Beyond Beliefs:

Experimental Evidence on Follow-Through Frictions at School Transitions*

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A key puzzle in economics is why households underinvest in education despite high returns. While standard explanations focus on information, this paper highlights the role of follow-through frictions in a school-level RCT in 231 primary schools in Quiché, Guatemala. We document a substantive intention-action gap: 65% of students plan to enroll in secondary school yet 34% of those fail to follow-through on their plans. To causally isolate the effect of follow-through frictions, we randomly assign schools to a pure control, an information treatment, and a mentoring treatment that delivers identical informational content, along with follow-through support comprising classroom based socio-emotional skill-building and personalized educational counseling. Information provision has no effect on enrollment. Mentoring increases enrollment by 8.4 percentage points (18%) relative to information, with larger effects among students who already planned to enroll. The evidence points to socio-emotional skills as the operative channel in how mentoring helped improve follow-through.

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1 Introduction

Across low and middle income countries, major losses in school progression occur around school transitions from primary to lower secondary and from lower to upper secondary even where tuition is free and the wage premium is substantial (UNICEF, 2021; Adelman and Székely, 2017). A frequent explanation centers on information frictions: households hold inaccurate beliefs about the returns to, costs of, or processes required for continued schooling, leading to underinvestment (Jensen, 2010; Nguyen, 2008). 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).

Yet information frictions are not the only barrier. Even when beliefs are broadly correct, students and families may fail to act on their intentions. We refer to these as *follow-through frictions*: inertia, planning failures, limited self-control, administrative complexity, and insufficient social support can all prevent the translation of educational plans into enrollment decisions (O'donoghue and Rabin, 1999; Bhargava and Manoli, 2015a; Alan et al., 2021). Despite their potential importance, the role of follow-through frictions in educational decision-making remains poorly understood, in large part because supporting students in following through on their plans typically also involves sharing information, making it difficult to disentangle these two channels.

This paper isolates the causal effect of relaxing follow-through frictions on educational investment. We conduct a school-level randomized controlled trial in 231 primary schools (with 3,273 sixth-grade students) in Quiché, Guatemala, where approximately half of students fail to transition to lower secondary school. We randomly assign schools to three arms: (i) control, (ii) an information treatment providing 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 but additionally provides individualized support and builds socio-emotional skills through a dedicated school-based mentor. Compliance is high: students in both treatment arms attended an average of 88% of sessions. Since the informational content is common across the two treatment arms, the information vis-à-vis the control group identifies the effect of correcting beliefs, while the mentoring vs information contrast identifies the effect of relaxing follow-through frictions.[2]

[2]The mentoring arm involves more contact time than the information arm (fourteen two-hour weekly sessions plus individual educational counseling, versus two one-hour group sessions). However, the additional mentoring time is devoted to socio-emotional skill-building and individualized

Our primary outcome in this study is school enrollment, and we have the universe of public and private school enrollments in Guatemala, yielding zero attrition. We complement this with in-class and at-home surveys to assess mechanisms such as beliefs, information retention, socio-emotional skills, and social support.

There is a 34% intention-action gap regarding school enrollment in our setting. In our baseline survey collected early in grade 6, the final year of primary, 65 percent of control subjects reported plans to enroll in lower secondary school next academic year. In reality, half enroll. Among those with plans to enroll, 34% fail to follow through. This intention-action gap is larger among poorer households, where 40% of planners do not enroll, but remains substantial at 29% even among less poor households. These patterns are difficult to reconcile with information frictions alone, as students already hold positive beliefs about returns to schooling at baseline and 83 percent say that they would enroll in lower secondary if it were up to them.

Information provision alone does not increase enrollment. The information treatment has no detectable effect on any educational outcome compared with the control group. In contrast, adding mentoring to this informational content increases enrollment by 8.4 percentage points ($p < 0.01$), an increase of approximately 18% relative to the control mean. Scaling for compliance, the LATE estimate implies a 13.6 percentage point effect for students who actively engaged with the program. This pattern of findings is similar for school attendance and alternative data sources on enrollment.

Heterogeneity patterns are consistent with follow-through frictions as the binding barrier. Mentoring effects are larger among students who already planned to enroll at baseline (the group for whom information is least likely to be the constraint). Effects are also larger among students from poorer households, who have fewer resources and less adult support to navigate the transition, and in larger schools, where individual guidance is otherwise scarce. In terms of mechanisms, we find a 0.19σ effect ($p < 0.05$) of mentoring relative to information on an enumerator-assessed soft-skills index that captures attentiveness and confidence. Effects on personal support measures are positive but not statistically significant. Although we cannot pinpoint the exact mechanism with certainty, the evidence is suggestive that mentoring may operate, at least in part, through socio-emotional skills.

This paper contributes to three literatures. First, we contribute to the literature on information frictions in educational decision-making. A large body of experimental

follow-up support, rather than to repeating informational content. Consistent with this, we do not find evidence that the mentoring group better recognizes session topics, informational content, or differentially updates beliefs about the returns to education more than the information group.

work provides households with information on returns to schooling (Jensen, 2010; Nguyen, 2008), enrollment processes (Dinkelman and Martínez A, 2014), or school quality (Avitabile and De Hoyos, 2018), with mixed effects on enrollment (see Kremer and Holla, 2009, for a review). Our null information effect is not simply another entry in this mixed ledger. In our setting, baseline beliefs are already broadly positive — 80 percent of students want to continue schooling, and perceived returns are high — yet half fail to enroll. The information null, combined with the strong mentoring effect, identifies the source of the gap: the binding constraint is not what families know but whether they can act on it.

Second, we contribute to the literature on behavioral barriers to take-up. A growing body of work documents that inertia, complexity, and present bias prevent individuals from acting on beneficial decisions in savings (Madrian and Shea, 2001), tax filing (Bhargava and Manoli, 2015a), and agricultural technology adoption (Duflo et al., 2011). We extend this framework to educational transitions, where the decision is high-stakes, one-shot, and effectively irreversible. We document a substantial intention-action gap (35% of students who plan to enroll fail to do so) and show experimentally that an intervention targeting follow-through frictions, rather than information, partially closes this gap. This provides some of the first pieces of experimental evidence isolating follow-through frictions as a binding barrier to human capital investment at a schooling transition.

Third, we contribute to the literature on mentoring and socio-emotional skills. Mentoring, soft-skills development, and tutoring programs produce positive effects on educational outcomes in several developing-country settings (Banerjee et al., 2007; Loyalka et al., 2013; Alan and Ertac, 2018; Alan et al., 2019; Dhar et al., 2022; Edmonds et al., 2023; Shah et al., 2024), but the mechanisms through which they operate are poorly understood because programs bundle information, social support, and skill-building. Our design holds informational content fixed across arms, allowing us to isolate the follow-through support component. The evidence in this paper points to socio-emotional skills — attentiveness, confidence, and persistence — rather than social support or belief updating as the channel through which mentoring raises enrollment. This connects to work showing that non-cognitive skills matter for life outcomes (Heckman et al., 2006) and that targeted interventions can build them (Alan et al., 2019; Sarzosa and Urzúa, 2021), but adds that these skills can serve as a mechanism for closing the gap between educational intentions and actions.

2 Research Design

2.1 Conceptual Framework

Educational underinvestment at key transition points is difficult to reconcile with the standard human capital model, under which 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, leading to underinvestment even when enrollment would be optimal (Jensen, 2010; Nguyen, 2008; Dinkelman and Martínez A, 2014). Second, students may be constrained by *follow-through frictions*: even with accurate beliefs, they may fail to act on them due to inertia (Madrian and Shea, 2001), administrative complexity (Bhargava and Manoli, 2015a), or planning and self-control problems that prevent sustained engagement with high-stakes decisions (O'donoghue and Rabin, 1999; Alan et al., 2021).

To help interpret our experimental results, we develop a simple stylized framework (see Online Appendix F) in which a student at a schooling transition decides whether to attempt enrollment in the next level. The student's decision depends on her perceived net return to continued schooling and on her capacity to follow through on an intended decision. Under the neoclassical benchmark, enrollment depends only on perceived returns exceeding costs. Once follow-through frictions are introduced, however, the effective enrollment threshold rises: a student who correctly perceives high returns may nonetheless fail to enroll if her ability to act on that perception is sufficiently low. The model highlights that, under the implicit assumption that students can costlessly act on their beliefs, information provision alone should change enrollment decisions (if beliefs are incorrect). When follow-through capacity is low, correcting beliefs may shift perceived returns without changing behavior.

While information frictions and the role of information provision in correcting them have been extensively studied (e.g., Jensen, 2010; Avitabile and De Hoyos, 2018; Nguyen, 2008), the role of follow-through frictions in educational decision-making is less well understood. The central identification challenge is to separate the two channels, as supporting students to follow through on their educational intentions typically also involves information sharing. We address this through a randomized trial with two treatment arms: (1) an information arm that provides students and caregivers with information about the returns to schooling and the enrollment process, and (2) a mentoring arm that delivers the same informational content but additionally provides

sustained personal support to help students put their plans into action. Because the informational content is identical across arms, both arms should equally shift beliefs about schooling returns, while mentoring additionally raises follow-through capacity. This allows us to isolate the effect of follow-through support from that of information provision.

2.2 Setting and Interventions

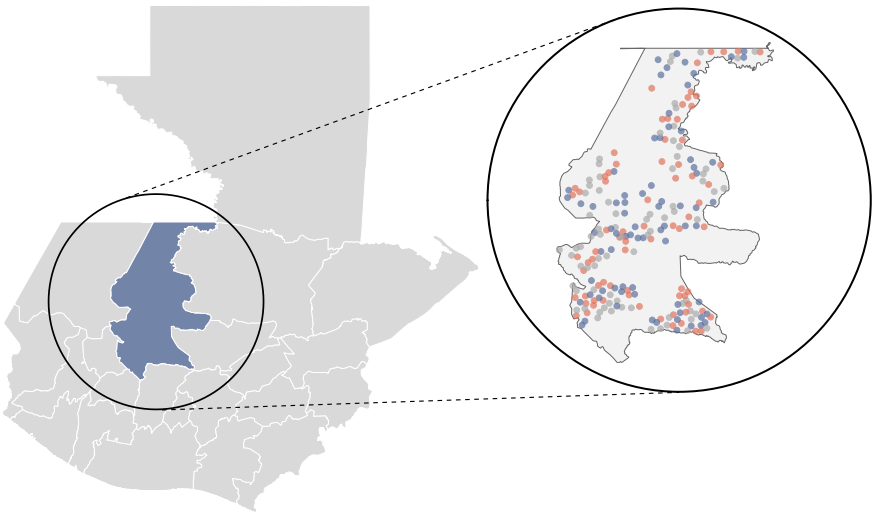


Figure 1: Study area

Notes: Grey indicates control schools, red indicates information-only, and blue indicates mentoring.

Our study is set in the department of Quiché, Guatemala (see Figure 1). 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). 89.2% of its population identify as indigenous (PNUD, 2022). The department also exhibits one of the sharpest dropout margins 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 16% (relative to primary). Although attendance is compulsory, progression is not automatic and requires active enrollment in a different school. This setting provides a natural context to study the mechanisms behind underinvestment in education.

