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Changing Perceptions of Educational Returns in Low- and Middle-Income Countries

A META-ANALYSIS

David K. Evans and Amina Mendez Acosta

Abstract

Campaigns to provide information about the returns to additional years of schooling have been lauded as low-cost ways to boost student engagement in school. We review 13 such programs in low- and middle-income countries across Africa, Asia, the Caribbean, and Latin America. On average, we find that information campaigns that provide information on the returns to education lead to more accurate student beliefs about the average value of further schooling, but also that those beliefs may be revised either upward or downward, depending on the direction of initial bias. We find positive and significant average impacts on school participation (with an average standardized effect size of 0.02) and on student learning (0.05), with significant variation across studies. Three of the studies with large samples show sizeable impacts on dropout rates specifically. Costs tend to be low, so providing information about the returns to additional years of schooling is likely cost-effective. We discuss variation across studies, design decisions, implementation challenges, heterogeneous effects, and ethical considerations.

KEYWORDS
Education, information, returns to education

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Changing Perceptions of Educational Returns in Low- and Middle-Income Countries: A Meta-Analysis

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Introduction

Do children, youth, and their parents hold accurate beliefs about the benefits of education? If people consistently underestimate those benefits, would communicating those benefits lead to reduced dropouts and increased learning? Households may have imperfect information about the returns to education. For example, if a household in a remote area only observes educated individuals who have not migrated out of that remote area, then they may underestimate the returns to education that accrue through migration to more active labor markets. On the other hand, if households are primarily aware of salient stories of educated acquaintances who have succeeded economically, then they may overestimate the returns to education.

With gaps in both educational access and quality in many countries and with thinly stretched budgets, providing information may be attractive as a low-cost intervention. In the last 15 years, a variety of programs have disseminated the returns to education to different populations via different technologies to understand how it affects student education outcomes. Some of these interventions have also provided information on the costs of education.

This meta-analysis synthesizes all available studies (to our knowledge) on the impact of disseminating information on the returns to education in low- and middle-income countries.[1] We identify 13 studies that evaluate the impact of sharing information on the returns to education. The studies take place in Africa (1 study), the Caribbean (2 studies), Eastern Europe (1 study), Latin America (6 studies), and Asia (3 studies). All but one of the studies take place in middle-income countries. The studies vary widely, targeting students from fourth grade to those entering university. Some interventions target students only while others include parents. Some use videos to implement the program; others rely on online or in-person information sessions.

On average, we find positive, statistically significant average impacts of sharing information about the returns to education on beliefs about the returns to education, on participation outcomes (like enrollment at a university and choosing a particular major), and on learning outcomes (like college admission test scores and standardized math test scores). Average effect sizes, pooled across these categories, are modest relative to other education interventions in low- and middle-income countries (Evans & Yuan, 2022). That said, three of the interventions with large samples—in the Dominican Republic (Jensen 2010) and in urban and rural Peru (Neilson et al., 2018) showed substantively sizeable impacts on dropout rates specifically. While cost data are not reported consistently, costs tend to be modest and so these programs are likely cost-effective on average.

The estimated impact on learning tends to be positive for youth who underestimated the average returns to education at baseline and negative for youth who overestimated the returns to education. For the subset of interventions that include children in primary school—in

1 Most of these studies provide other information in addition to the returns to education, but what all the studies in this review have in common is that they provide information on the returns to education.

Madagascar (Nguyen, 2008) and Peru (Neilson et al., 2018)—we find no average, substantive impact on access or learning.[2] In the discussion section, we explore heterogeneous effects in more detail, as well as common implementation challenges for these programs and ethical considerations.

Our study complements previous synthesis work that includes interventions that share returns to education. Angrist et al. (2020) examine effectiveness and cost-effectiveness of a range of education interventions. That study ranks providing information on the benefits of schooling (the set of interventions that are the topic of this paper) as the most cost-effective for the novel metric "learning adjusted years of schooling," but that average is based on just two estimates, one with a large impact and one with a small impact. The current synthesis covers a larger collection of studies (including studies in the gray literature), covering more geographical settings and more modes of implementing the program. Arias Ortiz et al. (2022) provide a narrative review of interventions that provide information on the returns to education in Latin America. Damgaard and Nielsen (2018) review nudge interventions in education, including interventions that provide information on the returns to schooling. They include a subset of the studies we identify in low- and middle-income countries, plus several studies from high-income countries.[3]

Of course, providing information on the average returns to education is just one type of information intervention in education. Other reviews have examined the impact of sharing information on the quality of schools either with parents or with schools (Cheng & Moses, 2016; Read & Atinc, 2018), and other studies have examined the impact of providing parents with information about their individual students' performance (Bergman, 2021; Bergman & Chan, 2021; Berlinski et al., 2022). The Global Education Evidence Advisory Panel labelled providing information on the "benefits, costs, and quality of education" as the single best buy (i.e., the only "great buy") out of all interventions they reviewed in their initial review (Global Education Evidence Advisory Panel, 2020).[4]

Our findings of modest but likely cost-effective impacts suggest that interventions providing information about the returns to education may be an appropriate tool within a broader toolkit to boost educational outcomes in low- and middle-income countries. The fact that providing information proves effective at changing beliefs in this area points to future innovations with these interventions. For example, future interventions may explore ways of simply communicating

2 For the two programs that include primary education, the point estimate on learning is positive, but it is not statistically significant with 95 percent confidence, only with 90 percent confidence.

3 Table 10 in Damgaard and Nielsen (Damgaard & Nielsen, 2018) qualitatively summarizes the effects across studies in the high-income countries (the U.S., Finland, Germany, and Norway) as well as a few studies in low- or middle-income countries (the Dominican Republic, Madagascar, and Chile, which was a middle-income country at the time of the intervention studied).

4 The second edition of the Global Education Evidence Advisory Panel's recommendations adds two other classes of interventions (targeted instruction and structured pedagogy programs) to the list of "great buys," but retains information interventions as first on the list (Global Education Evidence Advisory Panel, 2023).

distributions (rather than averages) to help beneficiaries make more informed decisions. Likewise, providing information on the benefits of education beyond earnings (e.g., the impact of girls' education on child survival) may be effective in environments where decision makers value other returns beyond financial remuneration. Helping individuals to make investment choices based on accurate information about the likely returns to those investments is worthwhile, even if—all by itself—it will not be sufficient to close access and quality gaps.

The design and objectives of interventions providing information on the returns to education

Interventions that provide information about the returns to education can be designed in a wide variety of ways. Variable features include the content, the mode of delivery, and the target recipients (Figure A1); decisions about these features then affect the mechanisms of change and potential outcomes.

Content: What information is provided?

The content of the intervention may include estimates of earnings (ranging from a role model's testimonial based on that person's experience to population averages from a national labor database), labor market needs (e.g., types of job opportunities that are available and employment rates), as well as total costs of education such as tuition fees, living expenses, and other requirements such as apprenticeships or internships.

Implementers may provide complementary information on resources for students, such as funding opportunities (e.g., scholarships, merit awards, or government loans), resources for career guidance (the existence of school or district career counselors, if they are available), or information on the school admission process that may not be generally known by the students, such as the role of test scores in the application process.

The level of disaggregation of the information presented is often limited by available data, but it will also be guided by the purpose of the intervention and may be tailored to expressed preferences of the recipients. For example, students about to choose between academic degrees or technical/vocational degrees or those about to choose their majors will benefit from information about earnings, the employment rate, and costs disaggregated by type of degree or school so that academic choices are underpinned by realistic market returns (in addition to accessibility and student interest).

Information could also be disaggregated according to the characteristics of the recipients. For example, programs that want to intentionally encourage girls or those from low-income households (groups where returns to education may be under-estimated) could provide returns information specifically for girls or invite role models specifically from low-income backgrounds.[5]

Delivery and recipients: Who receives the information and how?

The medium of dissemination can range from in-person delivery (invited speakers, career counselors, or teachers), to technology-facilitated (phone-calls, text messages, online surveys, or emails), to interventions that use the popular media (radio stations or TV channels), to interventions that use written pamphlets or posters, or combinations of any of these (such as an implementer showing a video and then leading a class discussion). Depending on the choice of medium, information can be delivered in school settings, at home, or virtually.

Finally, those seeking to implement information interventions must determine the target recipients of the intervention. In earlier grades, both parents and students might benefit from more accurate returns to schooling, especially since parents allocate the household resources for education. In later years of schooling, students may have more agency in the choice to attend school.

Mechanisms of change

Information interventions will likely work differently depending on baseline expectations. Parents and students who initially under-estimate returns to education may upgrade their beliefs in response to the intervention, increase aspirations, and increase demand for education. Similarly, parents and students who initially over-estimate returns may adjust their beliefs downward. In the case of post-primary students, for whom there are usually greater schooling costs (both out-of-pocket financial costs and opportunity costs), they may align their aspirations with labor market realities which can help them make financially sound choices around specific programs of study. In some cases, this might mean reduced investment in education; in other cases, it may mean more strategic choices of courses of study or institutions for study.

In addition to how much the intervention leads recipients to update their beliefs, the size of the impact will depend on the importance of this misinformation relative to other barriers to schooling entry that remain, but where correcting information asymmetry can tip the scales and increase demand for education and willingness to invest time, effort, and resources, information interventions could potentially increase school attendance, reduce dropout, improve learning outcomes and completion rates, and improve matches between graduates and labor market needs.

5 At the same time, providing estimated income disaggregated by gender to households with children of multiple genders could reduce investments for whichever gender has lower returns. Although such a shift in investment could be economically rational, public programs that could lead households to reduce investments in children's human capital should be held to a high standard of scrutiny, particularly because expected returns represent just one point in the distribution and will likely not reflect an individual child's actual returns. We discuss these and other ethical issues in the discussion section.

Methods

Search

In this review, we analyze studies that are (a) experimental or quasi-experimental evaluations of (b) interventions that provide information on the returns to education, which are (c) conducted in low- and middle-income countries (according to the classification of the country at the time of the intervention). We include journal articles, working papers, and conference papers in English. Because searching for articles using keywords such as “information” and “returns to education” leads to large numbers of search results with few relevant results (because these terms are so general), we instead started with six anchoring key papers known to the authors (Avitabile & de Hoyos, 2018; Jensen, 2010; J-PAL, 2018; Loyalka et al., 2013; Neilson et al., 2018; Nguyen, 2008).[6] We then reviewed for eligibility both the references cited in these papers and all papers that cite these original papers. We complemented that targeted search with a keyword search in Google Scholar using the terms “information intervention,” “returns to education,” and similar terms. We reviewed the first 1,000 hits and did not identify any additional studies beyond those identified previously.[7] In total, we identified 13 studies that passed our eligibility criteria (see Table A1 for a list of these studies).[8]

Data extraction and meta-analysis

We collected data on the type of information provided, who receives the information (parents or students), the recipient students’ school level, the level about which they are receiving information, any cost information about the program or intervention, and the identification strategy of the evaluation. To perform the meta-analysis, we collected data on each specific outcome, and—whenever available—the effect size, the difference between the means of treatment and control, the standard error of the difference, and the relevant sample sizes. For studies with multiple treatment arms, we only encode outcomes from treatments that directly include information on returns to education.[9] We also do not encode outcomes that have no clear positive or negative interpretation within a context, such as outcomes that document choosing one type of university over another type

6 The use of key papers in systematic reviews is a practice seen in major systematic review outlets, such as the Campbell Collaboration (e.g., Nakamura et al., 2023).

7 The Google Scholar search turned up many studies on how to estimate returns to education, just not on the impact of sharing those returns.

8 We are aware of at least two additional, ongoing studies, both in Peru. One uses text messages to inform parents of students in primary and secondary school with information on the returns to completing primary and secondary school. The other targets students and parents of students in the first year of secondary school. Students see a video and participate in a discussion in school about the returns to finishing high school and continuing with their education. Parents then receive WhatsApp messages with links to a website with average earnings by level of education (Cristia, 2022).

9 For example, we exclude the outcomes for the treatment arm that only provides information on tuition costs and financial aid opportunities in Bernal et al. (2022).

of university, when characteristics of these universities are not directly comparable.[10] See Appendix A for statistical details of the meta-analysis.

In our results, we report two forest plots for each class of outcomes (Panel A and Panel B of each figure). Panel A shows the study-level average and the meta-analytic average across the studies. For the overall meta-analytic average effect, we use average estimates within the entire study (i.e., across all treatment arms within a given study) and then average those across the studies, since treatment arms within a given study do not satisfy the requirement of independence of effect sizes and thus cannot be used to compute the inter-study average (Tanner-Smith & Tipton, 2014).

Panel B shows estimates for each treatment arm within each experiment. From a policy perspective, the treatment arms are of principal interest as they represent specific treatments that a policymaker might replicate.

The sample of studies

Thirteen studies met our eligibility criteria. They cover nine countries: China (2 studies), Chile (2 studies), Colombia (2), the Dominican Republic (2), Georgia (1), India (1), Madagascar (1), Mexico (1), and Peru (1). We list all the individual studies and briefly summarize the interventions and findings in Table A1. Our sample of 13 studies includes mostly studies in upper-middle-income countries (11),[11] with one study in a lower-middle-income country (India) and one in a low-income country (Madagascar). All of the studies are randomized controlled trials. Ten of the thirteen interventions provide information to students only; the other three explicitly seek to share information with both students and their parents (Table A2). Most of the studies focus on reaching students in seventh grade or higher, although two target younger students: Nguyen (2008) targets fourth grade students in Madagascar and Neilson et al. (2018) target students from fifth to eleventh grade in Peru (Figure A2). Six of these studies are published in peer-reviewed journals and the remaining seven studies are working papers or reports.[12]

We extracted over 890 estimates of outcomes from these studies across four dimensions: beliefs and perceptions on returns to education, school participation, learning, and labor market outcomes. Of these, over 290 outcomes are reported for the full sample. Over half of these outcomes are measures of beliefs and perceptions: does sharing information on the returns to education lead

10 For example, Busso et al. (2017) document whether students shift from expensive private universities offering traditional 5-year courses to professional and vocational schools offering shorter and less costly degrees. (Neither is a non-rational choice.) Bernal et al. (2022) document what kinds of information students want more of (e.g., on fees, credits, accredited institutions). Rao (2016) documents whether or not students take an education loan.

11 Chile transitioned from being an upper-middle income country in 2013 according to World Bank classification (World Bank, 2013), but we include the studies that evaluate interventions in this country because the interventions were implemented either before or during the transition.

12 We ran a simple linear regression between the average effect size of each study for each outcome category and whether or not the study is published as a crude measure of publication bias and found no statistically significant association.

people to change their beliefs about the returns to education—likely a necessary condition for subsequent impacts? A fifth of these outcomes are measures of learning, and another fifth are measures of school access and participation. The distribution of these outcomes by category and the number of studies that report these outcomes are reported in Table 1.

Results

In this section, we summarize the findings from the thirteen studies in our sample. We first discuss the baseline accuracy of participants' perceptions of the returns to education. We then report on how the interventions' impact those perceptions. Next, we consider the effect of the interventions on access outcomes, on learning outcomes, and—in the case of a few studies—on other outcomes.

Perceptions on returns to education

All of the studies in our sample that report baseline beliefs (12 of the 13 studies in our sample) find that people either hold inaccurate beliefs about the returns to education or that they have high uncertainty about those returns. However, the hypothesis that these interventions would increase educational investments presumes that people underestimate the return to education. This is not consistently the case. (Table 2 summarizes the particular inaccuracy of participants' estimates of returns to education at baseline in each of the twelve studies.) Four of the twelve studies—in China, one of the Colombia studies, one of the Dominican Republic studies, and in Peru—report that perceptions generally underestimate returns to education. For example, in Peru, Neilson et al. (2018) report that both students and parents surveyed underestimate earnings to education for all levels of schooling, but overestimate returns when moving up to a higher level of education: students expect earnings to jump three times moving from high school to university, but actual returns only increase by a little over twice. In the Dominican Republic, students underestimate the share of university graduates in the richest quintile of the population by almost half while overestimating the share of university graduates in the poorest quintile by double; in other words, they perceived university graduates to earn far less than they actually do (J-PAL, 2018).

