section 4.1 takes this implication to the data.

2.2 Setting and Interventions

Our study is set in the department of Quiché, Guatemala. Quiché ranks among the poorest departments in the country, with a multidimensional poverty rate of 76.5% compared to a national average of 61.6% (MIDES, 2025), and 89.2% of its population identify as indigenous (PNUD, 2022). The department also exhibits one of the highest dropout rates in Latin America: approximately half of sixth graders do not continue to lower secondary school (grades 7–9, known as *básico*), despite the absence of tuition fees and returns to lower secondary completion of 13 percent (relative to primary) Patrinos and Psacharopoulos (2020); Montenegro and Patrinos (2021). Although attendance is compulsory, progression is not automatic and requires active enrollment in a different school. The school year in Guatemala follows the calendar year, starting in January and ending in October.

We partnered with PRODESSA, a Guatemalan NGO experienced in educational support for indigenous communities, to develop two interventions addressing information and follow-through frictions during the transition from primary to lower secondary school. The

information treatment comprised five one-hour sessions for students, caregivers, and teachers, covering returns to schooling, enrollment logistics, and the risks of alternative paths such as child labor and illegal migration (see Appendix A.16). Sessions included additional materials such as classroom posters, leaflets for students, caregivers, and teachers, and culturally adapted comics for students. The *mentoring treatment* delivered the same informational content while adding follow-through support through (i) individual school counseling and (ii) sessions focused on building students' socio-emotional skills. A dedicated mentor at each school conducted weekly two-hour group sessions, individual student sessions, caregiver meetings, and teacher sessions (see Appendix A.15). Both interventions lasted four months and were implemented in sixth grade, the year before the transition to lower secondary school.

2.3 Sampling, Randomization, and Data

From the universe of 1,561 public schools offering sixth grade in Quiché, we excluded schools with a mean sixth-grade dropout rate below 35 percent based on Ministry of Education administrative records for 2013–2021, schools deemed inaccessible due to security or transport constraints, and schools located in the six municipalities where USAID was concurrently operating a separate program. From the remaining schools, we randomly selected 231 schools with 3,273 sixth-grade students, imposing a 2-kilometer buffer between schools to limit spillovers. The sample was not designed to be representative of Quiché or Guatemala, but to maximize power to study educational underinvestment: 69% of sixth graders in sample schools failed to transition to lower secondary in 2021, compared to 55% in Quiché and 38% nationally. We randomly assigned the 231 schools to one of three arms, stratified by interacting bins of historical dropout rates with bins of sixth-grade enrollment, yielding 77 schools per arm across 16 strata.

In our analyses, we use administrative records, in-school surveys, household surveys, and community surveys. The Ministry of Education maintains enrollment and performance records for all students in public and private schools in Guatemala, which we use

for our primary outcome. A baseline in-school survey (April–July 2024, 2,357 students) captures household context, educational plans, and socio-emotional outcomes. We define our study population as students who complete the baseline survey, which we use for balance checks, baseline controls, and heterogeneity analyses. A midline in-school survey (October–December 2024, after all interventions concluded) measures treatment recall and future intentions. At endline (in the fall of 2025, a year post treatment and at the end of the subsequent school year), we attempted to survey all baseline subjects and their caregivers at home, and interviewed community leaders to enumerate the location and activity status of all students in our baseline survey or who were enrolled in our study schools according to administrative records in the 2024 school year. Enrollment in 2025 is our primary outcome. As prespecified, we construct enrollment from the mode of enrollment responses across the student endline survey, the caregiver endline survey, the endline census, and administrative records. Question wording, variable definitions, and index construction follow our pre-analysis plan (Coccia et al., 2024).

2.4 Validity

We find no evidence of systematic differences between experimental groups at baseline (the p-value of the joint balance test is 0.641; Table A1). For our primary outcome, attrition is zero by construction, as the administrative records cover the full universe of students enrolled in any public or private school in Guatemala. In the survey data, we observe substantial differential attrition at midline, with more than 55 percent of control students attriting compared to approximately 35 percent in each treatment arm, driven almost entirely by school-level dropout from the survey (Tables A2 and A4). We therefore restrict midline comparisons to the two treatment arms, where attrition rates are similar and uncorrelated with differences in baseline characteristics (Tables A2 and A4). At endline, attrition is 31 percent for the student survey and 24 percent for the caregiver survey, with no evidence of differential attrition across arms (Table A2).

2.5 Compliance

Compliance with treatment was high: 96 percent of students in the information arm and 98 percent in the mentoring arm attended at least one session, mean attendance was 88 percent of sessions, and mentors conducted at least one individual session with 77 percent of students. Student reports of engagement with PRODESSA also align with the design: the median student in the mentoring arm reports weekly interactions with PRODESSA, compared to monthly interactions in the information arm.

2.6 Empirical Strategy

For each outcome Y_{iv} for student i in school v, our pre-specified main estimates are based on an intention-to-treat (ITT) regression of the form

$$Y_{iv} = \alpha + \gamma_I D_v^I + \gamma_M D_v^M + \lambda_1(A_i, F_i) + \lambda_2(S_v) + \upsilon_{iv}, \tag{1}$$

where D_v^I denotes school-level assignment to an intervention that provides information (both arms), D_v^M denotes assignment to the mentoring arm, $\lambda_1(A_i, F_i)$ includes baseline age and sex indicators, and $\lambda_2(S_v)$ includes strata fixed effects. With this specification, γ_I is our measure of belief updating, γ_M is our measure of follow-through frictions and our primary estimator of interest, and $\gamma_I + \gamma_M$ is the mentoring–control comparison. Standard errors are clustered at the school level.

3 The Intention–Action Gap

We examine educational intentions and enrollment patterns at the primary-to-lower-secondary transition in our control group before presenting the experimental results. At baseline, 80 percent of students in the control group express a desire to continue to lower secondary, and 65 percent report plans to do so (Figure 1). Caregivers share positive views, with 78 percent of students reporting that their parents want them to enroll. This aligns with fa-

vorable perceptions of schooling returns: only 7 percent of students believe they will be unable to find self-supporting employment after completing lower secondary, compared to 26 percent for primary completion.

Only half of students actually enroll in school the following year. Among students who report plans to enroll at the beginning of sixth grade, only 66 percent follow through, resulting in a 34 percent intention-action gap. Notably, 45 percent of non-enrollment occurs among those who planned to enroll. The gap does not reflect a long forecast horizon: among students who still report plans to enroll at midline in October, two months before the January enrollment window, 35 percent do not enroll.

While several factors could contribute to this intention-action gap, including economic realities catching up with students or social desirability in reporting, the overall pattern suggests that understanding it may require going beyond beliefs. Broad support for secondary schooling among students and caregivers, along with a sizable gap even among more affluent households, points to potential frictions that hinder follow-through. Failing to follow through on enrollment plans is correlated with below-median wealth, higher student age, being female, having both parents at home, and being in a school with a high dropout rate (Table A6).[1] At endline, we ask dropouts why they dropped out. Answers for the pooled sample are in Figure 2. We separate students who did not plan to enroll at baseline (left panel) from those who did plan to enroll but later failed to do so (right panel). Subjects can select multiple causes of dropout. We see that failed planners are typically more likely to answer with household related concerns (chores, employment, family constraints) and unenrolled non-planners emphasis school constraints more (access barriers, disengagement from school, problems with the school), suggesting that the nature of the follow through frictions in our failed planners come from unanticipated challenges in managing home obligations rather than unanticipated logistical challenges with accessing

[1]The differences by socioeconomic background are consistent with reduced support to follow through on plans in lower-SES households, but could also reflect an incorrect assessment of economic realities at baseline that only becomes apparent at the time of the transition. However, because the gap remains substantial even among more affluent households (Table A6), economic constraints alone are unlikely to account for the full pattern.

schools.

4 Results

4.1 Treatment Effects on Educational Investment

Following equation (1), we estimate effects for two comparisons of interest: the information–control contrast, our estimate of the role of information frictions, and the mentoring–information contrast, our estimate of the role of follow-through frictions. Treatment was conducted in the 2024 school year, and our primary findings on enrollment are for the following 2025 school year. Enrollment is defined as any indication that child ever enrolled in school in the 2025 year.

About half (51.6 percent) of control subjects enroll in school in the academic year after treatment (Figure 3 and Appendix Table A7). The information arm has 2 percentage points lower enrollment, with a 95 percent confidence interval spanning a 9 percentage point decline and a 4.7 percentage point increase. Mentoring raises enrollment by 5.7 percentage points relative to the control group and 7.6 percentage points relative to the information-only arm.[2]

Among students who planned to enroll at baseline, mentoring raises enrollment to 73.7 percent, against 66.7 percent in the control group and 66.4 percent in the information arm. Within this group, the mentoring–information contrast is 7.2 percentage points, while the information–control contrast is negligible, consistent with the framework's prediction that when baseline beliefs are already favorable, there is little updating to induce. Among students without baseline plans, enrollment is 21–23 percent in all three arms. This configuration also matches the framework's prediction.

We have several different measures of school engagement. Attendance differs from enrollment in that it implies on-going engagement during the school year to the time of

[2]Figure 3 sticks with the PAP pre-specified enrollment definition and not the lower secondary school enrollment appropriate for Figure 1. The tenor of our findings is not sensitive to different definitions of the outcome, Appendix Table A8.

measurement. Results for attendance differ in tenths of a percentage point and are in Appendix Table A7. We also have several different data sources that go into our prespecified enrollment and attendance measures. Results by data source are in Appendix Table A8. Student, caregiver, and administrative sources are all similar. Community leader "short census" reports lead to smaller treatment effects, consistent with more measurement error in that data.