We partnered with PRODESSA, a Guatemalan NGO with extensive experience in educational support in indigenous communities, to develop two interventions targeting information and follow-through frictions at the transition from primary to lower

secondary school (see Figure 2): (1) an information treatment and (2) a mentoring treatment. The *information treatment* consisted of five condensed one-hour sessions for students, caregivers, and teachers, covering topics such as returns to schooling, enrollment logistics, and risks associated with alternative paths such as child labor and illegal migration. Sessions were accompanied by posters left in classrooms, leaflets distributed to students, caregivers, and teachers, and culturally adapted comics read with students during sessions. The *mentoring treatment* delivered the same informational content but added follow-through support through (i) individual school counseling and (ii) sessions aimed at building students' socioemotional skills. A dedicated mentor per school conducted weekly two-hour group sessions with all students, individual student sessions, caregiver meetings, and teacher sessions. Both interventions lasted four months and were delivered during sixth grade, the year immediately preceding the transition to lower secondary school (see Appendix E.1 and E.2 for details on the curriculum of each treatment).

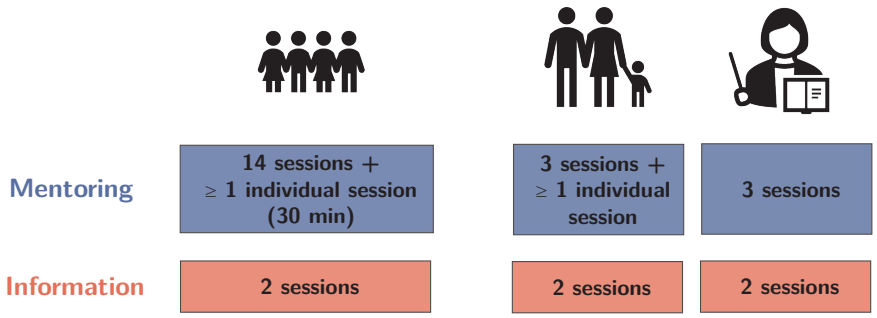


Figure 2: Intervention structure

2.3 Sampling and Randomization

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 set, we randomly selected 231 schools with a total of 3,273 sixth-grade students. To

limit spillovers, this selection step imposed a 2-kilometer buffer between sites.[3] The resulting sample was not designed to be representative of Quiché or Guatemala, but to maximize power to study educational underinvestment. Sample schools are more rural, more likely to be bilingual, and have fewer teachers than the average school in the department (Appendix Table A.1). By design, they also have substantially higher dropout rates: based on pre-intervention administrative records, 69 percent of sixth graders in sample schools failed to transition to lower secondary in 2021, compared to 55 percent in Quiché and 38 percent nationally (Appendix Table A.2).

The 231 schools were randomly assigned to one of three arms (mentoring, information, or control). Randomization was stratified by interacting five bins of historical dropout rates with three bins of sixth-grade enrollment.[4] Within each stratum, schools were assigned in equal proportions, yielding 77 schools per arm (see Appendix Figure A.1).

2.4 Data

The analysis in this study depends on administrative records, in-school surveys, household surveys, and community surveys.

Administrative records. The Ministry of Education (MOE) maintains detailed school enrollment and student performance data for all students enrolled in public and private schools in Guatemala. In the fall of 2023, they provided us with historical records for all schools in Quiché that allowed us to compute school size, historical dropout rates for 6th grade, and location for the purposes of sample selection and stratification. In February 2026, MOE provided us with 6th and 7th grade grades, year end pass / fail, and 7th grade enrollment status for all study subjects.[5]

[3]Of the 231 schools in the final sample, 16 were not selected during the initial draw: 10 schools declined participation prior to learning their treatment status and were replaced with randomly chosen backup schools, and 6 additional schools were added as resources became available. Of these 16, one incorrect school was visited at baseline as well as during treatment and ended up replacing the originally selected school. Additionally, during midline instrument design, we discovered that baseline enumerators had inadvertently collected data from the wrong location for two schools. Both schools remain in the analytic sample, but baseline data are unavailable for them.

[4]The 16 schools added after the initial randomization were placed in one additional stratum, yielding a total of 16 strata.

[5]MOE records came in waves before February 2026 but were finalized in late February. MOE provided 6th grade pass / fail data in May 2025 for study subjects. In November 2025, MOE provided 7th grade enrollment data for all study subjects.

In-school student surveys. We administered two 35-minute in-school surveys. The baseline survey was conducted between April 19 and July 15, 2024. 2,357 students in 231 schools were interviewed. The baseline survey captures household context, students' educational plans and aspirations and socio-emotional outcomes, along with broader outcomes such as child labor and migration intentions. It is used for balance checks, baseline controls, and heterogeneity analyses. The midline survey was collected between October 16, and December 9, 2024 after the completion of all interventions. All students present in grade 6 in the 231 study schools at the time of contact were interviewed regardless of their baseline status. The survey was designed to measure treatment recall, future intentions, and a broad set of potential mechanisms. High rates of attrition in control schools limit the usefulness of this data for another other than information vs mentoring arm comparisons (Section 2.5).

Household surveys. For endline, we visited a subset of baseline subjects in their homes. For study schools with a large number of study subjects, we randomly sampled a subset of students rather than interviewing all students from the baseline. As a result, our endline survey targeted 2,175 study subjects. For each subject, we attempted a subject interview and an adult caregiver interviewer. Caregivers are any coresident adult who identified as being primarily responsible for the care of the child was selected. The student survey lasted approximately 20 minutes, and the caregiver survey lasted 15 minutes.

Community surveys. Concurrent with the endline household surveys, we interviewed a local community leader to collect basic data on location and activity status for any 6th child we had any interaction with during the program period (April - November 2024). This includes baseline survey respondents, midline survey respondents, and mentoring or information session attendees for a total of 3,273 students from our 231 schools. We label these community surveys as a *Census* since they represent a full enumeration of all students with whom we had contact.

Variable definitions. Details on question wording, variable definitions, aggregation, and index constructions can be found in our pre-analysis plan. Enrollment in AY 2025 is our primary outcome of interest. As prespecified, enrollment is constructed from the mode of enrollment responses from the student endline survey, the caregiver endline survey, the endline census, and the administrative records. In cases with two modes, we prioritize the student survey first, if available, and the administrative

records second, that are available for all subjects. We explore sensitivity to constructing enrollment as the mean across these data sources in the appendix.

2.5 Validity

To assess the internal validity of our estimates, we examine (1) balance at baseline, (2) attrition across data sources and experimental arms, and (3) compliance with treatment assignment.

Baseline characteristics and balance. The average student in our sample is 12 years old, 65 percent speak Spanish at home, and 72 percent live with both parents. Most households have running water (80 percent) and electricity (87 percent), and just over a third have internet access. Roughly two-thirds plan to enroll in lower secondary school, a majority are in child labor (56 percent), and just under 15 percent plan to eventually migrate outside of Guatemala. We assess balance using three approaches: variable-by-variable F-tests, a joint SUR test, and bivariate pairwise orthogonality tests. Across all three checks, we find no evidence of systematic differences between experimental groups at baseline (Table B.1).[6]

Attrition. 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.[7] In our survey data, we observe substantial differential attrition at midline: more than 55 percent of control students attrit compared to approximately 35 percent in each treatment arm, driven almost entirely by school-level dropout from the survey (Table B.2).[8] We therefore restrict midline comparisons to the two treatment arms, where attrition rates are similar. 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 B.2).

[6]Baseline balance cannot be evaluated for the full administrative sample as baseline covariates are not observed for all individuals. We report balance on sex and age, which are available for the full sample, finding no evidence of imbalance (Table B.1).

[7]Students are linked across years using their national student ID. Students not appearing as enrolled in any lower secondary school in the following year are coded as having dropped out. Non-enrollment may therefore also capture matching errors, though these are likely minimal given that only two students in the sixth-grade administrative records (where coverage is universal) could not be matched using the national ID.

[8]Schools exited the survey due to decisions by parent councils, recommendations from teacher unions, and severe weather conditions impeding access. Reports citing rainfall are concentrated among control schools as well, suggesting that this category may partly capture non-weather-related refusals.

Compliance. Compliance with treatment was high across both arms (Appendix Table B.3). Among baseline respondents, 96 percent of students in the information arm and 98 percent of students in the mentoring arm attended at least 1 session. Caregiver attendance of at least one session was somewhat lower, at 89 percent in the information arm and 93 percent in the mentoring arm. Mentors additionally conducted at least one individual session with 77 percent of students and 42 percent of caregivers. Student reports at midline are consistent with an intervention occurring (Appendix Table B.4). 52 percent of the information arm and 46 percent of our mentoring arm report hearing of our implementing partner organization and more than half of students report school visits. Reported visit frequency aligns with the design: the median student in the mentoring arm reports weekly visits, compared to monthly visits 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 $\lambda_1(A_i, F_i)$ includes baseline age and sex indicators, and $\lambda_2(S_v)$ includes strata fixed effects.[9] D_v^I denotes school-level treatment assignment to an intervention that provides information (both arms), and D_v^M denotes school-level treatment assignment to the Mentoring arm. With this specification, γ_I is the mean difference in Y_{iv} is our measure of belief updating, γ_M is our measure of follow though frictions, and $\gamma_I + \gamma_M$ is the mean difference in Y_{iv} for subjects assigned to the mentoring arm compared to the control group. Standard errors are clustered at the school level.

We additionally report 2SLS estimates scaling for compliance, where we instrument for treatment receipt using random assignment, defining compliance as attending all sessions in the information arm and at least 12 out of 14 group sessions in the mentoring arm. For outcomes measured by multiple related survey items, we construct standardized indices following Anderson (2008). The full empirical strategy is documented in our pre-analysis plan;[10] deviations are listed in Appendix D.

[9] Our age effects are binned as we have a very small number of subjects age 11 and under and 14 and over. Hence *age effects* are: 11 and under, 12, 13, 14 and older.

[10] `https://www.socialscienceregistry.org/trials/13889`

3 Results

3.1 The Intention–Action Gap

Before presenting our experimental results, we first examine descriptive patterns of educational intentions and realized enrollment at the primary-to-lower-secondary transition. At baseline, 80 percent of students in the control group express a desire to continue to lower secondary, and 65 percent report planning to do so (Figure 3). Caregivers share positive views, with 78 percent of students reporting that their parents want them to enroll. This aligns with favorable 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.

