

# Subsidizing Remittances for Education: A Field Experiment Among Migrants from El Salvador\*

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## Abstract

We study the intersection of two research areas: educational subsidies and migrant remittances. We implement a randomized experiment offering Salvadoran migrants subsidies for education, which are channeled directly to a beneficiary student in El Salvador chosen by the migrant. The subsidies – in the form of matching grants – lead to increases in educational expenditures, higher private school attendance, and lower labor supply of youths in El Salvador households connected to migrant study participants. We find substantial “crowd in” of household educational investments, particularly for female students: for each \$1 received by female beneficiary students, educational expenditures on that student increase by close to \$5. There is no evidence of shifting of educational expenditures from other students in the household to the target student, and the subsidy has no substantial effect on remittances sent by the migrant.

**Keywords:** education, subsidy, crowd in, migration, remittances, transnational household, El Salvador

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## 1. Introduction

Economists have widely acknowledged that a variety of market failures lead private markets to provide suboptimal levels of education. Privately-chosen levels of educational investments may be lower than the social optimum due to imperfections in credit markets, failures in intergenerational contracting, imperfect information, or the existence of positive externalities from human capital investments (Becker 1981, Loury 1981, Acemoglu and Angrist 2000, Galor and Zeira 1993, Mookherjee and Ray 2003, Banerjee 2004, among others). A common policy response is to stimulate educational demand with subsidies. Conditional cash transfer programs, in which households receive cash payments conditional on behaviors such as school attendance and primary health care utilization, are perhaps the most widespread policy approach to subsidizing educational investments in the developing world.<sup>1</sup> Subsidies for private education are another approach, of which the Colombian voucher program is an example (Angrist et al. 2002). Given the growing popularity and expense of such subsidy programs, there is interest from policy makers in ways to mitigate the costs of such subsidies through alternative forms of financing. In this paper we study an educational subsidy program in a developing country with novel features designed to tap such alternative financing.

The key new feature of the program we study is that it seeks, via an innovative payment mechanism, to supplement donor-financed educational subsidies with the resources of migrants working overseas. On a global scale, migrant remittances are one of the largest types of international financial flows to developing countries, amounting in 2012 to over US\$400 billion (World Bank 2012).<sup>2</sup> There is substantial interest among policymakers in the economic impacts of migrant remittances, and in policy options for leveraging international migrant populations from developing countries for the economic development of their origin countries.<sup>3</sup>

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<sup>1</sup> CCT programs now exist in a many countries, and have been shown to lead to increased school enrollment and reduced dropout. Studies include Schultz (2004), Behrman et al (2005), Barrera et al (2011), Baird et al (2011), and Glewwe and Kassouf (2012). See Fiszbein and Schady (2009) for a review.

<sup>2</sup> By contrast, developing country receipts of official foreign development assistance in 2012 amounted to just US\$126 billion (OECD 2013). It is also worth noting that while migrant remittance flows are large in aggregate, in practice they amount to only a minority of the total developed-country earnings of migrant workers from developing countries (Clemens et al. 2009, Clemens 2011, Yang 2011).

<sup>3</sup> Policy-oriented publications include Pew Hispanic Center (2002), Terry and Wilson (2005), and World Bank (2006, 2007). Yang (2011) reviews recent research on the economics of migrant remittances. Cox-Edwards and Ureta (2003) and Yang (2008) examine the impact of migration and remittances on educational investments in migrant-origin households.

The educational subsidy program we analyze provides Central American immigrants in the U.S. with matching funds to be used for education in their home country. The program's target population is migrants from El Salvador in the Washington, DC metro area, and households in El Salvador that are connected to these migrants. In collaboration with partner organizations (a U.S.-based money transfer operator and an educational foundation in El Salvador), we designed and offered migrants a new product, named "EduRemesa." The EduRemesa product allowed migrants to channel money, in US dollars, towards the education of a particular student in El Salvador for the 2012 school year.<sup>4</sup> Migrants chose the specific student and the exact level of support provided. Students in El Salvador who were beneficiaries of an EduRemesa received a debit card, in their name, providing access to the funds. Beneficiary students were told that the funds were for expenditures related to their own education, but this was not enforced in any way.

We conducted a randomized controlled trial to measure the impacts of the EduRemesa mechanism at various levels of subsidy. We randomly assigned Salvadoran migrants (who were recruited in metro Washington, DC) to a control group or one of a number of treatment conditions. The treatments varied in the degree to which our research project subsidized, via a matching contribution, EduRemesa funds for the beneficiary student. In the "3:1 match" treatment, each dollar contributed by the migrant was matched with \$3 in project funds. In the "1:1 match" treatment, each dollar contributed by the migrant was matched with \$1 in project funds.<sup>5</sup> In a final treatment group ("no match"), migrants were simply offered the EduRemesa product without matching funds: migrants were expected to fully fund each dollar in support of the beneficiary student.<sup>6,7</sup>

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<sup>4</sup> "Remesa" is the Spanish word for "remittance." El Salvador uses the US dollar as its national currency.

<sup>5</sup> For example, in the 3:1 match treatment, an EduRemesa providing \$300 in total to a student would cost a migrant only \$75, with the remaining \$225 funded out of project resources. In the 1:1 match treatment \$300 in total support would cost a migrant \$150, with the remaining \$150 contributed by our project.

<sup>6</sup> The no match treatment tests migrant demand for the EduRemesa mechanism itself, without the match. Migrants might value the mechanism if they sought to better control how remittance recipients in El Salvador use the funds they send, and if they perceived the EduRemesa's product features as providing a greater degree of assurance that the funds would be used for the target student's education, compared to a regular cash remittance to the household. The no match treatment is also a benchmark for comparing the impact of the match treatments, allowing an estimate of the impact of the matching funds themselves, separately from the impact of the payment mechanism or of the marketing pitch that accompanied the offer of the EduRemesa.

<sup>7</sup> Matching programs to stimulate the use of remittances for investment in home countries have been implemented by home country governments, but to date have not been evaluated using randomized methods. For example, the Mexican government's "Tres por Uno" ("Three for One") program encourages Mexican migrants abroad to invest in

Several months after the EduRemesa offers to migrants, we conducted follow-up surveys to establish impacts of our treatments. Migrants could have sent EduRemesas to many possible students in El Salvador, so it was important that at baseline we elicited from migrants, in both the control and treatment groups, the identity of a “target” student in El Salvador whom they would be highly likely to fund *if offered the EduRemesa product*. We did this by telling migrants, at the start of the baseline survey, that a prize in the form of educational funding for one student in El Salvador would be awarded by lottery. Migrant survey respondents were invited to nominate one “target” student in El Salvador to be entered into this lottery.<sup>8</sup> It appears that this elicitation method was successful: 85% of migrants who sent an EduRemesa sent one to the target student they identified in this manner. Our measurement of impacts in El Salvador relies on surveys of these target students, and of a knowledgeable adult in the target student’s household.<sup>9</sup>

Take up of the EduRemesa was monotonically related to the match level. 18.5% of migrants in the 3:1 match treatment executed at least one EduRemesa transaction, compared to 6.9% in the 1:1 match treatment and zero in the no-match treatment. 15.1% and 6.0% of migrants in the 3:1 and 1:1 match treatments respectively chose to send an EduRemesa to their target student. In the 3:1 match treatment, migrants taking up the EduRemesa used it for 1.4 students, and provided total funding (inclusive of the match) of \$719 on average, of which \$465 was for the target student.

Our most noteworthy finding is that the 3:1 match treatment leads to large increases in educational expenditures on the target student. We find substantial “crowd in” of household educational investments in response to the subsidy. Not only are the EduRemesa funds supplementing (rather than substituting for) existing expenditures on education, the funds stimulate additional educational investments on the target student. Across all target students, we find a “crowd-in ratio” (ratio of increased target student educational expenditure to EduRemesa funds received) of 3.72 (each dollar of EduRemesa funds leads to \$3.72 in additional spending).

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their communities of origin. Each dollar invested by migrants abroad is matched by \$3 from the Mexican government. Migrants have contributed an average of \$15 million annually since the program began (Hazán, 2012).

<sup>8</sup> This was done prior to treatment so that choice of target students was not influenced by treatment status.

<sup>9</sup> Migrants also sent EduRemesas to other students in other households. Our approach is unable to identify impacts outside of the target student’s household, and thus we underestimate total impacts on El Salvador households.

Crowd in is driven entirely by females: the crowd-in ratio for female target students is 4.99 vs. only 1.73 for males.<sup>10</sup>

The 3:1 match also has substantial effects on other related outcomes. It leads target students to be more likely to attend private school, which is likely related to the observed higher educational expenditure. This impact is also concentrated among female target students. In addition, the 3:1 match leads to lower labor supply of target students (an effect that, by contrast, exhibits no strong heterogeneity with respect to target student gender).

To our knowledge, this is the first research to provide evidence of crowd in of education expenditures (or any household investment) in response to a subsidy. Crowd in is of course a theoretical possibility, simply representing the case where education is a normal good while “all other goods” are collectively inferior goods. Crowd in becomes more likely (and can be large in magnitude) in the case where increasing one’s consumption of education requires a discrete increase in expenditure after a certain point. In practice, this could be the case when a subsidy induces a shift from public to private school, and where private schools require discretely higher expenditures.<sup>11</sup> Our results are consistent with this theoretical case, in that the match treatment leads to large increases in private school attendance, and that typical expenditures on private schools in El Salvador are substantially higher than on public schooling.

Budget constraints prevented us from fielding full income, consumption and expenditure modules in the follow-up survey, so we are unable to say definitively where the funds for additional crowded-in educational expenditures came from. That said, data we did collect reveals where these crowded-in funds *did not* originate. They did not come from additional remittances sent by the migrant, since we find no large or statistically significant change in target student household remittance receipts. We also find that increased expenditures on target students are not funded via reductions in expenditures on other students in the household. Several other possible sources of funds exist (on which we cannot shed light directly), including reductions in other

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<sup>10</sup> The crowd-in ratio is significantly different from 1 at conventional levels (meaning there is crowd in) for the pooled sample of all target students and for female target students, but not for male target students.

<sup>11</sup> Peltzman (1973) makes a version of the same point, showing theoretically and empirically how subsidies for higher education in the form of state universities can lead to overall *reductions* in expenditures on higher education because the subsidy is in-kind and not valid at private institutions.

household expenditure categories, borrowing, other transfer receipts, and increases in earnings on the part of others in the household.<sup>12</sup>

This paper is related to research on crowd out of public transfers, in which findings of incomplete crowd out are referred to as “flypaper effects” (see Payne’s 2009 review.) Existing research finds no crowd out of resources within households in response to transfers provided to households for particular purposes, such as Jacoby (2002), Islam and Hoddinott (2009) and Afridi (2010) in the context of child nutrition programs. Shi (2012) documents a flypaper effect in the context of a change in school fees in rural China.<sup>13</sup> Das et al (2013) find crowd-out of household educational expenditures in response to anticipated public grants to schools. The Angrist et al (2002) study of the Colombian private school voucher program comes closest to finding crowd-in in response to a subsidy (finding a crowd-in ratio of 1.26), but that paper does not provide a formal statistical test of the hypothesis that household educational expenditures rose by more than the value of the subsidy (i.e., that the crowd-in ratio is greater than 1).<sup>14</sup> In contrast to these studies, we find evidence of crowd in of household resources in response to a transfer that is large in magnitude and statistically significant.<sup>15</sup>

While existing analyses of cash transfer programs have not examined impacts on household education expenditures,<sup>16</sup> our results are reminiscent of certain findings in that literature. Baird et al. (2011) and Edmonds and Schady (2012) find (in Malawi and Ecuador, respectively) that unconditional cash transfers have very large effects on school attendance, implying substantial elasticities of school attendance with respect to income. Angelucci et al. (2009) find that the Mexican conditional cash transfer program, Progresa, increased secondary school enrollment only when eligible secondary school students had eligible primary school students in their family network. In these circumstances, it appears that the Progresa transfer to a

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<sup>12</sup> The match treatments led to reductions in target student labor supply, so any increase in earnings would have to have been on the part of other household members.

<sup>13</sup> Duflo and Udry (2004) find evidence of a related type of flypaper effect: the effect of shocks to certain crops in Cote d’Ivoire are differential with respect to the gender of the individual typically farming that crop.

<sup>14</sup> The Angrist et al (2002) crowd-in ratio of 1.26 incorporates the opportunity cost of student labor hours (which fell in response to the voucher). The corresponding figure in our study is therefore 5.38 (column 1, Panel 4, Table 8), which similarly takes into account the opportunity cost of student time. Exclusive of the opportunity cost of student time, Angrist et al (2002) estimate a crowd-in ratio of 0.70; in our study the corresponding figure is 3.72 (column 1, Panel 2, Table 8). All figures in this footnote are for results pooling male and female students.