Two studies—the other Colombia study (with a different sample and asking about a different aspect of returns) and India—report baseline perceptions that are generally overestimates of actual returns. In Colombia, Bonilla-Mejía et al. (2019) report that four out of five students overestimate actual college earnings by between 60 and 100 percent. In India, Rao (2016) finds that up to three in four students over-estimate earnings (with more students over-estimating earnings for science majors than for art majors).

Most commonly, six of the twelve studies—in Chile, the Dominican Republic, Georgia, and Mexico—report mixed perceptions, with estimates that differ depending on gender or socio-economic status of the respondent or on the level of schooling for which returns are being estimated. In one study in

Mexico, students underestimate the average earnings associated with the completion of high school by about 11 percent but overestimate earnings associated with completion of university by between 27 to 37 percent (Avitabile & de Hoyos, 2018). In general, students in the Mexico study tend to predict higher earnings for themselves than for their peers (even for the same level of education attained). In the Dominican Republic, students overestimated earnings of those who finished only primary by around 10 percent, and underestimated earnings of those who finished secondary by 15 percent and those who finished tertiary by almost half (Jensen, 2010).

In some contexts, even rough estimates are hard to come by. In rural Madagascar, around one-third of primary school students answered "don't know" when asked about perceived earnings; those from households where the adults had less education were the most likely to report not knowing (Nguyen, 2008).

Impact on beliefs and perceptions around returns to education

Given that students and parents often hold inaccurate beliefs about the earnings associated with different levels of education, the next step in the process of changing behavior is changing those beliefs with information. Eight (of the thirteen) studies report impacts on beliefs (Figure 1 Panel A). On average, we find a positive impact (i.e., information makes beliefs more accurate), with an effect size (ES) of 0.08 SDs (95 percent confidence interval [CI]: 0.01–0.14). We do not observe a significantly higher likelihood of updating for those who underestimated returns versus those who overestimated returns (Figure 2).

In terms of individual treatments, students in the earlier Dominican Republic study (Jensen 2010) initially overestimated the earnings of those who finished primary only and underestimated the earnings of those with higher education. After receiving information on returns, the students corrected their beliefs, as reported in a follow-up survey four to six months later (*ES = 0.22, CI = 0.15–0.30*) (Figure 1 Panel B). In Peru, the video series combined with an app that provides information on returns increased the accuracy of students' and parents' estimates of earnings (from a baseline where they underestimated earnings across all levels) and increased their perceived feasibility of pursuing higher education in both urban and rural areas, with the largest point estimate (by far) from providing the app directly to students in urban areas (*ES = 0.37, CI = 0.31–42*) (Neilson et al., 2018). The study in Georgia that provided information leaflets also led to students changing their intended college major to those with higher employment rates (*ES = 0.23, CI = 0.12–0.34*) (Kudashvili & Todua, 2022).[13] Individually, the other five studies report zero to small positive (but imprecise) impact.

13 We include intended college major among the belief outcomes. We include actual college major choices among access outcomes.

Impact on school participation outcomes

Do information interventions increase or otherwise change school participation? In this section, we include outcomes such as total schooling completed, dropout rates, and changes in the choice of school. We find a small average effect (*ES = 0.02; CI = 0.00–0.34*), but a p-value of 0.01, indicating that the lower bound of the confidence interval is above zero in later digits (Figure 3 Panel A). Of the thirteen studies with 23 treatment arms, only three treatments from three studies report an individually positive, statistically significant effect: two of those are large (Jensen, 2010; Kudashvili & Todua, 2022) and the third is small (Neilson et al., 2018) (Figure 3 Panel B).

In Jensen (2010) in the Dominican Republic, students who received the information completed on average 0.20–0.35 more years of school over the next four years. The point estimates for other indicators of schooling (e.g., whether a child returned to school the next year and whether they finished school) and the impacts on poorer households are generally positive but not significant. In Kudashvili and Todua (2022) in Georgia, receiving information on actual earnings and the employment rate led to 14 percentage point more students changing their college majors (significant with 95 percent confidence). In Nguyen (2008) in Madagascar, providing statistics on returns led to 3.5 percentage points higher attendance, although that result was only significant with 90 percent confidence. In Neilson et al. (2018) in Peru, the video series reduced dropout rates by 1.8 percentage points in urban areas and 7.2 percentage points in rural areas (both significant at 1 percent); these appear insignificant in Figure 3 because they are combined with other participation measures that did not show any significant impact.

While the effect is small on enrollment, some studies report that conditional on enrollment, the information intervention led students to choose higher quality schools and tracks with higher returns. For example, in Colombia (Bernal et al., 2022), the intervention led to more students enrolling in tertiary education (by 4 to 6 percentage points) and—conditional on attendance—led to fewer students enrolling in public institutions (by 5 to 7 percentage points, but not significant) and more students enrolling in accredited universities (by 5 to 6 percentage points, also not significant), which in this context, is a (weak) signal for increased demand in higher-quality education.[14]

Information alone is likely not sufficient to increase school participation where other barriers are more salient, but for households that are able to send their children to school, information interventions modestly nudged them to enroll in pathways with better returns.

14 Again, we do not observe a significant effect in Figure 3 because the study tested several other measures of school participation for which there were no significant impacts.

Impact on learning outcomes

For learning outcomes, we find that the average effect on learning outcomes is positive and significant (*ES = 0.05, CI = 0.00–0.11*) from six studies (Figure 4 Panel A)—i.e., half the studies in our sample did not test for changes in learning outcomes. We observe individually positive, statistically significant effects in three studies: Mexico (Avitabile & de Hoyos, 2018) and then certain treatment arms in the Dominican Republic (J-PAL, 2018) and Mozambique (Nguyen, 2008) (Figure 4 Panel B). Point estimates suggest larger effects for those who initially underestimated the return to education (Figure 2), although that difference is not statistically significant.

In Avitabile and de Hoyos (2018), where youth underestimated earnings in high school and overestimated earnings in college in Mexico, the intervention had no impact on taking the national exam; but conditional on taking the exam, it increased test scores by 0.23 standard deviations. Students who received the information intervention reported higher levels of effort. In Neilson et al. (2018), the video series implemented in urban areas improved test scores by 0.03 standard deviations for the math test and 0.04 standard deviations for the verbal test in Peru. Similar to the study in Mexico, these outcomes followed a large impact on beliefs: the intervention raised estimates of returns from a baseline of below-actual expected returns.

In the Dominican Republic (J-PAL, 2018), showing videos about returns significantly improved test scores, with a larger estimated impact by videos with statistics (0.05 to 0.06 SDs) as opposed to videos with qualitative statements on returns. The effect is stronger for those who saw the video twice in succeeding years (0.07 to 0.13 SDs) and for those who were baseline high performers (around 0.10 SD for the upper three deciles). The intervention in Nguyen (2008) improved average test scores by 0.2 standard deviations, with the effects larger for those who underestimated returns at baseline (0.37 SDs). The role model from a poor background improved average test scores by 0.17 standard deviations, mainly driven by impact on poor students (0.27 standard deviations). There is no impact on test scores by the role model from rich background.

One of the Colombia studies (Bonilla-Mejía et al., 2019) demonstrates the key channel for affecting learning through a null: the average effect of that intervention on beliefs was virtually zero, which makes a subsequent zero impact on test scores unsurprising.

Two studies report outcomes that are not strictly on beliefs, school access, or learning. These include predicted earning gains, monthly debt and net value of degrees chosen by students (from Hastings et al., 2015) and prevalence of child labor, likelihood of being employed in hazardous conditions, and total students' work hours (from Neilson et al., 2018). The average impact on these outcomes is positive but not significant (Figure A3).

Discussion

Heterogeneous treatment effects

The average effect of providing information on returns to education may not be massive in general, but the impact may be amplified for some groups depending on several factors including the gender of the student, whether the recipients of information overestimate or underestimate returns at baseline, and household income. We discuss these differential impacts in the sub-sections below, and Figure 2 shows a summary of the impacts by subgroups.

Across other parameters such as the age of students targeted by the intervention and whether the recipient of the information is the parent or the student, the variation is too limited to draw even tentative conclusions. For example, the two studies that include primary age children do not show average effects on access outcomes, and the effects on learning outcomes are positive but statistically significant only at the 90 percent level (Figure A4). But only one of those studies (Nguyen, 2008 in Madagscar) is exclusively targeted to primary age children. Likewise, while those two studies include some variation in whether students or their parents are targeted, the interventions are distinct enough that one cannot clearly attribute differences in outcomes to the inclusion of parents.

Differential impacts by gender

Under- or over-estimating returns to education can be costly to students regardless of gender. Parents may have differential propensities to invest in boys versus girls, either because of beliefs about returns or due to discrimination: in one sample of 30 countries, investment in girls' education is more sensitive to income among the poorest (Evans et al., 2022). Student aspirations may also vary across genders: in Ethiopia among children age 12 to 15 years old, boys are significantly more likely to aspire to complete university than girls of the same age (Favara, 2016).

Is providing information on educational returns equally effective for girls and boys? There are six studies that report gender disaggregated impacts, four of which report estimates that allow them to be included in the meta-analysis.[15] Of these four, three studies report larger impacts for girls across belief, learning, and access outcomes based on selected measures. However, taking into account all outcomes reported in the paper, we find that on average, point estimates look similar for girls and boys, with slightly higher but not statistically significantly different estimates for girls on beliefs and learning. (Figure 2 shows the average across studies; Figure A5 shows results for each study.)

15 Of the six studies, two only report estimates for the interaction term on gender but not the effects and standard errors separate for each gender (Loyalka et al., 2013; Ding et al., 2021). Hence, we do not include these two in the meta-analysis in Figure 2, but include them in Figure A5 for reference.

In terms of individual study findings, the information intervention for tenth graders in Mexico improved student test scores across the board, and the effect is larger for girls: it eliminated the initial 0.30 standard deviation gender gap in the test scores of 12th graders. A likely channel is that girls report three times higher school-related effort than boys (Avitabile & de Hoyos, 2018). In Neilson et al. (2018) in Peru, the intervention increased math test scores for boys and girls, with higher gains for girls such that the gender gap in test scores fell by about one third. In Bernal et al. (2022) in Colombia, girls who received information on returns were more likely than boys to change preferred universities as a response to the new information and to choose higher quality universities.

The last study that reports results by gender—Bonilla-Mejía et al. (2019) in Colombia—find no average, significant impacts on beliefs, access, and learning outcomes for either boys or girls.

Differential impacts based on whether returns are over-estimated or under-estimated at baseline

Three studies report outcomes disaggregated by whether the students and parents over-estimate or under-estimate returns at baseline (Figure 2). One study only reports on belief outcomes, which we cover in the main results section, another study report on learning outcomes, and a third study report on all three outcomes. Across these studies, we see slightly bigger point estimates on updating beliefs for those who initially over-estimated returns (*ES = 0.03, CI = −0.03–0.09*), and slightly bigger impacts on access (*ES = 0.05, CI = −0.01–0.10*) and learning (*ES = 0.05, CI = −0.02–0.12*) for those who initially under-estimated returns, although none of these estimates are statistically significant. In particular, under-estimators at baseline had consistently higher test scores across the different treatment arms in Nguyen (2008) in Madagascar and in Bonilla-Mejía et al. (2019) in Colombia.[16]

Differential impacts based on poverty level

Five studies report outcomes disaggregated by household income. In general, we find that treatments have no significant impact on both poor and less poor households across belief and access outcomes (Figure 2), but we do find more positive impacts on learning for less poor households (*ES = 0.08, CI = 0.02–0.17*) than poor households (*ES = 0.05, CI = −0.03–0.13*), consistent across the three studies that report learning outcomes by poverty level (Avitabile & de Hoyos, 2018; Bonilla-Mejía et al., 2019; Nguyen, 2008). The Dominican Republic study by Jensen (2010) is an exception: students in less poor households upgraded their beliefs at a higher rate and completed more years of schooling compared to poor households.[17]

16 Figure A6 report these estimates by study in more detail.

17 Figure A7 report these estimates by study in more detail.

While we only have one study from a low-income country (Madagascar), the fact that the point estimates on learning outcomes are relatively high in that environment could point to the possibility of higher returns in the lowest income environments, where information may be less readily available. While we certainly caution against drawing strong conclusions from a single study, this suggests the potential value for low-income country systems of testing these interventions further.

Implementation challenges

Some studies report challenges in implementation along the lines of monitoring compliance, providing technical support, and ensuring adequate infrastructure in more remote areas, especially for interventions delivered through technology. Two studies—perhaps due to their nature of being project completion reports rather than journal articles—discussed those issues.

First, information interventions that are implemented through schools may compete with already existing priorities and—as a result—fall through the cracks. In Peru (Neilson et al., 2018), researchers sent packets with an introductory letter, video discs containing four 15-minute-long videos, and an instruction manual to principals of treated schools in urban areas. The principals were responsible for projecting the videos. However, less than half of the treated schools received the information packets, and only three in four schools that received the packets watched them. As a result, only one in three schools designated to receive the intervention ended up showing the videos during the first round. In the second year, follow-up measures greatly improved urban implementation.[18] In contrast, research staff visited the treatment schools in rural areas to organize a one-time session to show the videos with portable projections. This meant higher costs to monitor compliance, but 100 percent of targeted schools in rural areas received the treatment. In the later Dominican Republic program, telephone operators called the school psychologists responsible for showing the videos to students to verify compliance (J-PAL, 2018).

In addition to non-compliance, information interventions relying on technology could also face glitches: a bug in the app used in Neilson et al. (2018) in Peru made it difficult to distinguish between treatment and control groups in some urban schools. In the Dominican Republic (J-PAL 2018), the telephone operators validating compliance also collected feedback on technical difficulties that hindered school officials from showing the videos and coordinated with the assigned technician to troubleshoot. The researchers in this setting were particularly concerned with the infrastructure since less than half of the schools in their setting had appropriate equipment for showing videos and less than one-third of schools had stable access to electricity. Information interventions that do

18 In the second round of implementation the following year, the researchers re-delivered the packages and conducted a survey to track the implementation challenges of the previous round. They also set-up a call-center which fielded around 8,000 calls to monitor dissemination and e-mailed the materials to schools that experienced damage or loss of the videos. They incentivized schools by conducting a raffle of two computers to schools which completed the implementation on time. Because of these steps, the compliance rate doubled to two out of three schools by the second round.

not rely on technology such as written pamphlets, posters, or live resource speakers may be more appropriate in these settings, with some considerations for cost and effectiveness.

Cost analysis

The cost of implementing an information intervention can be grouped into two parts: the fixed cost of developing the information packet (in whatever format), and the variable costs of producing and delivering the information packets to recipients (Figure A8). Information on returns tailored to individual baseline preferences of students (as in Busso et al., 2017) or disaggregated by university and degree combinations (as in Bonilla-Mejía et al., 2019) may cost more to produce than generalized information on returns by level of schooling. Similarly, in-person delivery of information by gathering students or parents in one setting and engaging teachers to deliver the statistics on returns or a role model to talk about personal experiences (as in Nguyen, 2008) would cost far more than a mass-email strategy, where much of the cost would comprise getting the email addresses of students. Interventions delivered via videos (such as the telenovelas in Neilson et al., 2018 and J-PAL, 2018) would sit somewhere in the middle with its high fixed costs in production but cheaper reproduction, distribution, and scale-up.

Studies tend to be inconsistent and incomplete in their reported cost analysis. Table 3 lists the cost information and cost-effective analysis available from the eligible studies. Four of the studies provide either cost data or cost-effectiveness analysis. Several others claim that the interventions are low-cost or cost-effective without providing any specific cost analysis.

The available cost data suggest that on average, these tend to be very cheap among the range of education interventions. For example, analysis of the cost-effectiveness of the Nguyen (2008) intervention in Madagascar found that the cost of distributing and discussing the statistics was 8 cents (USD) per student. In Peru, the marginal cost of the campaign was 5 cents per student (after producing the video, which would add to the average cost but divided across a large number of students, the cost would still be low).