Two qualifications are important for this section. First, we cannot reject the null that the impact of mentoring is the same for planners and non-planners. The difference in the mentoring impact for planners relative non-planners is 7.8 percentage points with a standard error of 4.5. The null hypothesis test has a P-value of 0.085, so we do not reject equal effects of mentoring across subgroups. Our framework does not predict that the effect of mentoring can only be in planners; it just implies that there is more margin to move planners than non-planners. This highlights that the primary point of this section's heterogeneity result is accounting for what which subjects drive our treatment effects. This helps us understand the mechanisms behind our findings but does not imply that mentoring cannot help non-planners. Second, baseline enrollment plans is one of several pre-specified heterogeneity dimensions, though it is the only the framework that motivates this paper implies. Results for the remaining dimensions are reported in the populated PAP (Coccia et al., 2025) and the dimensions most relevant here are reproduced in Online Appendix Tables A9–A13 as well. Mentoring effects are also larger for students from below-median wealth households, who have fewer resources and less adult support, and in larger schools, where individual guidance is otherwise scarce, though those interaction effects are imprecisely estimated and the planning x mentoring interactions survive when included simultaneously with wealth and school size in the same regression.

4.2 Testing Content Parity

The interpretation of the mentoring–information contrast as the effect of follow-through support rests on content parity. Delivery parity is imposed by design and confirmed by im-

plementation records, but the more intensive and interactive format of the mentoring arm may have led to greater absorption of the common informational content. This subsection tests for that directly. Because differential attrition precludes control-group comparisons at midline (Section 2.4), the recall and knowledge tests compare the two treatment arms, which have similar midline attrition.

Table 1 reports the results. Columns 1 and 2 test content parity in the pooled sample with columns 3 and 4 reflect non-planners and 5 and 6 planners. For education and child labor related measures, our findngs are consistent with content parity. We find no indication that mentored students are more likely to recall having someone visit the school to provide sessions, to remember the informational topics of these sessions on education or child labor, or to report correct knowledge of educational laws. As an implementation check, we examine recall of socio-emotional topics taught exclusively in the mentoring arm and find recall there is 8.5 percentage points higher ($p < 0.05$) from an information-arm base of 23.6 percent. The arms differ where they should and, on these measures.

We do not in general observe variation in mentoring - information differences between planners and non-planners although there are two interesting distinctions. First, non-planners are more likely to report saliency of school visits. Second, non-planners are more likely to report increases in returns to education as a result of the mentoring treatment. This is in-line with our hypothesis that planners were already so persuaded as to the value of education that there was no scope for a differential effect along the information friction margin. That need not be true of non-planners.

The one place where information absorption seems to be different between mentor and information arms is in human trafficking knowledge where human trafficking knowledge take-up seems less in the mentoring arm for planners (this is driven by answers to one question on domestic human trafficking, Appendix Table A14). We do not think this difference is important in understanding our findings and likely reflects selective attention to information that is relevant in our multifacted information treatment (planners aren't likely to migrate in the near future, so aren't paying attention to human trafficking information in

the context of all the layered levels of schooling information in the mentoring intervention).

Overall, while we cannot fully rule out violations of content parity especially along the intensity of internalization of information margin, the evidence gives no indication that differential information absorption accounts for the enrollment differences between the two treatment arms. Where absorption differs, it differs on the socio-emotional content unique to mentoring, not on the common informational content, and human trafficking knowledge where mentoring subjects learn less from the intervention. This seems unlikely to drive enrollment and might reflect that human trafficking information is less salient to our planners who were not planning to migrate.

4.3 Interpreting the Mentoring Effect

That the impact of mentoring works through helping subjects with plans to enroll in lower secondary helps us understand that the mechanism for mentoring's effect is in resolving follow through frictions. While that is the mechanism behind the mechanism effect, this section speculates as to why mentoring might do that.

Mentoring is a bundle of individualized counseling and classroom-based socio-emotional skill-building delivered by a dedicated adult. Whether one component of that bundle drives the resolution of follow through frictions is a question for future research, but we prespecified several proxies for how mentoring might influence schooling and the goal of this section is to narrow candidate channels behind our follow-through mechanism. We distinguishing two broad channels: (i) sustained personal support that helps students and families follow through on their plans, and (ii) socio-emotional skills that help students translate intentions into action.

For the personal support channel, we collected data on social accountability, such as whether students felt that someone outside their family would notice or feel disappointed if they stopped studying, and on social support, including parental engagement with school, the existence of a pro-education peer group, and the presence of a figure inspiring them to work harder. As Table 2 shows, we observe positive point estimates for most outcomes,

but none remain statistically significant after the false-discovery-rate adjustment. These indices measure perceived support at endline; they need not capture the support the mentor provided during the transition itself, so the null results limit but do not eliminate the personal support channel. Morevoer, it is notable that adding mentoring to the information increases social accountability for planners (not so for non-planners). The important component question of this accountability index is whether there is someone outside of their family that would notice if the subject stopped studying. Given the magnitude of the mentoring - information difference for planners, we are uncomfortable excluding accountability as part of what leads planners to overcome follow through frictions with mentoring.

For socio-emotional skills, self-reported indices of self-regulation, planning, and agency show no effects. An enumerator-assessed soft-skills index rises by 0.19 standard deviations relative to information ($p = 0.015$), with increases in all subcomponents: enumerators rate children in the mentoring arm as more attentive, careful, alert, and confident. We treat this index as a secondary measure and interpret it with caution for two reasons. First, it is an enumerator's rating of the child's engagement during a survey interaction with an unfamiliar adult. Mentored children spent months interacting with an outside adult, so their greater ease in this interaction may partly reflect familiarity with such interactions rather than a general skill gain, acquired either from the program or from the additional schooling the program induced. Second, the index was measured after the school transition, so it may partly reflect the consequences of enrollment rather than its causes. Third, we do not see meaningful differences in our soft-skills for planners vs non-planners. It could be that planners are more effective in leveraging their improved soft-skills to promote school while non-planners use them in other ways, but we do not observe anything about how improved ability to interact with adults are leveraged.

It appears that whatever channel drives mentoring impact on follow-through frictions, mentoring's impact is not acting on one particular barrier to follow through. In Figure 4, we plot the reasons planners failed to enroll in school by treatment arm. Broadly, we do not see any dramatic changes in reasons for failure for the mentoring group compared to other

groups. Chores are a bit more prevalent an answer and paid work is a bit less prevalent, but there is no clear "smoking gun" to explain what reason for dropout is being eliminated for planners by the mentoring treatment.

Taken together, the evidence points to intensive personal mentoring, rather than information, as what moves enrollment. Within the mentoring bundle, socio-emotional skills are the leading candidate channel for resolving follow-through frictions: the soft-skills index is the only mechanism measure that responds, the arms differ in socio-emotional content by design, and the heterogeneity by baseline plans matches the framework's prediction for an intervention that raises students' capacity to act. But the mechanism evidence is not decisive, and we cannot rule out that mentoring also nudged subjects to overcome follow through frictions by providing a person outside of the family that the subject and their family felt accountable to.

5 Conclusion

This paper studies why many students who plan to continue in school do not enroll. In a randomized evaluation across 231 primary schools in rural Guatemala, providing information on schooling options, returns, and enrollment logistics has no detectable effect on enrollment. Adding school-based mentoring that delivers the same information alongside individualized support and socio-emotional skill-building increases lower secondary enrollment by 7.6 percentage points relative to information, a 15 percent increase.

We interpret the contrast between mentoring and information as evidence on follow-through frictions. In the control group, 34 percent of students who plan to enroll do not do so, and 45 percent of all non-enrollment occurs among planners. The framework's heterogeneity prediction is borne out: among students with baseline plans, the group where follow-through support should operate, mentoring raises enrollment by 7 percentage points relative to control as well as to information, while non-planners are unaffected by either treatment, and the attenuated full-sample mentoring–control contrast is what dilution by

the planner share implies. This interpretation rests on content parity between the two treatment arms. The mentoring arm does not differentially recall informational content, know education laws, or update beliefs about returns relative to the information arm, while recalling more of the socio-emotional content unique to it; we cannot fully rule out parity violations, but the measured evidence gives no indication of them. Taken together, the results indicate that the primary barrier in this setting is not what families know, but whether students can act on plans they already hold. The effective intervention is intensive personal mentoring. Socio-emotional skills are the leading candidate channel behind this mechanism, supported by a 0.19 standard deviation increase in an enumerator-assessed soft-skills index, but this measure is secondary, may partly capture children's greater ease with unfamiliar adults acquired through the program or through schooling itself, and does not rule out a role for personal support.

These findings point to a class of frictions between plans and realized behavior that is distinct from information frictions. They suggest that models of educational investment should allow for gaps between stated intentions and subsequent actions, and that interventions can be effective by targeting these gaps without changing beliefs or prices. A testable implication is that impacts of similar programs should be larger in settings with high baseline intentions and multi-step enrollment processes, and smaller where beliefs about returns or access are incorrect.

External validity depends on context. In our setting, baseline motivation to continue in school is high, which may explain the absence of effects of information alone; in settings with weaker demand or less accurate beliefs, information interventions may play a larger role. Our evidence comes from rural indigenous communities, and we do not observe long-run outcomes (our endline is a year post treatment), so persistence of enrollment gains and downstream effects on learning and labor market outcomes remain open. For policy, light-touch information campaigns are unlikely to close enrollment gaps where intentions are already favorable. Programs that provide structured, individualized support to help students complete required steps, meet deadlines, and sustain effort can raise educa-

tional investment at transition points. More generally, distinguishing between information frictions and follow-through frictions can guide both theory and program design.

6 Generative AI Disclosure Statement

Generative AI systems, including Claude Code, Claude Cowork, Claude Chat, ChatGPT, Codex, and Refine.ai, were used in the course of this research. Their use included translation of project documents, code generation and refinement under the author's direction, editorial revision to improve clarity and readability, and reviews to identify potential issues related to clarity, logic, exposition, internal consistency, and consistency between reported results, the manuscript text, and other project documents. Generative AI tools were used to assist these processes, but they did not determine the research questions, study design, empirical strategy, statistical analysis, interpretation of results, or conclusions. Generative AI was not available at the time of project design and implementation and had no role in fieldwork or project material development. The authors are solely responsible for all aspects of the analysis, the accuracy of the results, the interpretation of the evidence, and the content of the manuscript.