<sup>15</sup> Carneiro et al (2012) find that a public health intervention (anti-malarial spraying) crowds in household purchases of another health good (insecticide-treated bednets) in Eritrea, but do not estimate the change in total household health expenditures.

<sup>16</sup> Some studies of the impacts of CCTs have gone beyond schooling measures to examine impacts on household consumption (Hoddinott and Skoufias, 2004; Angelucci and Attanasio, 2009; Angelucci and de Giorgi, 2009).

household with a secondary school student crowded in transfers from other eligible households (those with primary school students) in the social network to enable secondary students to attend school.<sup>17</sup>

Our research is also related to experimental research on matching funds for charitable contributions. Karlan and List (2007) find that matching offers (at the same 3:1 and 1:1 ratios we study) increase the giving response rate and the amount donated, regardless of the size of the matching offer. Eckel and Grossman (2008) find that matching increases charitable donations more than rebates of equivalent size. Karlan et al. (2011), by contrast, find only weak evidence for the effectiveness of matches, and find that under some presentations matches may even have negative effects. Meier (2007) finds that after the matching period ended, voluntary contributions decreased, concluding that matching may have negative effects in the long run. Our study differs from these studies of matching in charitable giving because migrants and EduRemesa beneficiaries are typically family members, so we study intra-family transfers rather than charitable donations to anonymous recipients.

This paper is organized as follows. Section 2 provides a theoretical discussion of the possibility of crowd-in in response to a transfer. Section 3 describes the project, and Section 4 provides an overview of the data and sample summary statistics. Section 5 presents the main empirical results. Section 6 provides a discussion and additional empirical results. Section 7 concludes.

## **2. A Simple Model**

In response to receiving additional funds from an external source to be used for education, how should household educational expenditures respond? We discuss here a simple model to guide interpretation of the empirical results to follow. The model, which we present in diagrammatic form, illustrates the cases where a transfer received by the household could lead to crowd out or crowd in. Furthermore, anticipating our empirical results, we discuss a case where crowd in could be especially large: when increasing one's consumption of education requires a discrete increase in expenditure after a certain point (which could represent the shift from public to private school).

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<sup>17</sup> The findings of Gertler et al. (2012) are broadly related as well, in that they find that a portion of Progresa transfers are put towards household investments (in this case in the form of productive assets).

Consider a unitary household, in a static context, choosing between purchases of education (for a particular student), and of all other goods. We abstract from the extensive margin (the decision to attend school at all), and consider that the purchase of education involves choosing a “quality level” of schooling  $E$ .<sup>18</sup> All other goods, denoted  $Y$ , are denominated in dollars. We are interested in the impact of receiving a transfer, in dollar amount  $s$ , on the optimal choice of  $E$ .

Figure 1 presents the case of crowd out of the transfer, the case where both education and all other goods are normal goods. Prior to the receipt of the transfer, the optimal consumption bundle is at point  $x$  at the tangency point of household indifference curve  $U$  with the budget line  $B$ . The transfer  $s$  leads the budget line to shift upwards to  $B'$ , where the new optimal consumption bundle is at point  $x'$  at the point of tangency with indifference curve  $U'$ . Consumption of all other goods and of education quality both rise. The dollar value of the increase in consumption of all other goods can be read off the vertical axis,  $\Delta Y$ . The increase in expenditure on education is therefore  $s - \Delta Y$ . The increase in educational expenditure is less than the amount of the transfer, so some of the transferred funds were “crowded out” by expenditures on all other goods.

Figure 2 illustrates the case of crowd in. All elements of the figure are identical to those in Figure 1, except for the position of indifference curve  $U'$  which implies that the post-transfer consumption bundle  $x'$  involves a reduction in expenditure on all other goods ( $\Delta Y < 0$ ). In this case, expenditure on education rises by more than the amount of the transfer ( $s - \Delta Y > s$ ). In this case, education is a normal good, while all other goods are – in aggregate – inferior goods.

Our empirical analysis will estimate the impact of a transfer on educational expenditures, and in particular will estimate the impact of each dollar transferred on educational expenditures. If each additional dollar leads to less than a dollar increase in educational expenditures, we will conclude that crowd out has occurred. If, on the other hand, each additional dollar leads to more than a one dollar increase in educational expenditures, then we will have found crowd in.

In anticipation of our empirical results, we turn to a discussion of an additional case where crowd in could be particularly large in magnitude. This is the case where it is impossible to purchase intermediate levels of educational quality, so that moving from lower to higher levels

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<sup>18</sup> The decision to abstract from the extensive margin anticipates our empirical results: the EduRemesa treatments have no impact on the extensive margin of school attendance.



of educational quality requires a household to make a discrete jump from a lower to a higher level, and to pay a fixed cost when doing so. This involves a modification to the standard budget constraint, as in Figure 3. The budget constraint is partitioned into two parts, with a void in between. At lower levels of education quality, it is only possible to purchase up to  $a$  units, and any increase after this point requires a discrete jump to  $b$  units or more and payment of a fixed cost  $F$ . In practice, this void could represent the gap in quality between public and private schools, where the assumption is that the quality of a private school is not just marginally higher than that of a public school, but significantly higher.

Figure 4 illustrates the potential impact of a transfer when intermediate educational quality levels are unavailable. Prior to the increase, the chosen consumption bundle is  $x$ , with relatively low educational quality (below  $a$ ). The transfer  $s$  shifts the partitioned budget constraint upwards in a parallel fashion, and it is possible for the consumer to desire to pay the fixed cost  $F$  to make a discrete jump to educational quality level  $b$ . The change in all other goods expenditure,  $\Delta Y$  (which is negative), is large with respect to the increase in funds, and the increase in expenditure on education,  $s - \Delta Y$ , is correspondingly large as well.

### **3. Project Description**

#### **A. Overview of education in El Salvador**

The education system in El Salvador is divided into four levels: primary (grades 1-6), lower secondary or middle school (grades 7-9), secondary (grades 10-12), and tertiary. The system is standardized across the country, but there are some variations, specifically in that students can often choose whether to complete a two- or three-year high school program. At the tertiary level there are a wide range of public and private options, including both traditional universities and technical programs.

Primary school enrollment rates are high in El Salvador, at 95 percent in 2009. However, enrollment quickly falls off at the middle and secondary levels. In 2009, enrollment rates in middle and secondary school were only 56 and 32 percent respectively (FUSADES 2011). A large government conditional cash transfer program has focused on primary school students despite the much lower enrollment rates for older students (de Brauw and Gilligan 2011). Although public schools below the tertiary level do not charge tuition or fees in El Salvador, the costs of attending secondary school are nonetheless higher than for primary school. Older

students have higher opportunity cost because of the higher value of their time, and secondary schools are often further away and require expenditures on uniforms and school supplies. These characteristics of the El Salvador educational system make it an appropriate setting within which to study a project that is targeted towards secondary and tertiary students.

Most students at the primary and secondary school level in El Salvador study in public schools. Appendix Table 1 shows figures from the 2010 *Encuesta de Hogares de Propósitos Múltiples* (EHPM), an annual, nationally representative, household survey in El Salvador. 89% of primary students and 79% of secondary students attend public schools. Although only 21% of students attend private school at the secondary level, the fact that that percentage doubles from the primary school level suggests that attending private school at the secondary level is valued. At the tertiary level, private institutions are much more important, with 60% of enrolled students attending a private institution.

At the secondary level, where Salvadoran students take a standardized national test, mean scores of private school students consistently exceed those of public school students by a large margin (FUSADES 2011). While these differences may be due to a variety of factors, such as the nature of selection into school type, these differences may be behind perceptions that private schools are of higher quality.

There are significant cost differences between attending public and private institutions. Appendix Table 2 shows average education expenditures in the follow-up survey data collected for this study (to be described below), for the control group only. At the secondary school level, average annual expenditures are roughly two-thirds higher in private than in public schools (\$2214 compared to \$1442). This difference is largely due to tuition costs as no school fees are charged for public secondary education in El Salvador, but expenditures in other categories are higher as well. This cost differential carries over to the tertiary level where private school costs are again about two-thirds higher than those for public schools (\$2834 compared to \$1868) despite the fact that both types of institutions charge fees at the tertiary level.

## **B. Project overview**

Migrants from El Salvador were recruited to participate in this project at the two locations of the Salvadoran consulate in the Washington, DC area (in Georgetown and Woodbridge, VA). Baseline field work began in early November 2011 and concluded in early February 2012, a period chosen to overlap with the vacation period between the end of the 2011

school year and the start of the 2012 school year.<sup>19</sup> While waiting for consular services, migrants were approached by project staff and asked if they wished to participate in the study. Because the product being evaluated was specifically targeted towards students at the secondary or tertiary level, migrants were required to have a relative in El Salvador who would be eligible for secondary or tertiary studies in the 2012 school year.<sup>20</sup> Migrants who agreed to participate in the study were administered a baseline survey.

A key objective of this research is to measure impacts on students and households in El Salvador. This being the case, a challenge that arises is determining which students and households in El Salvador to survey, since migrants who are offered EduRemesas could use them for students in multiple potential households. In addition, it is important to determine the identity of surveyed students and households in El Salvador in a consistent manner across treatment conditions, so as to avoid the possibility that treatment status would affect which El Salvador student and household the migrant study respondent chose to identify.

Our approach was to identify, for all migrants, the student in El Salvador whom they would prioritize to receive additional educational financing. Our presumption was that this student would be the one they would finance with an EduRemesa (if offered the EduRemesa facility, and choosing to take up). Specifically, we asked migrants to enter a student of their choosing in El Salvador (who would be eligible for secondary or tertiary schooling in the coming year) into a lottery to receive a \$500 scholarship for the 2012 school year.<sup>21</sup> This was done at the beginning of the baseline survey, before any individual learned of their treatment status, and so helps rule out differential selection of target students and households on the basis of treatment status. Throughout the paper we will refer to this student as the “target student” and to the student’s household as the “target household.” The rest of the baseline survey collected basic demographic information on the migrant, information on remittances, and information about the target student and household.

Immediately following the baseline survey, our project staff implemented the randomized treatments. Treatments were conducted immediately after the baseline survey so as to reduce attrition. All migrants, including those in the control group, were offered general information

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<sup>19</sup> Public schools in El Salvador began the school year on January 23, 2012.

<sup>20</sup> Relatives were defined as “close family members” or children, siblings, nieces and nephews, grandchildren, and cousins.

<sup>21</sup> Target students were not required to be currently enrolled in school.

about the importance of education in El Salvador, and suggestions on how to maximize the impact of their remittances on the educational outcomes of their family members. Migrants in the treatment groups were offered the EduRemesa with a subsidy level corresponding to their treatment group.<sup>22</sup>

Following the baseline interaction, follow-up surveys were conducted from July to October 2012 (the last third of the 2012 school year), in random order. A phone survey of migrant respondents collected information about remittances sent to the target household. Information about the El Salvador household was also collected via phone surveys, where we separately interviewed the target student and a knowledgeable adult in the target student's household. Target students provided information related to their education and labor supply, while knowledgeable adults provided information related to the education of other students in the household. We use the information in these follow-up surveys, combined with administrative information about the take up of the EduRemesas, to analyze treatment impacts.

### **C. Details of EduRemesa treatments**

We partnered with the Fundación Empresarial para el Desarrollo Educativo (FEPADE),<sup>23</sup> an educational NGO in El Salvador, to develop the EduRemesa. The EduRemesa was a product that would allow migrants to directly send money to high school and college students to use for their education. Migrants participating in the project were randomly assigned to be either part of a control group or one of three treatment groups that received offers for the EduRemesa at varying subsidy levels. In order to avoid spillovers between participants, a first-stage randomization was conducted at the day-by-location level that assigned migrants to either the control group or to a group that would receive an offer of the EduRemesa. In other words, on each day and at each recruitment location all migrants were either in the control group or not. One third of days were allocated to the control group and two thirds to the EduRemesa group. This randomization was stratified by week and location.

In a second randomization, all migrants who had been selected to receive an EduRemesa offer were divided into three groups: those who received no match offer, those who received a

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<sup>22</sup> Following the conclusion of the baseline interaction with the migrant, the target household in El Salvador was administered a phone survey. These mainly serve to establish a first contact with the El Salvador household, with the intention of reducing attrition in the later follow-up survey. Because some time had passed between the migrant treatment in the United States and the survey in El Salvador (the mean time between surveys was fifteen days), responses and behaviors by El Salvador respondents could have already been influenced by the treatments, so these phone El Salvador surveys cannot be considered “baseline” data.