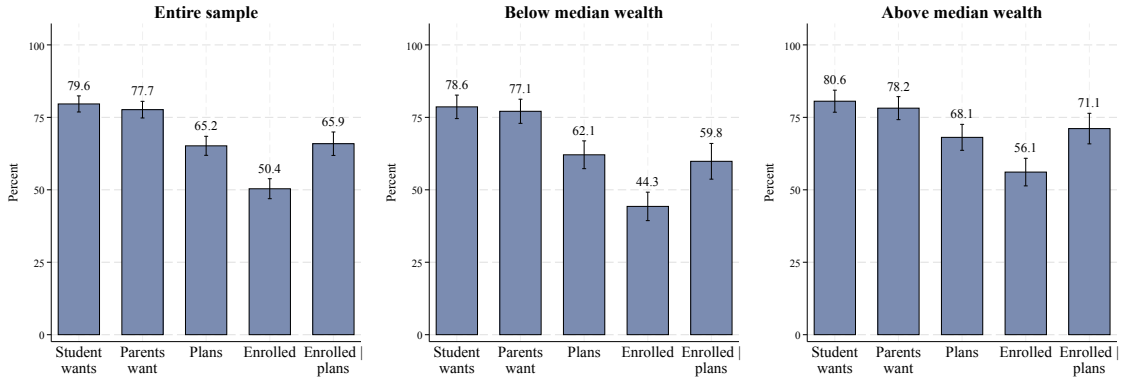


Figure 3: Intention-action gap by socioeconomic background (control group)

Notes: The figure reports the share of students in the control group 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 secondary school, and (v) enroll in secondary school conditional on answering that you have plans to do so. The sample is split by median household wealth. All "don't know" responses are coded as zero. Error bars represent standard errors.

Yet only 42 percent of students in our control group actually enroll in lower secondary school the following year. Figure 3 limits the sample to controls subjects that participated in the baseline school survey and responded to questions about their plans. Among the students who report plans to enroll at the beginning of sixth grade, only 66 percent follow through, a 34 percent intention-action gap. This gap is larger for poorer households, with 40 percent of poor households failing to follow through on plans to attend lower secondary compared to 29 percent of less poor households. Failing to follow through on enrollment plans is also correlated with a less supportive

home environment, higher student age, and lower educational engagement (Appendix Figure A.3).[11]

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, together with a sizable gap even among more affluent households, points toward a potential role for frictions that hinder follow-through. We now turn to causal evidence to examine this proposition.

3.2 Treatment Effects on Educational Investment

Following equation 1, we estimate effects for two comparisons of interest: the information–control contrast, identifying the role of information frictions, and the mentoring–information contrast, identifying the role of follow-through frictions (our primary estimator of interest).

Table 1 reports our **main effects** on school transition outcomes. For the *information treatment*, we find no detectable effect on any outcome. This is consistent with our descriptive evidence and suggests that in a setting where beliefs about returns are already broadly positive, providing accurate information may not shift enrollment. We find strong positive effects of *mentoring relative to information* in the academic year subsequent to treatment. Enrollment increases by 8.4 percentage points in the reduced form, corresponding to an increase of approximately 18 percent (column 1) relative to the information arm. Scaling for compliance, the 2SLS estimate implies an effect of 13.6 percentage points, or approximately 30 percent, for students who actively engaged with the mentoring program relative to those who received only information. Relative to control, mentoring increases enrollment by 5 percentage points or 11 percent, with a corresponding 2SLS estimate of 9 percentage points or 20 percent in the year following treatment.

Effects on enrollment in lower secondary school are somewhat smaller at 7.3 percentage points (17 percent), reflecting that not all enrolled students progress to lower secondary (column 2). Students induced to enroll also attended regularly: attendance effects are nearly identical in magnitude to enrollment effects (columns 3 and 4).

[11]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, given that the gap remains substantial even among more affluent households, economic constraints alone are unlikely to account for the full pattern.

We find no effects of either intervention on grade progression during the treatment year (column 5), when all students are enrolled and 88 percent pass 6th grade (last year of primary). There is, however, suggestive evidence that mentoring increased study effort during this year, with a 11.6 percentage point increase in the share of students working at least one hour per day on homework (Appendix Table C.4). In the subsequent year, mentoring increases the probability of passing grade 7 (first year of lower secondary) relative to the information arm by 5.2 percentage points (column 6). This effect is fully accounted for by increased enrollment in grade 7: conditional on enrollment, mentoring reduces pass rates by 5.2 percentage points relative to the information arm (column 7), with similar results relative to control. This likely reflects the retention of marginal or lower-performing students in lower secondary, rather than a decline in performance caused by mentoring.

To assess **robustness**, we examine whether results hold for (i) different aggregation strategies across data sources and (ii) individual data sources separately. Results using the mean rather than the mode across sources remain largely unchanged (Appendix Table C.1–mean-based version of Table 1). Survey-based results are consistent with our main effects too but somewhat smaller in magnitude and less precise, reflecting the smaller samples available from household surveys (Appendix Table C.2). Estimates are also very similar when using enrollment from administrative records alone, using different samples (Appendix Table C.3)

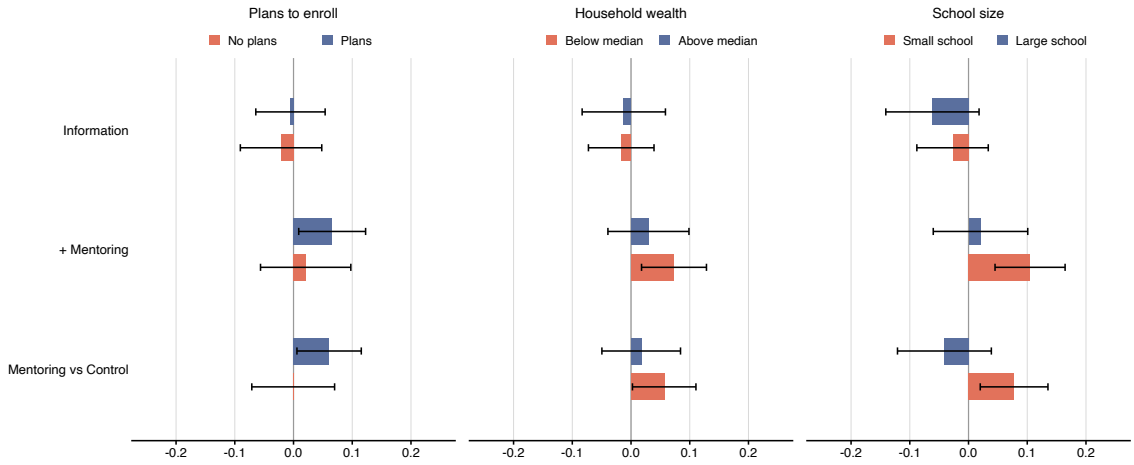


Figure 4: Heterogeneity in treatment effects on enrollment

Notes: Each panel shows point estimates and 90% confidence intervals for two subgroups defined by the heterogeneity variable. Red indicates the lower subgroup (no baseline enrollment plans, below-median wealth, small school); blue indicates the upper subgroup (plans to enroll, above-median wealth, large school). "Mentoring vs Control" reports the sum of the Information and + Mentoring coefficients, corresponding to the mentoring–control comparison.

We also explore **effect heterogeneity** along pre-specified dimensions. If mentoring alleviates follow-through frictions, we would expect the largest effects among students who already intend to enroll at baseline, since mentoring helps them act on existing intentions, those from poorer households who have fewer resources and less adult support to navigate the transition, and those in larger and more anonymous schools where individual guidance is otherwise scarce. The heterogeneity patterns are broadly consistent with this interpretation, though interaction effects are imprecisely estimated (Table C.8). Mentoring effects are larger for students who plan to enroll at baseline, for those from below-median wealth households, and in larger schools. We also examine heterogeneity along additional pre-specified dimensions, including gender, dropout risk, and school remoteness, and find no systematic pattern of differential effects (Appendix Table C.8). [12]

Overall, these results suggest that personal support matters beyond information provision. Both the treatment effects and the heterogeneity patterns are broadly consistent with a role for follow-through frictions, though interaction effects are imprecisely estimated.

3.3 Mechanisms

We use survey data from the midline and endline to examine two broad mechanisms driving the observed enrollment effects, related to (i) information frictions and content parity, and (ii) follow-through frictions.

Information frictions and content parity. We assess whether information provision led to belief updating (information–control contrast) and whether the content parity assumption holds (mentoring–information contrast). In line with the null enrollment effect of the information treatment, we find limited evidence of belief updating due to mere information provision across nearly all outcomes (Table 2). The one notable exception is the returns-to-schooling index at endline, where information increases perceived returns by 0.13 standard deviations ($p < 0.05$). This could reflect a chance finding, as it is not mirrored in any other measure, or it could suggest that belief updating occurred but did not translate into higher enrollment.

Regarding content parity, a key concern is that the more intensive and interactive format of the mentoring arm may have led to greater information absorption. We find no indication that mentored students are more likely to recall having someone

[12] Mentoring effects are also larger in low-dropout schools and among high-risk students relative to information, but not relative to control.

visit the school to provide sessions or informational content, to remember the topics of these sessions, or to report correct knowledge of educational laws (Table 3). As an implementation check, we examine recall of socio-emotional topics taught exclusively in the mentoring arm and find significantly higher recall there, confirming that the arms differ only where expected. For effects on beliefs about schooling, the results are somewhat less clear. Reassuringly, midline outcomes show no differences between arms, and endline effects are also null for several outcomes. Yet, compared to the information arm, mentoring students tend to perceive lower returns to education, value education more highly than work, and have caregivers report a higher ideal age to stop schooling (driven by a reduction in the age for the information group). The interpretation of these mutually inconsistent effects is further complicated by the fact that these questions were asked after the school transition and may therefore partly reflect the consequences of differential enrollment rather than its causes.[13] Overall, while we cannot fully rule out violations of content parity, the evidence suggests that differential information absorption is unlikely to account for the enrollment differences between the two treatment arms.

Follow-through frictions. The enrollment difference between the mentoring and information arms is consistent with follow-through frictions constituting a binding constraint on educational transitions in this setting. We distinguish between two broad channels through which mentoring could alleviate these constraints: (i) providing sustained personal support to help students and families follow through on their plans and (ii) building students' socio-emotional skills needed to 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 items such as parental engagement with school, the existence of a pro-education peer group, or the presence of a figure inspiring them to work harder. As Table 4 shows, we observe positive effects for most outcomes, but none reach statistical significance. For *socio-emotional skills*, we use an enumerator-assessed soft skills index that captures dimensions such as attentiveness, carefulness, alertness, and confidence, as well as several self-reported measures, including self-regulation, planning, and agency. We

[13]Regardless of treatment assignment, subjects that are in school say that schooling is more valuable than work and subjects who are working respond the opposite. For example, in the control population, subjects enrolled in school and not employed have a pro-work index difference of 0.66 standard deviations compared to subjects that are employed and not enrolled. Hence, any treatment assignment that increases enrollment will mechanically decrease pro-work beliefs.

observe effects of 0.19 SD (p=0.015) for the enumerator-assessed index at endline. Increases are present in all subcomponents, with enumerators rating children in the mentoring arm as more attentive, careful, alert, and confident than their peers in the information arm. [14]

Overall, the evidence is more supportive of the socio-emotional skills channel as the mechanism through which mentoring raised enrollment, but we cannot rule out that personal support also played a role in helping students follow through on their intentions.

4 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 8.4 percentage points, an 18 percent increase relative to the control mean.

We interpret the contrast between mentoring and information as evidence on follow through frictions. In the control group, 35 percent of students who plan to enroll do not do so, indicating a substantial intention action gap. The mentoring arm does not change beliefs or information retention relative to the information arm, which rules out differential content as an explanation. Instead, mentoring increases socio emotional skills by 0.19 standard deviations. 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. Our findings are consistent with our hypothesis that mentoring improved soft skills which are useful in acting on plans, but we cannot rule out the argument that school enrollment builds soft skills.