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7 Figures and Tables

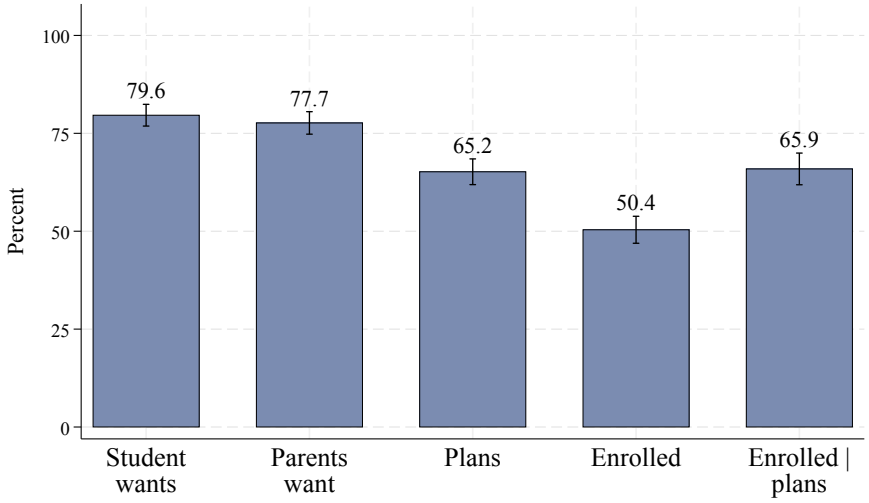


Figure 1: Intention-action gap, control group

Notes: The figure reports the share of baseline students in the control group with an observed response to the baseline lower secondary enrollment-plans question who (i) report at baseline that they want to continue to secondary school, (ii) report at baseline that their parents want them to continue, (iii) report plans to enroll at baseline, (iv) enroll in lower secondary school, and (v) enroll in lower secondary school conditional on reporting plans to do so. All "don't know" responses are coded as zero. Error bars represent 95 percent confidence intervals.

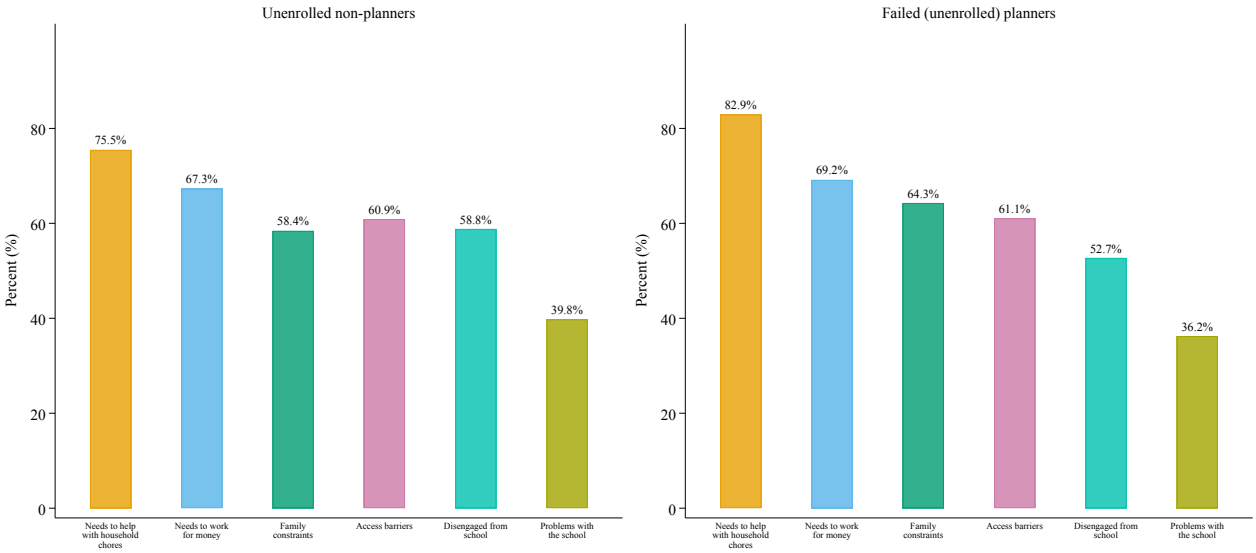


Figure 2: Reasons for not being in school by baseline enrollment plans

Notes: The figure reports reasons students gave in the endline survey for not being in school separately for students who did not report plans to enroll at baseline (left panel) and those who reported plans to enroll but were not in school at endline (right panel). The sample is restricted to students with completed baseline surveys, an observed response to the baseline lower-secondary enrollment plans question, and an available response to the endline reason question. Students could report more than one reason, so the categories are not mutually exclusive and percentages do not sum to 100. Family constraints combines lack of parental support and school costs; access barriers combines distance and safety; disengagement combines disliking school and poor school performance; and problems with the school combines poor school quality and being denied attendance. Variable-specific sample sizes range from 253 to 255 for non-planners and from 238 to 240 for planners.

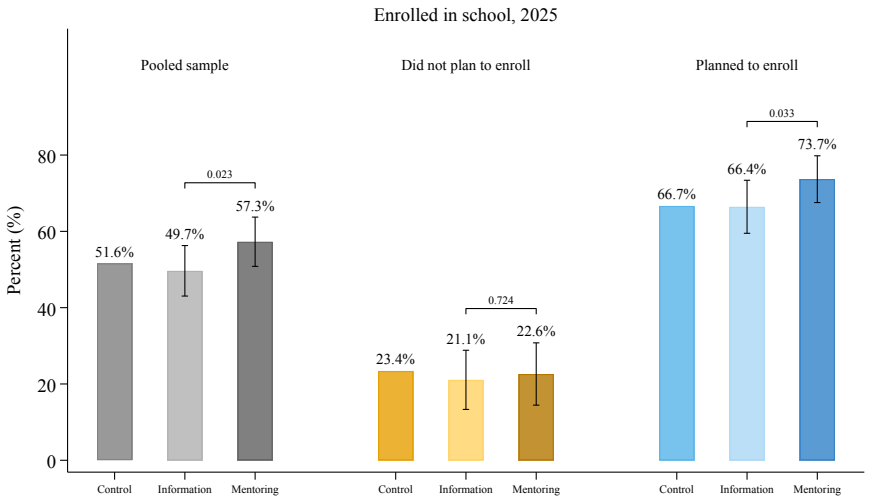


Figure 3: Treatment effects on enrollment by baseline enrollment plans

Notes: The figure reports school enrollment in 2025 by treatment arm, for the pooled sample and separately for students who did and did not report plans to enroll at baseline. Enrollment is defined as enrollment in any level of schooling (primary or lower secondary), as pre-specified (Coccia et al., 2024); Figure 1 reports lower secondary enrollment. Error bars represent 95 percent confidence intervals. Brackets denote mentoring–information comparisons within each group, with the corresponding *p*-value reported above each bracket. Regressions include strata, age-bin, and sex fixed effects, with standard errors clustered at the school level.

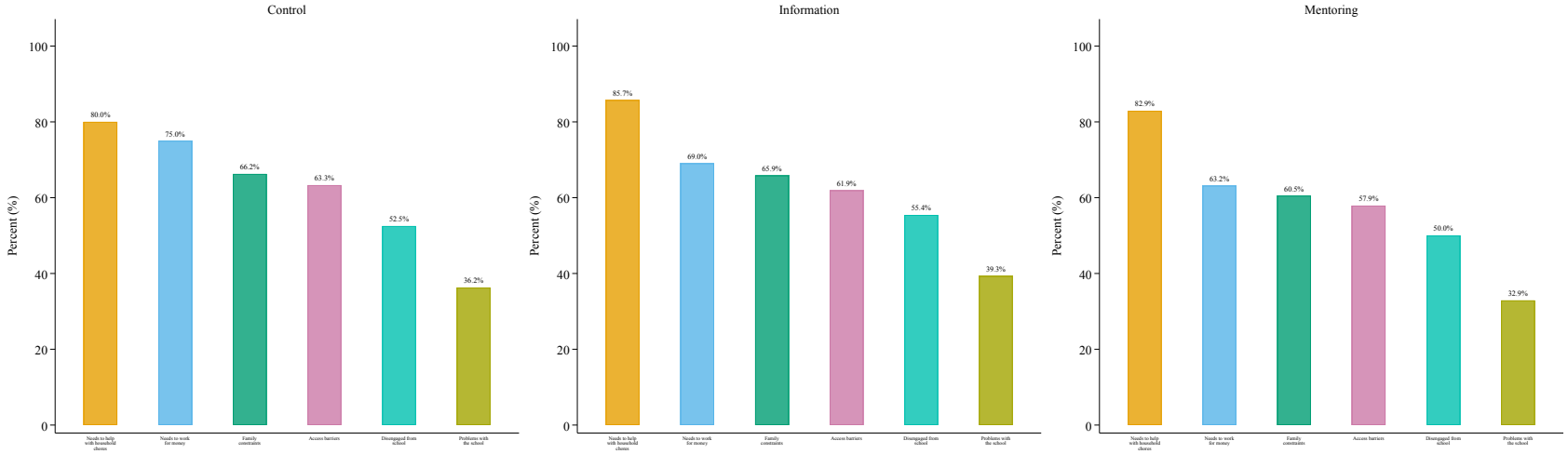


Figure 4: Reasons for not being in school among failed planners, by treatment arm

Notes: The figure reports reasons students gave in the endline survey for not being in school separately by treatment arm among students who reported plans to enroll at baseline but were not in school at endline. The sample is restricted to students with completed baseline surveys, an observed response to the baseline lower-secondary enrollment plans question, and an available response to the endline reason question. Students could report more than one reason, so the categories are not mutually exclusive and percentages do not sum to 100. Family constraints combines lack of parental support and school costs; access barriers combines distance and safety; disengagement combines disliking school and poor school performance; and problems with the school combines poor school quality and being denied attendance. Variable-specific sample sizes range from 238 to 240.