<sup>23</sup> In English, “Business Foundation for Educational Development.”

1:1 match offer, and those who received a 3:1 match offer. This randomization was done at the individual level and was stratified within sequentially-numbered groups of six surveys. On days when the EduRemesa treatment was being offered, the match treatments offered to the migrants varied randomly at the individual level. All treatment materials were contained in a sealed envelope attached to each survey that was opened by the surveyor when the survey concluded and the treatment began. Surveyors did not know before opening the envelope which match treatment had been assigned. The randomization process is depicted in Figure 5. The following is a brief description of the information provided to the different groups.<sup>24</sup>

*Control group: Encouragement to send remittances for education*

Migrants in the control group were provided with a handout that discussed the importance of supporting education in El Salvador and suggested that sending remittances directly to students (as opposed to their parents) in monthly installments was an effective way to do this. Project staff reviewed and discussed the handout with the migrant and gave it to the migrant to take home. The purpose of providing the control group with this information was to help ensure that any effects found of the EduRemesa could be interpreted as due to the product itself, and not due to the encouragement that it provided for directing remittances towards education or to specific suggestions on how to send remittances for education (e.g., sending in monthly installments).

*Treatment group 1: EduRemesa with no match (without subsidy)*

Migrants in this treatment group were provided with the same handout given to the migrants in the control group. Following the discussion of the importance of directing education funds directly to the student in monthly installments, migrants were then introduced to the EduRemesa, a product that would make it simpler for them to do this. Migrants were given a pamphlet that they reviewed with the surveyor that contained all relevant information and contact information for US based project staff and FEPADE in El Salvador.

EduRemesas were available in the fixed amounts of \$300 or \$500 for secondary school students and \$600 or \$800 for tertiary students. As part of the project, migrants were exempted from paying the administrative fees usually charged by FEPADE, and they received a coupon

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<sup>24</sup> Copies of the materials provided to study participants can be accessed at the following website: [www.umich.edu/~deanyang/eduremesa/ambler\\_aycinena\\_yang\\_2013\\_EduRemesas\\_marketing\\_materials.pdf](http://www.umich.edu/~deanyang/eduremesa/ambler_aycinena_yang_2013_EduRemesas_marketing_materials.pdf).

with the informational pamphlet that informed them of this.<sup>25</sup> Migrants who were interested in sending an EduRemesa filled out a short application indicating the identity of the student beneficiary and then sent the desired amount directly to FEPADE through a money transfer company, Viamericas Corporation, our other collaborating organization in this study. Student beneficiaries would receive an ATM card from FEPADE and one tenth of the amount sent by the migrant would be deposited into their accounts every month during the ten months of the school year. This money was intended to be used by the student for expenses related to their education, but this was not enforced.<sup>26</sup> The purpose of offering the EduRemesa without any subsidy was to analyze the demand for and impact of a product that allowed migrants to directly channel remittance funds toward education, and additionally to provide a benchmark group that allows us to isolate the impacts of the match subsidies themselves, separately from the EduRemesa payment mechanism and marketing pitch.

*Treatment group 2: EduRemesa with a 1:1 match subsidy*

Migrants in this treatment group received the same information as migrants in treatment group 1, but the coupon they received informed them that in addition to not having to pay the administrative fees, they were being offered a one to one match on every dollar they sent as part of an EduRemesa. For example, in order to send a \$300 EduRemesa, they would have to provide only \$150 and the project would provide the remaining \$150.

*Treatment group 3: EduRemesa with a 3:1 match subsidy*

This treatment was identical in all respects to treatment group 2, with the only difference being that the match rate was three to one. For example, in order to send a \$300 EduRemesa, they would have to pay only \$75 and the project would provide the remaining \$225. A description of the amount to be sent by the migrant for each treatment group and EduRemesa amount is in Table 1.

In all three treatment groups, the interaction ended by asking the migrants whether or not they were at all interested in the EduRemesa and whether they would like to receive a follow up call from the project in a few days. Migrants who indicated that they were interested were contacted by phone several days later to further discuss their interest and answer any questions. Project staff continued to follow up with all interested participants until they indicated that they

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<sup>25</sup> FEPADE charges administrative fees of 15% of the total EduRemesa amount.

<sup>26</sup> The system used for the distribution of funds is the same system already used by FEPADE for the distribution of funds in their existing scholarship program.

were no longer interested. Migrants additionally had contact information for project staff in the United States and FEPADE in El Salvador.

Migrants who decided to take up the EduRemesa did so by visiting any Viamericas authorized remittance agent and sending the required remittance amount. Once FEPADE had received the remittance, they contacted the beneficiary student to request a copy of the student's identification card needed to issue their ATM card. Upon receipt of this documentation, the student came to FEPADE's central offices in San Salvador to complete the paperwork.<sup>27</sup> Students and their guardians were reimbursed by our project for travel expenses. Before receiving their bank card, students signed a letter acknowledging the amount of their EduRemesa and the accompanying rules. The rules required that the students turn in proof of enrollment, that students must attend school, comply with academic requirements, and inform FEPADE if they stopped attending school for any reason.<sup>28</sup>

FEPADE's standard arrangement when administering educational scholarships for other donors involves requiring students to provide official copies of report cards, which are then forwarded to the scholarship sponsor. In our partnership with FEPADE on the EduRemesa project, we implemented an additional cross-randomization to test the impact of offering this monitoring mechanism. Migrants in treatment groups 1, 2, or 3 were cross-randomized into being offered one of two versions of the EduRemesa: one in which the migrant was additionally offered the benefit of receiving a report of the student's grades after each grading period ("EduRemesa with grades"), and one in which migrants were not given this option ("EduRemesa without grades"). This cross-randomization allows us to test whether impacts of the EduRemesa are different when combined with an improved ability for migrants to monitor student grades.<sup>29</sup>

#### **4. Sample, balance tests, and attrition**

As described in the previous section, study participants are migrants from El Salvador recruited in the Washington, DC area, and the target students identified by the migrants during the baseline survey. Three main samples will be used for analysis: the full sample of migrant-student pairs with a completed migrant baseline survey (the "full" sample), the sample of

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<sup>27</sup> A few students living in the far eastern portion of the country completed their paperwork at a FEPADE center in the department of San Miguel.

<sup>28</sup> In four cases, FEPADE suspended monthly transfers to EduRemesa recipients who had stopped attending school.

<sup>29</sup> The grades/no grades cross-randomization was also randomized at the day-location level. See section 6 for further discussion of the impact of this cross-randomized treatment.

migrant-student pairs with completed El Salvador follow-up surveys (the “El Salvador follow-up” sample), and the sample of migrant-student pairs with a completed migrant follow-up survey (the “migrant follow-up” sample). There are 991 migrant-student pairs in the full sample, 728 in the El Salvador follow-up sample (73 percent completion), and 735 in the migrant follow-up sample (74 percent completion). Because the main outcome variables of interest are collected in the El Salvador follow-up survey, the main tables in the paper will display results in the El Salvador follow-up sample.<sup>30</sup> Outcomes related to educational expenditures and remittances are derived through a series of questions and imputed (in a few cases) when missing to allow for a consistent sample. The substance of the results does not change when excluding imputed observations. Further information about the variable construction for all variables and imputation procedures can be found in Appendix A.

Table 2 provides baseline summary statistics for the El Salvador follow-up sample for variables related both to the migrant and to the target student. The migrants are 50 percent female, 37 years old on average, and have been in the United States for an average of 11 years. Average annual remittances to the target household are \$2,684, suggesting that even though an existing remittance relationship was not a requirement, most migrants in our sample do remit to the target households.<sup>31</sup> The target students are 53 percent female and 18.5 years old on average. They are related to the migrant in a diverse set of ways: 26 percent are the migrant’s child, 25 percent the migrant’s sibling, 33 percent the migrant’s niece or nephew, and 10 percent are the migrant’s cousin. 92 percent of target students are in school at baseline. Because the main analyses will examine heterogeneity of treatment effects by gender of the target student, we present summary statistics by gender in Appendix Table 3A. Appendix Tables 3B and 3C provide summary statistics for the full sample and the migrant follow-up sample respectively, both for the overall samples and by target student gender. No meaningful differences are apparent across the three samples at baseline.

Because this is a randomized experiment, it is important to confirm that the randomization was successful in ensuring balance in baseline variables across treatment conditions. Table 3 examines balance across the treatment groups in the El Salvador follow-up

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<sup>30</sup> All regression results in the paper are similar when performed in a sample that was restricted to those migrant-student pairs where both follow-ups were complete, although precision suffers due to the reduced sample size.

<sup>31</sup> At baseline, 86 percent of migrants report sending nonzero remittances to the target household during the past year.



sample using the same variables reported in Table 2. Appendix Table 4A examines balance by gender of the target student and Appendix Tables 4B and 4C examine balance in the full sample and the migrant follow-up samples respectively. The first four columns of the tables report the mean of each variable in the control group and each treatment group. The tables also report the p-values on the F-tests for equality of those means. The samples are well-balanced at baseline. The number of p-values below 0.10 or 0.05 is small and not different from what would be expected given sampling variation.

Given that it was not possible to complete follow-up surveys with all members of the full sample it is also important to analyze whether or not this attrition is in any way related to treatment. Appendix Table 5 presents regression estimates on whether survey completion varies in each of the three treatment groups compared to the control group, overall and by gender of the target student. The table also reports the p-values from tests of the equality of survey completion between the different treatment groups. The dependent variable in column 1 is completion of the El Salvador follow-up, the dependent variable in column 2 is completion of the migrant follow-up, and column 3 examines completion of both surveys. Attrition is not related to treatment status in the full sample, the female target student subsample, and (for the most part) in the male target student subsample.<sup>32</sup>

## 5. Empirical results

### A. Estimation

Random treatment assignment allows us to estimate the causal impact of the different EduRemesa treatments on a variety of outcomes. The main results in this paper are estimated using the following equation:

$$outcome_{ijt} = \beta_0 + \beta_1 3:1 match_{ijt} + \beta_2 1:1 match_{ijt} + \beta_3 no match_{ijt} + \delta_{jt} + \varepsilon_{ijt} \quad (1)$$

where  $i$  indexes each migrant/target-student pair,  $j$  indexes the location of the initial interaction with the migrant, and  $t$  indexes the week of the initial interaction. The outcomes consist of take-up measures from the EduRemesa administrative data and variables from the migrant and El Salvador follow-up surveys relating to educational expenditures, educational outcomes, labor force participation, and remittances.  $\beta_1$ ,  $\beta_2$ , and  $\beta_3$  are the average difference between an

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<sup>32</sup> The one exception is that there is lower migrant follow-up survey completion for the 1:1 match treatment in the subsample with male target students. This is not a treatment cell, subsample, or survey relevant for any key results, so we do not concern ourselves with this one case where there may be treatment-related attrition.

outcome variable in the 3:1 match treatment, the 1:1 match treatment, and the no match treatment, respectively, and its value in the control group. They are the intent to treat (ITT) effects of the three EduRemesa treatments on the outcomes of interest.  $\delta_{jt}$  are stratification cell fixed effects representing the week and location of the observation's baseline survey. There are 28 week-location stratification cells in all analysis samples. Robust standard errors are clustered by unique combinations of day and location of the baseline interaction (the level of the EduRemesa randomization).

Additionally, most analyses in this paper will be considered both in the overall sample and separately by gender of the target student. Panel 1 of these tables will display results for the overall sample, panel 2 for female target students only, and panel 3 for male target students only. The tables will also display the p-values on statistical tests of equality of the treatment effects across the different treatment groups.

## **B. Take-up**

Before we consider how receipt of the EduRemesa may have affected behavior, we first examine the take up of the EduRemesa and how that take up differs by treatment group. All take-up related variables come from the EduRemesa administrative data, provided by both Viamericas Corporation and FEPADE. Table 4 reports summary statistics related to the take up of the EduRemesa. Panel 1 describes the basic characteristics of the EduRemesas sent. 52 EduRemesas were sent overall by 41 migrants. 85 percent of migrants who sent an EduRemesa (35 out of 41) sent one to the target student they named during the baseline survey. 17 non-target students received EduRemesas, most sent by migrants who sent more than one EduRemesa overall. 40 EduRemesas were sent in the 3:1 match group and 12 were sent in the 1:1 match group. No migrants in the no match treatment group chose to send an EduRemesa.