These findings point to a broader class of frictions between plans and realized

[14] These soft skill effects are not mirrored in either the self-reported measures or the midline assessment. One possible explanation is measurement: the midline assessment was conducted in school, where the more chaotic environment may make reliable assessment more difficult, while the endline was conducted at home. Note also that the endline was conducted after the school transition, so effects on soft skills may partly reflect the consequences of enrollment rather than its causes. Yet, conditioning on non-enrollment does not materially reduce the point estimate (0.14 SD; p = 0.22). Because the intervention appears to induce enrollment among marginal students in terms of performance or commitment (see Table 1, column 7), this conditioned comparison is likely conservative, suggesting that soft skills operate more as a mediator than a consequence of enrollment.

behavior that can be 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. A testable implication is that impacts of similar programs should be larger in settings with high baseline intentions and complex or 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, 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 that helps students complete required steps, meet deadlines, and sustain effort can raise educational investment at transition points. More generally, distinguishing between information frictions and follow through frictions can guide both theory and program design.

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5 Tables

Table 1: Treatment effects on school transition outcomes

Variable	Enrolled in school 2025 (1)	Enrolled in secondary 2025 (2)	Attending school 2025 (3)	Attending secondary 2025 (4)	Passed 6th grade 2024 (5)	Passed 7th grade 2025 (6)	Passed 7th grade, conditional (7)
Reduced Form							
Information	-0.034	-0.028	-0.028	-0.024	0.005	-0.029	-0.006
	(0.030)	(0.030)	(0.029)	(0.029)	(0.016)	(0.030)	(0.027)
+ Mentoring	0.084***	0.073***	0.084***	0.071**	-0.014	0.052*	-0.052*
	(0.030)	(0.028)	(0.030)	(0.028)	(0.017)	(0.029)	(0.029)
Mentoring vs Control	0.050*	0.045	0.057*	0.048	-0.010	0.023	-0.059**
	(0.029)	(0.029)	(0.029)	(0.029)	(0.018)	(0.030)	(0.028)
2SLS							
Information	-0.046	-0.038	-0.037	-0.032	0.006	-0.039	-0.008
	(0.040)	(0.039)	(0.039)	(0.038)	(0.022)	(0.039)	(0.031)
+ Mentoring	0.136***	0.118***	0.139***	0.117***	-0.024	0.079*	-0.080**
	(0.049)	(0.044)	(0.048)	(0.045)	(0.028)	(0.047)	(0.039)
Mentoring vs Control	0.090*	0.080	0.102*	0.085*	-0.018	0.040	-0.088**
	(0.053)	(0.051)	(0.053)	(0.052)	(0.032)	(0.052)	(0.040)
Control mean	0.457	0.421	0.428	0.400	0.883	0.356	0.856
Observations	3273	3273	3273	3273	3273	3138	1302

Notes: All regressions include strata, age, 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 outcomes are constructed as the mode across four data sources: administrative records, student endline survey, caregiver survey, and endline short census, except columns 2 and 4 which do not use the short census (no grade level in census) and columns 6 and 7 which are only available in administrative data only. Results using the mean across sources for columns 1–5 are reported in Appendix Table C.1. 2SLS estimates scale for compliance as described in Section 2.6. The sample for this table includes all subjects (N=3,273) except for column 6, which is restricted to students with 2025 administrative enrollment records that have a final passing status available (n=3,138). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Treatment effects on beliefs

	Panel A: Student beliefs				Panel B: Caregiver beliefs		
	Midline		Endline		Endline		
	Higher wage	Effort returns	Returns index	Pro-work index	Complete lower sec.	Age stop school	Age for work
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Information			0.133**	-0.020	-0.021	-0.454	-0.222
			(0.062)	(0.080)	(0.026)	(0.321)	(0.256)
+ Mentoring	-0.001	0.020	-0.095	-0.129*	0.013	0.593*	-0.074
	(0.026)	(0.034)	(0.063)	(0.073)	(0.028)	(0.311)	(0.252)
Mentoring vs Control			0.038	-0.148**	-0.008	0.140	-0.296
			(0.066)	(0.073)	(0.027)	(0.330)	(0.230)
Information/Control mean	0.761	0.619			0.835	18.379	16.576
Observations	1171	1171	1284	1283	1505	1491	1503

Notes: Columns 1–2 are from the midline survey. For these columns, "+ Mentoring" reports the mentoring–information contrast only, as the control group is excluded from midline comparisons due to differential attrition (see Section 2.5). Columns 3–7 are from the endline survey. "Information" reports the ITT effect relative to control; "+ Mentoring" reports the effect of mentoring relative to information; "Mentoring vs Control" reports the sum of both coefficients. Robust standard errors are shown in parentheses. All regressions include strata, age, and sex fixed effects, with standard errors clustered at the school level. Control mean is not reported for columns 3–4 as endline belief indexes are standardized. For columns 1–2, the sample is students in the mentoring and information arms with available midline surveys (n=1,171). For columns 3–4, the sample is subjects with complete, full endline student surveys (n=1284) and non-missing responses to all index components. For columns 5–7, the sample is subjects with completed full endline caregiver surveys (n=1505) and non-missing responses to each variable. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Content parity and information absorption

	Topic recall			Ed. Law knowledge	
	Any session	Educational Information	Socio-emotional Content	Compulsory primary	Compulsory lower sec.
	(1)	(2)	(3)	(4)	(5)
Mentoring vs Info	-0.016	-0.028	0.079**	-0.004	-0.015
	(0.035)	(0.026)	(0.033)	(0.036)	(0.034)
Information mean	0.585	0.209	0.230	0.643	0.626
Observations	1171	1171	1171	1171	1171

Notes: Midline survey, mentoring–information contrast only (control excluded due to differential attrition, Section 2.5). Column 3 serves as a placebo check. All regressions include strata, age, and sex fixed effects, standard errors clustered at the school level (in parentheses). The sample for this table is students in the mentoring and information arms with available midline surveys (n=1,171). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Treatment effects on follow-through frictions

	Panel A: Personal support		Panel B: Socio-emotional skills			
	Social accountability index	Social support index	Soft skills index (enum.)	Self-regulation index	Planning index	Agency index
	(1)	(2)	(3)	(4)	(5)	(6)
Information	0.012	0.016	-0.022	-0.088	0.086	0.048
	(0.079)	(0.053)	(0.086)	(0.070)	(0.069)	(0.085)
+ Mentoring	0.087	0.022	0.188**	0.060	-0.016	-0.101
	(0.072)	(0.053)	(0.077)	(0.066)	(0.075)	(0.078)
Mentoring vs Control	0.098	0.038	0.165**	-0.028	0.070	-0.053
	(0.080)	(0.046)	(0.083)	(0.068)	(0.074)	(0.074)
Observations	1,283	1,284	1,284	1,284	1,284	1,283

Notes: All outcomes are from the endline survey. All regressions include strata, age, and sex fixed effects, with standard errors clustered at the school level (in parentheses). "Information" reports the ITT effect relative to control; "+ Mentoring" reports the effect of mentoring relative to information; "Mentoring vs Control" reports the sum of both coefficients (p-value in brackets). The soft skills index in column 3 is based on enumerator assessments. All other indexes are self-reported. All indexes are standardized. 2SLS estimates and index components are reported in Appendix Tables **??** and C.7. The sample for each column is subjects with complete, full-length endline surveys (n=1,284) and non-missing responses to all index components. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Supplemental Appendices

A Sampling and Data

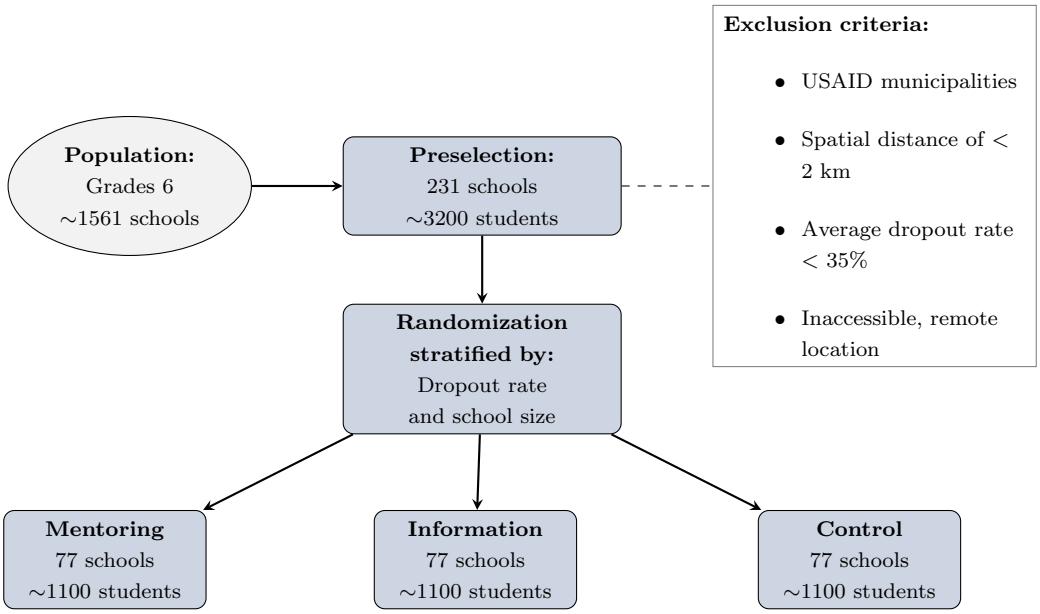


Figure A.1: Overview of sampling and randomization procedure

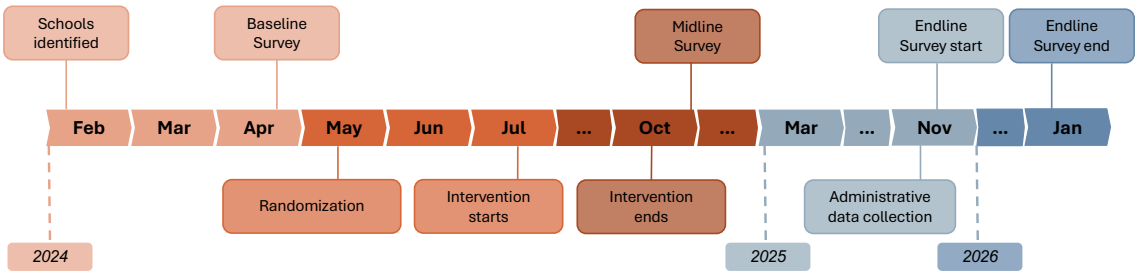


Figure A.2: Timeline of data collection and intervention

Table A.1: Differences between schools in the experiment and those not participating (in 2018)