Table 1: Content parity and information absorption

Outcome	Pooled		Non-planners		Planners	
	Information mean (1)	Mentoring vs Info (2)	Information mean (3)	Mentoring vs Info (4)	Information mean (5)	Mentoring vs Info (6)
Recalls any school visit	0.570	0.009	0.491	0.112*	0.616	−0.045
		(0.035)		(0.059)		(0.041)
		[0.931]		[0.341]		[0.673]
Recalls education-related information	0.196	−0.004	0.150	0.044	0.222	−0.045
		(0.027)		(0.047)		(0.032)
		[0.953]		[0.690]		[0.606]
Recalls socio-emotional content	0.236	0.085**	0.168	0.066	0.276	0.069*
		(0.035)		(0.051)		(0.038)
		[0.308]		[0.658]		[0.341]
Recalls child labor content	0.117	0.000	0.087	0.051	0.135	−0.021
		(0.028)		(0.042)		(0.031)
		[1.000]		[0.658]		[0.786]
Recalls migration/human trafficking content	0.147	0.063*	0.121	0.042	0.162	0.065
		(0.033)		(0.045)		(0.041)
		[0.341]		[0.690]		[0.478]
Expects higher earnings from post-primary education	0.751	0.014	0.647	0.092*	0.811	−0.032
		(0.028)		(0.047)		(0.035)
		[0.845]		[0.341]		[0.690]
Needs more education for the life they want	0.939	0.009	0.886	0.036	0.970	−0.006
		(0.015)		(0.033)		(0.014)
		[0.845]		[0.673]		[0.883]
Identifies compulsory primary schooling	0.657	−0.026	0.601	−0.052	0.690	−0.009
		(0.037)		(0.061)		(0.043)
		[0.786]		[0.690]		[0.949]
Identifies compulsory lower-secondary schooling	0.638	−0.031	0.584	0.000	0.670	−0.049
		(0.036)		(0.052)		(0.041)
		[0.690]		[1.000]		[0.658]
Child labor knowledge index	0.028	−0.023	−0.144	0.014	0.128	−0.050
		(0.078)		(0.104)		(0.095)
		[0.931]		[0.953]		[0.845]
Human trafficking knowledge index	0.114	−0.181**	−0.082	−0.067	0.229	−0.236**
		(0.088)		(0.149)		(0.099)
		[0.341]		[0.861]		[0.308]

Notes: The sample consists of 978 baseline students in the mentoring and information arms with an observed response to the baseline enrollment plans question and an available midline survey, including 332 non planners and 646 planners. The control arm is excluded from these midline comparisons because of differential attrition (Section 2.4). Columns 1–2 report results for the pooled sample, Columns 3–4 for non-planners, and Columns 5–6 for planners. Within each group, the first column reports the information arm mean and the second the mentoring–information difference. All regressions include strata, age-bin, and sex fixed effects, with standard errors clustered at the school level and reported in parentheses. False discovery rate adjusted q-statistics are reported in square brackets and are computed jointly across all 33 mentoring–information tests in the table, following Anderson (2008). Binary outcome means are proportions and their treatment effects are differences in proportions; the child labor and human trafficking knowledge indexes are standardized. The socio-emotional recall outcome combines recall of socio-emotional topics with recall of some topics that are also part of the informational content; it is thus not a clean mentoring-only measure. All outcomes are from the midline student survey. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Personal support and socio-emotional skills

Outcome	Pooled			Non-planners			Planners		
	Control mean	Information vs Control	Mentoring vs Info	Control mean	Information vs Control	Mentoring vs Info	Control mean	Information vs Control	Mentoring vs Info
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Personal support									
Social accountability index	0.000	0.006	0.093	−0.274	0.147	−0.107	0.111	−0.031	0.139*
		(0.079)	(0.073)		(0.129)	(0.152)		(0.080)	(0.080)
		[0.987]	[0.717]		[0.717]	[0.976]		[0.987]	[0.616]
Social support index	0.015	0.016	0.017	−0.194	−0.001	0.038	0.099	0.031	−0.008
		(0.053)	(0.053)		(0.087)	(0.105)		(0.061)	(0.060)
		[0.987]	[0.987]		[0.993]	[0.987]		[0.987]	[0.987]
Panel B: Socio-emotional skills									
Soft skills index, enumerator-assessed	0.000	−0.027	0.191**	−0.022	−0.041	0.216*	0.009	−0.043	0.198**
		(0.086)	(0.078)		(0.146)	(0.123)		(0.100)	(0.096)
		[0.987]	[0.480]		[0.987]	[0.616]		[0.987]	[0.480]
Self-regulation index	0.000	−0.088	0.052	−0.009	−0.135	0.015	0.004	−0.072	0.060
		(0.070)	(0.066)		(0.118)	(0.122)		(0.087)	(0.086)
		[0.717]	[0.972]		[0.717]	[0.987]		[0.972]	[0.976]
Planning index	0.000	0.087	−0.004	0.086	−0.167	0.026	−0.035	0.183**	−0.015
		(0.069)	(0.076)		(0.134)	(0.134)		(0.082)	(0.088)
		[0.717]	[0.987]		[0.717]	[0.987]		[0.480]	[0.987]
Agency index	0.000	0.046	−0.089	−0.125	−0.008	−0.041	0.050	0.080	−0.139
		(0.085)	(0.078)		(0.128)	(0.131)		(0.101)	(0.093)
		[0.987]	[0.717]		[0.987]	[0.987]		[0.972]	[0.717]

Notes: Estimation samples include up to 1,270 students with completed baseline surveys, an observed response to the baseline enrollment plans question, and a nonmissing endline value for the relevant index. Columns 1–3 report results for the pooled sample, Columns 4–6 for students without baseline plans to enroll, and Columns 7–9 for students with baseline plans to enroll. Within each group, the first column reports the control arm mean in the corresponding regression sample, the second the information–control difference, and the third the mentoring–information difference. Regressions have strata, age-bin, and sex fixed effects. Standard errors are clustered at the baseline school level and reported in parentheses. False discovery rate adjusted q-statistics are reported in square brackets and are computed jointly across all 36 treatment-effect tests in the table, following Anderson (2008). All indices are standardized relative to the pooled control group, so means and treatment effects are in standard deviation units. The soft-skills index is based on enumerator assessments of student attentiveness, carefulness, alertness, and confidence during the survey interaction; all other indices are self-reported. Outcomes are from the endline student survey. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A On-Line Appendix

A.1 Balance

Table A1: Balance of baseline characteristics

Baseline characteristic	Mentoring mean (1)	Information mean (2)	Control mean (3)	F-statistic (p-value) (4)
Age (years)	12.334	12.406	12.295	1.023
	(1.083)	(1.113)	(1.035)	(0.361)
Female	0.514	0.489	0.490	0.579
	(0.500)	(0.500)	(0.500)	(0.562)
Speaks Spanish at home	0.653	0.581	0.642	1.312
	(0.476)	(0.494)	(0.480)	(0.271)
Household size	7.066	7.284	7.458	1.713
	(3.059)	(3.121)	(3.340)	(0.183)
Mom and dad are coresidents	0.715	0.694	0.699	0.351
	(0.452)	(0.461)	(0.459)	(0.704)
Household has running water	0.798	0.775	0.801	0.249
	(0.402)	(0.418)	(0.399)	(0.780)
Household has electricity	0.866	0.879	0.883	0.089
	(0.341)	(0.326)	(0.322)	(0.915)
Household has internet	0.373	0.347	0.372	0.435
	(0.484)	(0.476)	(0.484)	(0.648)
Plans to enroll in lower secondary	0.687	0.625	0.652	2.290
	(0.464)	(0.484)	(0.477)	(0.104)
Student is in child labor	0.559	0.583	0.583	0.458
	(0.497)	(0.493)	(0.493)	(0.633)
Plans to migrate internationally	0.143	0.134	0.147	0.129
	(0.351)	(0.341)	(0.354)	(0.879)
Joint orthogonality χ^2				15.301
				(0.641)
Bivariate orthogonality F-statistic	0.940^a	0.513^b	1.123^c	
	(0.493)	(0.864)	(0.350)	
Observations	761	761	810	

Notes: The sample is restricted to students with completed baseline surveys and an observed response to the baseline enrollment plans question. Columns 1–3 report treatment arm means; standard deviations in parentheses. Column 4 reports the F-statistic and associated p-value for the null hypothesis that treatment assignment does not predict each baseline characteristic. Regressions include age-bin, sex, and strata fixed effects; the age regression excludes age-bin fixed effects and the female regression excludes the sex fixed effect. The joint orthogonality test evaluates whether treatment assignment jointly predicts the included baseline characteristics other than age and female. The bivariate tests compare mentoring and control (^{a}), information and control (^{b}), and mentoring and information (^{c}). Orthogonality tests include age-bin, sex, and strata fixed effects. Standard errors are clustered at the school level. The child-labor measure is observed for 757 mentoring, 755 information, and 805 control students.