Panel 2 shows the number of EduRemesas sent by amount of the EduRemesa. Within each education level, migrants appear to take advantage of the match offer by choosing to send the larger available amount. 28 of the 34 EduRemesas sent for secondary schooling were for \$500 (compared to 6 at the \$300 level), and 13 of the 18 sent for tertiary schooling were for \$800 (compared to 5 at the \$600 level). Panel 3 displays average characteristics of EduRemesas, conditional on the migrant sending at least one EduRemesa. Migrants supported 1.2 students on average in the 1:1 match group and 1.3 students in the 3:1 match group. In the 1:1 and 3:1 groups respectively migrants sent (inclusive of the match) an average of \$690 and \$719 in total, \$540

and \$465 of which went towards target student beneficiaries. Finally, panel 4 compares the distribution of the education level of target students overall to the education level of those who received an EduRemesa. Those who received EduRemesas are broadly similar to those that did not, with the exception that fewer of the EduRemesa recipients were still in primary school at the time of the baseline interview (17 percent in the overall sample compared to 8.6 percent among EduRemesa recipients).<sup>33</sup>

Table 5 estimates the impact of the treatments on take-up using equation (1). The results shown in Table 5 are obtained using the El Salvador follow-up sample and the results of the same analyses in the full sample and the migrant follow-up sample are shown in Appendix Tables 6A and 6B. Panel 1 describes results in the overall sample and panels 2 and 3 show results among migrants whose chosen target students were female and male, respectively.

Take-up in both the control group and the no match treatment group is zero. Both the 3:1 and 1:1 match treatments encourage take-up relative to the no-match treatment group and the control group, but the larger subsidy offered by the 3:1 match is much more effective. Column 1 examines whether a particular migrant sent any EduRemesa, and column 2 the total number of EduRemesas sent by the migrant. Migrants in the 3:1 match group were 18.5 percentage points more likely to send an EduRemesa at all and those in the 1:1 match group were 6.9 percentage points more likely. The 3:1 group sent 0.25 EduRemesas on average and the 1:1 group sent 0.08. Migrant contributions to EduRemesas average \$23 and \$35 in the 1:1 and 3:1 match groups respectively (column 3). This resulted in an average of \$50 in total EduRemesa funds (migrant contribution plus subsidy) being sent in the 1:1 group and \$140 being sent in the 3:1 group (column 4).

Columns 5, 6, and 7 examine only EduRemesas sent to the target student. The 1:1 match offer increased the likelihood that an EduRemesa was sent to the target student by 6.0 percentage points relative to the control group; the corresponding figure in the 3:1 match group was 15.1 percentage points (column 5). Migrants contributed \$18 and \$22 in the 1:1 and 3:1 match groups (column 6), for average total receipts by the target student of \$37 and \$86, respectively (column 7).

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<sup>33</sup> Although the EduRemesa is for secondary and tertiary level students, some target students may have been in primary at baseline because they would have been eligible had they been in their last year of primary school, preparing to begin their first year of secondary school in 2012. However, it is also possible that there were some target students who did not truly meet the requirement.

Some differences in take up by gender are present. Although overall take up (columns 1-4) does not seem to be strongly related to target student gender, use of EduRemesas for target students specifically does vary by gender. In the 3:1 match group female target students are 18 percentage points more likely to receive an EduRemesa than target students in the control group, while male target students are only 11.5 percentage points more likely (column 5). Female target students in the 3:1 match group receive an average of \$108 in total EduRemesa funds while male target students in the same group receive only \$56 (column 7). The same trend is present in the 1:1 match group where female target students receive an average of \$60 in total EduRemesa funds, while the estimated amount received by male target students is low and not statistically significantly different from zero. Migrants do seem to be more likely to send EduRemesas to their target students when those students are female.<sup>34</sup>

### **C. Impact on educational expenditures**

We now turn to the principal question of the paper: how did the EduRemesa affect the education spending of recipients? Although the EduRemesa was specifically marketed and designed as a tool to provide education funds directly to students, because money is fungible it is not obvious that EduRemesa funds would result in an increase in education expenditures. Follow-up data collected from the target students and responsible adults in their households allow us to answer this question. Given that 85 percent of migrants who sent an EduRemesa chose to send one to their target student, it appears that our method of determining the target sample was largely successful. We now examine impacts of the EduRemesa on target students.

Table 6 reports impacts on target student education expenditures, both overall and for female and male target students separately. Column 1 examines total annualized expenditures on the target student's education and columns 2 through 9 examine expenditures by category. The main result in Table 6 is that the target students in the 3:1 match group spend an average of \$301 more on educational expenses, an increase of 22 percent over expenditures in the control group. As would be expected, given lower take-up, there is a smaller increase in the 1:1 match group, but it is not statistically significant. The overall increase in the 3:1 match group is driven by large increases in tuition (\$106), transportation (\$77), and food (\$143). The only statistically significant increase in the 1:1 match group is for tuition (\$83). Despite the fact that there was no

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<sup>34</sup> In column 7 of Table 5 (total EduRemesa funds received by target student) the p-value on the statistical test for equality of treatment effects across female and male target students is 0.134 for the 3:1 match treatment and 0.038 for the 1:1 match treatment.

take-up in the no match group, there is an increase in tuition expenditures of \$67. However, this does not translate to an increase in overall education expenditures.

These results are heterogeneous with respect to gender of the target student. The impacts of the 3:1 match treatment on female target students are large and statistically significant. The 3:1 match treatment leads to a \$509 average increase in total education expenditures, a 36 percent increase from the mean expenditures in the control group. As in the overall sample, this increase is coming from large increases in tuition, transportation and food expenditures. There are no positive, statistically significant impacts of either match treatment on education expenditures among male target students, and the main coefficients are much smaller in magnitude.<sup>35</sup> Male target students were less likely than female target students to receive an EduRemesa, but differences in take up alone cannot account for the differences in impacts on educational expenditures.

These results are shown graphically in Figure 6, which plots the cumulative distribution function of total target student expenditures separately for the control group and the three treatment groups. Panel 1 shows all target students, panel 2 female target students, and panel 3 male target students. For both the overall sample and the sample of female target students, the distribution of the 3:1 match group is clearly shifted to the right compared to the control group, the no match group, and the 1:1 match group. Target students in the 3:1 match group are spending more across the entire distribution.

In order to fully understand how the EduRemesa is affecting resources allocated towards education it is also instructive to examine total household education expenditures. If total household expenditures go up by less than target student expenditures, then the increases documented in Table 6 may be partly due to shifting of resources away from other students in the household towards the target student. We perform this analysis by summing the reports of expenditures on the target student with the reports of expenditures for others aged 22 or under in the household. The impact of the match treatment on total household educational expenditures is presented in Table 7. The set-up of the table is parallel to Table 6, but all the outcomes are for total household expenditures on education. The results mirror those for target student education expenditures. Total expenditures increase both overall and for female target students and these

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<sup>35</sup> The p-value on the statistical test for equality of the effect of the 3:1 match treatment across male and female target students is 0.086.

increases are driven by increases in tuition, transportation, and food. However, the estimates on total household expenditures are generally less precise than those for target student expenditures and not all the impacts are statistically significant. Despite this, the coefficients are similar in magnitude and somewhat greater than the coefficients for the impacts on target student expenditure alone. This indicates that the increases in target student expenditures are not accompanied by reductions in expenditures for other students in the household.

Tables 6 and 7 reveal that the 3:1 match treatment increases target student education expenditures. In the 3:1 match group, the increase in total target student expenditures is \$301 overall and \$509 among females, which should be compared to average target student EduRemesa funds received of \$85 overall and \$108 among females resulting from that treatment (Table 5, column 7). It appears that not only does education spending increase by the total amount of the EduRemesa, but that the EduRemesa may actually encourage further investment in education by the target household. In other words, receipt of the EduRemesa may actually be “crowding in” educational expenditure.

To examine this explicitly, Table 8 reports the results of instrumental variables regression estimating the impact of each dollar of EduRemesa funds on target student educational expenditures. Because the large increases in educational expenditures occur only in the 3:1 match group, we utilize only the control group and the 3:1 match group in this analysis. We instrument for total target student receipt of EduRemesa funds with the 3:1 match group treatment indicator and estimate the model by two stage least squares. As in equation 1, the instrumental variables regressions include stratification cell fixed effects and standard errors are clustered at the day-location level. Panel 1 reports the first stage regression and panel 2 the second stage. Column 1 presents the estimate for the overall El Salvador follow-up sample, and separate estimates for female and male target students are in columns 2 and 3 respectively. F-statistics for the first stage regressions indicate that the instrument is strong according to the Stock and Yogo (2005) thresholds in both the overall sample and for female target students.

The estimated coefficient in panel 2 reveals the impact of each dollar of EduRemesa funds on target student educational expenditures. As discussed in Section 2, it can be interpreted as a test of crowd out vs. crowd in: a coefficient statistically significantly smaller than 1 would reveal crowd out, while a coefficient statistically significantly larger than 1 would reveal crowd in. In the overall El Salvador follow-up sample, the coefficient is 3.72. Each dollar of the

EduRemesa leads to an increase of \$3.72 in target student education expenditures. Among female target students the coefficient is even larger: each EduRemesa dollar leads to an increase of \$4.99 in target student education expenditures. These estimates are both statistically significantly different from unity, at the 10% and 5% levels in the overall and female target student subsamples, respectively. For male target students, the coefficient is also positive, but is smaller in magnitude and is not significantly different from either zero or unity. Because all these coefficients exceed 1, we refer to these coefficients elsewhere in the paper as “crowd-in ratios.”

#### **D. Impact on other target student outcomes**

Given the finding of a large crowd-in ratio for female target students, the empirical results are suggestive of the situation (discussed in Section 2) where the presence of fixed costs for high levels of education quality can result in large crowd-in ratios (as depicted in Figure 4). We therefore now turn to the impacts of this spending on other education-related outcomes to see if other results are also consistent with the model. First, in Table 9 we examine impacts on school enrollment and type of school. Column 1 examines whether or not the target student is enrolled in school at follow-up and columns 2 through 4 whether the target student is in any private school, parochial school, or non-parochial private school respectively (the latter two are subcategories of private schools). As in the previous tables, panel 1 examines all target students in the El Salvador follow-up sample, panel 2 is restricted to female target students, and panel 3 is restricted to male target students.

The treatments do not have statistically significant effects on school enrollment overall. The coefficient on the 3:1 match in column 1 among female target students is positive and economically meaningful, but falls short of statistical significance. There is, however, a large impact on the probability that the target student is attending private school, and as in the results on expenditures, this result is concentrated among the female target students. Female target students in the 3:1 match group are 18 percentage points more likely to be in private school, and those in the 1:1 match group are 12 percentage points more likely. These are large increases relative to the control group private school attendance rate of 26 percent. These increases in private school attendance concord with the increases in expenditure on tuition and other educational expenditures discussed above. The amount needed to enroll in a private institution may be higher than what is provided by the EduRemesa (in fact the EduRemesa amounts were designed for public, not private, school), but the extra funds provided by the EduRemesa were

enough to encourage households to provide the remaining funds needed. In other words, this increase in private school attendance corresponds to the situation described in Section 2 where a fixed cost associated with an increase in educational quality can result in a large crowd in of funds in response to a transfer.

We also examine the impact of the treatments on target students' labor supply. Because the EduRemesa has no effect on overall enrollment, it is not expected that student labor supply would be lower because of decreased drop out, but the receipt of the EduRemesa funds may have reduced the need of the students to work while in school to pay for the costs related to their education. Additionally, increased attendance at private schools may have required target students to dedicate more time and effort to their studies, reducing their ability to work. On the other hand, it is possible that target students would have had to increase their labor supply, given the large crowd in of expenditures. We examine target student labor force participation in Table 10 for the overall sample (panel 1) and female and male target students separately (panels 2 and 3). We examine the impacts of the match treatments on both the extensive margin (whether a student worked) and the intensive margin (hours worked per week). We focus here on columns 1 and 2 which examine all work, but also present results for paid and unpaid work separately (columns 3 through 6).

Both the 3:1 and the 1:1 match treatments had a significant effect on target student labor supply. Target students in the 3:1 match group are 14 percentage points less likely to do any work at all and work an average of 4.4 hours less per week than students in the control group. Students in the 1:1 match group are 7.5 percentage points less likely to do any work and work 3.2 hours less per week. These are large relative effects: the 3:1 match group is a 64 percent reduction compared to the control group. Figure 7 shows the cumulative distribution functions of total hours worked by treatment for the overall sample (panel 1), female target students (panel 2), and male target students (panel 3). The distributions of both the 3:1 and 1:1 match groups are shifted to the right compared to those of the no match and control groups. This is evidence of effects on both the extensive and intensive margins. Target students in the 3:1 and 1:1 match groups are much less likely to work at all, but they are also less likely to work a large number of hours, as evidenced by the much longer tails of the no match and control group distributions.