Ultimately, these tend to be cost-effective interventions. While individual studies do not always have the statistical power to identify significant impacts, our meta-analytic results suggest that these interventions are nudging students towards more accurate beliefs and—on average—more school participation and better learning outcomes. Relative to coaching teachers or building schools (both important classes of interventions), these will be much cheaper and still deliver positive impacts.

Ethical considerations in information interventions

One concern with changing student and parent behavior by providing information on the average returns to different levels of schooling is that almost no one earns the average. Some will earn less, some will earn more, and students may have no way of knowing where they are likely to fall.

As a result, while we can know whether the intervention is leading student beliefs about the average returns to be more accurate, we do not know whether the interventions are leading student beliefs to be more accurate about their likely personal returns. One study in India (Rao, 2016) sought to go beyond averages and share the 25th and 75th percentile of returns. Further work on how better to communicate distributions—especially among populations with limited statistical literacy—may allow future interventions to mitigate this concern.

A second concern is that among populations where individuals overestimate the returns to education (which is common, as demonstrated in Table 2), parents may choose to reduce investments in children. Likewise, if returns are broken down by gender and one gender is predicted to gain higher returns, parents may reallocate across genders. As a practical example, in an experiment that provided parents with more accurate information about each of their children's school performance in Malawi, parents re-allocated resources away from poorer performing children and toward higher performing children (Dizon-Ross, 2019). While this may be the parents' prerogative, interventions that have the potential to lead parents to reduce investments in children should be held to a very high level of scrutiny by the external actors supporting those interventions.

Conclusion

On average, providing information about the returns to education leads to more accurate beliefs about the returns to education, greater rates of school participation, and larger impacts on student learning outcomes. All of these impacts are statistically significant with 95 percent confidence or higher. The average effects on beliefs are the largest (a standardized effect size of 0.08), with a small average effect on school participation (0.02) and a substantive impact on learning (0.05). There is significant variation across studies. Available evidence suggests that these interventions tend to be relatively low-cost. Several of the studies were implemented at some scale (with more than a thousand schools in the second Dominican Republic study and a similarly sized sample in Peru). All of this suggests that information campaigns on the returns to education are a useful part of the education policymaker's toolbox.

That said, two considerations may give pause. First, as discussed in the section on ethical considerations, these interventions may reduce investments in education as well as increase them. While the average effect is positive, policymakers must concern themselves with not only the average student but also the most marginalized, at-risk students. In some cases, reduced investment may be optimal—e.g., among secondary school students who were considering tertiary courses of study with low returns. In other cases, reduced investments may be problematic—e.g., with very young students where the uncertainty around returns is particularly high, potentially limiting future opportunities.

Second, while these seem to be highly cost-effectiveness interventions, the actual size of the average impacts is modest. A recent review of effect sizes in international education interventions found a median effect size on access outcomes of 0.07 and on learning outcomes of 0.10 (Evans & Yuan, 2022). The average effect of the class of information interventions that we review in this paper is substantially lower for both classes of outcomes. Policymakers in countries at all levels of income have limited bandwidth to design and manage programs effectively, so in places seeking to make big improvements in education, cost-effectiveness may not be a sufficient metric by itself if the substantive impact of the intervention is limited.

Both of these considerations are potentially addressable. First, identifying innovative ways to communicate distributions rather than mere averages may help beneficiaries to understand the range of possible outcomes and invest appropriately. Second, while policymakers do have limited bandwidth, there are many programs that are completely ineffective, and so replacing an ineffective program with a modestly effective program is likely to be a net benefit to beneficiaries. Ultimately, helping individuals and households in low-income environments to make the best choices with the limited resources they have is clearly a worthy part of the education policy maker toolkit.

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Tables and figures

TABLE 1. Distribution of outcomes from the eligible studies

Type of Outcome	Outcomes Reported for the Full Sample (# of Studies that Report these Outcomes)	Outcomes Reported by Gender (# of Studies that Report these Outcomes)	Outcomes Reported by Poverty Level (# of Studies that Report these Outcomes)	Outcomes Reported by Baseline Estimates of Returns (# of Studies that Report these Outcomes)	Total Outcomes (# of Studies that Report these Outcomes)
Belief	164 (8)	226 (3)	32 (3)	22 (2)	468 (8)
School attendance	63 (12)	40 (4)	48 (5)	10 (1)	198 (12)
Learning	51 (6)	16 (3)	34 (3)	28 (2)	129 (6)
Other outcomes	19 (2)	26 (1)	13 (1)	0 (0)	99 (2)
Total	297 (12)	308 (4)	127 (6)	60 (3)	894 (13)

TABLE 2. Studies that characterize perceptions on returns to education

Study	Country	Baseline Beliefs vs Measured Earnings	
		Brief	Detailed
Avitabile and de Hoyos (2018)	Mexico	• Underestimate earnings to completing secondary school • Overestimate earnings to completing university	At the baseline, students underestimate the average earnings associated with the completion of high school but overestimate earnings associated with completion of university. Both boys and girls underestimate their earnings by 11 percent, conditional on high school completion. Conditional on university completion, boys overestimate their earnings by 27 percent, and girls overestimate their earnings by 37 percent. Actual earnings of girls are about 25 percent lower than those earned by boys on both levels of school attainment, and the students' estimations of their earnings reflect this gender gap on wage.
Bernal et al. (2022)	Colombia	• Underestimate earnings from higher education	In a survey where students are asked to identify which statements are myths or facts, in general, about half of students believe that the salary earned after higher education is not enough to pay for educational loans and that graduates from accredited institutions in general do not earn higher salaries than graduates from non-accredited institutions. Both of these statements are wrong.
Bonilla-Mejía et al. (2019)	Colombia	• Overestimate earnings from higher education	"Four out of five students in our sample tend to overestimate actual college earnings. Students overestimate the average premiums to two- and four-year college degrees by about 60% and 100%."
Busso et al. (2017)	Chile	• Underestimate employment probability • Overestimate earnings	At baseline, self-reported probability of being employed after study is around 70 percent, which underestimates actual employment rates of 84 percent. "At the same time, students tend to overestimate wages for people with their preferred degree. Three-quarters of respondents with non-missing data expect that monthly wages are over 600,000 pesos. This is 1.6 times the median monthly wage for similarly educated young adults in 2013."

TABLE 2. (Continued)

Study	Country	Baseline Beliefs vs Measured Earnings	
		Brief	Detailed
Ding et al. (2021)		• No information on baseline expected earnings and returns	The paper reports that "on average, female students expressed less preference for a STEM major than male students" but do not report baseline estimates on income or returns based on choice of major.
Hastings et al. (2015)	Chile	• Overestimate earnings (among low-achieving students)	"Low-income and low-achieving students who apply to low-earning college degree programs overestimate earnings for past graduates by over 100%, while beliefs for high-achieving students are correctly centered."
J-PAL (2018)	Dominican Republic	• Underestimate earnings	Students consistently underestimated the benefits of higher education levels. "Students underestimate the percentage of university graduates who are in the richest quintile by almost 40 percentage points, while overestimating their proportion in the poorest quintiles by almost double the actual proportion." This finding is consistent for across gender and socio-economic level.
Jensen (2010)	Dominican Republic	• Overestimate earnings from primary • Underestimate earnings from secondary and tertiary	Students in the baseline survey were asked what they thought were the monthly earnings of someone based on school attainment. The students overestimated earnings of those who finished only primary by around 10 percent, and underestimated earnings of those who finished secondary by 15 percent and those who finished tertiary by almost half. In effect, they were underestimating the premium of secondary education over primary and the premium of tertiary education over secondary by about 75 percent for each level.
Kudashvili and Todua (2022)	Georgia	• Underestimate earnings from secondary • Overestimate earnings from tertiary	Students underestimated earnings of those with no university education by 25 percent and overestimated earnings of those with university degrees by 60 percent (estimates by major are between 22 percent to 113 percent of actual earnings). Students also significantly overestimated unemployment rates of both those with no tertiary degrees (off by a scale of 4.5) and those with tertiary degrees (scale of 1.2). Multiplying mean earnings with unemployment rate, students' expected earnings are smaller than actual for those with no university degree (by 50 percent) and bigger than actual for those with degrees (by 31 percent).
Loyalka et al. (2013)	China	• High uncertainty in earning predictions • Overestimate cost of attending secondary • Overestimate rate of enrollment to secondary	"Although students do perceive that higher levels of schooling lead to higher wages, there is substantial variation among students in perceived wages for each level of education… For example, student estimates for wages earned by university graduates range from 1300 to 13,000 yuan per month." Students also overestimate costs of attending vocational schools: "the median expectation is 2000 yuan higher than the actual net cost, 3000 yuan/year. More than 25% of students believed that attending vocational high school would cost 10,000 yuan/year or more. This is 7000 yuan (or more than two times higher) than the actual net cost." In addition, only 5 percent of students believe they will enroll to secondary school (instead of entering the labor force early), when only two-thirds actually do based on recent data.

TABLE 2. (Continued)

Study	Country	Baseline Beliefs vs Measured Earnings	
		Brief	Detailed
Neilson et al. (2018)	Peru	• Underestimate earnings for all levels of education • Overestimate returns for all levels of education	"We found that students and parents underestimate the economic returns to all levels of education." Based on students estimates, completing university vs completing high school would increase wages by 2.9 times while real returns is around 2.2 times.
Nguyen (2008)	Madagascar	• High uncertainty and low willingness to predict earnings • Accurate median estimate among those who report	Rural households face uncertainty about the returns to education: "73% of the respondents report that it is difficult to learn about their peers and neighbors' income; 53% say there are frequent incidences of educated people out-migrating from the village." Around one-third of respondents answered "don't know" when asked about perceived earnings; those from poor households and with less education are slightly more likely not to report perceived earnings. Of those that report perceived earnings, the median estimate is close to actual average earnings, although the distribution is widely dispersed. In addition, parents associate higher education with government jobs when only 33 percent of high school graduates work for the government and 40 percent in commerce and the private sector. Poorer households report consistently lower perceived earnings across all education levels (including low baseline earnings with low education), but estimate substantial income earnings with more education.
Rao (2016)	India	• Overestimate earnings from tertiary	Majority of students (between 64 to 75 percent) over-estimate earnings across the different academic strands. Both genders over-estimate earnings, but more males do so (66 to 83 percent) than females (49 to 70 percent).

TABLE 3. Cost-effectiveness data from studies

Study and Country of Implementation	Cost Information	Cost-Effectiveness Analysis
Avitabile and de Hoyos (2018) (Mexico)	The program is characterized as "essentially zero-cost."	None
Bernal et al. (2022) (Colombia)	The intervention is characterized as zero cost.	None
Bonilla-Mejía et al. (2019) (Colombia)	The program is characterized as "low-cost."	None
Busso et al. (2017) (Chile)	The interventions' cost has three components: "the cost of collecting student contact details (emails) at baseline, along with their baseline school and career preferences" (in this case, 1.8USD per student); the cost of building the tailored returns information (around 2.68USD per treated student); and of building a database on financial aid information (1.5USD per student).	None
Ding et al. (2021)	None	None
Hastings et al. (2015) (Chile)	None	None
J-PAL (2018) (Dominican Republic)	"The major investment was executed in the production and elaboration of the videos ($104,000) and further improvements for the implementation of the second year ($25,400). The second highest costs was the training of technicians to effectively deliver the videos to the schools they were in charge. The training expenses, which included transportation, food and personnel ranged from $22,000 to $63,000 from years 2015 to 2016. Other costs such as monitoring, CDs and materials were no higher than $13,000."	Per $100, the videos increased test scores by between 0.75 to 1.66 standard deviations (Table 7 in the report). Showing the videos in both years is more cost-effective. The biggest cost comes from producing the videos, so scaling up would further improve cost-effectiveness.
Jensen (2010) (Dominican Republic)	None	The program is described as "cost-effective."
Kudashvili and Todua (2022) (Georgia)	None	None
Loyalka et al. (2013) (China)	None	None
Neilson et al. (2018) (Peru)	"The marginal cost of the campaign was less than US$0.05 per student (not including the fixed costs of producing the video)."	None
Nguyen (2008) (Madagascar)	None	"The [statistics] program cost 0.08 USD per student but increased student attendance by 3.5 percentage points and improved test scores by 0.20 standard deviations after three months. This implies a program cost of 2.30 USD for an additional year of schooling and 0.04 USD for additional 0.10 standard deviations in test scores, more cost-effective than previous interventions evaluated in a randomized experiment."
Rao (2016) (India)	None	None

FIGURE 1. Average effect on beliefs and perceptions (positive effect = more accurate beliefs)

Panel A: Clustered by study

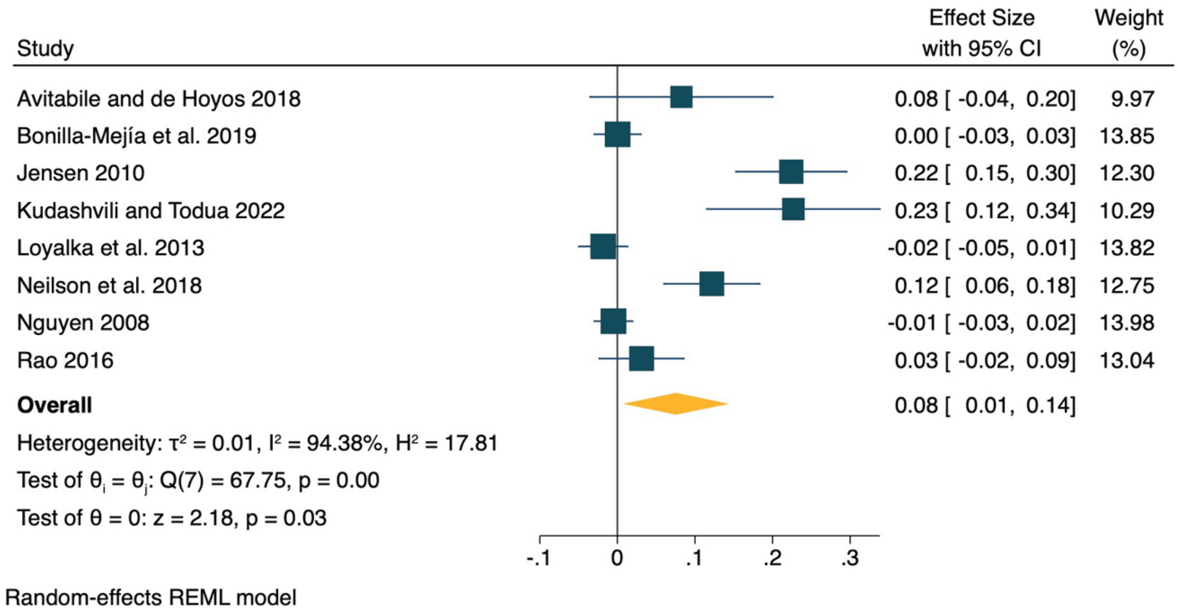
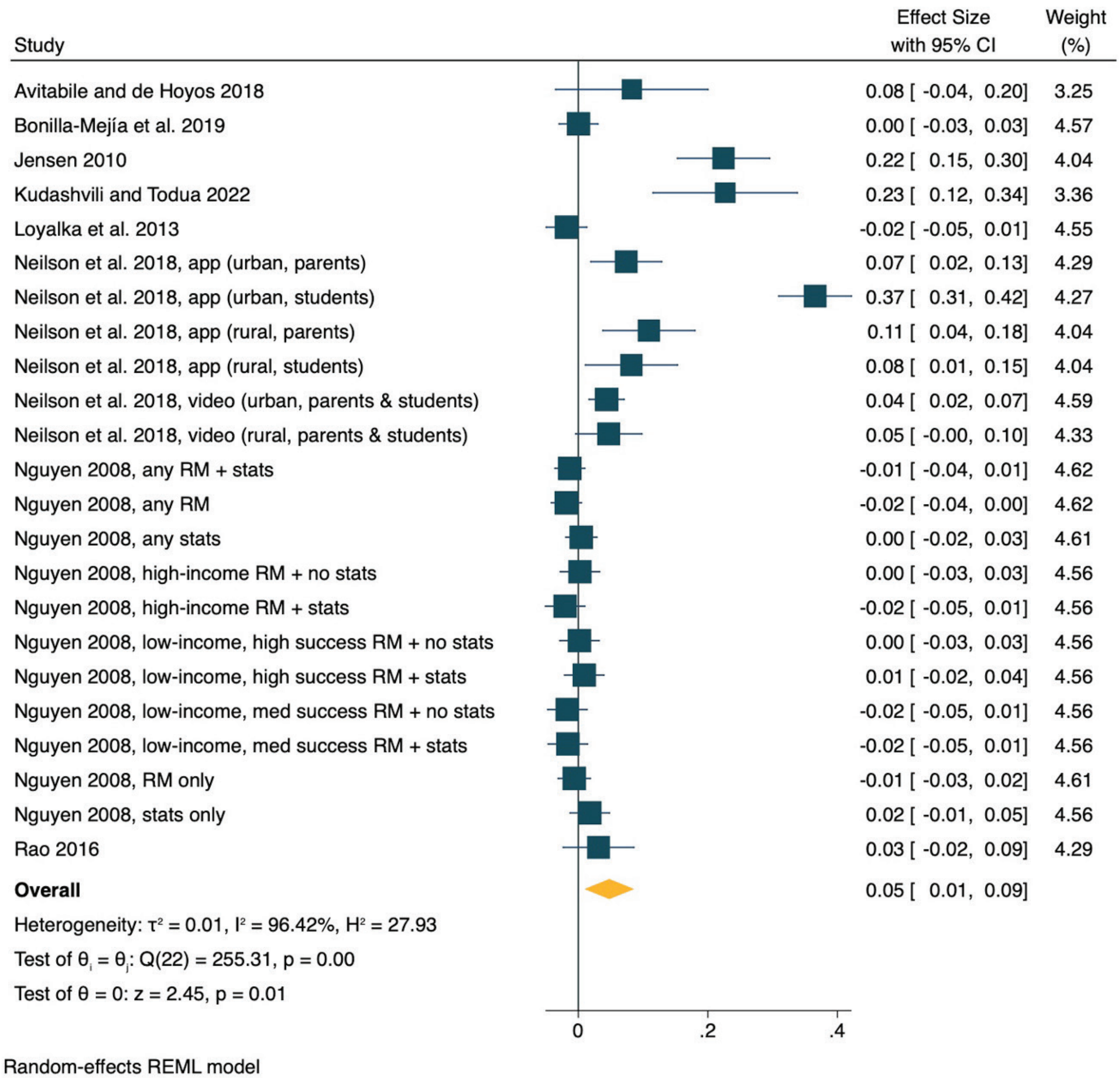