	Other schools in the country (1)	Other schools in Quiché (2)	In the experiment (3)	Difference (3)-(2) (4)
% rural	90.98	95.12	100.00	4.88***
	(28.65)	(21.56)	(0.00)	(0.62)
	[15,419]	[1,229]	[231]	
% morning shift	94.57	97.72	98.70	0.98
	(22.67)	(14.93)	(11.35)	(0.86)
	[15,419]	[1,229]	[231]	
% bilingual	46.88	81.18	88.74	7.56***
	(49.90)	(39.10)	(31.67)	(2.36)
	[15,699]	[1,254]	[231]	
Total enrollment	135.95	118.80	115.02	-3.78
	(135.38)	(124.70)	(48.32)	(4.77)
	[15,363]	[1,226]	[231]	
Grd 6 enrollment	16.99	13.94	13.02	-0.92
	(19.34)	(16.03)	(6.00)	(0.60)
	[15,699]	[1,254]	[231]	
Number of teachers	5.68	4.90	4.57	-0.33*
	(4.95)	(4.63)	(1.91)	(0.18)
	[15,363]	[1,226]	[231]	
% with a school board	85.65	94.78	96.97	2.19*
	(35.06)	(22.24)	(17.18)	(1.30)
	[15,377]	[1,227]	[231]	

Notes: Column 1 displays the mean, standard deviation (in parentheses), and number of observations (in square brackets) for schools not in the experimental sample. Column 2 displays the mean, standard deviation (in parentheses), and number of observations (in square brackets) for schools not in the experimental sample, but in Quiché. Column 3 shows the mean, standard deviation (in parentheses), and number of observations (in square brackets) for schools in the sample. Column 4 reports the differences between the other schools in Quiché and those in the experimental sample, as well as the standard error of the difference (in parentheses). This data comes from Ministry of Education administrative records collected for Haimovich et al. (2021). Standard errors, clustered at the school level, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.2: Students enrolled in grade 6 in experimental vs. non-experimental schools

	Other schools in the country (1)	Other schools in Quiché (2)	In the experiment (3)	Difference (3)-(2) (4)
Panel A: 2018 academic year				
% male	51.26	52.03	52.69	0.66
	(49.98)	(49.96)	(49.94)	(1.05)
	[266,718]	[17,482]	[3,008]	
Age (Jan 1st, 2018)	12.83	13.01	13.14	0.13***
	(1.25)	(1.34)	(1.31)	(0.04)
	[264,439]	[17,338]	[2,998]	
GPA	7.61	7.40	7.35	-0.05
	(0.87)	(0.82)	(0.81)	(0.04)
	[260,656]	[17,279]	[2,984]	
% repeat grade	1.41	1.81	1.93	0.11
	(11.77)	(13.34)	(13.75)	(0.40)
	[266,661]	[17,480]	[3,008]	
% at-risk (statistical model)	41.44	66.55	86.89	20.33***
	(49.26)	(47.18)	(33.76)	(2.97)
	[248,695]	[16,229]	[2,837]	
% dropout	31.38	51.48	65.43	13.95***
	(46.40)	(49.98)	(47.57)	(2.02)
	[266,718]	[17,482]	[3,008]	
Panel B: 2021 academic year				
% male	51.10	52.14	50.91	-1.23
	(49.99)	(49.96)	(50.00)	(1.10)
	[282,955]	[18,349]	[3,204]	
% dropout	37.67	55.04	69.41	14.37***
	(48.46)	(49.75)	(46.08)	(1.96)
	[282,955]	[18,349]	[3,204]	

Notes: Column 1 displays the mean, standard deviation (in parentheses), and number of observations (in square brackets) for schools not in the experimental sample. Column 2 displays the mean, standard deviation (in parentheses), and number of observations (in square brackets) for schools not in the experimental sample, but in Quiché. Column 3 shows the mean, standard deviation (in parentheses), and number of observations (in square brackets) for schools in the sample. Column 4 reports the differences between the other schools in Quiché and those in the experimental sample, as well as the standard error of the difference (in parentheses). This data comes from Ministry of Education administrative records collected for Haimovich et al. (2021). Standard errors, clustered at the school level, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

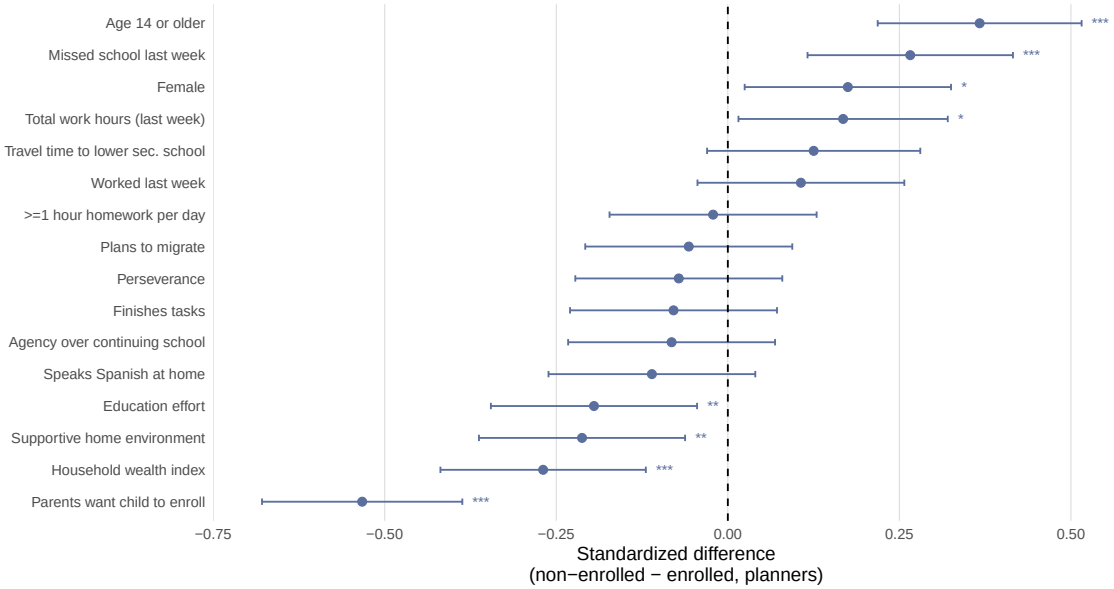


Figure A.3: Who fails to follow through on enrollment plans?

Notes: This figure plots standardized differences in baseline characteristics between students who report plans to enroll but do not enroll and those who both plan and enroll. Positive values indicate higher levels among students who do not follow through on their enrollment plans. All variables are standardized to have mean zero and standard deviation one in the estimation sample. Estimates are obtained from separate regressions of each characteristic on an indicator for not enrolling among students with enrollment plans. Error bars denote 90% confidence intervals. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B Validity

Table B.1: Balance of baseline variables (baseline sample)

Variable	Mentoring mean (1)	Information Mean (2)	Control Mean (3)	F-Stat (p-value) (4)
Age	12.328	12.406	12.295	1.017
	(1.082)	(1.112)	(1.035)	(0.363)
Female	0.514	0.488	0.490	0.628
	(0.500)	(0.500)	(0.500)	(0.535)
Spanish	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 Chi-Square				15.301
				(0.641)
Bivariate Orthogonality F-Stat	0.940 [a]	0.513 [b]		1.123 [c]
	(0.493)	(0.864)	(.)	(0.350)
Observations, female/age	775	763	810	
Observations, other	761	761	810	

Notes: Columns 1–3 report the mean of each variable for the mentoring, information, and control groups, respectively, with standard deviations shown in parentheses. Column 4 reports the F-statistic and its associated p-value (in parentheses) for the null hypothesis that treatment status does not predict each individual outcome variable, controlling for age bin, gender, and stratum fixed effects, except for Age (does not include age fixed effects) and Female (does not include gender fixed effects). The chi-square joint orthogonality tests the null that treatment assignment does not predict baseline characteristics. Baseline characteristics are defined as the variables shown in the table excluding age and female. [a] The bivariate test in Column 1 tests the hypothesis that there are no differences in baseline characteristics between the mentoring and control arms. [b] The bivariate test in Column 2 tests the hypothesis that there are no differences in baseline characteristics between the information and control arms. [c] The bivariate test in Column 4 tests the hypothesis that there are no differences in baseline

Table B.2: Attrition

Variable	Attrition rate (1)	Are treatment arms equal (2)	Treatment correlation (3)	Balance covariates (4)	Randomization valid (5)	Target N (6)
Midline						
Enrolled	0.425	1.037	7.933	2.933	13.027	2348
		(0.310)	(0.000)	(0.009)	(0.790)	
Endline						
Enrolled	0				15.301	3273
					(0.641)	
Census, some info	0.158	0.284	0.342	0.590	13.746	3273
		(0.594)	(0.711)	(0.738)	(0.746)	
Census, all info	0.232	1.159	0.586	0.578	12.092	3273
		(0.283)	(0.558)	(0.748)	(0.842)	
At least 1 HH survey	0.186	0.243	1.200	2.852	9.398	2175
		(0.622)	(0.303)	(0.011)	(0.950)	
Caregiver survey	0.240	0.018	1.542	2.208	8.066	2175
		(0.893)	(0.216)	(0.043)	(0.978)	
Student survey	0.314	0.194	2.757	1.565	7.792	2175
		(0.660)	(0.066)	(0.158)	(0.982)	

Notes: Attrition rates in column 1 are calculated as a fraction of targeted subjects in column 5. Column 2 shows the F-stat (p-value) associated with the null that attrition does not differ between treatment arms. Column 3 shows the F-stat (p-value) associated with the null that random assignment does not predict attrition. Column 4 shows the F-stat (p-value) associated with the null that there are no differences (jointly) in baseline balance attributes between attritors and non-attritors. Column 5 shows the chi-square joint orthogonality test for the null that treatment assignment does not jointly predict baseline characteristics within the non-attritors sample. There are 3,273 individuals who have had prior contact with the project. A total of 2,175 individuals were targeted for both the endline student and caregiver surveys. The short census was attempted for all 3,273 individuals. The midline survey aimed to recapture 2,348 students in the baseline survey sample. Standard errors are clustered at the school level for all regressions.

Table B.3: Session attendance by treatment assignment

Variable	Students			Student's caregiver	
	Attended at least 1	Attendance rate 92%+	Had private session	Attended at least 1	Attended all offered
	(1)	(2)	(3)	(4)	(5)
Information					
Full sample	90.9	74.3		85.0	68.9
Baseline sample	95.9	80.9		89.1	74.7
Mentoring					
Full sample	89.1	55.8	67.2	84.1	41.2
Baseline sample	97.5	66.2	76.8	93.4	48.5

Notes: Column 1 reports the percent of students that attended at least one session. Column 2 reports the percent of students with high compliance, defined as attending 92% or more of sessions offered (equivalent to all sessions offered for the information arm). Column 3 reports the percent of mentoring arm students who received a private session, offered at mentor discretion based on student need. Column 4 reports the percent of students whose caregiver attended at least one session. Column 5 reports the percent of students whose caregiver attended all sessions offered. The full sample includes all individuals with prior contact with the project (N=3,273). The baseline sample is restricted to students attending at baseline (N=2,348). There was one school in each treatment arm where no sessions were offered. All attendance rates are unconditional on whether sessions were offered at the school.