Interestingly, and in contrast to the previous results in the paper, there are similar impacts of the 3:1 match treatment on the labor supply of both male and female target students.<sup>36</sup> These large reductions in labor supply for both male and female target students can be thought of as representing another way in which target students are “spending” their EduRemesa funds, further strengthening the evidence that the EduRemesa leads to crowd in of resources. We examine this directly in Table 8. First, in panel 3, we estimate the impact of total EduRemesa funds received by the target student on the wages earned by the target student, where the EduRemesa funds are instrumented by the 3:1 match group treatment indicator. Because wages are not reported in our survey, we perform an approximation by multiplying the gender- and age-specific mean hourly wage reported in the nationally-representative 2010 *Encuesta de Hogares de Propósitos Múltiples* by the number of annual paid hours worked by the target student. This approximation suggests that for every dollar received as an EduRemesa, female target students reduce their earnings by \$0.86 and male target students reduce their earnings by \$3.08, although the male estimate is not statistically significant.

Finally, we can combine our data on education expenditures with these earnings estimates to understand the impact of the EduRemesa on total resources devoted to target student education.<sup>37</sup> This is shown in panel 4 of Table 8 where the dependent variable is total target student education expenditures minus target student estimated earnings (in other words, the household’s contribution to the target student’s educational expenditures, net of the target student’s earnings). As in panel 3, we instrument for total EduRemesa funds with the 3:1 treatment indicator. With the addition of the foregone earnings, we find large crowd-in ratios for both females (5.8) and males (4.8), although the male estimate is not statistically significant. Because of the crude manner in which wages were estimated, strong conclusions should not be drawn from the exact magnitudes of these estimates. We view the results of panel 4 as giving a rough sense of how the estimated crowd-in ratio would change when considering the reduction in target student earnings as an additional resource contribution to the target student’s education.<sup>38</sup>

## **E. Impact on remittances**

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<sup>36</sup> For the 1:1 match treatment there are impacts only for female target students, however because take-up of the EduRemesa among male target students in the 1:1 group was so low, we would not expect to see any results of that treatment among males.

<sup>37</sup> Of course there may be other resources that we do not measure that are also being affected.

<sup>38</sup> It should be noted that these estimates are conservative in that they place no value on unpaid work.

Given that the EduRemesas were initiated and partially funded by migrant family members, an open question is whether the positive crowd-in ratios reflect (at least in part) an increase in funds remitted to the target household by the migrant. In other words, did migrants “top-up” the EduRemesa resources with additional remittances?

We therefore analyze impacts on remittances sent by the migrant. Table 11 presents these results for the overall sample and separately by target student gender. The dependent variable of interest is the remittances sent by the migrant between January 1, 2012 and the follow-up survey date to the target household (column 1), other households in El Salvador (column 2), and to all households (column 3).<sup>39</sup> Because of several large outliers in the remittance data, we also show results that trim the top one percent of values (columns 4-6) and results that utilize the inverse hyperbolic sine transformation of the remittance variable (columns 7-9).<sup>40, 41</sup>

There is no evidence in Table 11 that the 3:1 match treatment results in higher remittances either to the target household or overall. In anything, there may be a negative effect, since the estimated coefficients are negative. An oddity is that in columns 1-3 there appear to be negative effects of the 1:1 and no match treatments on remittances. However, these effects are not robust to trimming of large outliers or to the inverse hyperbolic sine transformation. Overall, the treatments do not seem to have had an important effect on remittances. Our findings of positive crowd-in ratios therefore do not appear to be explained by via additional inflows of funds from migrants.

## **6. Discussion and Additional Analyses**

In this section we provide additional discussion and analyses to clarify the interpretation of results. We also report on results of an additional cross-randomization that we have so far mentioned only in passing.

### *Ruling out marketing effects*

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<sup>39</sup> The information was reported by the migrant during the migrant follow-up survey, and therefore the analysis sample differs slightly from the analyses thus far in the paper that use information from the El Salvador follow-up survey. The remittance figures are derived through a series of questions and imputed when missing to allow for a consistent sample. The substance of the results does not change when excluding imputed observations. Further information about the variable construction and imputation procedure can be found in Appendix A.

<sup>40</sup> The inverse hyperbolic sine transformation is  $\log(y_i + (y_i^2 + 1)^{1/2})$ . It can be interpreted in the same way as a logarithmic dependent variable, but does not suffer the same problem of being undefined at zero (Burbidge et al. 1988).

<sup>41</sup> All the previously-reported results in Tables 6 and 7 relating to education expenditures are robust to trimming of the top 1% and the inverse hyperbolic sine transformation.

One might be concerned that some other aspect of the 3:1 match treatment is contributing to the observed increase in education expenditures, aside from the EduRemesa funds provided. In particular, participants received encouragement to channel remittances to education as part of the marketing of the EduRemesa, so it is possible that some of the increase in expenditures could be the result of a marketing effect.

Our experiment was designed precisely to eliminate such concerns. While migrants in the control group did not receive the offer of an EduRemesa, they did receive a flyer that suggested ways migrants could enhance remittance impacts on education that highlighted the features of the EduRemesa (specifically, the flyer suggested sending funds directly to the sponsored student and disbursing funds in monthly installments).

In addition, we can compare the results in the 3:1 match group to the no match group where the EduRemesa was also offered but without subsidy. The marketing effect should be the same in both groups, while take up was zero in the no match group, so the difference in outcomes between these groups should only be due to the EduRemesa funds received. Across all the outcomes where the 3:1 match treatment had a statistically significant effect (target student education expenditures, household education expenditures, private school attendance, and the labor supply outcomes), the 3:1 match effect is also statistically significantly different from the effect of the no match treatment. We therefore view the results as ruling out the possibility that the 3:1 match effect is partly due to the encouragement to invest in education that was part of the marketing of the EduRemesa.<sup>42</sup>

#### *Relative magnitudes of the 3:1 and 1:1 match treatment effects*

We focus most of our attention on the substantial impacts of the 3:1 match, but it is also important to consider these effects next to the effects of the 1:1 match. Take up was highest in the 3:1 match group, but it was also positive in the 1:1 group. Among female target students, for example, take up was 10% in the 1:1 group compared to 19% in the 3:1 match group (Table 5, column 5). Given this level of take up and the large effects of the 3:1 treatment, one might have expected to see positive, but smaller, effects of the 1:1 treatment on expenditures and other outcomes. We do find this for some key outcomes: for female target students, the 1:1 match

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<sup>42</sup> We also note that the marketing treatments were administered to the migrants, not the family members. If the marketing of the EduRemesa increased migrant interest in promoting education in target student households, we would expect to see increases in remittances sent to these households. But as discussed above, we find no increase in remittances sent by migrants to the target households.

raises private school attendance and reduces labor supply (point estimates are smaller in magnitude than those of the 3:1 match, but not statistically significantly so). However, we do not find statistically significant increases in female target student expenditures due to the 1:1 match (although the coefficient on total expenditures for the 1:1 match treatment in column 1 of Table 6 is positive.) Looking across outcome variables, the broad pattern of these findings is that the 1:1 match also has positive effects but that are smaller in magnitude and less often statistically significant compared to the effects of the 3:1 match.

### *Migrant screening*

Beyond the use of matching funds and the payment mechanism, an additional characteristic of the EduRemesa project that sets it apart from other subsidy programs is the fact that the beneficiaries of the subsidies are chosen by the migrant study participants. Given the large magnitude of the increases in expenditures and private school attendance and the decreases in labor supply, it seems that the migrants were quite effective in selecting the students who were most likely to use the subsidy to make large investments in their education. This result suggests that requiring contributions from family members can do more than simply alleviate the financial burden of such educational transfer programs for governments, but can also serve to target those transfer programs towards students who will benefit even with only minimal oversight. Of course policy makers considering such a program must first consider whether these marginal students are the appropriate target population in every circumstance. Additionally, because our results are limited to the specific context that we study, it is not known whether screening would be as effective other situations, for example if the product was offered to families in a developing country instead of to international migrants.

### *EduRemesa with and without monitoring of beneficiary student grades*

As mentioned in Section 3.C above, migrants offered the EduRemesa were cross-randomized into being offered one of two versions of the product: half of migrants were randomly assigned to be offered a version of the EduRemesa where they would receive official reports of their beneficiary students' grades at the end of every grading period ("EduRemesa with grades"), and the remaining migrants were offered the EduRemesa without this grade reporting ("EduRemesa without grades").

We included this cross-randomization to test whether the impact of the EduRemesa offer could be enhanced by providing the migrant improved monitoring of student performance. We

hypothesized that migrants offered the EduRemesa with grades might take up the product at higher rates. In addition, conditional on taking up, the EduRemesa with grades could have provided greater incentive for households to spend more on education.

Appendix Table 7 analyzes take up separately for the EduRemesa with grades and the EduRemesa without grades. Take up in the 3:1 match group does not vary by whether or not the migrant was offered grade reports, and this is true across all measures of take up. The similarity in treatment effects for the EduRemesa with and without grades is also evident in the analysis of target student educational expenditures (Appendix Table 8). The only evidence of differences across the EduRemesa with and without grades is in take up in the 1:1 match group, which is higher for the EduRemesa without grades. It is not obvious why the EduRemesa without grades would have led to higher take up, but we speculate that migrants may have not wanted to bear the effort cost of monitoring students in El Salvador that would be expected with the EduRemesa with grades treatment. We do not place great emphasis on this result, however, since the corresponding pattern (higher take up for the EduRemesa without grades) does not hold for the 3:1 match treatment. Overall, we conclude from this analysis that migrants do not appear to place value on monitoring the performance of students funded via the EduRemesa.

## **7. Conclusion**

We report the results of a randomized experiment testing take up and impacts of a novel educational subsidy program. The program provided a payment mechanism, called EduRemesa, through which Salvadoran migrants in the United States could channel funding for education to secondary- and tertiary-level students (of their choice) in El Salvador. We randomly assigned the offer of the EduRemesa mechanism to migrants, at (also randomly assigned) varying levels of subsidy via a matching contribution: no match, 1:1 match, and 3:1 match. Take up of EduRemesas was zero without subsidy, roughly 7% in the 1:1 match treatment, and approximately 19% in the 3:1 match treatment. The sums received by El Salvador beneficiaries were substantial: in the 3:1 match treatment, conditional on take up, about \$465 was transferred on average to beneficiary students in El Salvador (inclusive of the matching funds).

The 3:1 match treatment led to large increases in educational expenditures on beneficiary students, over and above amounts transferred via the EduRemesa mechanism. These effects are concentrated among female beneficiary students. Each EduRemesa dollar received by females

led to \$5 in additional spending on education for the beneficiary student; in other words, each EduRemesa dollar “crowded in” an additional \$4 in female student educational expenditure by the recipient household. The 3:1 match treatment also led female beneficiary students to have substantially higher private school attendance (which is likely closely related to the large increase in expenditures) and lower labor supply. For male beneficiary students, corresponding effects of the 3:1 match are smaller and not statistically significant, with the exception of a reduction in labor supply that is similar in magnitude to that found for females.

These results can help guide policy related to increasing the development impact of migrant remittances. They indicate that donor- or government-funded programs aiming to subsidize education in developing countries can extend the resources available to them via contributions from two additional sources: 1) international migrants, who respond positively to matching grant programs for home-country education, and 2) beneficiary households themselves, who respond to subsidies by contributing additional resources toward student education. Our estimates indicate that each \$1 of donor funds provided for secondary or tertiary education can generate additional contributions amounting to \$0.33 from international migrants and \$3.62 from beneficiary households themselves.<sup>43</sup>

Our finding of zero take up in the no-match treatment may reveal that migrants have no (unsubsidized) demand for control over remittance recipient expenditures on education. This interpretation contrasts with Ashraf et al.’s (2012) evidence of migrant demand for control over savings in remittance-recipient households, but is consistent with Torero and Viceisza’s (2011) findings that migrants do not seek control over grocery expenditures of remittance-recipient households. Another explanation for zero take up in the “no match” treatment is that migrants have a demand for control over educational expenditures in El Salvador, but they *ex ante* believed that the EduRemesa did not assure that funds would be used for education (even though we find that this was not the case *ex post*). The fact that the monitoring of target student grades had no positive effect on take-up or target student educational expenditures may be interpreted as supportive of the first explanation. Migrants may not desire greater control over all

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<sup>43</sup> These figures are implied on the crowd-in ratio of 3.72 that pools female and male target students (column 1, Panel 2, Table 8): of the increase in expenditures of \$3.72, target student households fund \$2.72, while the EduRemesa funds \$1 (of which \$0.75 is donor-funded and \$0.25 is migrant-funded).

dimensions of economic decision making at home, so perhaps migrant lack of control is not significant in this domain.<sup>44</sup>

As in all empirical work, it is important to replicate this study in other populations and contexts to gauge the generalizability of these results. In particular, it would be worth examining whether similar crowd in would be found outside the context of transnational households (households with an international migrant member). Also, since in our experiment the transition from public to private schooling appears central to mediating the effects found, future work should examine whether similar crowd in would occur in contexts where private schooling options are not as widely available.