FIGURE 1. (Continued)

Panel B: Clustered by treatment arms



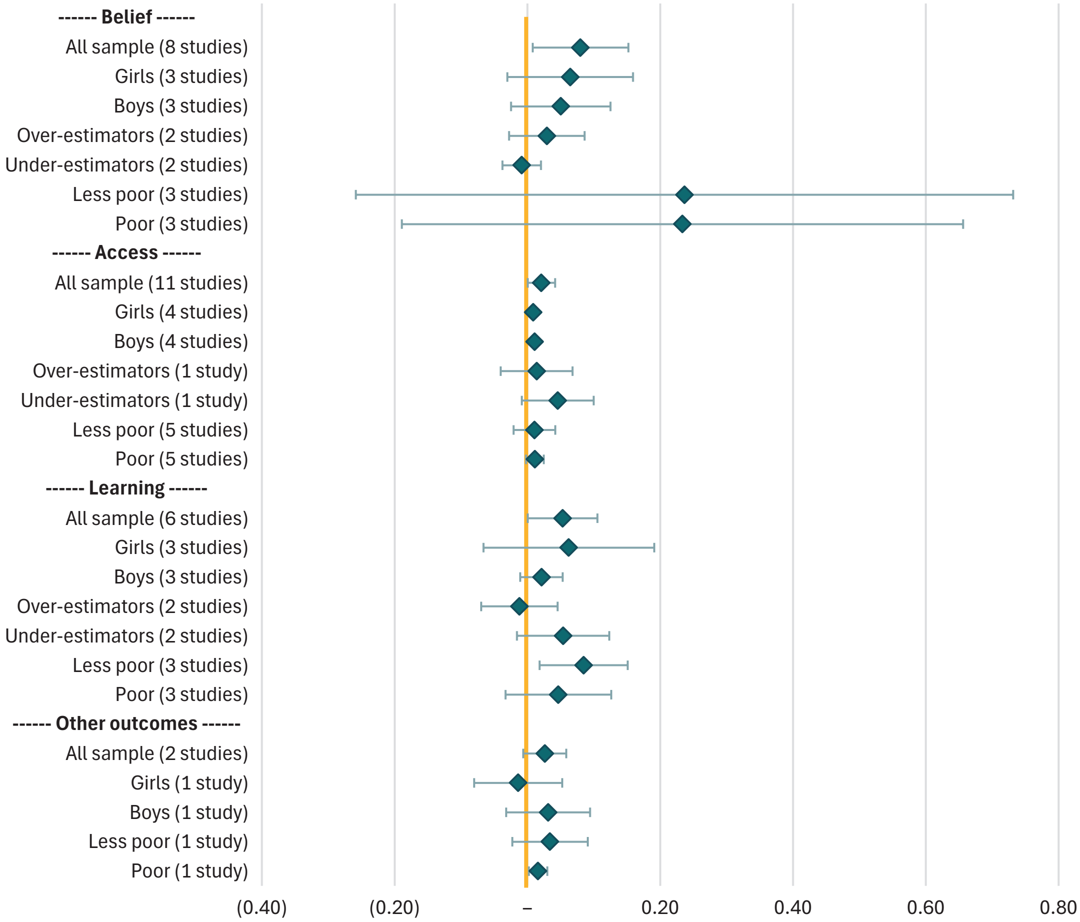
Note: RM means role-model. We report the outcomes here such that a positive coefficient means an increase in the accuracy of belief and perception. For example, the signs of the coefficients for the absolute gap between perceived and actual returns to education are flipped, since a negative coefficient (hence, a reduction in the gap between perception and actual) is a positive outcome. Similarly, for those who initially under-estimate returns, a positive outcome is that they update their perception upwards. For those who initially over-estimate returns, a positive outcome is that they update their perceptions of returns downwards (and hence, we flip the sign of the coefficient).

Note on clustering: Many of the studies report multiple estimates for the same type of outcome. We cluster outcomes to avoid giving undue weight to studies that provide multiple estimates. In this forest plot, we use the method of aggregation proposed by Borenstein et al. (2009) assuming within-study correlation of dependent outcomes to be 0.5. See Appendix B for more details in how we cluster the outcomes.

Note on weights: We weight the outcomes of the studies by the inverse of the variances of their effect estimates (i.e., more precise estimates and those from larger studies with smaller standard errors receive more weight) (Higgins et al., 2022). The weight attributed to each study (in the case of Panel A) and each treatment (in the case of Panel B) is represented by the size of the square marker in the location of the point estimate. These weights are the default for the Stata command for meta-analysis that we employ: commands *meta summarize* and *meta forestplot* in Stata 16.1 (StataCorp, 2019).

Source: Authors' construction from the eligible studies described in the methods section.

FIGURE 2. Heterogenous effect by gender, poverty, and baseline estimates



Note: Many of the studies report multiple estimates for the same type of outcome. We cluster outcomes to avoid giving undue weight to studies that provide multiple estimates. In this forest plot, we use the method of aggregation proposed by Borenstein et al. (2009) assuming within-study correlation of dependent outcomes to be 0.5. See Appendix B for more details in how we cluster the outcomes. The number of studies reporting the relevant outcomes are inside parentheses.

Source: Authors' construction from the eligible studies described in the methods section.

FIGURE 3. Average effect on school participation outcomes

Panel A: Clustered by study

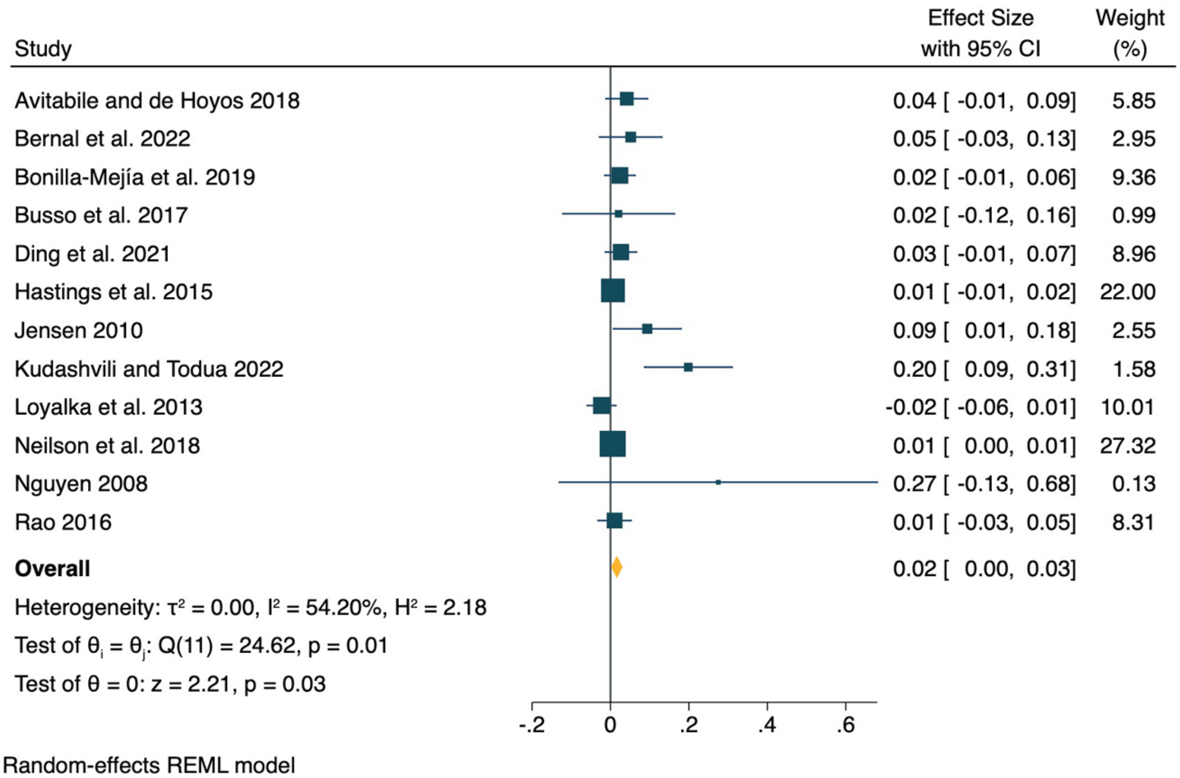
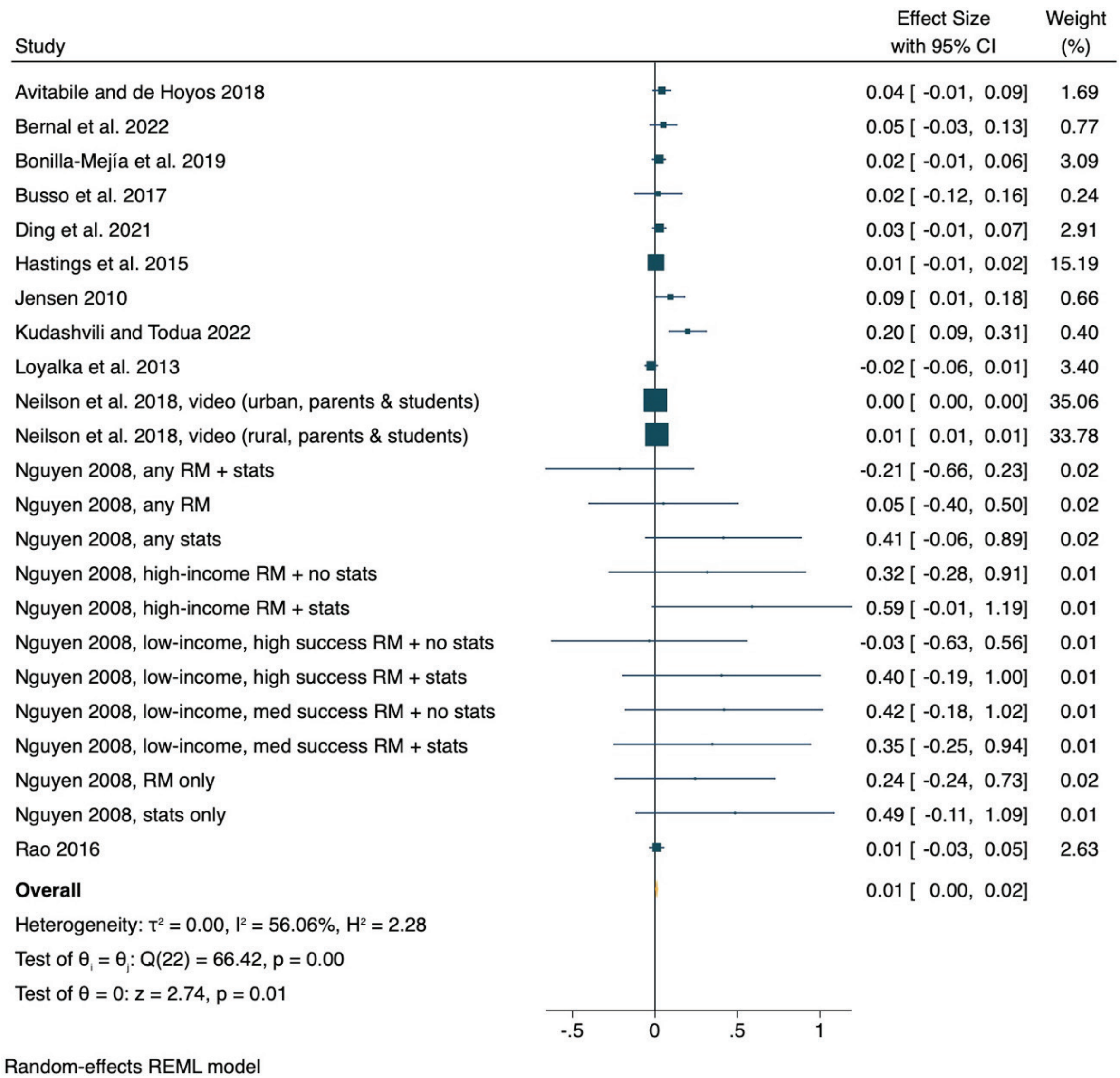


FIGURE 3. (Continued)

Panel B: Clustered by treatment arms



Note: RM means role-model. Many of the studies report multiple estimates for the same type of outcome. We cluster outcomes to avoid giving undue weight to studies that provide multiple estimates. In these forest plots, we use the method of aggregation proposed by Borenstein et al. (2009) assuming within-study correlation of dependent outcomes to be 0.5. See Appendix B for more details in how we cluster the outcomes.

Note on weights: We weight the outcomes of the studies by the inverse of the variances of their effect estimates (i.e., more precise estimates and those from larger studies with smaller standard errors receive more weight) (Higgins et al., 2022). The weight attributed to each study (in the case of Panel A) and each treatment (in the case of Panel B) is represented by the size of the square marker in the location of the point estimate. These weights are the default for the Stata command for meta-analysis that we employ: commands *meta summarize* and *meta forestplot* in Stata 16.1 (StataCorp, 2019).

Source: Authors' construction from the eligible studies described in the methods section.