A.2 Attrition

Table A2: Attrition

Variable	Attrition rate (1)	Are treatment arms equal (2)	Treatment correlation (3)	Balance covariates (4)	Randomization valid (5)	Target *N* (6)
Midline						
Enrolled	0.428	0.816	7.523	2.696	13.027	2,332
		(0.367)	(0.001)	(0.005)	(0.790)	
Endline						
Enrolled	0.000				15.301	2,332
					(0.641)	
Employed	0.089	0.052	0.095	0.930	11.151	2,332
		(0.820)	(0.910)	(0.500)	(0.888)	
Migrated	0.010	0.924	0.870	1.301	14.446	2,332
		(0.338)	(0.420)	(0.237)	(0.700)	
Census, some info	0.045	1.933	1.364	1.146	13.746	2,332
		(0.166)	(0.258)	(0.332)	(0.746)	
Census, all info	0.116	0.020	0.014	1.057	12.092	2,332
		(0.888)	(0.986)	(0.396)	(0.842)	
At least 1 HH survey	0.186	0.200	1.146	3.326	9.398	2,156
		(0.655)	(0.320)	(0.001)	(0.950)	
Caregiver survey	0.242	0.001	1.420	3.411	8.066	2,156
		(0.972)	(0.244)	(0.001)	(0.978)	
Student survey	0.314	0.183	2.681	3.384	7.792	2,156
		(0.670)	(0.071)	(0.001)	(0.982)	

Notes: The sample is 2,332 students with completed baseline surveys and an observed response to the baseline enrollment plans question. Attrition rates in Column 1 are calculated as a fraction of targeted students in Column 6. Column 2 reports the F-statistic and *p*-value for the null that attrition does not differ between mentoring and information arms. Column 3 reports the F-statistic and *p*-value for the null that treatment assignment does not jointly predict attrition. Column 4 reports the F-statistic and *p*-value for the null that the included baseline covariates do not jointly predict attrition. Included baseline covariates are Spanish spoken at home, household size, parental coresidence, running water, electricity, internet access, baseline enrollment plans, child labor, and migration plans. Column 5 reports the chi-square and *p*-value from a joint test of whether treatment assignment is associated with these baseline covariates among non-attritors. Regressions include strata, age-bin, and sex fixed effects, with standard errors clustered at the baseline school level. All 2,332 students were targeted at midline and through the short census; 2,156 were targeted for both the endline student and caregiver surveys. Enrollment is observed for all 2,332 students because it is constructed from the endline student survey, caregiver survey, short census, and administrative records. For outcomes constructed from multiple sources, Target *N* is the number of students targeted in at least one of those sources.

A.3 Balance by Baseline Plans

Table A3: Balance of baseline characteristics by baseline enrollment plans

Baseline characteristic	Mentoring mean (1)	Information mean (2)	Control mean (3)	F-statistic (p-value) (4)
Panel A: Did not plan to enroll				
Age (years)	12.399	12.491	12.383	0.402
	(0.983)	(1.177)	(1.078)	(0.670)
Female	0.513	0.491	0.504	0.402
	(0.501)	(0.501)	(0.501)	(0.670)
Speaks Spanish at home	0.517	0.516	0.596	2.240
	(0.501)	(0.501)	(0.492)	(0.109)
Household size	7.450	7.411	7.468	0.009
	(2.857)	(3.415)	(3.164)	(0.991)
Mom and dad are coresidents	0.748	0.667	0.674	2.549
	(0.435)	(0.472)	(0.470)	(0.081)
Household has running water	0.794	0.723	0.777	0.578
	(0.405)	(0.448)	(0.417)	(0.562)
Household has electricity	0.878	0.888	0.879	0.098
	(0.328)	(0.316)	(0.326)	(0.907)
Household has internet	0.336	0.330	0.337	0.045
	(0.473)	(0.471)	(0.473)	(0.956)
Student is in child labor	0.517	0.578	0.566	0.762
	(0.501)	(0.495)	(0.497)	(0.468)
Plans to migrate internationally	0.164	0.126	0.152	0.827
	(0.371)	(0.333)	(0.360)	(0.439)
Joint orthogonality χ^2				20.337
				(0.205)
Bivariate orthogonality F-statistic	1.258[a]	0.939[b]		1.220[c]
	(0.271)	(0.487)		(0.292)
Observations	238	285	282	
Child-labor observations	238	282	281	

Table A3: Balance of baseline characteristics by baseline enrollment plans (continued)

Baseline characteristic	Mentoring mean (1)	Information mean (2)	Control mean (3)	F-statistic (p-value) (4)
Panel B: Planned to enroll				
Age (years)	12.304	12.355	12.248	0.806
	(1.125)	(1.071)	(1.009)	(0.448)
Female	0.514	0.487	0.483	0.304
	(0.500)	(0.500)	(0.500)	(0.738)
Speaks Spanish at home	0.715	0.620	0.667	1.846
	(0.452)	(0.486)	(0.472)	(0.160)
Household size	6.891	7.208	7.453	2.972
	(3.133)	(2.932)	(3.434)	(0.053)
Mom and dad are coresidents	0.700	0.710	0.712	0.020
	(0.459)	(0.454)	(0.453)	(0.981)
Household has running water	0.799	0.807	0.814	0.044
	(0.401)	(0.395)	(0.389)	(0.957)
Household has electricity	0.860	0.874	0.884	0.336
	(0.347)	(0.332)	(0.320)	(0.715)
Household has internet	0.390	0.357	0.390	0.409
	(0.488)	(0.480)	(0.488)	(0.665)
Student is in child labor	0.578	0.586	0.592	0.251
	(0.494)	(0.493)	(0.492)	(0.779)
Plans to migrate internationally	0.134	0.139	0.144	0.102
	(0.341)	(0.346)	(0.351)	(0.903)
Joint orthogonality χ^2				13.890
				(0.607)
Bivariate orthogonality F-statistic	1.196[a]	0.405[b]		0.923[c]
	(0.305)	(0.916)		(0.500)
Observations	523	476	528	
Child-labor observations	519	473	524	

Notes: The sample contains 2,332 baseline students with an observed enrollment plans response: 805 non-planners (Panel A) and 1,527 planners (Panel B). Columns 1–3 report treatment arm means; standard deviations in parentheses. Column 4 reports the F-statistic and p-value for the null that treatment assignment does not predict each characteristic. Regressions include age-bin, sex, and strata fixed effects, omitting age-bin fixed effects for age and the sex fixed effect for female. Joint tests use the included baseline characteristics other than age and female; bivariate tests compare mentoring–control ([a]), information–control ([b]), and mentoring–information ([c]), with the same controls. Standard errors are clustered at the school level. Binary variable means are proportions; age is in years and household size in persons. Child labor sample sizes are reported separately because of missing baseline responses.

A.4 Balance at Baseline for Recaptured at Midline Baseline Subject Survey Respondents

Table A4: Balance of baseline characteristics among midline respondents

Baseline characteristic	Mentoring mean (1)	Information mean (2)	Control mean (3)	F-statistic (p-value) (4)
Age (years)	12.25	12.31	12.22	0.283
	(1.03)	(1.09)	(1.01)	(0.754)
Female	0.514	0.517	0.511	0.030
	(0.500)	(0.500)	(0.501)	(0.970)
Speaks Spanish at home	0.679	0.617	0.646	0.828
	(0.467)	(0.487)	(0.479)	(0.439)
Household size	6.872	7.317	7.360	1.323
	(2.963)	(3.181)	(3.302)	(0.269)
Mom and dad are coresidents	0.693	0.696	0.654	0.411
	(0.462)	(0.461)	(0.476)	(0.664)
Household has running water	0.801	0.794	0.792	0.024
	(0.400)	(0.405)	(0.406)	(0.976)
Household has electricity	0.880	0.894	0.907	0.400
	(0.325)	(0.309)	(0.290)	(0.671)
Household has internet	0.413	0.349	0.430	2.015
	(0.493)	(0.477)	(0.496)	(0.136)
Plans to enroll in lower secondary	0.687	0.632	0.626	1.091
	(0.464)	(0.483)	(0.484)	(0.338)
Student is in child labor	0.530	0.560	0.565	0.523
	(0.500)	(0.497)	(0.496)	(0.593)
Plans to migrate internationally	0.144	0.134	0.157	0.281
	(0.351)	(0.341)	(0.365)	(0.755)
Joint orthogonality χ^2				13.027
				(0.790)
Bivariate orthogonality F-statistic	0.623[a]	0.589[b]		1.041[c]
	(0.775)	(0.804)		(0.410)
Observations	508	470	356	
Child-labor observations	506	468	352	

Notes: The sample contains 1,334 baseline students with an observed enrollment plans response who were recaptured at midline. Columns 1–3 report treatment arm means; standard deviations in parentheses. Column 4 reports the F-statistic and associated p-value for the null that treatment assignment does not predict each baseline characteristic. Regressions include age-bin, sex, and strata fixed effects; the age regression excludes age-bin fixed effects and the female regression excludes the sex fixed effect. Joint orthogonality test uses the displayed baseline characteristics other than age and female. Bivariate tests compare mentoring–control ([a]), information–control ([b]), and mentoring–information ([c]), with the same controls. Standard errors are clustered at the school level. Binary variable means are proportions; age is measured in years and household size in persons. Child-labor sample sizes are reported separately because of missing baseline responses.

A.5 Balance at Baseline for Recaptured at Endline Baseline Subject Survey Respondents

Table A5: Balance of baseline characteristics among endline student survey respondents

Baseline characteristic	Mentoring mean (1)	Information mean (2)	Control mean (3)	F-statistic (p-value) (4)
Age (years)	12.30	12.36	12.22	1.593
	(1.08)	(1.09)	(1.05)	(0.206)
Female	0.557	0.506	0.505	1.612
	(0.497)	(0.500)	(0.501)	(0.202)
Speaks Spanish at home	0.663	0.591	0.655	1.275
	(0.473)	(0.492)	(0.476)	(0.281)
Household size	7.091	7.234	7.271	0.218
	(3.070)	(2.994)	(2.947)	(0.805)
Mom and dad are coresidents	0.702	0.695	0.697	0.189
	(0.458)	(0.461)	(0.460)	(0.828)
Household has running water	0.785	0.756	0.797	0.476
	(0.411)	(0.430)	(0.402)	(0.622)
Household has electricity	0.878	0.870	0.906	0.653
	(0.327)	(0.337)	(0.292)	(0.521)
Household has internet	0.366	0.339	0.356	0.204
	(0.482)	(0.474)	(0.479)	(0.816)
Plans to enroll in lower secondary	0.721	0.681	0.704	0.913
	(0.449)	(0.467)	(0.457)	(0.403)
Student is in child labor	0.564	0.565	0.585	0.242
	(0.496)	(0.496)	(0.493)	(0.785)
Plans to migrate internationally	0.130	0.128	0.156	0.504
	(0.336)	(0.334)	(0.363)	(0.605)
Joint orthogonality χ^2				7.792
				(0.982)
Bivariate orthogonality F-statistic	0.390[a]	0.526[b]		0.469[c]
	(0.938)	(0.854)		(0.893)
Observations	517	492	469	
Child-labor observations	514	490	465	

Notes: The sample contains 1,478 baseline students with an observed enrollment plans response who were recaptured in the endline student survey. Columns 1–3 report treatment arm means; standard deviations in parentheses. Column 4 reports the F-statistic and associated p-value for the null that treatment assignment does not predict each baseline characteristic. Regressions include age-bin, sex, and strata fixed effects; the age regression excludes age-bin fixed effects and the female regression excludes the sex fixed effect. The joint orthogonality test uses the displayed baseline characteristics other than age and female. Bivariate tests compare mentoring–control ([a]), information–control ([b]), and mentoring–information ([c]), with the same controls. Standard errors are clustered at the school level. Binary variable means are proportions; age is measured in years and household size in persons. Child-labor sample sizes are reported separately because of missing baseline responses.