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<sup>44</sup> It might also be the case that the grades monitoring option was not perceived by the migrant as being enough to give them control over education expenditures, despite their latent demand for control in this domain.

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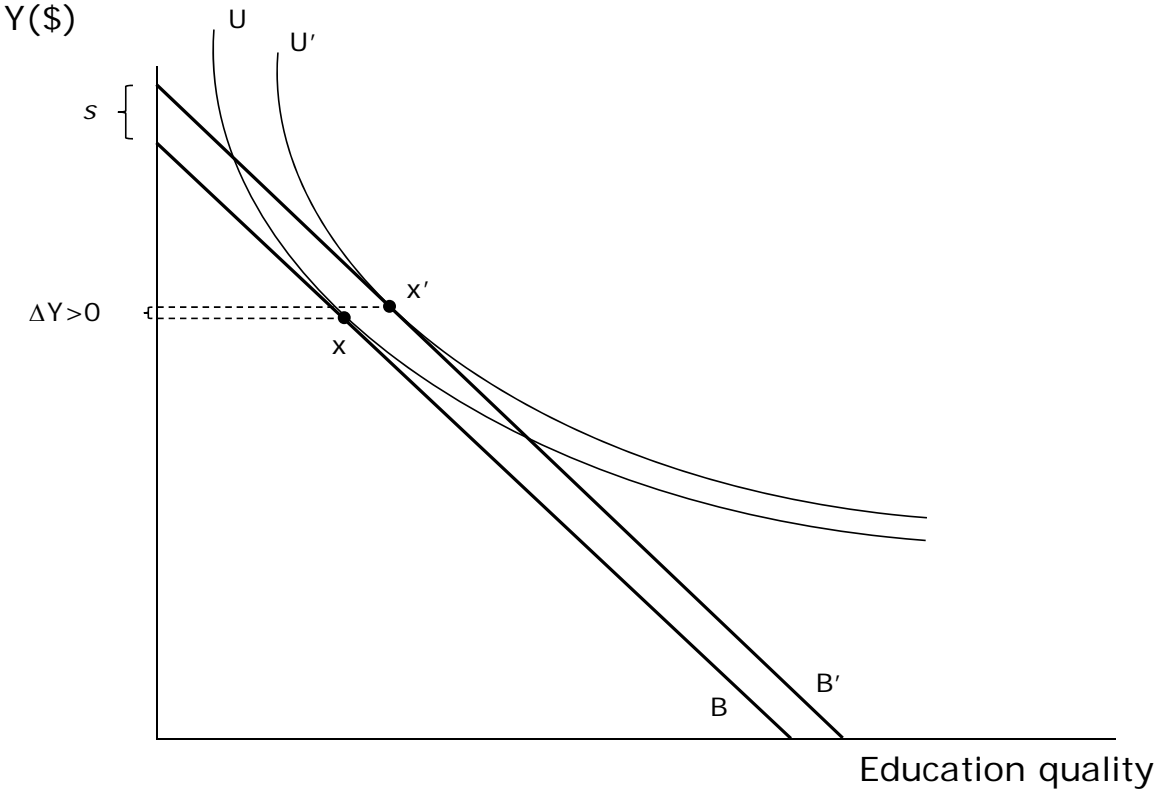
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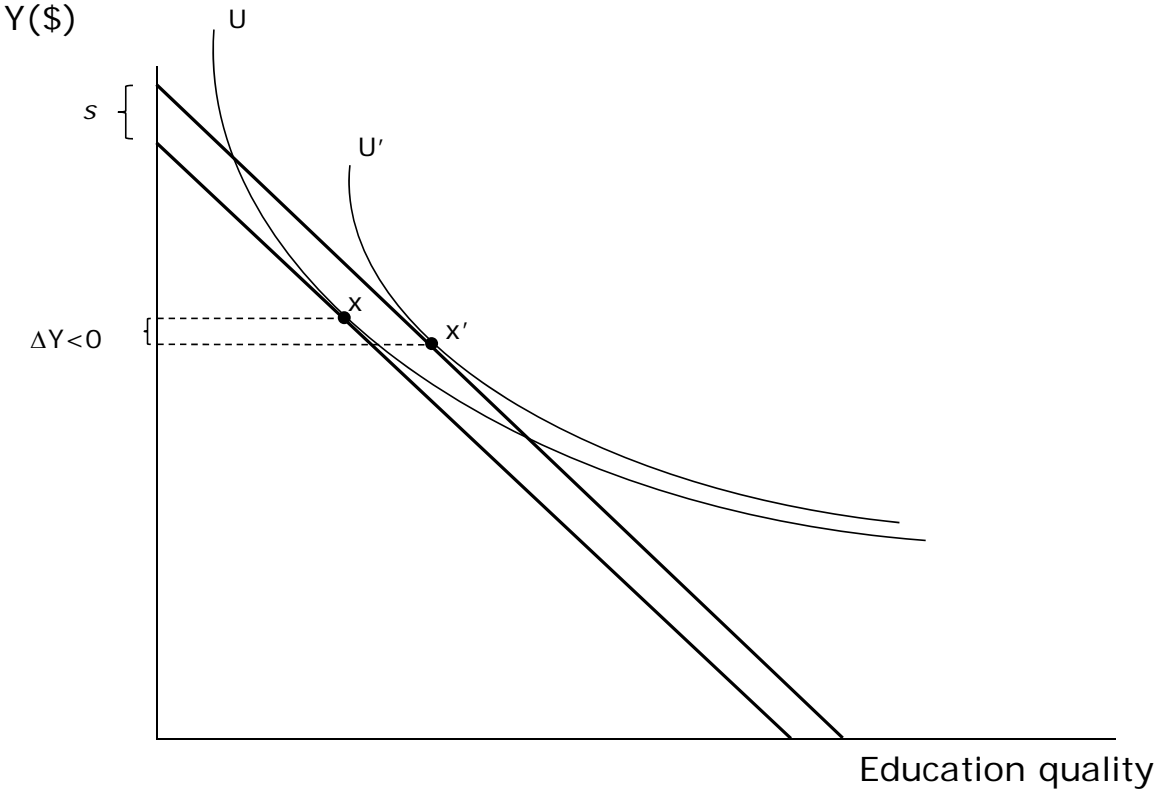
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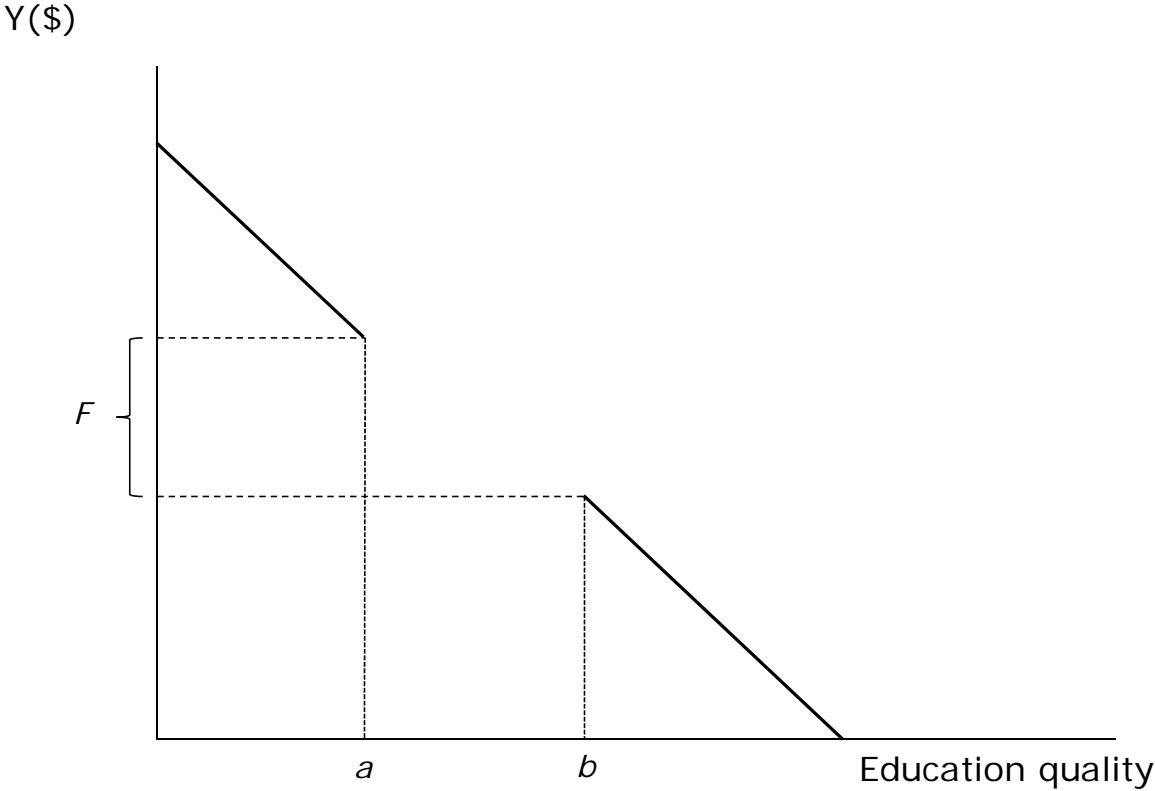
**Figure 1: Standard budget constraint, crowd-out**



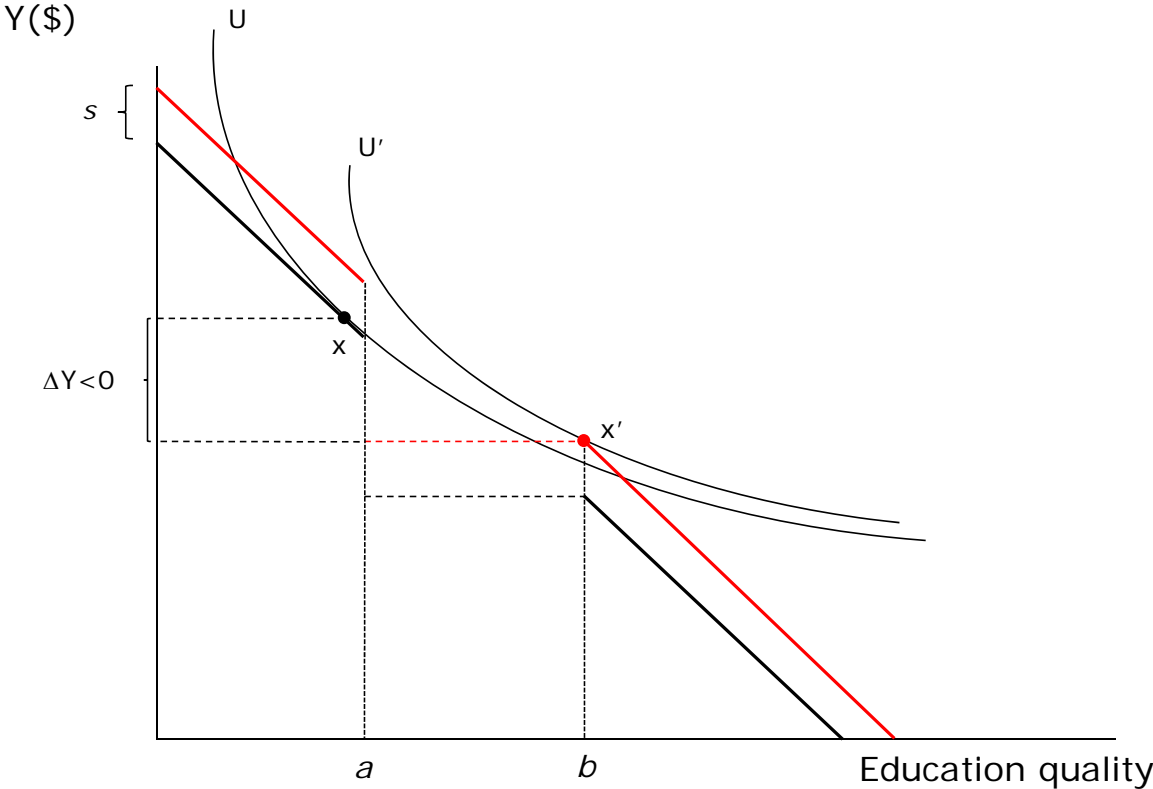
**Figure 2: Standard budget constraint, crowd-in**