FIGURE 4. Average effect on learning outcomes

Panel A: Clustered by study

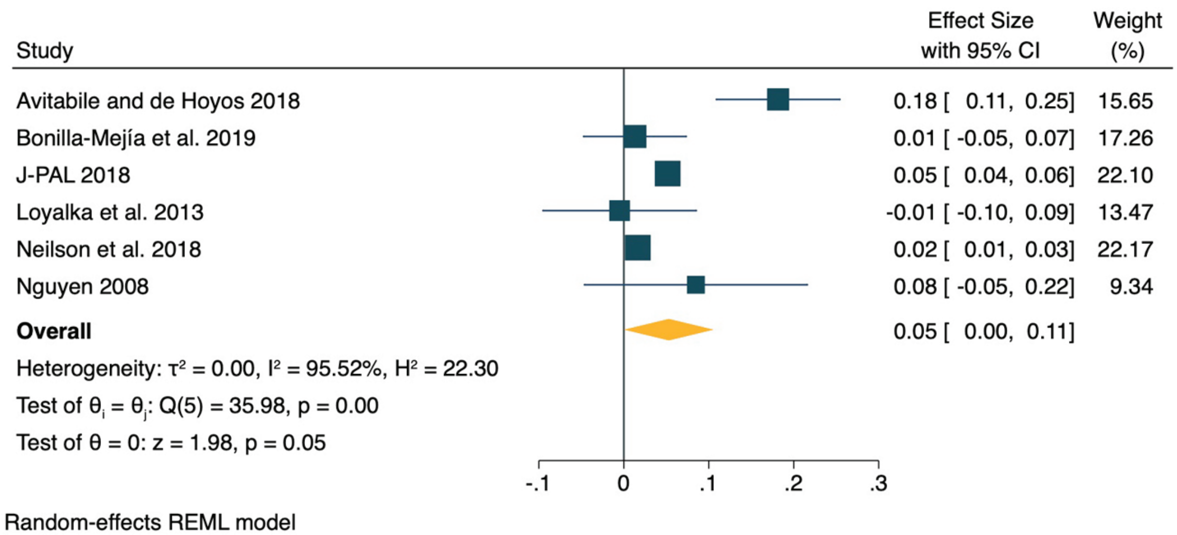
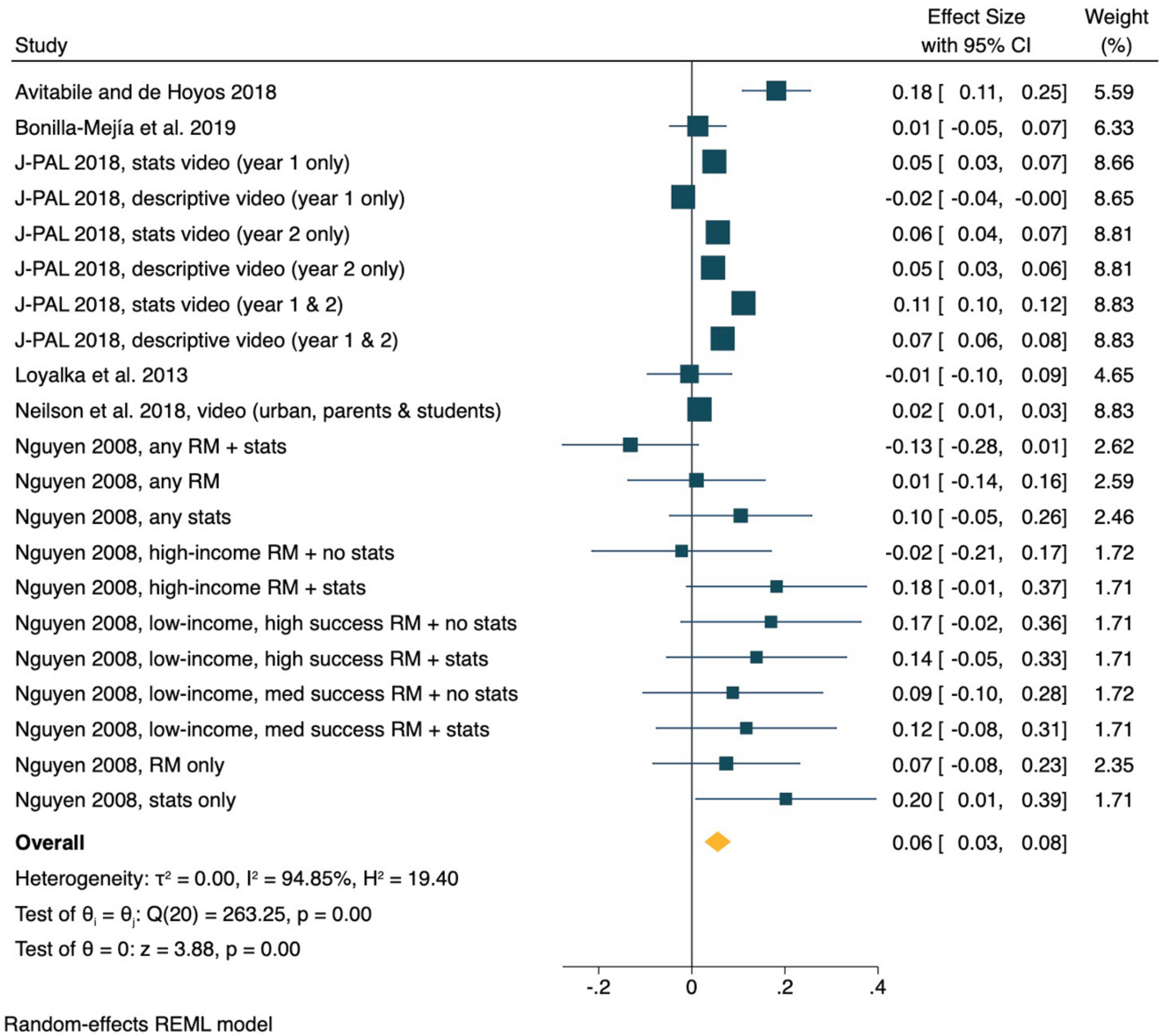


FIGURE 4. (Continued)

Panel B: Clustered by treatment arms



Note: RM means role-model. Many of the studies report multiple estimates for the same type of outcome. We cluster outcomes to avoid giving undue weight to studies that provide multiple estimates. In these forest plots, we use the method of aggregation proposed by Borenstein et al. (2009) assuming within-study correlation of dependent outcomes to be 0.5. See Appendix B for more details in how we cluster the outcomes.

Note on weights: We weight the outcomes of the studies by the inverse of the variances of their effect estimates (i.e., more precise estimates and those from larger studies with smaller standard errors receive more weight) (Higgins et al., 2022). The weight attributed to each study (in the case of Panel A) and each treatment (in the case of Panel B) is represented by the size of the square marker in the location of the point estimate. These weights are the default for the Stata command for meta-analysis that we employ: commands *meta summarize* and *meta forestplot* in Stata 16.1 (StataCorp, 2019).

Source: Authors' construction from the eligible studies described in the methods section.

Appendices

Appendix A: Standardizing effect size

When the standardized effect size is not reported, we follow equation 4.18 in Borenstein et al. (2009) and use the point estimates to compute the standardized effect size or Cohen's d:

$$d = \frac{D}{S_{pooled}} = \frac{Y_T - Y_C}{S_{pooled}} \tag{A.1}$$

where D is the mean difference between the outcomes for the treatment group (Y_T) and the control group (Y_C) at the follow-up, and S_{pooled} is the pooled standard deviation for the treatment and control groups combined. In the studies in our sample, this is often missing so we calculate this using the following formula:[19]

$$S_{pooled} = SE_D \sqrt{\frac{n_T n_C}{n_T + n_C}} \tag{A.2}$$

where n_T is the sample size of the treatment group at follow-up, n_C is the sample size of the control group at follow-up, and SE_D is the reported standard error of D.

For the studies that do not report the effect size or for studies that do report the effect size but not the variance of the effect size, we follow equation 4.20, again in Borenstein et al. (2009), to compute the variance of Cohen's d:

$$V_d = \frac{n_T + n_C}{n_T n_C} + \frac{d^2}{2(n_T + n_C)} \tag{A.3}$$

where n_T is the sample size of the treatment group at follow-up, n_C is the sample size of the control group at follow-up, and d is the Cohen's d computed in equation (A.1) above. We take the square root of this statistic to compute the standard error.

Appendix B: Clustering the outcomes by study

Many of the studies in our sample report multiple outcomes within the same category (such as the impact of intervention on math test scores and reading test scores, or impact on beliefs immediately after the intervention versus on follow-up, or impacts of different treatment arms). These multiple outcomes within the same study are not independent, since they draw from the same sample and are compared against the same control group. This is a challenge because (1) univariate meta-analysis assumes independent effect sizes and (2) not clustering dependent effects within a study will give undue weight to some studies simply because they report more measures of the same outcomes. There are two methods to overcome these challenges.

19 The derivation of this equation can be found in Appendix C: Mathematical Appendix in Evans and Popova (2017).

One method is to first calculate a synthetic within-study average effect of the dependent outcomes. These synthetic effect sizes are independent across the studies and can now be aggregated using conventional meta-analytic methods. We implement this by simply taking the mean of the individual effect sizes in the study as discussed from Chapter 24 from Borenstein et al. (2009), or formally, equation 24.4:

$$\bar{Y} = \frac{1}{m}\sum_{j}^{m} Y_j \tag{B.1}$$

where m is the number of outcomes in the study.

The variance of this mean is computed by equation 24.5 in the same chapter:

$$V_{\bar{Y}} = \left(\frac{1}{m}\right)^2 \left(\sum_{j=1}^{m} V_i + \sum_{j \neq k}\left(r_{jk}\sqrt{V_j}\sqrt{V_k}\right)\right) \tag{B.2}$$

where m is the number of outcomes in the study, V_i is the variance of each effect size in the study and r_{jk} is the correlation between two different effect sizes. Here, we assume this correlation to be the same across all effect sizes (i.e., $r = r_{jk}$ for all combinations of j and k). We use the command *agg* from the Meta-Analysis with Mean Differences package in *R* (Del Re & Hoyt, 2022) using three assumptions on correlation r (0, 0.5, and 0.99) to check for robustness of result.[20]

We then use Stata's meta-analysis package to summarize the effect sizes across the studies under a random effects model (commands *meta summarize* and *meta forestplot* in Stata 16.1) (StataCorp, 2019). The random effects model assume that the studies' true effect sizes are different (as opposed to a common-effect model that assumes that there is one true effect across all the studies) and that the studies only capture a random sample of the larger population of studies (as opposed to assuming that all the studies in the meta-analysis define the whole population of interest). We use the default weighting scheme where the outcomes of the studies are weighted by inverse of the variances of their effect estimates (essentially, more precise estimates and those from larger studies with smaller standard errors are given more weight) (Higgins et al., 2022).

Another method is to employ a robust variance estimation (Hedges et al., 2010) that allows for the inclusion of dependent effect sizes within each study and uses an estimate of the variance that is robust to assumptions around the covariance structure of the effect sizes being reported (Hedges et al., 2010). In particular, equations 10 and 11 of Hedges et al. (2010) shows the application of the

20 The documentation on *agg* command requires Hedges *g* effect size, which corrects for small sample bias present in Cohen's *d* (Lin & Aloe, 2021), but given the relatively large sample sizes in our studies (the smallest sample size is 44, median is 1,890), Cohen's *d* should be a sufficient effect size input (Ben-Shachar et al., 2023).

method to mean effect sizes that we use here. The weighted mean of a sample of m studies taking into account multiple outcome clusters k_j (for $j = 1, \ldots, m$) is given by the intercept b_1:

$$b_1 = \frac{\sum_{j=1}^{m} \sum_{i=1}^{k_j} w_{ij} T_{ij}}{\sum_{j=1}^{m} \sum_{i=1}^{k_j} w_{ij}} \tag{B.3}$$

where T_{ij} is the estimate and w_{ij} is the weight for the estimate i in the study j.

Assuming all estimates in the same study is given the same weight (while weights across the study may vary), the variance of b_1 is given by:

$$V^R = \frac{\sum_{j=1}^{m} w_j^2 \left(\bar{T}_j - b_1\right)^2}{\left(\sum_{j=1}^{m} w_j\right)^2} \tag{B.4}$$

where $\bar{T}_j$ is the unweighted mean of the estimates in the study j, b_1 is the estimate of the mean given in the previous equation and w_j the total weight for all estimates in the study j.

We implement this by using the *robumeta* package in Stata (Tanner-Smith & Tipton, 2014) using three assumptions on correlation (0, 0.5, and 0.99) and using the random effects weighing scheme discussed above.

We present the average effect sizes using both estimates in Table A3.

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Supplementary tables and figures

TABLE A1. Studies included in the review

Study and Country of Implementation	Intervention and Evaluation Method	Summary of Findings
Avitabile and de Hoyos (2018) (Mexico)	The program provided information on average earnings associated with attending high school versus university, associated life expectancy, and how to obtain funding for higher education to incoming 10th grade students. (RCT, N = 54 schools) *Delivery of information:* Interactive computer software in a school setting *Recipients:* Students only (upper-secondary)+ *Data disaggregation:* By gender and educational level *Source of information on returns:* National labor force survey	• The program led to 0.23 standard deviation higher test score, with effects higher for girls (0.25 standard deviation higher test scores). This effect may be explained by girls switching to more math-intensive tracks as a result of the program. • There is a positive but not significant effect on whether students went on to take the standardized 12th grade test on time, a proxy for on-time graduation. • There is also a positive but not statistically significant effect of the treatment on how realistic earnings perception are. At baseline, students underestimate earnings associated with high school completion but overestimate earnings of university graduates; these get updated to more realistic outcomes after treatment.
Bernal et al. (2022) (Colombia)	The program provided information on available financial aid and on tuition costs in one treatment (the "costs/aid treatment") and facts on returns to higher education at accredited universities and enrolment rates in accredited universities (the "returns/rates treatment") to 10th graders, all of whom had already expressed their intention to enroll in tertiary education. (RCT and discrete choice experiment, N = 331 schools) *Delivery of information:* One-time online survey sent to participating schools; paper-based surveys were used in schools without internet connection. *Recipients:* Students only (upper-secondary) *Data disaggregation:* By type of university (accredited or public) *Source of information on returns:* National labor force survey	• The program was associated with more students enrolling in tertiary education (by 4 percentage points in the returns/rates treatment, not significant, and by 5.8 percentage points in the costs/aid treatment, statistically significant). • Conditional on attendance, the program had no statistically significant impacts on enrollment in public or accredited universities. Point estimates suggest that fewer students in both treatments enrolled in public institutions (by 5 to 8 percentage points, but not significant for either treatment) and more students enrolling in accredited universities (by 5 to 7 percentage points, also not significant), which in this context, is a signal for increased demand in higher-quality education. • In a discrete choice experiment administered immediately after the information was provided (in both treatments), girls and higher-income students exhibited less preference for public universities than other participants.

TABLE A1. (Continued)

Study and Country of Implementation	Intervention and Evaluation Method	Summary of Findings
Bonilla-Mejía et al. (2019)* (Colombia)	Recent Colombian college graduates provided a 35-minute presentation on the earning premiums of college vs high school (just attending and completing), including average salary differences across fields of study and between two- and four-year degrees. The presentation included information on student loan programs as well as a demonstration of a government website that provides information on average earnings by-degree and university, available funding opportunities and financial aid to cover costs, and the importance of test scores in the admission process to 8th graders. (RCT, N = 115 schools) *Delivery of information:* In-person by "model" speakers (local college graduates) *Recipients:* Students only (lower-secondary) *Data disaggregation:* By educational level *Source of information on returns:* Household survey	• The intervention led to higher awareness of financial aid options (5 percentage points). • There was a slightly positive but not significant effect on college enrollment rates. • There was a significant effect on enrollment on top-10 colleges, which increased by 5 percentage points. • The intervention did not change students' inflated beliefs about earnings of college graduates. • There was no significant impact of the intervention on overall test scores or on test scores for language or math.
Busso et al. (2017) (Chile)*	After identifying their preferences for careers and schools, 12th grade students received personalized emails containing information on financial aid for both open-access (less selective) schools and merit-based (more selective) schools, including municipality-specific financial aid opportunities. Half of the students received additional information on average earnings and employment rates; however, that information arrived after the application deadline for more selective schools, but not for less selective schools. The information is tailored to students' preferences on schools and careers: they received data for their school-career combination and for other schools offering the same career. (RCT, N = 300 schools) *Delivery of information:* Emails containing information are sent to email addresses collected during school fairs.	• "Students tend to overestimate wages for people with their preferred degree." • There was low uptake: "at most, half of the students received and opened" the emails. • There was no impact of financial aid information on application to or enrollment in selective schools. • The combined information and returns treatment led to 8 percentage point higher probability that a student enrolls in a school suggested by the intervention. • Likewise, the combined information and returns treatment led to a 10-percentage point higher probability that the student enrolled in a school suggested by the intervention and that this school was of higher quality than the school the student preferred at baseline.