Table B.4: Midline compliance

Variable	Heard of Prodessa (1)	Reports school visit (2)	Median visit frequency (3)
Information			
Full sample	51.5	58.5	Once a month
Baseline sample	52.1	57.2	Once a month
Mentoring			
Full sample	45.9	58.0	Once a week
Baseline sample	46.4	59.2	Once a week

Notes: Column 1 shows the percent of students that report hearing of the implementing NGO Prodessa. Column 2 reports the percent of students that report a school visit. Column 3 shows the median frequency of visits reported by students. The sample for this column is students with midline survey in either treatment arm (n=1,171). The baseline row additionally restricts to students attending at baseline (n=994).

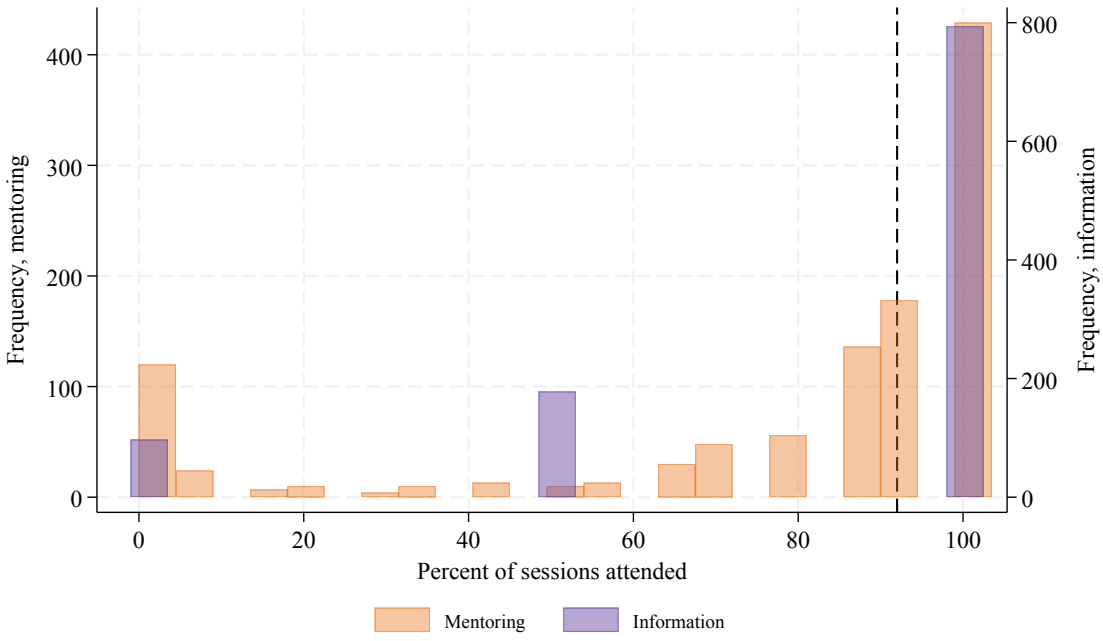


Figure B.1: Percent of sessions attended by treatment arm

Table B.5: Treatment saliency

Data source:	Prodessa records		Midline			Endline
Variable	Information (1)	+ Mentoring (2)	Heard of Prodessa (3)	Any school visits (4)	Attended visits (5)	Treatment saliency index (6)
Reduced Form						
Information	0.745	0.001				0.080
	(0.027)	(0.008)				(0.073)
+ Mentoring	-0.186	0.557	-0.071	-0.016	0.021	0.078
	(0.040)	(0.031)	(0.050)	(0.035)	(0.036)	(0.070)
Mentoring vs Control	0.559	0.558				0.157
	(0.030)	(0.030)				(0.071)
2SLS						
Information						0.096
						(0.089)
+ Mentoring			-0.104	-0.024	0.031	0.136
			(0.071)	(0.051)	(0.053)	(0.096)
Mentoring vs Control						0.233
						(0.104)
Control mean			0.515	0.585	0.494	
Observations	3273	3273	1171	1171	1171	1284

Notes: All regressions include strata fixed effects, age effects, and sex effects. Standard errors are clustered at the school level (in parentheses). Columns 1–2 report the first stage regression used throughout the paper. Columns 3–5 are measures from the midline survey and are limited to students in the information and mentoring arms with a midline survey (n=1,171). Column 6 is a standardized index of measures from the endline survey; individual components are shown in Appendix Table B.6. 2SLS instruments for compliance using random assignment. The first endogenous variable is an indicator for complying with either arm (attending at least 92% of mentoring sessions or all information sessions offered). The second is an indicator for compliance specifically with the mentoring arm. Mentoring vs Control reports the total effect of the mentoring arm relative to the control group, computed as the linear combination of the Information and + Mentoring coefficients.

Table B.6: Components of treatment saliency index at endline

Variable	School visits (1)	Inspired (2)	Someone cares I succeed (3)	Talked to someone about education (4)	Someone cares if stop studying (5)	Someone notice if stopped trying (6)
Reduced Form						
Information	0.064*	-0.009	0.025	0.023	-0.042	0.026
	(0.035)	(0.040)	(0.037)	(0.043)	(0.048)	(0.043)
+ Mentoring	0.026	0.000	-0.012	0.024	0.072	0.019
	(0.033)	(0.046)	(0.041)	(0.042)	(0.047)	(0.036)
Mentoring vs Control	0.089***	-0.009	0.012	0.047	0.030	0.045
	(0.033)	(0.043)	(0.038)	(0.043)	(0.047)	(0.044)
2SLS						
Information	0.077*	-0.011	0.030	0.028	-0.052	0.032
	(0.042)	(0.048)	(0.045)	(0.052)	(0.058)	(0.052)
+ Mentoring	0.055	-0.002	-0.012	0.042	0.097	0.035
	(0.045)	(0.063)	(0.056)	(0.058)	(0.064)	(0.050)
Mentoring vs Control	0.132***	-0.014	0.018	0.070	0.045	0.067
	(0.048)	(0.063)	(0.056)	(0.064)	(0.069)	(0.065)
Control mean	0.634	2.881	2.871	2.774	2.752	2.787
Observations	1284	1282	1280	1281	1282	1282

Notes: All regressions include strata fixed effects, age effects, and sex effects. Standard errors are clustered at the school level (in parentheses). 2SLS instruments for compliance using random assignment. Mentoring vs Control reports the total effect of the mentoring arm relative to the control group.

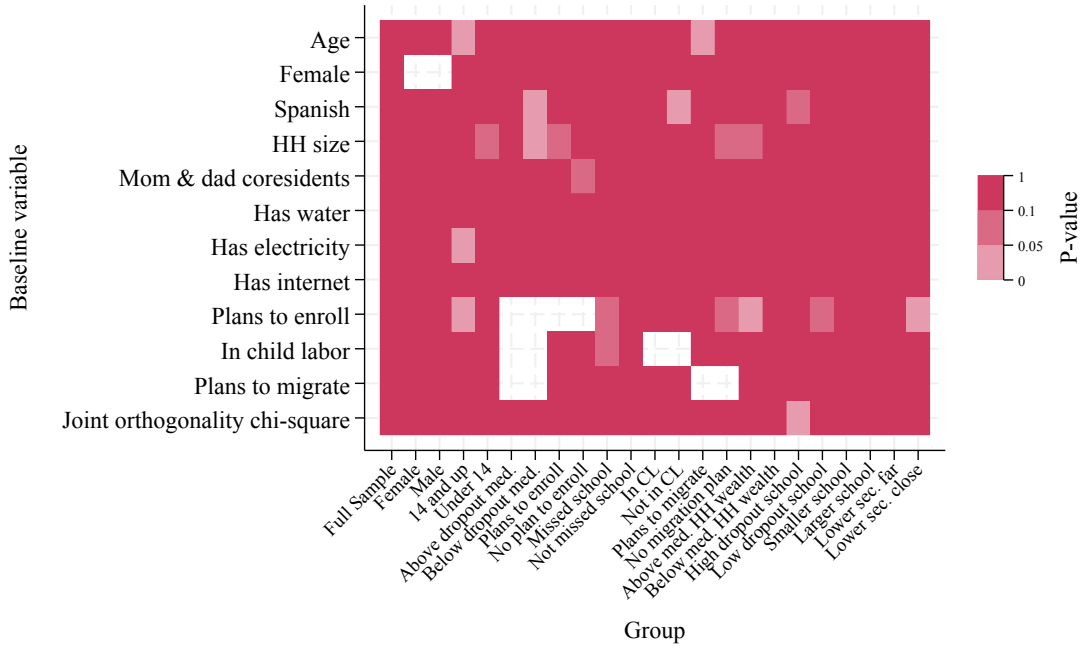


Figure B.2: Balance within subgroups

Notes: This figure presents a heatmap of the p-values associated with each baseline variable, for the null hypothes that treatment status does not predict each individual variable. The chi-square joint orthogonality tests the null th treatment assignment does not predict baseline characteristics for all variables except female and age, which are used fixed effects. Strata fixed effects included, with standard errors clustered at the school level.

C Additional Results

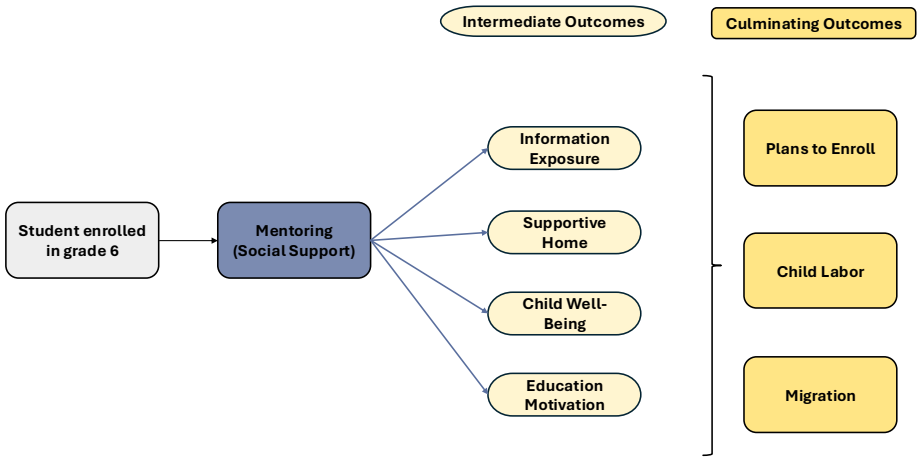
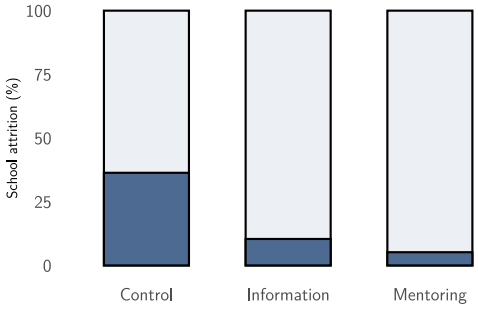
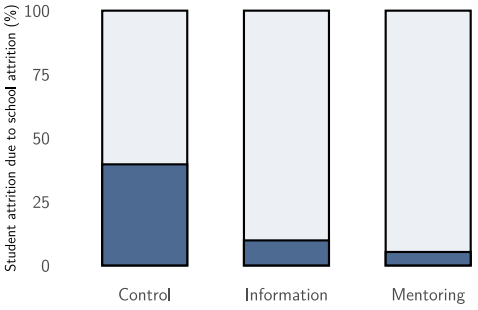