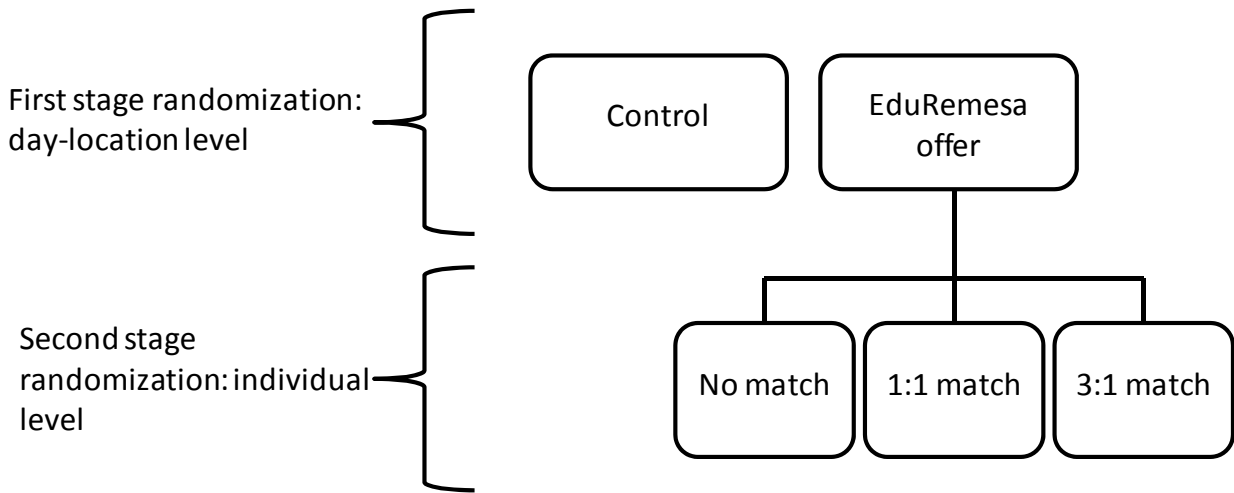
**Figure 3: Budget constraint when intermediate quality levels are unavailable**



**Figure 4: Impact of increased funds when intermediate quality levels are unavailable**

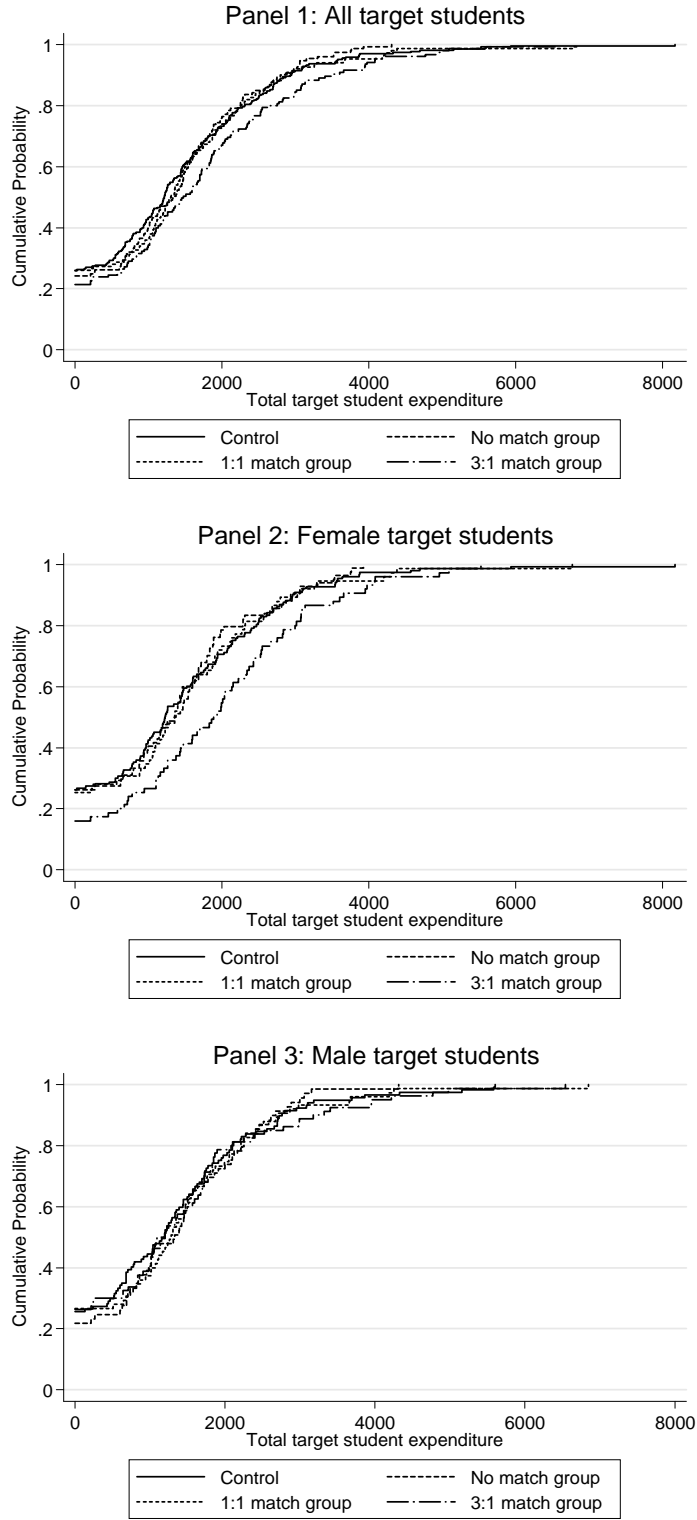


**Figure 5: Treatment groups**

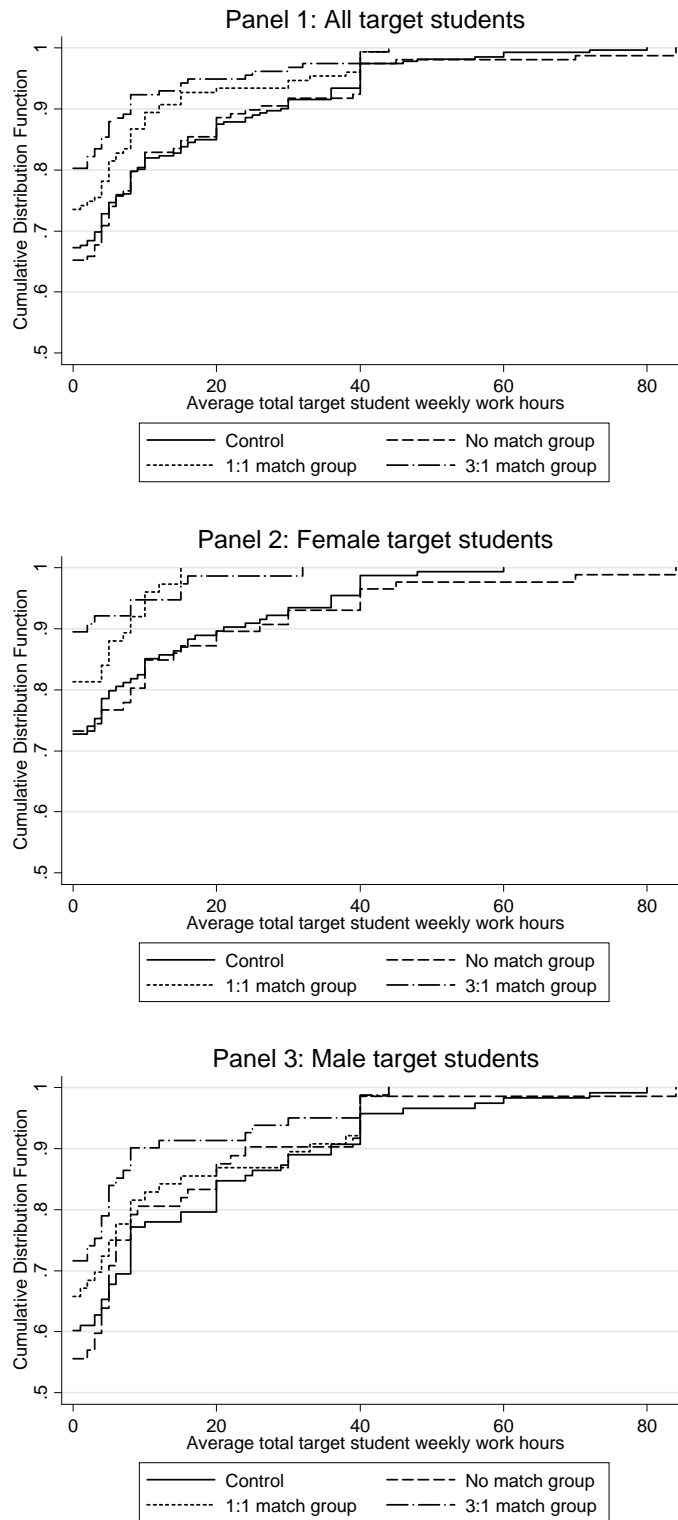




**Figure 6: Cumulative distribution functions of total target student education expenditure**



**Figure 7: Cumulative distribution functions of total target student hours worked**



**Table 1: EduRemesa amounts and migrant contributions by treatment group**

		<i>Treatment groups</i>			
		<b>No match</b>	<b>1:1 match</b>	<b>3:1 match</b>	
<i>EduRemesa amounts</i>	<b>Secondary</b>	<b>\$300</b>	\$300	\$150	\$75
		<b>\$500</b>	\$500	\$250	\$125
	<b>Tertiary</b>	<b>\$600</b>	\$600	\$300	\$150
		<b>\$800</b>	\$800	\$400	\$200

**Table 2: Baseline summary statistics**

<b>Variable</b>	<b>Mean</b>	<b>Std. Dev.</b>	<b>Min</b>	<b>10th pct.</b>	<b>Median</b>	<b>90th pct.</b>	<b>Max</b>	<b>N</b>
Migrant is female	0.50	0.50	0	0	0	1	1	728
Migrant age	36.88	9.43	15	26	36	49	74	709
Migrant is married	0.60	0.49	0	0	1	1	1	724
Migrant hh size in US	4.48	2.09	1	2	4	7	13	728
Migrant years of education	9.12	4.66	0	1	9	14	21	717
Migrant years in US	11.22	6.37	0	5	10	21	38	726
Migrant annual remittance to target hh	2,684	3,463	0	0	1,750	7,050	31,620	713
Migrant annual remittances to other hhs	1,182	2,002	0	0	0	3,600	15,600	721
Target student is female	0.53	0.50	0	0	1	1	1	728
Target student age	18.50	3.20	11	15	18	23	38	713
<i>Target student is migrant's...</i>								
...child	0.26	0.44	0	0	0	1	1	727
...sibling	0.25	0.43	0	0	0	1	1	727
...niece/nephew	0.33	0.47	0	0	0	1	1	727
...cousin	0.10	0.31	0	0	0	1	1	727
Target student is in school	0.92	0.27	0	1	1	1	1	728
Target student years of education	11.81	2.18	8	9	12	15	24	678

Notes: Sample is all migrant-student pairs with completed El Salvador follow-up surveys. Variables all come from migrant baseline survey. Sample size varies slightly with missing values for each variable.

**Table 3: Baseline balance**

	<i>Means</i>				<i>P-values</i>				
	Control	No match	1:1 match	3:1 match	C = NM = 1:1 = 3:1	C = NM	C = 1:1	C = 3:1	<i>N</i>
Migrant is female	0.47	0.49	0.53	0.53	0.239	0.551	0.116	0.104	728
Migrant age	36.76	36.84	36.83	37.16	0.995	0.923	0.883	0.799	709
Migrant is married	0.60	0.55	0.68	0.59	0.168	0.180	0.187	0.914	724
Migrant hh size in US	4.55	4.50	4.41	4.39	0.705	0.988	0.304	0.611	728
Migrant years of education	9.14	8.78	8.74	9.80	0.207	0.450	0.534	0.217	717
Migrant years in US	10.90	11.24	11.09	11.88	0.492	0.447	0.649	0.141	726
Migrant annual remittance to target hh	2,964	2,582	2,408	2,556	0.586	0.396	0.167	0.395	713
Migrant annual remittances to other hhs	1,248	1,054	1,031	1,342	0.515	0.380	0.327	0.577	721
Target student is female	0.57	0.55	0.50	0.48	0.281	0.928	0.190	0.139	728
Target student age	18.34	18.44	18.68	18.69	0.524	0.394	0.254	0.160	713
<i>Target student is migrant's...</i>									
...child	0.27	0.22	0.27	0.26	0.515	0.158	0.812	0.608	727
...sibling	0.23	0.31	0.22	0.25	0.147	0.036	0.699	0.453	727
...niece/nephew	0.30	0.33	0.39	0.33	0.233	0.517	0.043	0.574	727
...cousin	0.12	0.12	0.08	0.09	0.427	0.841	0.236	0.465	727
Target student is in school	0.92	0.90	0.93	0.94	0.562	0.369	0.740	0.549	728
Target student years of education	11.79	11.51	12.04	11.91	0.337	0.416	0.261	0.486	678

Notes: Sample is all migrant-student pairs with completed El Salvador follow-up surveys. Variables all come from migrant baseline survey. Sample size varies slightly with missing values for each variable. P-values come from regressions of each baseline variable on the treatment variables, including stratification cell fixed effects for week and location of baseline survey, with standard errors clustered at the level of the day and location of the baseline survey.