TABLE A1. (Continued)

Study and Country of Implementation	Intervention and Evaluation Method	Summary of Findings
	Recipients: Students only (upper-secondary) *Data disaggregation:* By degree-school combination *Source of information on returns:* Publicly-available database based on tax returns	• Conditional on enrollment, treated students were more likely to choose schools and careers with lower estimated wages (8.8 percentage), probably because students shift away from private universities and toward professional schools, with shorter programs but also lower expected wages.
Ding et al. (2021) (China)	The program provided the average wage after one year of graduation for each major to high school seniors. *Delivery of information:* Earnings information provided through online survey before asking for preferred degree *Recipients:* Students only (upper-secondary) *Data disaggregation:* By degree program *Source of information on returns:* National Survey of College Graduate Employment	• 39 percent of students changed their preferred majors after being shown earnings by major. • The intervention did not lead to statistically significant impact on actual enrollment to STEM or engineering majors on average. • Male students are more likely to switch applications to STEM/Engineering majors by 2.5 percentage points and to be admitted to these majors by 3 percentage points. The effects on female students are not statistically significant.
Hastings et al. (2015) (Chile)	The program provided degree-specific average monthly earnings and loan repayment costs to incoming university students applying for a government student loan. They also show the 15-year repayment period of the federal loan against earnings during that period. (RCT, N = 49,166 or 30% of all student loan applicants in Chile) *Delivery of information:* Online survey at the time of application for student loans *Recipients:* Students only (upper-secondary) *Data disaggregation:* By degree program *Source of information on returns:* National tax returns	• "Students who choose the lowest earning degree programs overestimate earnings for past graduates of those programs by more than 100%… Students choosing high-earning programs underestimate earnings for past graduates." • The intervention did not lead to any discernable impact on the decision to enroll in a degree program. • The intervention did affect which degrees students chose. • Treatment raises earnings predicted earnings by 1.4 percent, with larger effects (3.2 percent) for the poorest students. • Treatment lowers enrollment in programs in the bottom third of the returns distribution by 3.3 percent, with larger impacts for poorer students (4.6 percent) and for students with low test scores (3.9 percent).

TABLE A1. (Continued)

Study and Country of Implementation	Intervention and Evaluation Method	Summary of Findings
J-PAL (2018) (Dominican Republic)	Students between 7th and 12th grade saw telenovela-style videos about the benefits of education. Two variations were tested. One was an "informative video" with income levels and distributions for each level of education, plus non-monetary benefits of education. The second was a "persuasive video" with a qualitative description of returns. The schools also received posters—some informative and some persuasive—in some cases broken down by gender. The videos were shown in two waves in two years; hence, some students saw the video only in the first year, some only in the second year, and some saw the video in both years. (RCT, N = 599 schools in the first round and 2,469 schools in the second round) *Delivery of information:* Story-telling videos *Recipients:* Students and parents (lower and upper-secondary) *Data disaggregation:* By education level and gender *Source of information on returns:* Not provided in the paper	• Neither type of video changed dropout rates in the year they watched the videos. • Both types of videos reduced dropout rates (by 2.5 to 3 percentage points) the year after students watched the videos. • Neither video type outperformed the other. • Watching the video the year before the test had no impact (by itself) on performance on a standardized 8th grade test. • Watching the video in the same year as the test boosted test scores for both types of video by comparable amounts—0.05–0.06 standard deviations. • Watching the video both in the year of the test and the year previous also boosted test scores, by 0.08 standard deviations for the persuasive video and 0.13 standard deviations for the informative video.
Jensen (2010) (Dominican Republic)	At the end of an in-person survey, 8th grade students received a seven-sentence pre-defined script comparing the average earnings of someone who completed primary school versus secondary school versus university. (RCT, N = 2,250 students) *Delivery of information:* in-person as part of a household survey *Recipients:* Students only (lower-secondary) *Data disaggregation:* By education level (for a man aged 30 to 40 years) *Source of information on returns:* Household survey on income covering non-rural households (rural households are excluded because of the difficulty in estimating agricultural income)	• Students who received the information completed on average 0.20–0.35 more years of school over the next four years. • This is driven by the impact of the treatment on least poor households (0.33 more years). • The impact on other indicators of schooling (returned next year and finished school) and on poorest households are generally positive but not significant. • The intervention corrected biases in perceived earnings, with a large decrease in the expectation for earnings with just primary school (RD$284 decrease) and a smaller increase (RD$80) in the expectation for earnings with secondary school.

TABLE A1. (Continued)

Study and Country of Implementation	Intervention and Evaluation Method	Summary of Findings
Kudashvili and Todua (2022) (Georgia)	"Information on actual earnings and unemployment" by college major are provided to 10th and 11th grade students. (RCT, N = 2015 students) *Delivery of information:* information leaflet given in a school setting *Recipients:* Students only (upper-secondary school) *Data disaggregation:* By choice of college major *Source of information on returns:* National household survey	• Students who received "information on actual earnings and unemployment" were 10 percent more likely to change their college major. • Students shifted toward majors with lower unemployment rates than the students expected. They did not shift toward majors with higher earnings than they expected. • Students in the same schools as students who received the intervention—particularly older students, closer to graduation—also changed majors, although at apparently lower rates.
Loyalka et al. (2013) (China)	Teachers underwent a half-day training to provide a scripted 45-min lesson to their grade 7 students. Students were given "statistics on the net returns (wages minus costs) associated with different levels of schooling", "wage differences between high school and junior high school graduates in percentage terms", and "national and provincial-level statistics on the average wage levels and wage differences associated with different levels of education." (RCT, N = 131 junior high schools and more than 12,000 students) *Delivery of information:* In-person by teachers *Recipients:* Students only (lower-secondary) Data disaggregation: By level of schooling, and by province *Source of information on returns:* 2005 National Census	• On average, receiving the information did not change students' dropout rates, math scores, or their likelihood going to high school either vocational, academic, or any high school. • Girls who received the program were 3 percentage points less likely to drop out than boys who received the program (both measures are close to zero: the program reduced the likelihood of dropping out by 0.01 percentage points for girls and increased the likelihood of dropping out by 0.02 percentage points for boys). Girls were also 7 percentage points more likely to go to academic high school and 6 percentage points less likely to go to vocational high school (considered to be of less quality than academic high schools) as a result of the information intervention than boys.
Neilson et al. (2018) (Peru)	Urban schools received a DVD packet with four 15-minute episodes of a "telenovela-style video series whose plot conveyed messages about the social value of education, real earnings information for different education levels and fields, and options for financing higher education."	*Video intervention* • Initial take-up of the urban intervention was initially weak—i.e., many schools did not receive the videos, and some that received the videos did not show them. In the second year, take-up was higher in urban schools but still only about 66 percent. • Take-up was high (nearly 100 percent) in rural schools because of direct delivery.

TABLE A1. (Continued)

Study and Country of Implementation	Intervention and Evaluation Method	Summary of Findings
	A sample of students attending urban schools also received app-based information package with more details and infographics on returns. In rural schools, enumerators went directly and organized a showing of one 30-minute video.[21] Participants include primary school students (5th and 6th graders) and secondary school students (7th to 11th graders). (RCT, N = 424 urban schools, 249 rural schools) *Delivery of information:* By video and through a tablet *Recipients:* Students and parents (primary, lower and upper-secondary) *Data disaggregation:* By educational level, field of study, and location (urban versus rural) *Source of information on returns:* National household survey	• In urban areas, the video series increased the perceived returns to finishing primary school by 8 percent, the perceived returns to finishing technical education by 4 percent, and the perceived returns to finishing university by 8 percent. • Parents adjusted upwards their expected earnings of their children after the treatment, but girls' parents did so only for higher education, and the increase is only a tenth of that seen with boys' parents. Similar results are seen in the implementation in rural areas. • In urban areas, the video series reduced dropout rates by almost 20 percent (or 1.8 percentage points). Rural dropout rates also fell by 50 percent (or 7.2 percentage points). • Test scores in urban areas also rose by between 3 to 4 percent of a standard deviation, with improvements driven by girls. No test scores reported for rural areas. • In urban areas, the intervention also reduced child labor for girls by 3 percentage points. *App-based intervention* • The intervention increased both students' and parents' perceptions of the likelihood of finishing high school and college by between 4.2 to 4.6 percentage points. • As aspirations rose, so did the willingness of parents to impose study time (by 2.1 hours, driven by parents of boys in urban areas). • The intervention had no impact on parental monetary or time investment.

21 The video interventions for urban and rural areas are different enough that we treat them as separate treatment arms (i.e., video series for rural areas and video series for urban areas). All statistics are also reported separately for urban and rural areas, and so treating these intervention variations as separate treatment arms allow us to include the outcomes of this study in the main meta-analysis.

TABLE A1. (Continued)

Study and Country of Implementation	Intervention and Evaluation Method	Summary of Findings
Nguyen (2008) (Madagascar)	The study evaluated three interventions: (a) college graduates with varying backgrounds (low-income versus high-income) shared their success story; (b) simply providing statistics on returns through teachers; and (c) a combination of both to grade 4 students and their parents in rural schools. (RCT, N = 640 schools) *Delivery of information:* In-person and written medium in a school setting. Teachers provided the statistics and distributed a half-page pamphlet. The role model shared their experience in person. *Recipients:* Students and parents (primary school) *Data disaggregation:* By education level and gender *Source of information on returns:* National household survey	*Role model intervention* • The role model from a poor background improved average test scores by 0.17 standard deviations, mainly driven by impact on poor students (0.27 standard deviations). • There is no impact on test scores by the role model from rich background. *Statistics intervention* • The statistics intervention reduced the gap in average perceived earnings (by 14.9 percentage points). • Average test scores rose by 0.2 standard deviations, and for those with initial beliefs about returns that were below those presented, test scores rose more (0.37 standard deviations). • "Student attendance in statistics schools is also 3.5 percentage points higher than attendance in schools without statistics." *Combined intervention* • Combining a role model from a poor background with statistics led to 0.18 standard deviations higher test scores, much smaller than the statistics intervention alone. • The role model from a rich background (combined with providing statistics) had no impact on test scores. • Combining the role model with statistics had no impact on other measures on beliefs, attendance, and test scores.

TABLE A1. (Continued)

Study and Country of Implementation	Intervention and Evaluation Method	Summary of Findings
Rao (2016) (India)	"The 20-minute information session discussed the average and the 25th and 75th percentile of the monthly earnings distribution of men and women who have completed each higher education alternative" to 12th grade students. (RCT, N = 1,525 students in nine schools) *Delivery of information:* In-person *Recipients:* Students only (upper-secondary school) *Data disaggregation:* By educational track and gender *Source of information on returns:* National household survey	• Receiving information led students to update their estimated earnings in the right direction according to point estimates, but the effect is small and not statistically significant except for the vocational strand. "The effect-sizes imply an upward revision of earnings-beliefs of around 6% (technical track) and downward revisions ranging from around 3 to 7.3 percent (general and vocational tracks)" relative to non-attendance. • Baseline under-estimators generally revise their perceived earnings upwards (small coefficients and none are significant). Baseline over-estimators similarly revise their beliefs downwards (significant for general and vocational streams) by 13.1 and 18.8 change in log wages, respectively. • The intervention increased enrollment, but only for science majors (0.317 change in log-odds of enrollment, significant at 5 percent).

Notes: Studies marked with an asterisk (*) are studies with treatment arms that provide information other than returns to education. We summarize here all relevant outcomes from all treatment arms but only consider in the meta-analysis the outcomes directly from treatment arms that provides returns to education information. (+) In this table, we classify grades 7 to 9 as lower-secondary level (ISCED level 2, usually the level after completing primary school) and grades 10 to 12 as upper-secondary level (ISCED level 3, usually the last three grades before attending college or university, or the first three years of vocational education) as consistent with international norms and standards (NCES, 2012; UIS, 2012).

TABLE A2. Target information recipients

Students only	• Chile (Busso et al., 2017) • Chile (Hastings et al., 2015) • China (Loyalka et al., 2013, Ding et al., 2021) • Colombia (Bernal et al., 2022) • Colombia (Bonilla-Mejía et al., 2019) • Dominican Republic (Jensen, 2010) • Georgia (Kudashvili & Todua, 2022) • India (Rao, 2016) • Mexico (Avitabile & de Hoyos, 2018)
Students and parents	• Dominican Republic (J-PAL, 2018) • Madagascar (Nguyen, 2008) • Peru (Neilson et al., 2018)

TABLE A3. Average effects by outcome and subgroup using different methods of aggregation

Sample	Outcome	Num. of Studies	Mean of the Effect Sizes (Borenstein et al., 2009)			Robust Variance Estimation (Hedges et al., 2010)		
			Correlation = 0	Correlation = 0.5	Correlation = 0.99	Correlation = 0	Correlation = 0.5	Correlation = 0.99
All sample	Access to schooling	12	0.03 [0.01, 0.06]	0.02 [0, 0.03]	0.01 [0, 0.01]	0.02 [−0.00, 0.05]	0.02 [−0.00, 0.05]	0.02 [−0.00, 0.05]
	Beliefs and perception	8	0.08 [0.01, 0.14]	0.08 [0.01, 0.14]	0.07 [0, 0.14]	0.08 [0, 0.16]	0.08 [0, 0.16]	0.08 [0, 0.16]
	Learning	6	0.06 [0, 0.11]	0.05 [0, 0.11]	0.05 [0, 0.1]	0.03 [−0.01, 0.06]	0.03 [−0.01, 0.06]	0.03 [−0.01, 0.06]
	Other outcomes	2	0.03 [0, 0.07]	0.03 [−0.01, 0.06]	0.02 [0, 0.05]	0.03 [−0.2, 0.26]	0.03 [−0.2, 0.26]	0.03 [−0.2, 0.26]
Baseline over-estimators	Access to schooling	1	0.01 [−0.02, 0.05]	0.01 [−0.04, 0.07]	0.01 [−0.06, 0.08]	NA	NA	NA
	Beliefs and perception	2	0.04 [−0.02, 0.09]	0.03 [−0.03, 0.09]	0.02 [−0.03, 0.08]	0.03 [−0.34, 0.4]	0.03 [−0.34, 0.4]	0.03 [−0.34, 0.4]
	Learning	2	−0.06 [−0.17, 0.06]	−0.01 [−0.07, 0.05]	−0.01 [−0.08, 0.06]	−0.02 [−0.56, 0.51]	−0.03 [−0.66, 0.6]	−0.04 [−0.71, 0.63]
	Other outcomes	0	NA	NA	NA	NA	NA	NA
Baseline under-estimators	Access to schooling	1	0.05 [0.01, 0.08]	0.05 [−0.01, 0.1]	0.05 [−0.02, 0.12]	NA	NA	NA
	Beliefs and perception	2	0 [−0.03, 0.04]	−0.01 [−0.04, 0.02]	−0.01 [−0.05, 0.03]	0 [−0.23, 0.23]	0 [−0.23, 0.23]	0 [−0.23, 0.24]
	Learning	2	0.06 [0.02, 0.09]	0.05 [−0.02, 0.12]	0.05 [−0.04, 0.14]	0.05 [−0.03, 0.13]	0.05 [−0.03, 0.13]	0.05 [−0.04, 0.14]
	Other outcomes	0	NA	NA	NA	NA	NA	NA
Boys only	Access to schooling	4	0.01 [0, 0.03]	0.01 [0.01, 0.01]	0.01 [0.01, 0.01]	0.01 [−0.02, 0.05]	0.01 [−0.02, 0.05]	0.01 [−0.02, 0.05]
	Beliefs and perception	3	0.06 [−0.02, 0.14]	0.05 [−0.02, 0.13]	0.04 [−0.03, 0.11]	0.05 [−0.11, 0.21]	0.05 [−0.11, 0.21]	0.05 [−0.11, 0.21]
	Learning	3	0.04 [−0.01, 0.09]	0.02 [−0.01, 0.05]	0.01 [0, 0.03]	0.01 [−0.1, 0.13]	0.02 [−0.15, 0.19]	0.02 [−0.16, 0.21]
	Other outcomes	1	0.03 [0.01, 0.06]	0.03 [−0.03, 0.09]	0.03 [−0.05, 0.12]	NA	NA	NA