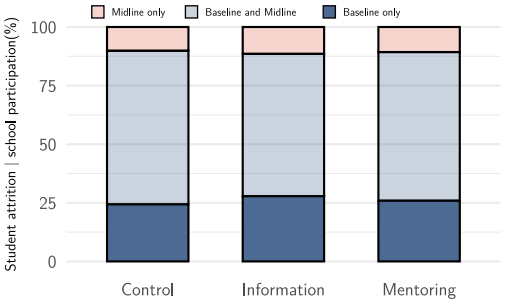
Figure C.1: Theory of change



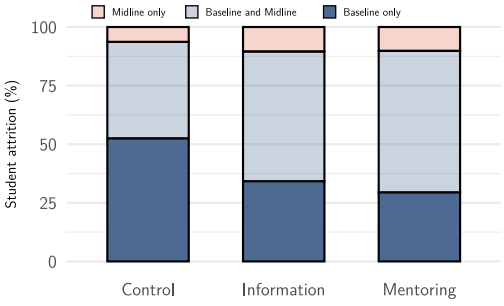
(a) School attrition (% of schools)



(b) School attrition (% of students)



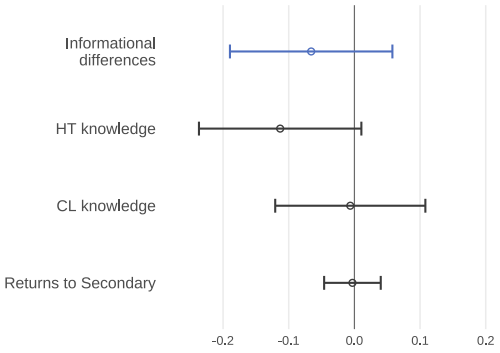
(c) Student attrition | school participation



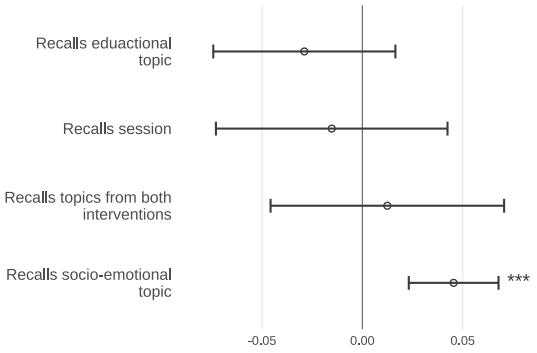
(d) Student attrition

Figure B.3: Midline attrition

Notes: In all panels, the dark blue fraction indicates attrition. Panel (a) shows school attrition measured as the percentage of sampled schools not visited at midline. Panel (b) reports school-level student attrition measured as the percentage of students enrolled in attriting schools. Panel (c) shows student attrition conditional on school participation at midline. Panel (d) shows overall student attrition at midline.



(a) Information differences



(b) Topic recall

Figure C.2: Information differences and topic recall

Notes: Effect sizes for information differences are shown in standard deviations. Effect sizes for topic recall are shown in percentage points.

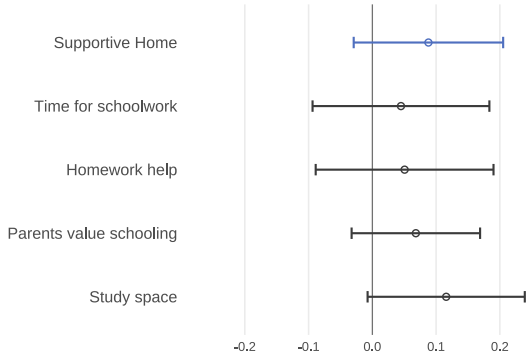
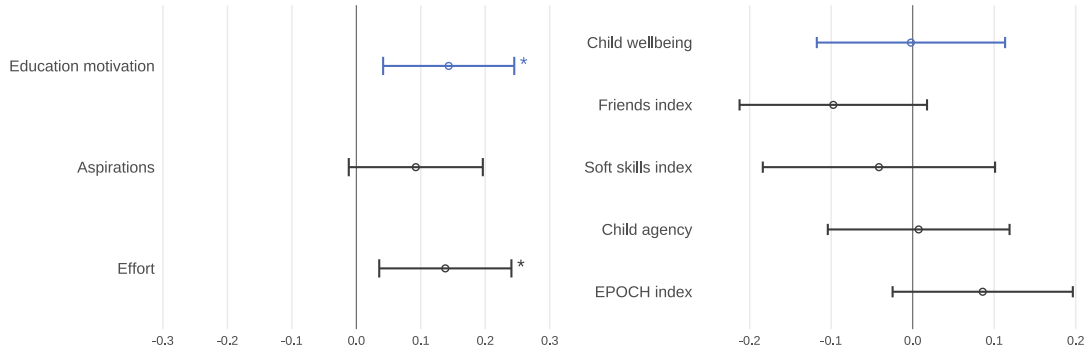


Figure C.3: Supportive home



(a) Commitment to schooling (b) Child well-being

Figure C.4: Commitment to schooling and child well-being

Table C.1: Treatment effects on secondary school transition outcomes

Variable	Enrolled in school (1)	Enrolled in secondary (2)	Attending school (3)	Attending secondary (4)	Passed 6th grade (5)
Reduced Form					
Information	-0.023	-0.022	-0.021	-0.023	0.001
	(0.030)	(0.029)	(0.030)	(0.028)	(0.016)
+ Mentoring	0.072**	0.070**	0.072**	0.071**	-0.011
	(0.030)	(0.028)	(0.030)	(0.027)	(0.017)
Mentoring vs Control	0.049	0.048*	0.051*	0.048*	-0.010
	(0.030)	(0.029)	(0.030)	(0.029)	(0.017)
2SLS					
Information	-0.031	-0.030	-0.028	-0.030	0.002
	(0.041)	(0.039)	(0.040)	(0.038)	(0.021)
+ Mentoring	0.120**	0.116***	0.120**	0.117***	-0.019
	(0.049)	(0.044)	(0.048)	(0.044)	(0.028)
Mentoring vs Control	0.088	0.086*	0.092*	0.086*	-0.017
	(0.054)	(0.051)	(0.053)	(0.051)	(0.031)
Control mean	0.466	0.418	0.441	0.397	0.881
Observations	3273	3273	3273	3273	3273

Notes: All regressions include strata, age, 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 outcomes are constructed as the mean across four data sources: administrative records, student endline survey, caregiver survey, and endline short census, except columns 2 and 4 which do not use the short census. 2SLS estimates scale for compliance as described in Section 2.6. The sample for this table includes all subjects (N=3,273). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.2: Effect on enrollment for each data source

Variable	Enrolled 2025 Student survey (1)	Enrolled 2025 Caregiver survey (2)	Enrolled 2025 Short Census (3)	Enrolled 2025 Admin (4)
Information	-0.012	0.005	-0.011	-0.028
	(0.036)	(0.035)	(0.037)	(0.031)
+ Mentoring	0.052	0.060*	0.040	0.087***
	(0.036)	(0.036)	(0.038)	(0.030)
Mentoring vs Control	0.041	0.065*	0.030	0.059*
	(0.037)	(0.035)	(0.034)	(0.031)
Control mean	0.595	0.552	0.526	0.449
Observations	1493	1652	2618	3244

Notes: This table reports ITT estimates of treatment effects on enrollment, measured using the data source indicated in each column. Samples are limited as follows: Column 1 includes students with a completed student survey. Column 2 includes students with a completed caregiver survey. Column 3 is restricted to students found in the endline short census with nonmissing enrollment status. Column 4 is restricted to students with 2024 enrollment in administrative data. All regressions include age group, sex, and strata fixed effects, with standard errors clustered at the school level (in parentheses).

Table C.3: Effect on administrative enrollment, by sample

Variable	Enrolled 2025	Enrolled 2025	Enrolled 2025	Enrolled 2025
Sample	Student survey	Caregiver survey	Short census	Enrolled 2024 admin
	(1)	(2)	(3)	(4)
Information	0.004	0.002	-0.030	-0.028
	(0.039)	(0.038)	(0.031)	(0.031)
+ Mentoring	0.063*	0.067*	0.083***	0.087***
	(0.037)	(0.036)	(0.031)	(0.030)
Mentoring vs Control	0.067*	0.069*	0.053	0.059*
	(0.040)	(0.038)	(0.032)	(0.031)
Control mean	0.569	0.545	0.486	0.448
Observations	1493	1652	2618	3244

Notes: This table reports ITT estimates of treatment effects on enrollment as measured with administrative data, for different sample restrictions. Columns 1 and 2 are restricted to subjects with an endline student or caregiver survey, respectively. Column 3 is restricted to subjects with nonmissing enrollment status in the endline short census, and column 4 is restricted to students enrolled in 2024 according to administrative data. All regressions include age group, sex, and strata fixed effects, with standard errors clustered at the school level (in parentheses).

Table C.4: Effort in school

Data source:	Midline					Admin	
Variable	Not missed school (1)	Works 1+ hours/day on homework (2)	Total minutes on homework (3)	When I start homework, I finish it (4)	I work hard in school (5)	Passed 6th 2024, admin (6)	Avg grade 2024, standardized (7)
Reduced Form							
Information						-0.007	0.047
						(0.017)	(0.075)
+ Mentoring	0.056*	0.041	5.919	0.029	0.044	-0.006	-0.068
	(0.034)	(0.031)	(3.706)	(0.028)	(0.031)	(0.018)	(0.081)
Mentoring vs Control						-0.013	-0.021
						(0.018)	(0.077)
2SLS							
Information						-0.010	0.062
						(0.023)	(0.097)
+ Mentoring	0.083*	0.060	8.712	0.043	0.065	-0.013	-0.097
	(0.049)	(0.045)	(5.392)	(0.041)	(0.046)	(0.029)	(0.124)
Mentoring vs Control						-0.023	-0.035
						(0.032)	(0.128)
Information/Control mean	0.585	0.674	72.522	0.710	0.587	0.885	
Observations	1171	1171	1171	1169	1170	3273	2976

Notes: All regressions include strata, age, 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. 2SLS estimates scale for compliance as described in Section 2.6. The sample for columns 1–5 is limited to subjects in the mentoring and information arms with a midline survey (n=1,171) and nonmissing response for each outcome. Columns 6-7 are outcomes at endline from administrative data, all subjects have passing status (3,273) while grades data is available for 2,976 students. 2SLS instruments for compliance using random assignment. Mentoring vs Control reports the total effect of the mentoring arm relative to the control group. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.5: Treatment effects on not missing school

	Student survey		Caregiver survey	
Variable	Not missed school (1)	Not missed, conditional (2)	Not missed school (3)	Not missed, conditional (4)
Reduced Form				
Information	0.018	0.029	0.025	0.030
	(0.037)	(0.027)	(0.034)	(0.028)
+ Mentoring	0.021	-0.025	0.033	-0.011
	(0.035)	(0.028)	(0.034)	(0.026)
Mentoring vs Control	0.038	0.004	0.058*	0.019
	(0.035)	(0.026)	(0.034)	(0.025)
2SLS				
Information	0.021	0.035	0.031	0.036
	(0.044)	(0.032)	(0.041)	(0.033)
+ Mentoring	0.035	-0.029	0.054	-0.009
	(0.048)	(0.037)	(0.046)	(0.033)
Mentoring vs Control	0.057	0.006	0.084*	0.028
	(0.051)	(0.038)	(0.049)	(0.035)
Control mean	0.520	0.859	0.483	0.862
Observations	1493	914	1652	956