A.6 Correlates of Follow Through Failures

Table A6: Baseline Predictors of Enrollment Plans, Enrollment, and Follow-Through

Baseline characteristic	Plans to enroll (1)	Enrolls in lower secondary (2)	Follows through on plans (3)
Age (years)	−0.027	−0.072***	−0.095***
	(0.017)	(0.015)	(0.023)
Female	−0.004	−0.042	−0.076*
	(0.028)	(0.030)	(0.038)
Speaks Spanish at home	0.058	−0.010	−0.004
	(0.036)	(0.035)	(0.043)
Household size	0.006	−0.017***	−0.017**
	(0.005)	(0.005)	(0.007)
Mom and dad are coresidents	0.018	−0.050	−0.073*
	(0.037)	(0.031)	(0.039)
Household has running water	0.017	−0.011	−0.077
	(0.053)	(0.041)	(0.049)
Household has electricity	0.020	−0.030	0.033
	(0.055)	(0.048)	(0.061)
Household has internet	0.037	0.019	−0.020
	(0.034)	(0.039)	(0.047)
Plans to enroll in lower secondary	—	0.329***	—
		(0.040)	
Student is in child labor	0.036	−0.036	−0.064
	(0.031)	(0.031)	(0.040)
Plans to migrate internationally	0.016	−0.052	−0.028
	(0.049)	(0.045)	(0.056)
Missed school in past week	−0.024	−0.014	−0.019
	(0.038)	(0.031)	(0.037)
Household wealth above median	0.022	0.069**	0.074*
	(0.041)	(0.030)	(0.042)
School dropout rate above median	−0.264***	−0.161**	−0.187**
	(0.087)	(0.063)	(0.087)
Grade 6 enrollment below median	0.102	0.108	0.054
	(0.105)	(0.082)	(0.132)
No nearby lower secondary school	−0.034	0.005	0.056
	(0.045)	(0.039)	(0.041)
Outcome mean	0.651	0.502	0.658
Observations	805	805	524

Notes: The sample is control arm students observed at baseline with a response to whether they planned to enroll in lower secondary and with nonmissing values for the listed predictors; Columns 1–2 contain 805 students and Column 3 contains 524 baseline planners. Columns 1–3 report coefficients from separate linear regressions of baseline enrollment plans, lower secondary enrollment, and enrollment among baseline planners, respectively. Plans to enroll is included as a predictor only in Column 2 because it is the outcome in Column 1 and Column 3 only includes students planning to enroll. All regressions include strata fixed effects. Standard errors clustered by baseline school are in parentheses. Sources: `2_finaldta.dta`, `admin_final.dta`, `final 231 school sample_fv.dta`, and `2_school_routes.dta`.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.7 Treatment Effects on Enrollment and School Attendance

Table A7: Treatment effects on enrollment and school attendance

	Enrolled in school, 2025			Attending school, 2025		
Sample	Full sample	No plans	Plans	Full sample	No plans	Plans
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Intent to Treat						
Information	−0.020	−0.023	−0.002	−0.016	−0.012	−0.003
	(0.034)	(0.040)	(0.035)	(0.034)	(0.038)	(0.035)
+ Mentoring	0.076**	0.015	0.072**	0.079**	0.013	0.074**
	(0.033)	(0.043)	(0.034)	(0.033)	(0.042)	(0.035)
Mentoring vs Control	0.057*	−0.008	0.070**	0.063*	0.000	0.070**
	(0.033)	(0.042)	(0.031)	(0.033)	(0.040)	(0.032)
Control mean	0.516	0.234	0.667	0.485	0.202	0.636
Observations	2,332	805	1,527	2,332	805	1,527

Notes: All regressions include strata, age-bin, and sex fixed effects, with standard errors clustered at the school level in parentheses. "Information" reports the effect relative to control. "+ Mentoring" reports the effect of mentoring relative to information. "Mentoring vs Control" reports the sum of both coefficients, corresponding to the mentoring–control comparison. All columns restrict the sample to students with completed baseline surveys and an observed response to the baseline enrollment plans question. Within each outcome, the first column reports the full restricted sample, the second students without baseline plans to enroll, and the third students with baseline plans to enroll. Enrollment is defined as enrollment in school in 2025 at any level, including primary and lower secondary. All outcomes are constructed as the mode across administrative records, the student endline survey, the caregiver survey, and the endline community census. Results using the mean across sources, individual data sources, and additional school transition outcomes are reported in the Online Appendix. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.8 Robustness and Additional School Transition Outcomes

Table A8: Robustness of enrollment and attendance results

Outcome	Full sample			No plans			Plans		
	Info.	+ Mentoring	N	Info.	+ Mentoring	N	Info.	+ Mentoring	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Mean across sources									
Enrolled in school, 2025	−0.016	0.072**	2,332	−0.017	−0.004	805	0.001	0.076**	1,527
	(0.033)	(0.032)		(0.039)	(0.040)		(0.035)	(0.033)	
Attending school, 2025	−0.017	0.072**	2,332	−0.011	−0.010	805	−0.004	0.078**	1,527
	(0.033)	(0.032)		(0.037)	(0.038)		(0.035)	(0.034)	
Panel B: Enrollment by data source									
Student report	−0.015	0.064*	1,507	−0.014	−0.008	454	−0.003	0.068*	1,053
	(0.035)	(0.036)		(0.056)	(0.059)		(0.037)	(0.035)	
Caregiver report	0.005	0.061*	1,639	−0.036	0.004	514	0.037	0.062*	1,125
	(0.035)	(0.036)		(0.050)	(0.055)		(0.036)	(0.035)	
Short census	−0.018	0.049	2,134	−0.004	−0.064	716	−0.003	0.069*	1,418
	(0.035)	(0.036)		(0.048)	(0.048)		(0.038)	(0.038)	
Administrative records	−0.013	0.079**	2,332	−0.005	0.008	805	0.001	0.078**	1,527
	(0.035)	(0.033)		(0.041)	(0.042)		(0.037)	(0.034)	
Panel C: Attendance by data source									
Student report	0.006	0.062*	1,478	0.040	−0.042	440	0.010	0.068*	1,038
	(0.038)	(0.037)		(0.054)	(0.060)		(0.038)	(0.036)	
Caregiver report	0.003	0.055	1,635	−0.030	0.000	513	0.033	0.049	1,122
	(0.034)	(0.035)		(0.047)	(0.051)		(0.037)	(0.037)	
Short census	−0.012	0.047	2,132	−0.007	−0.064	715	0.008	0.067*	1,417
	(0.036)	(0.036)		(0.048)	(0.047)		(0.038)	(0.038)	
Administrative records	−0.020	0.082**	2,332	−0.006	0.012	805	−0.009	0.080**	1,527
	(0.035)	(0.033)		(0.039)	(0.041)		(0.038)	(0.035)	

Notes: The sample is 2,332 students with completed baseline surveys and observed baseline enrollment plans (805 without plans; 1,527 with plans). Columns 1–3 report the full sample, Columns 4–6 students without plans, and Columns 7–9 students with plans. Within each group, "Information" is the information–control difference, "+ Mentoring" is the mentoring–information difference, and N is the number of regression observations. Panel A replaces the mode across sources used in Table A7 with the mean; Panels B and C report individual sources. Regressions include strata, age-bin, and sex fixed effects. Standard errors are clustered at the baseline school level and reported in parentheses. Outcomes are coded on a zero to one scale, so coefficients are differences in proportions. Row-specific sample sizes vary with data availability. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Robustness of enrollment and attendance results (continued)

Outcome	Full sample			No plans			Plans		
	Info.	+ Mentoring	N	Info.	+ Mentoring	N	Info.	+ Mentoring	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel D: Additional school transition outcomes									
Enrolled in lower secondary	−0.036	0.077**	2,332	−0.024	0.023	805	−0.027	0.069**	1,527
	(0.034)	(0.032)		(0.039)	(0.041)		(0.036)	(0.034)	
Passed grade 6, mean across sources	−0.022**	−0.005	2,332	−0.003	−0.005	805	−0.029***	−0.009	1,527
	(0.010)	(0.011)		(0.015)	(0.016)		(0.011)	(0.013)	
Passed grade 6, mode across sources	−0.017*	−0.009	2,332	0.006	−0.015	805	−0.028**	−0.010	1,527
	(0.010)	(0.012)		(0.017)	(0.017)		(0.011)	(0.014)	
Likes going to school	0.044**	−0.020	1,270	0.091*	−0.076**	375	0.019	−0.002	895
	(0.019)	(0.016)		(0.047)	(0.038)		(0.017)	(0.015)	
Passed grade 6 and attending, mean	−0.031	0.074**	2,332	−0.019	0.016	805	−0.023	0.067*	1,527
	(0.033)	(0.032)		(0.036)	(0.038)		(0.036)	(0.035)	
Passed grade 6 and attending, mode	−0.031	0.074**	2,332	−0.027	0.016	805	−0.021	0.069*	1,527
	(0.034)	(0.033)		(0.037)	(0.041)		(0.037)	(0.035)	
Did not miss school last week, mean	0.022	0.023	1,754	0.012	−0.058	560	0.041	0.024	1,194
	(0.033)	(0.033)		(0.045)	(0.046)		(0.036)	(0.036)	
Did not miss school last week, mode	0.022	0.020	1,754	0.011	−0.056	560	0.042	0.018	1,194
	(0.035)	(0.034)		(0.049)	(0.049)		(0.038)	(0.037)	
Passed first year of lower secondary	−0.025	0.046	2,263	−0.016	0.014	792	−0.010	0.028	1,471
	(0.034)	(0.034)		(0.034)	(0.036)		(0.041)	(0.039)	