TABLE A3. (Continued)

Sample	Outcome	Num. of Studies	Mean of the Effect Sizes (Borenstein et al., 2009)			Robust Variance Estimation (Hedges et al., 2010)		
			Correlation = 0	Correlation = 0.5	Correlation = 0.99	Correlation = 0	Correlation = 0.5	Correlation = 0.99
Girls only	Access to schooling	4	0.01 [0, 0.03]	0.01 [0.01, 0.01]	0.01 [0.01, 0.01]	0.02 [−0.01, 0.04]	0.02 [−0.01, 0.04]	0.02 [−0.01, 0.04]
	Beliefs and perception	3	0.07 [−0.02, 0.16]	0.06 [−0.03, 0.16]	0.06 [−0.04, 0.16]	0.07 [−0.13, 0.27]	0.07 [−0.13, 0.27]	0.07 [−0.13, 0.27]
	Learning	3	0.08 [−0.07, 0.22]	0.06 [−0.07, 0.19]	0.03 [0.01, 0.05]	0.04 [−0.18, 0.25]	0.04 [−0.18, 0.25]	0.04 [−0.18, 0.26]
	Outcomes	1	−0.01 [−0.04, 0.01]	−0.01 [−0.08, 0.05]	−0.01 [−0.1, 0.08]	NA	NA	NA
Less poor households	Access to schooling	5	0.03 [−0.02, 0.8]	0.01 [−0.02, 0.04]	0.01 [−0.02, 0.05]	0.02 [−0.03, 0.07]	0.02 [−0.04 0.08]	0.02 [−0.04, 0.08]
	Beliefs and perception	3	0.24 [−0.26, 0.73]	0.24 [−0.26, 0.73]	0.24 [−0.26, 0.73]	0.23 [−0.84, 1.29]	0.23 [−0.84, 1.3]	0.23 [−0.84, 1.3]
	Learning	3	0.11 [0, 0.22]	0.08 [0.02, 0.15]	0.08 [0.01, 0.16]	0.1 [−0.19, 0.4]	0.11 [−0.18, 0.39]	0.11 [−0.17, 0.39]
	Outcomes	1	0.03 [0.01, 0.06]	0.03 [−0.02, 0.09]	0.03 [−0.04, 0.11]	NA	NA	NA
Poor households	Access to schooling	5	0.02 [0, 0.03]	0.01 [0, 0.02]	0.01 [−0.01, 0.03]	0.01 [−0.03, 0.05]	0.01 [−0.03, 0.05]	0.01 [−0.03, 0.05]
	Beliefs and perception	3	0.23 [−0.19, 0.66]	0.23 [−0.19, 0.66]	0.23 [−0.19, 0.66]	0.23 [−0.68, 1.13]	0.23 [−0.68, 1.13]	0.23 [−0.68, 1.13]
	Learning	3	0.06 [−0.02, 0.13]	0.05 [−0.03, 0.13]	0.04 [−0.04, 0.12]	0.03 [−0.26, 0.32]	0.04 [−0.23, 0.3]	0.04 [−0.21, 0.29]
	Outcomes	1	0.02 [0.01, 0.02]	0.02 [0, 0.03]	0.02 [0, 0.03]	NA	NA	NA

Note: We compute the average effect sizes across these studies taking into account multiple outcomes within a study by two methods of aggregation: Borenstein et al. (2009) and Hedges et al. (2010). For more details, see Appendix B.

FIGURE A1. Components of interventions that provides information on returns to education and intended outcomes

COMPONENTS OF AN INTERVENTION THAT PROVIDES INFORMATION ON RETURNS TO EDUCATION

CONTENT

Type of information

- Earnings
- Job opportunities
- Total costs of education
- Resources (funding opportunities, importance of test scores, existence of career counselors in schools, etc.)

Level of disaggregation of information

Returns to education by...

- Completed level of schooling (primary only, secondary only, post-secondary)
- By type of specialization (technical or vocational streams) or major (in tertiary education)
- By geographic location (local regions and international)
- By gender

DELIVERY

Medium of information dissemination

- Written (pamphlets, e-mail)
- In-person (invited speakers, career counselors)
- Popular media (telenovela, radio program)
- By phone (calls, mass text messages, e-mails)

Location

- In school
- At home
- Virtual (such as over phones or emails)

RECIPIENT

Receiver of information

- Parents only (often early childhood centers or earlier grades)
- Parents and students (primary and secondary levels)
- Students only (often late secondary and onwards)

MECHANISMS OF CHANGE

Parents and students who initially underestimate returns

- Adjust upwards their expected returns to education
- Increase aspirations based on realistic earning potential
- Increase demand for education, especially for girls

Parents and students who initially overestimate returns (for some schooling level or careers)

- Adjust downwards their expected returns on specific schooling level or career choices
- Increase aspirations grounded on actual earnings, actual costs and funding opportunities
- Increase demand for education tracks with reasonable employability and earnings

INTENDED IMPACT

Outcomes

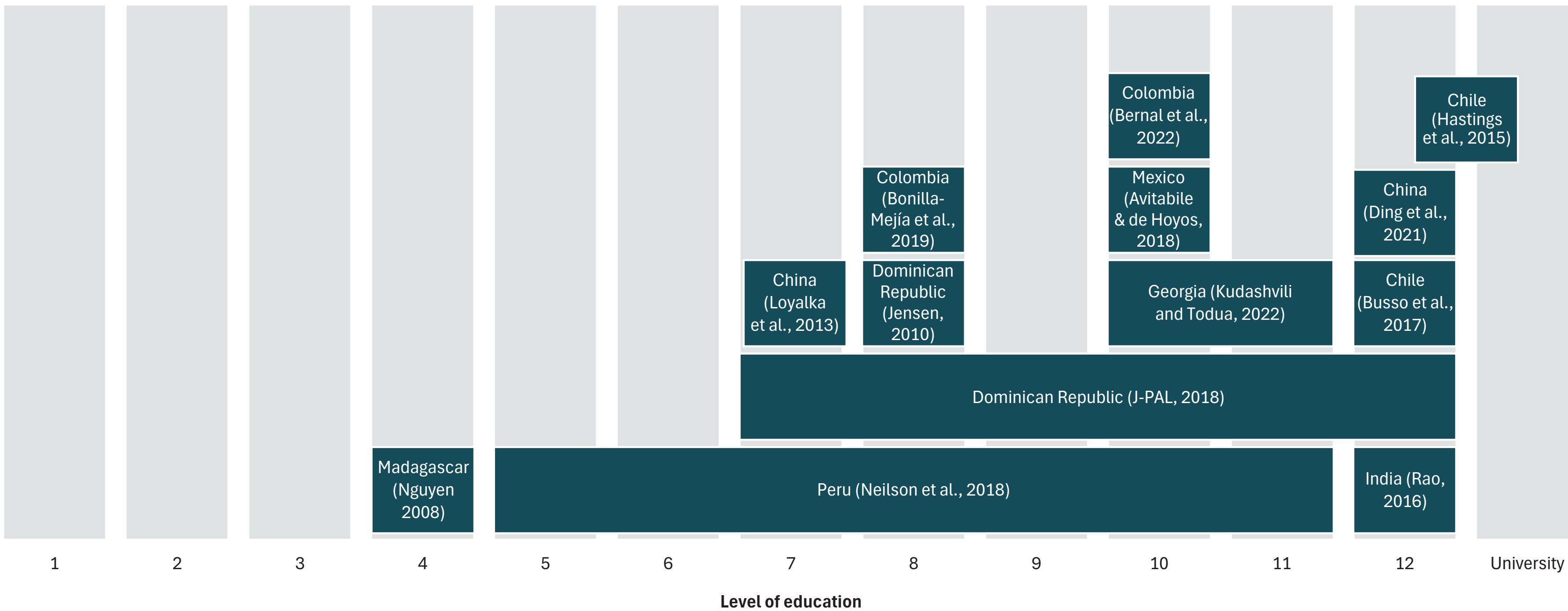
- Increase school attendance rate
- Improve learning outcomes
- Reduce dropouts and improve completion rates
- Improve match between choices of specialization or majors and labor market needs
- Improve later life outcomes (labor market outcomes and quality of life)

All of which lead to:

- Improved human capital

Source: Authors' construction.

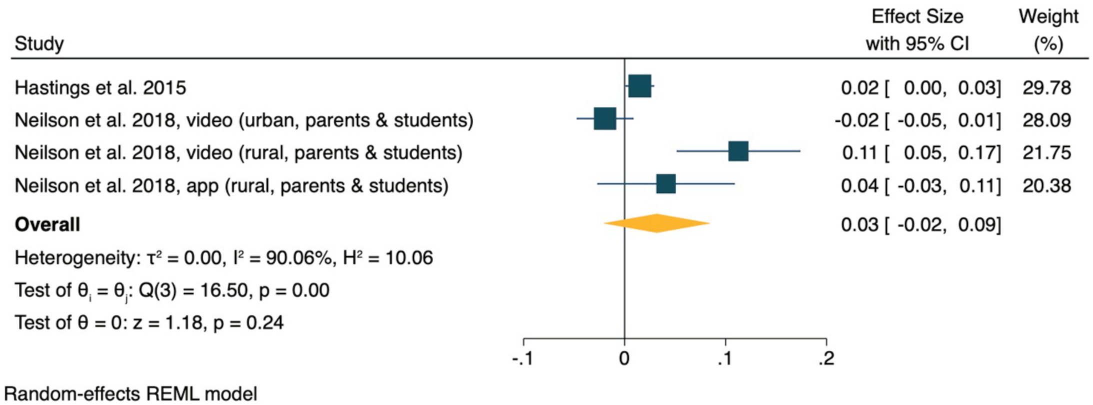
FIGURE A2. Grade levels of recipient students for informational interventions



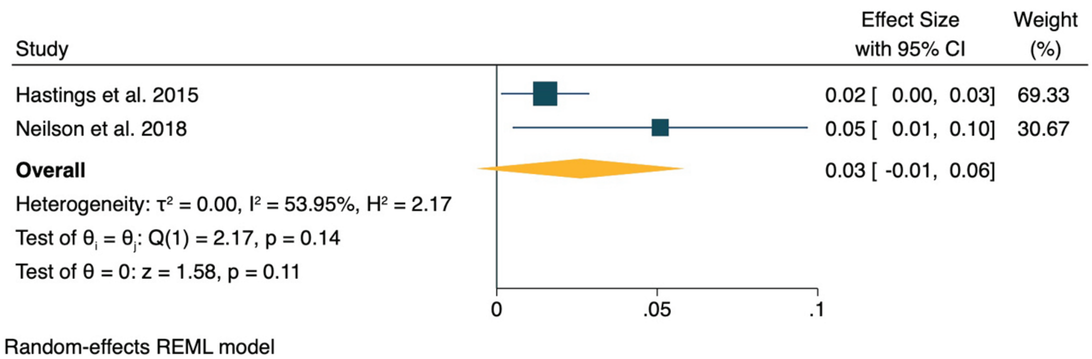
Source: Authors' construction from the eligible studies described in the methods section.

FIGURE A3. Average effect on other outcomes

Panel A: Clustered by treatment arm



Panel B: Clustered by study



Note: Outcomes shown in this figure are those that do not fit in the previous categories but are still relevant to our paper. These include predicted earning gains, monthly debt and net value of degrees chosen (from Hastings et al., 2015) and prevalence of child labor, likelihood of being employed in hazardous conditions, and total work hours (from Neilson et al., 2018).

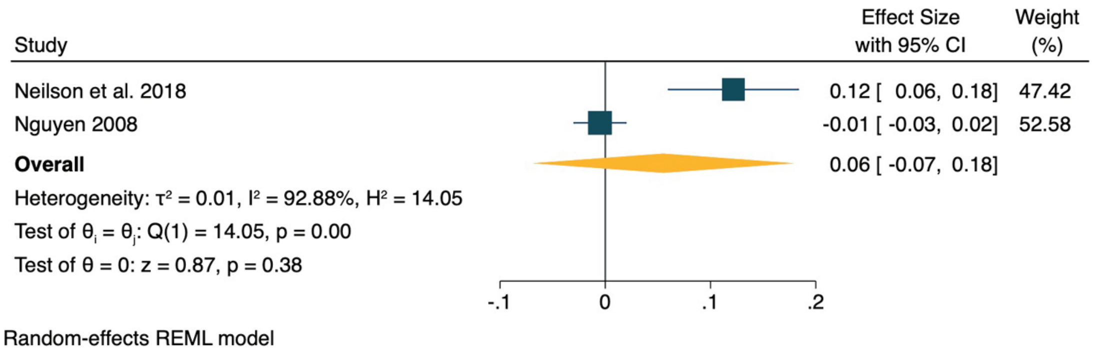
Many of the studies report multiple estimates for the same type of outcome. We cluster outcomes to avoid giving undue weight to studies that provide multiple estimates. In this forest plot, we use the method of aggregation proposed by Borenstein et al. (2009) assuming within-study correlation of dependent outcomes to be 0.5. See Appendix S.2 for more details in how we cluster the outcomes.

Note on weights: We weight the outcomes of the studies by the inverse of the variances of their effect estimates (i.e., more precise estimates and those from larger studies with smaller standard errors receive more weight) (Higgins et al., 2022). The weight attributed to each study (in the case of Panel A) and each treatment (in the case of Panel B) is represented by the size of the square marker in the location of the point estimate. These weights are the default for the Stata command for meta-analysis that we employ: commands *meta summarize* and *meta forestplot* in Stata 16.1 (StataCorp, 2019).

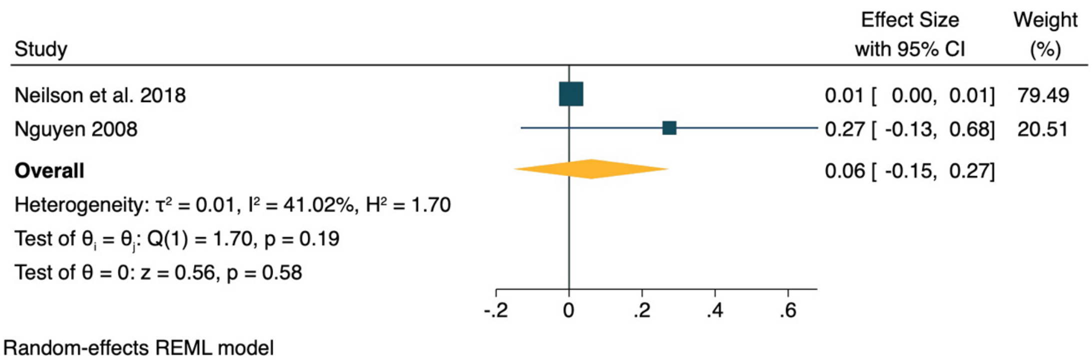
Source: Authors' construction from the eligible studies described in the methods section.