Notes: All regressions include strata, age, 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. The sample for columns 1 and 3 is subjects with endline student and caregiver surveys, respectively. Columns 2 and 4 restrict to students who report attending school at some point during the academic year in the respective survey. 2SLS estimates scale for compliance as described in Section 2.6. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.6: Components of subject belief indexes

	People with basic:					Will move:	
Variable	Earn more (1)	Meet job requirements (2)	Have a better life (3)	Work=useful skills (4)	Choose work over schooling (5)	Out of community (6)	Outside Guatemala (7)
Reduced Form							
Information	0.060	0.075*	0.051	-0.006	-0.011	-0.086*	-0.090
	(0.046)	(0.040)	(0.047)	(0.053)	(0.052)	(0.051)	(0.057)
+ Mentoring	-0.044	-0.042	-0.052	-0.085*	-0.059	0.009	0.012
	(0.047)	(0.039)	(0.045)	(0.051)	(0.045)	(0.048)	(0.054)
Mentoring vs Control	0.016	0.034	-0.001	-0.092*	-0.070	-0.078*	-0.078
	(0.044)	(0.040)	(0.047)	(0.049)	(0.048)	(0.047)	(0.048)
2SLS							
Information	0.073	0.092*	0.062	-0.007	-0.013	-0.105*	-0.109
	(0.056)	(0.048)	(0.056)	(0.064)	(0.062)	(0.061)	(0.068)
+ Mentoring	-0.050	-0.042	-0.064	-0.129*	-0.091	-0.010	-0.006
	(0.063)	(0.053)	(0.061)	(0.069)	(0.060)	(0.066)	(0.072)
Mentoring vs Control	0.024	0.049	-0.002	-0.136*	-0.103	-0.114*	-0.115
	(0.064)	(0.058)	(0.069)	(0.072)	(0.070)	(0.069)	(0.072)
Control mean	2.745	2.833	2.690	2.560	2.432	2.596	2.456
Observations	1274	1281	1280	1280	1281	1274	1270

Notes: All regressions include strata fixed effects, age effects, and sex effects. Standard errors are clustered at the school level (in parentheses). 2SLS instruments for compliance using random assignment. Mentoring vs Control reports the total effect of the mentoring arm relative to the control group. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.7: Components of soft skills and planning indexes

Variable	Knows how to continue school (1)	Breaks down tasks (2)	Plans for goals (3)	Attentive (4)	Careful (5)	Alert (6)	Confident (7)
Reduced Form							
Information	-0.048	0.019	0.005	-0.078	0.027	-0.027	0.007
	(0.044)	(0.036)	(0.037)	(0.078)	(0.073)	(0.068)	(0.077)
+ Mentoring	0.055	0.033	0.017	0.123*	0.147**	0.156***	0.151**
	(0.040)	(0.036)	(0.034)	(0.070)	(0.066)	(0.058)	(0.067)
Mentoring vs Control	0.006	0.052	0.023	0.045	0.173**	0.129**	0.158**
	(0.043)	(0.036)	(0.034)	(0.071)	(0.069)	(0.063)	(0.078)
2SLS							
Information	-0.059	0.023	0.006	-0.095	0.031	-0.034	0.008
	(0.053)	(0.043)	(0.044)	(0.094)	(0.088)	(0.082)	(0.092)
+ Mentoring	0.069	0.054	0.027	0.162*	0.225**	0.225***	0.226**
	(0.054)	(0.048)	(0.046)	(0.093)	(0.088)	(0.078)	(0.091)
Mentoring vs Control	0.010	0.077	0.033	0.067	0.257**	0.191**	0.234**
	(0.063)	(0.053)	(0.049)	(0.103)	(0.100)	(0.092)	(0.112)
Control mean	2.787	2.988	3.119	3.577	3.535	3.582	3.431
Observations	1282	1283	1283	1284	1284	1284	1284

Notes: All regressions include strata fixed effects, age effects, and sex effects. Standard errors are clustered at the school level (in parentheses). 2SLS instruments for compliance using random assignment. Mentoring vs Control reports the total effect of the mentoring arm relative to the control group. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.8: Heterogeneity

Variable	Enrolled mode (1)	Enrolled mode (2)	Enrolled mode (3)	Enrolled mode (4)	Enrolled mode (5)	Enrolled mode (6)
Information	0.022	-0.023	-0.022	-0.012	0.021	0.042
	(0.040)	(0.039)	(0.038)	(0.041)	(0.044)	(0.077)
+ Mentoring	0.041	0.015	0.097**	0.093**	0.064	0.005
	(0.039)	(0.043)	(0.039)	(0.040)	(0.042)	(0.086)
**Above Median Dropout Risk*						
Information	-0.068					-0.051
	(0.050)					(0.059)
+ Mentoring	0.033					0.067
	(0.047)					(0.059)
**Plans to enroll*						
Information		0.021				-0.004
		(0.048)				(0.062)
+ Mentoring		0.057				0.097
		(0.050)				(0.065)
**Above Median Wealth*						
Information			0.014			0.012
			(0.043)			(0.041)
+ Mentoring			-0.046			-0.072*
			(0.045)			(0.041)
**Small school*						
Information				-0.022		-0.022
				(0.067)		(0.060)
+ Mentoring				-0.051		-0.042
				(0.070)		(0.064)
**Not remote*						
Information					-0.119*	-0.078
					(0.065)	(0.057)
+ Mentoring					0.066	0.041
					(0.065)	(0.057)
Observations	2332	2332	2332	2325	2332	2325

Notes: The sample for this table is students with completed baseline surveys (N=2,332). All regressions include strata fixed effects, age effects, and sex effects. Standard errors are clustered at the school level (in parentheses). Each column reports a separate regression where random assignment is fully interacted with the heterogeneity indicator and fixed effects. In column 5, the indicator for small school is not available for one school (n=2325). Column 6 includes all heterogeneity indicators simultaneously. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D Deviations from Pre-Analysis Plan

- **Self-regulation index:** In the PAP, this index had as a component: "If I make an effort, I can find enough time to study and do homework at home." This question is only asked of non-dropout respondents, and therefore cannot be included in the index. We drop this component.
- **Evaluation of randomization validity within subgroups:** In our PAP, we evaluated the validity of randomization within subgroups. However, we inadvertently omitted the age and sex controls used in all our regression analyses from that analysis. We have reproduced the analysis herein, including those controls.
- **School effort:** The analysis in Section **??** was not prespecified. The variables used in the analysis were prespecified except for the three individual school effort variables from the midline.
- **Planning Index:** Our PAP included intent to enroll in school in our planning index. This was an error that has been corrected in the paper.
- **Spillovers:** We prespecified a spillover analysis, but our Ministry of Education partners did not provide the promised data.
- **Employment and Migration Outcomes:** We have cut these outcomes out of this paper for economy. They are in the populated PAP.

E Interventions

E.1 Syllabus of Mentoring Sessions

E.1.1 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 E.1: 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.

E.1.2 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

E.1.3 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

E.1.4 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.

E.2 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.

F Conceptual Framework

This appendix presents a simple formal framework that clarifies how the two intervention arms map into the parameters governing adolescents' schooling decisions. The model is not intended as a structural object for estimation, but rather as a device that separates the role of information from that of follow-through capacity, providing a clear benchmark for interpreting the experimental results.

A grade 6 student at time $t = 0$ chooses whether to attempt continuation into lower secondary at $t = 1$. Let the intended action be $i \in \{S, O\}$, where S denotes continuation in school and O denotes an outside option that aggregates work, migration, or other non-school activities. Preferences are fixed and are not directly affected by the interventions.

Let $u_O \in \mathbb{R}$ denote utility from the outside option. If schooling is implemented, the student obtains net utility

$$u_O + \Delta V - c, \tag{2}$$

where ΔV is the (unknown) net lifetime payoff difference between schooling and the outside option, and $c \geq 0$ is the contemporaneous cost of schooling, known at $t = 0$.
Beliefs. At $t = 0$, the student has an information set $\mathcal{I}$ and subjective beliefs over ΔV. Let

$$m(\mathcal{I}) \equiv \mathbb{E}[\Delta V \mid \mathcal{I}] \tag{3}$$

denote the subjective expected net return to continuation. This mirrors standard subjective-expectations models of schooling investment in which choices are governed by perceived returns rather than objective returns (Dominitz and Manski, 1994; Attanasio and Kaufmann, 2014).
Capacity or Follow-through constraints. Even if the student intends to continue, the decision may fail to be implemented due to planning, follow-through, administrative, or coordination frictions. Let $C \in \mathbb{R}$ be a capacity state, where a higher C means fewer constraints. Conditional on intending S, the probability of successful implementation is $\pi(C) \in (0, 1)$ with $\pi'(C) > 0$. If implementation fails, the realized action is O.

In addition, intending S requires paying an attempt cost $\kappa \geq 0$, which is incurred whenever $i = S$, regardless of whether implementation succeeds. This allows capacity to affect both intention and realization, while keeping preferences fixed. Reduced-form implementation wedges of this kind are consistent with models that emphasize self-control, attention, and effort frictions (O'donoghue and Rabin, 1999) and with evidence that reducing psychological or procedural frictions can materially shift take-up of high-stakes decisions (Bhargava and Manoli, 2015b).

If the student intends O, the realized action is O and expected utility is $EU(O) = u_O$. If the student intends S, implementation succeeds with probability $\pi(C)$. Expected utility is

$$EU(S) = u_O + \pi(C)\big(m(\mathcal{I}) - c\big) - \kappa. \tag{4}$$

The student chooses $i = S$ if and only if $EU(S) \geq EU(O)$ which requires:

$$m(\mathcal{I}) \;\geq\; c + \frac{\kappa}{\pi(C)}. \tag{5}$$

The threshold in equation (5) is increasing in the attempt cost κ and decreasing in the implementation probability $\pi(C)$. When follow-through capacity is high ($\pi(C)$ close to one and κ close to zero), the condition reduces to the standard net-return comparison $m(\mathcal{I}) \geq c$. When capacity is low, even students with correct and favorable beliefs may choose the outside option because the effective cost of attempting enrollment exceeds the perceived gain.

The two experimental arms target different parameters of this framework. The information arm provides content designed to shift the subjective return $m(\mathcal{I})$ while leaving follow-through capacity C unchanged. The mentoring arm delivers the same informational content — and therefore shifts $m(\mathcal{I})$ by the same amount — but additionally raises C through sustained personal support, socioemotional skill-building, and logistical guidance. Thus, the mentoring-information contrast isolates the causal effect of relaxing follow-through constraints. If information alone changes behavior, incorrect beliefs bind. If mentoring changes outcomes beyond information, follow-through constraints bind. If neither arm matters, the neoclassical benchmark holds.