Notes: The sample is 2,332 students with completed baseline surveys and observed baseline enrollment plans (805 without plans; 1,527 with plans). Columns 1–3 report the full sample, Columns 4–6 students without plans, and Columns 7–9 students with plans. Within each group, "Information" is the information–control difference, "+ Mentoring" is the mentoring–information difference, and N is the number of regression observations. Panel D reports additional school-transition outcomes. Regressions include strata, age-bin, and sex fixed effects. Standard errors are clustered at the baseline school level and reported in parentheses. Outcomes are coded on a zero to one scale, so coefficients are differences in proportions. Row-specific sample sizes vary with data availability. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.9 Heterogeneity by Household Wealth

Table A9: Heterogeneity in treatment effects by household wealth

	Baseline household wealth		Above - below
Treatment contrast	Below median	Above median	Difference
	(1)	(2)	(3)
Information	−0.022	−0.008	0.014
	(0.038)	(0.042)	(0.043)
+ Mentoring	0.097**	0.052	−0.046
	(0.039)	(0.042)	(0.045)
Mentoring vs Control	0.075**	0.044	−0.032
	(0.038)	(0.040)	(0.043)
Control mean	0.463	0.566	–
Observations	1,172	1,160	2,332

Notes: The sample is 2,332 students with completed baseline surveys and an observed response to the baseline enrollment plans question. The outcome is enrollment in any school in 2025. Columns 1 and 2 report treatment effects below and above median baseline household wealth; Column 3 reports the above minus below difference in effects. "Information" is the information–control difference, "+ Mentoring" is the mentoring–information difference, and "Mentoring vs Control" is their sum. Observations in Columns 1 and 2 are group counts; Column 3 uses the full regression sample. The regression includes strata, age-bin, and sex fixed effects interacted with the wealth indicator. Standard errors clustered at the baseline school level are in parentheses. The outcome is binary, so coefficients are differences in proportions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.10 Heterogeneity by School Size

Table A10: Heterogeneity in treatment effects by school size

	Baseline school size		Smaller - larger
Treatment contrast	Larger	Smaller	Difference
	(1)	(2)	(3)
Information	−0.012	−0.034	−0.022
	(0.041)	(0.053)	(0.067)
+ Mentoring	0.093**	0.043	−0.051
	(0.040)	(0.057)	(0.070)
Mentoring vs Control	0.082**	0.009	−0.073
	(0.040)	(0.054)	(0.067)
Control mean	0.527	0.483	–
Observations	1,731	594	2,325

Notes: The sample is 2,325 students with completed baseline surveys, an observed response to the baseline enrollment plans question, and an observed school size measure. Seven students in the 2,332 sample have no school size measure. The outcome is enrollment in any school in 2025. Columns 1 and 2 report treatment effects in larger and smaller schools; Column 3 reports the smaller minus larger difference in treatment effects. Smaller schools have below-median baseline Grade 6 enrollment. "Information" is the information–control difference, "+ Mentoring" is the mentoring–information difference, and "Mentoring vs Control" is their sum. Observations in Columns 1 and 2 are group counts; Column 3 uses the full regression sample. The regression includes strata, age-bin, and sex fixed effects interacted with the school size indicator. Standard errors clustered at the baseline school level are in parentheses. The outcome is binary, so coefficients are differences in proportions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.11 Heterogeneity by Baseline Plans and Household Wealth

Table A11: Heterogeneity in treatment effects by baseline enrollment plans and household wealth

	Below-median wealth		Above-median wealth		Treatment-effect differences	
Treatment contrast	No plans	Plans	No plans	Plans	Plans - no plans	Above - below
	(1)	(2)	(3)	(4)	(5)	(6)
Information	−0.033	−0.014	−0.002	0.017	0.019	0.031
	(0.044)	(0.039)	(0.045)	(0.043)	(0.049)	(0.040)
+ Mentoring	0.052	0.116***	−0.034	0.030	0.064	−0.086**
	(0.046)	(0.037)	(0.049)	(0.042)	(0.049)	(0.040)
Mentoring vs Control	0.019	0.102***	−0.036	0.047	0.083*	−0.055
	(0.044)	(0.036)	(0.047)	(0.037)	(0.045)	(0.038)
Control mean	0.215	0.615	0.256	0.711	–	–
Observations	436	736	369	791	2,332	2,332

Notes: The sample is 2,332 students with completed baseline surveys and an observed response to the baseline enrollment plans question. The outcome is enrollment in any school in 2025. Columns 1–4 report treatment effects for the four groups with baseline enrollment plans and household wealth. Columns 5 and 6 report the plans minus no-plans and above minus below treatment interactions. Interactions are estimated together, so the plans interaction is conditional on wealth and the wealth interaction is conditional on plans. "Information" is the information–control difference, "+ Mentoring" is the mentoring–information difference, and "Mentoring vs Control" is their sum. Observations in Columns 1–4 are group counts; Columns 5 and 6 use the full regression sample. The regression includes strata, age-bin, and sex fixed effects separately interacted with plans and wealth. Standard errors clustered at the baseline school level are in parentheses. The outcome is binary, so coefficients are differences in proportions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.12 Heterogeneity by Baseline Plans and School Size

Table A12: Heterogeneity in treatment effects by baseline enrollment plans and school size

	Larger schools		Smaller schools		Treatment-effect differences	
Treatment contrast	No plans	Plans	No plans	Plans	Plans - no plans	Smaller - larger
	(1)	(2)	(3)	(4)	(5)	(6)
Information	−0.027	0.004	−0.042	−0.011	0.031	−0.015
	(0.043)	(0.040)	(0.060)	(0.056)	(0.049)	(0.062)
+ Mentoring	0.027	0.079**	0.020	0.072	0.052	−0.007
	(0.048)	(0.037)	(0.065)	(0.062)	(0.051)	(0.067)
Mentoring vs Control	0.000	0.083**	−0.022	0.061	0.083*	−0.022
	(0.046)	(0.037)	(0.062)	(0.055)	(0.046)	(0.063)
Control mean	0.225	0.700	0.266	0.579	–	–
Observations	591	1,140	210	384	2,325	2,325

Notes: The sample is 2,325 students with completed baseline surveys, an observed response to the baseline enrollment plans question, and an observed school size measure. Seven students in the 2,332 sample have no school size measure. The outcome is enrollment in any school in 2025. Columns 1–4 report treatment effects for the four groups defined by baseline enrollment plans and school size. Columns 5 and 6 report the plans minus no-plans and smaller minus larger treatment interactions. The interactions are estimated together, so the plans interaction is conditional on school size and the school size interaction is conditional on plans. Smaller schools have below-median baseline Grade 6 enrollment. "Information" is the information–control difference, "+ Mentoring" is the mentoring–information difference, and "Mentoring vs Control" is their sum. Observations in Columns 1–4 are group counts; Columns 5 and 6 use the full regression sample. The regression includes strata, age-bin, and sex fixed effects interacted separately with plans and school size. Standard errors clustered at the baseline school level are in parentheses. The outcome is binary, so coefficients are differences in proportions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.13 Heterogeneity by Baseline Plans, Household Wealth, and School Size

Table A13: Heterogeneity in treatment effects by baseline enrollment plans, household wealth, and school size

Treatment contrast	Reference group (1)	Plans interaction (2)	Wealth interaction (3)	School size interaction (4)
Information	−0.037	0.029	0.030	−0.012
	(0.048)	(0.050)	(0.040)	(0.063)
+ Mentoring	0.063	0.058	−0.086**	−0.002
	(0.051)	(0.051)	(0.040)	(0.069)
Mentoring vs Control	0.026	0.087*	−0.056	−0.014
	(0.049)	(0.046)	(0.038)	(0.063)
Reference-group control mean	0.195	–	–	–
Observations	2,325	2,325	2,325	2,325

Notes: The sample is 2,325 students with completed baseline surveys, an observed response to the baseline enrollment plans question, and observed wealth and school size measures. Seven students in the 2,332 sample have no school size measure. The outcome is enrollment in any school in 2025. Column 1 reports treatment effects for the reference group: students without baseline enrollment plans, with below median baseline household wealth, in larger schools. Columns 2–4 report the plans minus no-plans, above minus below wealth, and smaller minus larger school size treatment interactions, respectively. The three interactions are estimated together, so each is conditional on the other two. Smaller schools have below-median baseline Grade 6 enrollment. "Information" is the information–control difference, "+ Mentoring" is the mentoring–information difference, and "Mentoring vs Control" is their sum. The regression includes strata, age-bin, and sex fixed effects interacted separately with each heterogeneity indicator. Standard errors clustered at the baseline-school level are in parentheses. The outcome is binary, so coefficients are differences in proportions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.14 Human Trafficking Knowledge Index Components