FIGURE A4. Effect for studies that include primary school students

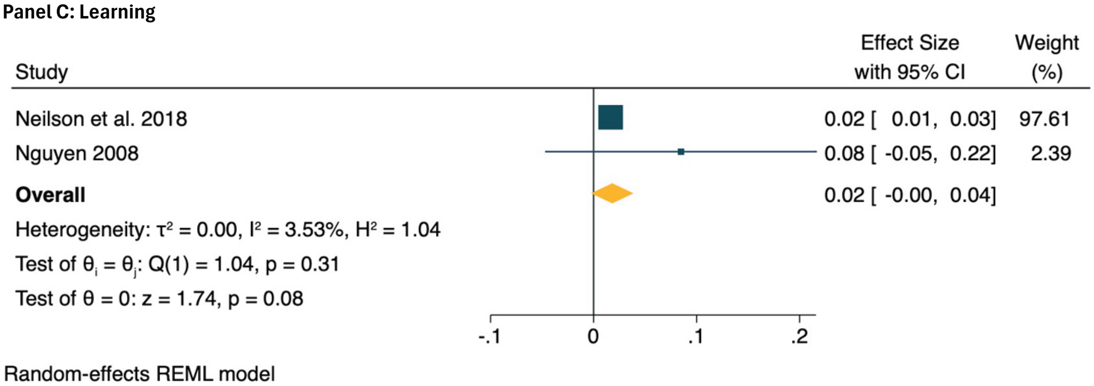
Panel A: Beliefs



Panel B: Access to schooling



Panel C: Learning

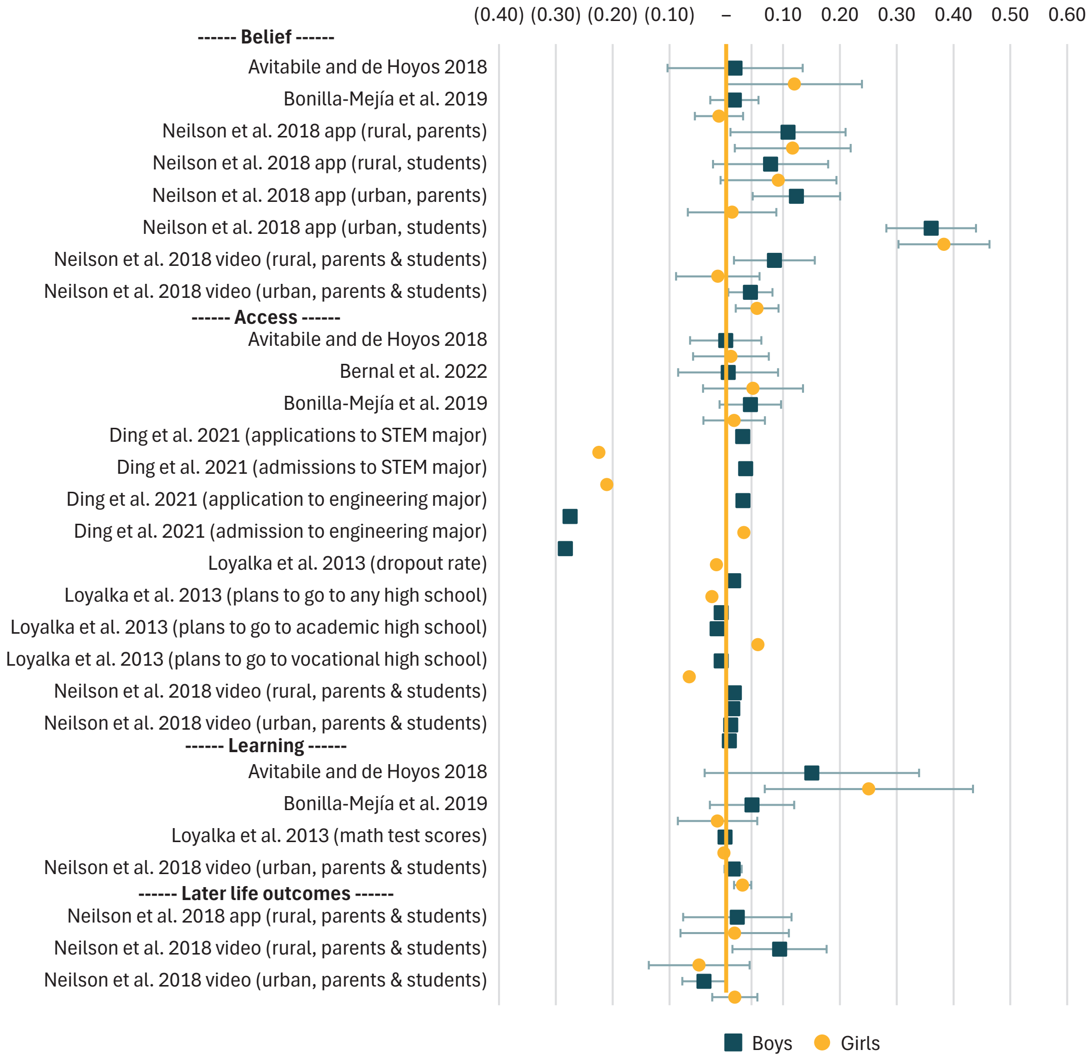


Note: Many of the studies report multiple estimates for the same type of outcome. We cluster outcomes to avoid giving undue weight to studies that provide multiple estimates. In this forest plot, we use the method of aggregation proposed by Borenstein et al. (2009) assuming within-study correlation of dependent outcomes to be 0.5. See Appendix S.2 for more details in how we cluster the outcomes.

Note on weights: We weight the outcomes of the studies by the inverse of the variances of their effect estimates (i.e., more precise estimates and those from larger studies with smaller standard errors receive more weight) (Higgins et al., 2022). The weight attributed to each study (in the case of Panel A) and each treatment (in the case of Panel B) is represented by the size of the square marker in the location of the point estimate. These weights are the default for the Stata command for meta-analysis that we employ: commands *meta summarize* and *meta forestplot* in Stata 16.1 (StataCorp, 2019).

Source: Authors' construction from the eligible studies described in the methods section.

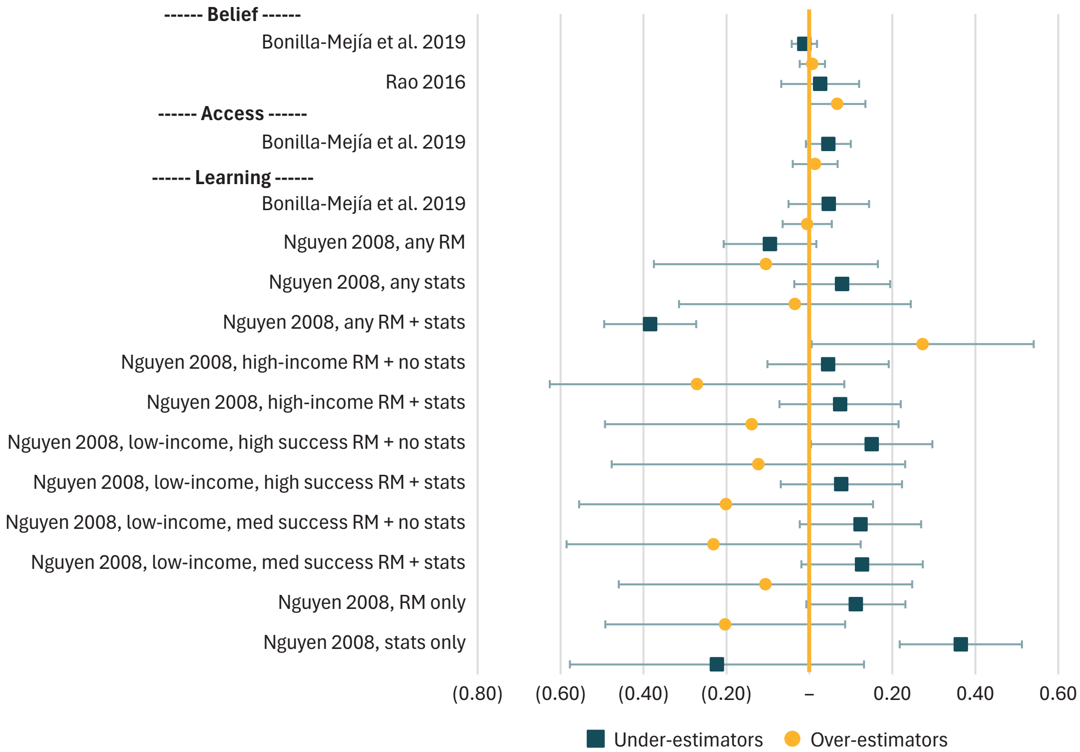
FIGURE A5. Heterogenous effect by gender



Note: Many of the studies report multiple estimates for the same type of outcome. We cluster outcomes to avoid giving undue weight to studies that provide multiple estimates. In this forest plot, we use the method of aggregation proposed by Borenstein et al. (2009) assuming within-study correlation of dependent outcomes to be 0.5. See Appendix S.2 for more details in how we cluster the outcomes. Two studies report standard errors for interaction terms (e.g., treatment * girl) but not for the overall effect on girls. In those cases, we include them in the figure (without error bars) but do not include them in the meta-analysis.

Source: Authors' construction from the eligible studies described in the methods section.

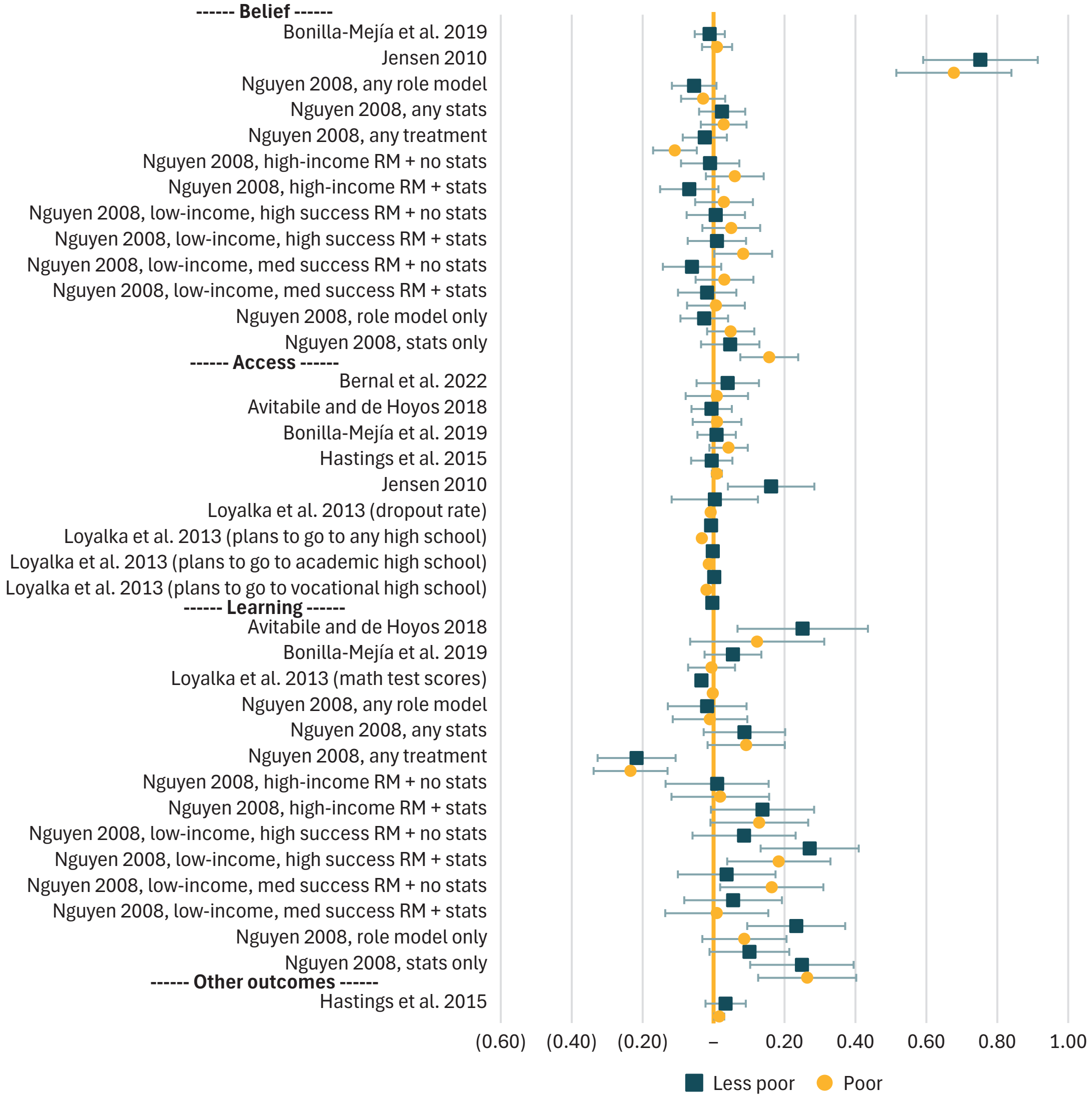
FIGURE A6. Heterogenous effect by baseline estimates



Note: Many of the studies report multiple estimates for the same type of outcome. We cluster outcomes to avoid giving undue weight to studies that provide multiple estimates. In this forest plot, we use the method of aggregation proposed by Borenstein et al. (2009) assuming within-study correlation of dependent outcomes to be 0.5. See Appendix S.2 for more details in how we cluster the outcomes.

Source: Authors' construction from the eligible studies described in the methods section.

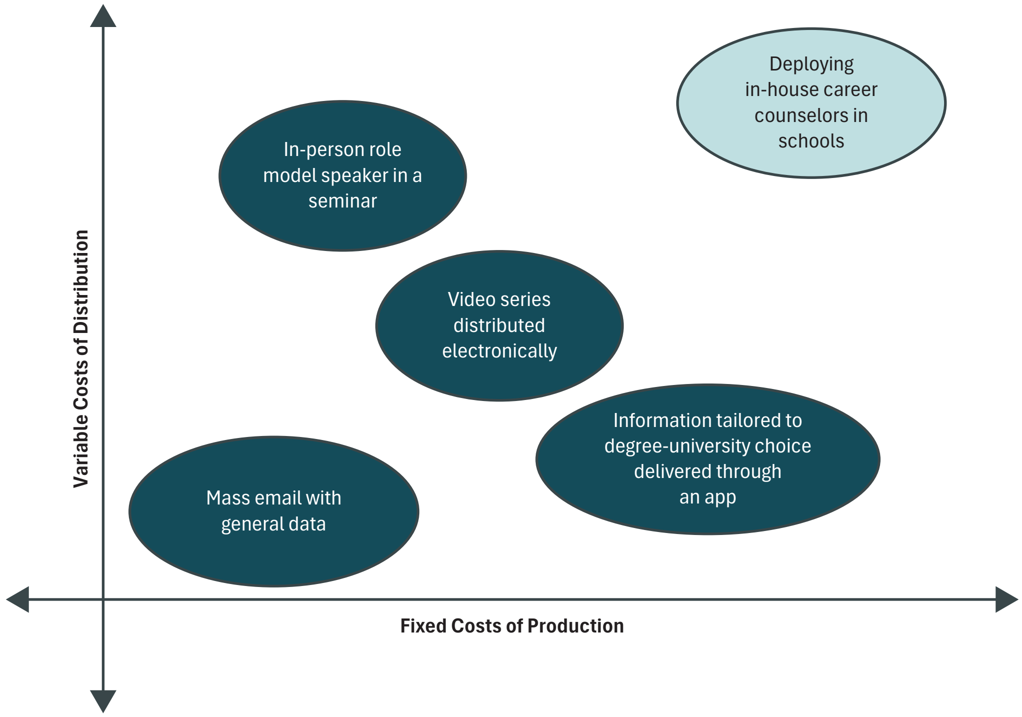
FIGURE A7. Heterogenous effect by poverty level



Note: Many of the studies report multiple estimates for the same type of outcome. We cluster outcomes to avoid giving undue weight to studies that provide multiple estimates. In this forest plot, we use the method of aggregation proposed by Borenstein et al. (2009) assuming within-study correlation of dependent outcomes to be 0.5. See Appendix S.2 for more details in how we cluster the outcomes. Some studies report standard errors for interaction terms (e.g., treatment * poor) but not for the overall effect on poor households. In those cases, we include them in the figure (without error bars) but do not include them in the meta-analysis.

Source: Authors' construction from the eligible studies described in the methods section.

FIGURE A8. Illustrative figure of the fixed costs of production and variable costs of distribution of select information interventions



Note: The circles in teal are select interventions from the studies in the sample. The circle in light blue is a hypothetical example of an intervention with high fixed cost of production (in this case: recruitment, training, and salary) and high variable cost (i.e., new counselor for each school or groups of school).

Source: Authors' construction.