Table A14: Components of the human trafficking knowledge index

Outcome	Pooled		Non-planners		Planners	
	Information mean (1)	Mentoring vs Info (2)	Information mean (3)	Mentoring vs Info (4)	Information mean (5)	Mentoring vs Info (6)
Has heard of human trafficking	0.372	−0.046	0.295	−0.003	0.418	−0.084*
		(0.038)		(0.063)		(0.044)
		[0.632]		[0.976]		[0.632]
Recognizes victims can be any age or sex	0.509	−0.045	0.439	0.014	0.549	−0.068*
		(0.036)		(0.060)		(0.041)
		[0.632]		[0.976]		[0.632]
Recognizes domestic human trafficking risk	0.430	−0.064**	0.462	−0.077	0.411	−0.051
		(0.032)		(0.057)		(0.040)
		[0.632]		[0.632]		[0.632]
Reports an action in response to trafficking	0.630	−0.025	0.526	−0.031	0.690	−0.044
		(0.038)		(0.061)		(0.043)
		[0.807]		[0.818]		[0.666]
Reports an appropriate local response	0.428	0.003	0.387	0.010	0.451	−0.005
		(0.037)		(0.064)		(0.043)
		[0.976]		[0.976]		[0.976]
Identifies a human trafficking risk situation	0.545	−0.026	0.474	0.047	0.586	−0.056
		(0.045)		(0.060)		(0.051)
		[0.807]		[0.768]		[0.663]
Identifies an appropriate response	0.636	−0.017	0.595	−0.002	0.660	−0.027
		(0.031)		(0.061)		(0.035)
		[0.807]		[0.976]		[0.768]
Identifies situations that increase trafficking risk	0.309	−0.047	0.277	−0.058	0.327	−0.039
		(0.037)		(0.049)		(0.048)
		[0.632]		[0.632]		[0.768]
Information-arm observations	470		173		297	
Regression observations		978		332		646

Notes: The sample consists of 978 baseline students in the mentoring and information arms with an observed response to the baseline enrollment plans question and an available midline survey, including 332 non-planners and 646 planners. The control arm is excluded from these midline comparisons because of differential attrition (Section 2.4). Columns 1–2 report results for the pooled sample, Columns 3–4 for non-planners, and Columns 5–6 for planners. Within each group, the first column reports the information arm mean and the second the mentoring–information difference. All regressions include strata, age-bin, and sex fixed effects, with standard errors clustered at the school level and reported in parentheses. False discovery rate adjusted q-statistics are reported in square brackets and are computed jointly across all 24 mentoring–information tests in the table, following Anderson (2008). All outcomes are binary components of the human trafficking knowledge index, so means are proportions and treatment effects are differences in proportions. Outcomes are from the midline student survey. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.15 Syllabus of Mentoring Sessions

A.15.1 Sessions with students

The following sessions are conducted in groups. After each group session, two individual 30-minute sessions are conducted. During these sessions, the mentor focuses on the particular situation of each student and provides personalized mentoring. Each student will have a minimum of two individual sessions as part of the program. The remaining individual sessions will be assigned to students who need them most as determined by the mentor.

Session 1: What do I want to be?

- Students become familiar with the program and the objective of the project
- Students reflect on their life plans and whether education will help them realize them

Session 2: Self-Esteem

- Children recognize that they have inner value and that they are important
- I can be wrong or make mistakes: Making mistakes is a normal part of life and does not make us bad or less valuable people
- Children recognize that they have the qualities to succeed in school and achieve their goals

Session 3: Getting to know professions and careers

- Children identify which professions and careers they could choose according to their tastes, preferences, goals, and dreams
- Children learn the routes of the educational system to become a professional

Session 4: Children's rights in the family context

- Explanation of children's rights: right to education, reproductive rights
- Discussion of the risks and consequences of dropping out of school
- Discussion of the risks, consequences, and prevention of teenage pregnancies

Session 5: Child Labor in Guatemala and Quiché

- Children learn about what is child labor and what is not, discussing consequences and risks
- Reflection on the distribution of each student's time in and out of school
- Life stages and priorities at each stage: Prioritizing school over work in childhood and adolescence

Session 6: Migration and Human Trafficking

- Children learn to identify types of migration and its causes
- Develop an understanding of independent migration and the risks faced by children and adolescents
- Definition and types of human trafficking
- Children learn about the frequent discourses used by traffickers and reflect on the risks faced by children and adolescents who become victims
- Learn about institutions and resources available for help

Session 7: Importance of lower secondary education for a better future

- Develop awareness of the impact that education in general, and lower secondary education in particular, can have on their lives
- Students recognize that learning is a beautiful thing that can help them grow and achieve their goals

Session 8: Getting to know my lower secondary school

- This session was actually conducted as the next to last session in all schools
- Mentors show students a video of a visit to a lower secondary school that is similar to a neighboring school (urban/rural)
- Mentors discuss with students how to enroll, attend, and transport themselves to the nearest lower secondary school

Session 9: Growth Mindset

- Children learn that skills and attitudes can be learned and developed with effort and practice, and are not fixed or determined
- Children recognize that they can develop the qualities they still lack to succeed in school and in life

Session 10: Looking for alternatives to study

- Children learn alternatives to continue studying if their families' resources limit these possibilities
- Invite NGOs that provide scholarships or CCTs to inform about their programs

Session 11: Perseverance and effort

- Children understand the importance of completing homework assignments
- Children learn to be self-motivated to achieve their goals and do good quality tasks

Session 12: Making decisions

- Children understand the steps of decision-making and goal-setting, developing independence and initiative skills to make decisions

Session 13: Communication and conflict resolution

- Children learn strategies for resolving conflicts or coping with problems
- Children can listen to and consider the opinions, views, and feelings of others

Session 14: My life project

- Children learn to differentiate between short-term and long-term goals
- Children make a life plan with their long-term goals
- Children define their short-term goals, which could help them realize their life project

Session Delivery Adjustments Some schools experienced session delays due to local holidays or school activities. To ensure all schools in the mentoring arm received every session before the school year ended, sessions 12 and 13 were merged into a single session for 41 schools, covering the content of both sessions. Other schools received double sessions—two full sessions in one day—and only one school received three full sessions in a single day. The table below shows the number of schools receiving double sessions.

Table A15: Number of schools experiencing multiple mentoring sessions in one day

Sessions given in the same day	Number of schools
2 and 3	4
3 and 4	1
4 and 5	5
5 and 7	2
6 and 7	8
7 and 9	2
9 and 10	1
9, 10 and 11	1
9 and 11	1
10 and 11	7
13 and 14	1

These are sessions delivered in the same day. Each session is delivered in full.

A.15.2 Sessions with caregivers

Work with caregivers involves three meetings with all caregivers, and individual meetings with prioritized caregivers in their home or a place of their choice. A prioritized caregiver is the person in charge of a student with a higher risk of dropping out of school (low grades, lack of motivation, lack of parental support).

Initial meeting with caregivers

- Presentation of the program: issues, objectives, program activities aimed at teachers, caregivers, and students
- Describe role of caregivers in program development
- Basic information on returns to education, dropout rates, risks associated with child labor and human trafficking

Second meeting with caregivers

- Conducted after the first eleven weeks of mentoring with students
- Provide information on the transition to lower secondary
- Highlight benefits of education for the future of students, and the importance of lower secondary school for subsequent education opportunities
- Provide information on scholarships for different education levels
- Introduce locally famous people and their professions
- Discussion of progress made, challenges faced, proposals for the future and the role of caregivers in the continuity of the program
- Receive caregiver suggestions

Third meeting with caregivers

- Highlight children's rights, like access to education
- Discuss causes and consequences of school dropout, and provide information on returns to education and salary ranges by career and education level in Guatemala
- Describe child labor, its consequences, and how to report situations of child labor
- Discuss migration and risks related to unaccompanied child migration, including types of human trafficking and where to report potential human trafficking situations

Individual visits at home

- Talk with caregivers about the importance of education for the student's future. The student's presence was optional in these discussions
- Listen to caregiver suggestions and alternatives are proposed to continue with the student's education
- General tips for parents to support the student's education as provided by the mentor. Tailored advice was provided depending on the family/student situation
- At the end of the talk, the agreed commitments are highlighted

A.15.3 Sessions with teachers

Initial session with teachers and director

- Presentation of the program: issues, objective, program activities aimed at teachers, caregivers and students
- Teacher suggestions for improving the program
- Discussion of each student's situation to focus on individual sessions

- Basic information on returns from education, school dropout rates, risks associated with child labor and human trafficking
- Role and commitment of teachers during the development of the program

Mid-term evaluation session *Note:* This session was merged with the second caregiver session. Only the follow-up for each student detailed below happened only with teachers, without the parents' presence.

- Discussion of progress made, challenges faced, proposals for the future after the first eleven student sessions
- Role and commitment of teachers for program continuity
- Teacher feedback and suggestions
- Follow up of each student's progress and challenges (this was not part of the merged session with caregivers)

Final evaluation session *Note:* This session was merged with the last caregiver session per teachers' request.

- Final presentation of program progress
- Role of teachers beyond the project, including agreements with teachers on how to promote the transition process to lower secondary education
- Final thanks to the teachers

A.15.4 Closing activity

After all sessions were completed, each school had a closing activity in which children, caregivers and teachers were awarded a diploma for completion of the sessions. Students were also gifted a student kit of school supplies (geometry tools, pencils, notebooks) and caregivers were given a bag of groceries and supplies worth 20 Guatemalan Quetzales.

A.16 Syllabus of Information Sessions

For the information treatment, two sessions were designed to align with the information covered in the mentoring sessions. Students received two separate sessions, while caregivers and teachers each had one session, followed by a joint session for both groups, totaling five scheduled sessions. Each session included a presentation followed by a discussion, with time for questions after each topic. The lack of computers and projectors in schools made it infeasible to have PowerPoint presentations. Instead, posters were printed to be explained and shown to the participants. At the end of each session, participants were given a leaflet summarizing the information covered.

Session 1

- Session content corresponds to student sessions 3, 7, 8, 10
- Leaflet includes information on returns to schooling, professions and educational opportunities, alternatives to study, etc.

Session 2

- Session content corresponds to student sessions 4, 5, 6, 7
- Leaflet includes information sheet on child labor, migration, human trafficking and returns to education (salaries)

Closing activity After all sessions were completed, each school had a closing activity in which children, caregivers and teachers were awarded a diploma for completion of the sessions. Students were also gifted a student kit of school supplies (geometry tools, pencils, notebooks) and caregivers were given a bag of groceries and supplies worth 20 Guatemalan Quetzales.