**Table 4: Summary of EduRemesa take up**

**Panel 1: Characteristics of EduRemesas sent by treatment group**

	No match	1:1 match	3:1 match	Total
Number of migrants sending ERs	0	10	31	41
Number of target students receiving ERs	0	9	26	35
Total number of ERs	0	12	40	52
ERs sent to other students	0	3	14	17

**Panel 2: Number of EduRemesas sent by amount and treatment group**

		Treatment groups				
		No match	1:1 match	3:1 match	Total	
EduRemesa amounts	Secondary	\$300	0	1	5	6
		\$500	0	6	22	28
	Tertiary	\$600	0	2	3	5
		\$800	0	3	10	13
	Total		0	12	40	52

**Panel 3: Average characteristics of EduRemesas conditional on takeup**

	1:1 match	3:1 match	Overall
Number of EduRemesas sent	1.20	1.29	1.27
Total amount sent by migrant	\$332	\$180	\$217
Total amount sent by migrant plus subsidy	\$690	\$719	\$712
Amount sent by migrant to target student	\$270	\$116	\$154
Amount sent by migrant to target student plus subsidy	\$540	\$465	\$483

**Panel 4: EduRemesas by education level**

	Baseline measure	
	% of target students overall	% of target students that received ER
Primary	17.0	8.6
Secondary	50.6	60.0
Tertiary	32.3	31.4

Notes: Data comes from EduRemesas administrative data. Sample is all migrant-student pairs interviewed at baseline.

**Table 5: Takeup of EduRemesa by treatment**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	EduRemesa sent	Number of EduRemesas sent	Total amount sent by migrant	Total amount sent by migrant plus subsidy	EduRemesa sent to target student	Total amount sent by migrant to target student	Total amount sent by migrant to target student plus subsidy
<b>Panel 1: All target students</b>							
3:1 match	0.185*** [0.0332]	0.248*** [0.0492]	35.09*** [6.984]	139.8*** [27.47]	0.151*** [0.0291]	21.61*** [4.236]	85.51*** [16.25]
1:1 match	0.0686*** [0.0201]	0.0841*** [0.0256]	23.14*** [7.107]	49.63*** [15.29]	0.0600*** [0.0190]	18.49*** [5.934]	37.15*** [12.18]
No match	-0.000367 [0.00985]	0.00532 [0.0129]	1.184 [2.445]	4.544 [7.153]	-0.000529 [0.00931]	0.559 [1.879]	1.311 [4.991]
<i>P-values for tests of equality of coefficients</i>							
3:1 = 1:1	0.002	0.004	0.246	0.005	0.011	0.667	0.021
3:1 = No match	0.000	0.000	0.000	0.000	0.000	0.000	0.000
1:1 = No match	0.001	0.004	0.003	0.004	0.002	0.002	0.002
3:1 = 1:1 = No match	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Observations	728	728	728	728	728	728	728
R-squared	0.133	0.114	0.08	0.102	0.114	0.075	0.097
Control group mean	0	0	0	0	0	0	0
<b>Panel 2: Female target students</b>							
3:1 match	0.178*** [0.0464]	0.233*** [0.0629]	35.82*** [9.666]	141.2*** [38.26]	0.178*** [0.0464]	27.60*** [6.985]	108.4*** [27.21]
1:1 match	0.101*** [0.0346]	0.111*** [0.0372]	31.13*** [10.79]	67.51*** [23.67]	0.101*** [0.0346]	29.23*** [10.56]	59.89*** [21.93]
No match	0.00990 [0.0136]	0.0176 [0.0185]	4.590 [3.966]	13.23 [12.25]	0.00990 [0.0136]	3.150 [3.239]	7.475 [8.565]
<i>P-values for tests of equality of coefficients</i>							
3:1 = 1:1	0.186	0.082	0.730	0.073	0.186	0.896	0.165
3:1 = No match	0.001	0.001	0.001	0.001	0.001	0.001	0.001
1:1 = No match	0.004	0.004	0.005	0.005	0.004	0.006	0.006
3:1 = 1:1 = No match	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Observations	387	387	387	387	387	387	387
R-squared	0.145	0.146	0.118	0.135	0.145	0.113	0.137
Control group mean	0	0	0	0	0	0	0
<b>Panel 3: Male target students</b>							
3:1 match	0.180*** [0.0435]	0.246*** [0.0724]	30.91*** [9.659]	127.0*** [37.66]	0.115*** [0.0366]	13.57*** [4.848]	55.96*** [18.83]
1:1 match	0.0281 [0.0230]	0.0508 [0.0364]	13.59 [8.474]	29.36 [20.01]	0.00842 [0.0184]	5.466 [4.737]	7.756 [10.88]
No match	-0.0123 [0.0162]	-0.0106 [0.0225]	-2.886 [4.292]	-6.829 [13.15]	-0.0129 [0.0143]	-2.389 [2.518]	-7.167 [7.916]
<i>P-values for tests of equality of coefficients</i>							
3:1 = 1:1	0.003	0.023	0.208	0.031	0.023	0.273	0.047
3:1 = No match	0.000	0.001	0.001	0.001	0.003	0.004	0.005
1:1 = No match	0.107	0.131	0.101	0.103	0.305	0.150	0.214
3:1 = 1:1 = No match	0.000	0.001	0.001	0.001	0.007	0.008	0.011
Observations	341	341	341	341	341	341	341
R-squared	0.178	0.135	0.109	0.131	0.161	0.118	0.146
Control group mean	0	0	0	0	0	0	0

Notes: Robust standard errors clustered at the level of the day and location of the baseline survey in brackets. Sample is all migrant-student pairs with completed El Salvador follow-up surveys. All regressions include stratification cell fixed effects for the week and location of the baseline survey. Dependent variables are from EduRemesa administrative data.

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 6: Target student education expenditures**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	<i>Dependent variable: Annualized target student expenditure on</i>								
	Total	Tuition	School supplies	Uniforms	Books	Transport	Food	Computer use	Other
<b>Panel 1: All target students</b>									
3:1 match	301.5**	105.8***	-3.343	6.962	7.323	76.67**	143.5**	0.0542	-35.49
	[125.5]	[32.52]	[7.791]	[6.069]	[7.797]	[37.81]	[57.33]	[26.29]	[28.62]
1:1 match	74.97	83.38**	-11.28	-8.662*	5.047	35.85	48.37	-29.75	-47.98
	[117.0]	[32.89]	[7.079]	[4.784]	[7.913]	[41.41]	[51.78]	[25.04]	[34.29]
No match	19.32	66.58*	-1.105	-7.527	-11.26*	1.060	35.94	-20.00	-44.37
	[111.5]	[34.93]	[7.508]	[4.815]	[5.802]	[31.04]	[47.20]	[25.29]	[28.77]
<i>P-values for tests of equality of coefficients</i>									
3:1 = 1:1	0.102	0.603	0.338	0.007	0.830	0.391	0.123	0.302	0.605
3:1 = No match	0.060	0.405	0.818	0.010	0.029	0.075	0.102	0.502	0.613
1:1 = No match	0.675	0.691	0.270	0.811	0.053	0.406	0.840	0.765	0.869
3:1 = 1:1 = No match	0.136	0.705	0.459	0.014	0.029	0.200	0.191	0.560	0.838
Observations	728	728	728	728	728	728	728	728	728
R-squared	0.033	0.052	0.032	0.052	0.033	0.042	0.045	0.037	0.051
Control group mean	1358	186.8	60.16	35.94	54.68	270.4	442.9	217.5	89.63
<b>Panel 2: Female target students</b>									
3:1 match	509.4***	202.2***	6.169	12.58	7.583	131.5**	216.0**	-6.485	-60.11
	[183.8]	[56.38]	[11.04]	[9.253]	[11.41]	[50.28]	[90.58]	[40.70]	[39.75]
1:1 match	45.60	98.91*	-0.808	-14.02**	4.872	61.80	41.95	-57.34	-89.76
	[185.7]	[51.68]	[12.96]	[5.491]	[12.42]	[63.79]	[84.40]	[39.72]	[57.80]
No match	-55.40	66.59	-2.196	-7.224	-12.20	-0.458	41.30	-52.00	-89.23*
	[169.1]	[50.75]	[9.509]	[5.854]	[8.562]	[40.37]	[68.63]	[43.64]	[50.01]
<i>P-values for tests of equality of coefficients</i>									
3:1 = 1:1	0.028	0.132	0.656	0.004	0.852	0.324	0.088	0.280	0.408
3:1 = No match	0.006	0.044	0.478	0.020	0.091	0.027	0.061	0.378	0.332
1:1 = No match	0.596	0.612	0.925	0.234	0.165	0.346	0.995	0.917	0.985
3:1 = 1:1 = No match	0.017	0.118	0.770	0.016	0.148	0.086	0.122	0.508	0.598
Observations	387	387	387	387	387	387	387	387	387
R-squared	0.103	0.109	0.047	0.094	0.069	0.101	0.09	0.083	0.109
Control group mean	1412	173.6	56.35	34.59	57.22	279.4	454.7	245.6	110.9
<b>Panel 3: Male target students</b>									
3:1 match	43.57	-4.661	-16.83	-1.432	5.697	19.85	53.46	-1.683	-10.83
	[186.7]	[51.66]	[11.26]	[7.368]	[11.89]	[52.56]	[70.17]	[35.84]	[32.94]
1:1 match	64.92	51.20	-29.41***	-7.723	6.234	12.80	40.63	-2.068	-6.742
	[195.1]	[55.47]	[9.984]	[7.979]	[12.08]	[54.94]	[69.66]	[34.18]	[60.15]
No match	-27.38	53.82	-6.742	-9.841	-11.52	-14.75	-12.80	-12.64	-12.91
	[189.5]	[58.89]	[12.21]	[6.830]	[9.414]	[49.21]	[71.23]	[32.92]	[36.09]
<i>P-values for tests of equality of coefficients</i>									
3:1 = 1:1	0.921	0.375	0.224	0.504	0.969	0.914	0.872	0.992	0.939
3:1 = No match	0.724	0.329	0.486	0.275	0.134	0.519	0.449	0.791	0.936
1:1 = No match	0.647	0.964	0.086	0.799	0.173	0.668	0.489	0.792	0.902
3:1 = 1:1 = No match	0.886	0.569	0.165	0.548	0.223	0.798	0.708	0.955	0.990
Observations	341	341	341	341	341	341	341	341	341
R-squared	0.058	0.067	0.089	0.065	0.061	0.069	0.052	0.065	0.103
Control group mean	1287	204	65.15	37.69	51.36	258.7	427.4	180.8	61.88

Notes: Robust standard errors clustered at the level of the day and location of the baseline survey in brackets. Sample is all migrant-student pairs with completed El Salvador follow-up surveys. All regressions include stratification cell fixed effects for the week and location of the baseline survey.

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1



**Table 7: Total household education expenditures**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	<i>Dependent variable: Annualized target student expenditure on</i>								
	Total	Tuition	School supplies	Uniforms	Books	Transport	Food	Computer use	Other
<b>Panel 1: All target students</b>									
3:1 match	332.8*	147.9***	-5.067	11.89	3.238	111.3*	95.97	6.577	-39.08
	[168.7]	[45.64]	[9.432]	[8.077]	[10.13]	[56.44]	[76.22]	[39.86]	[28.46]
1:1 match	84.86	95.87**	-19.29**	-4.093	-4.331	90.10	-16.69	-10.74	-45.96
	[169.9]	[42.80]	[8.978]	[7.705]	[8.934]	[71.90]	[71.63]	[35.86]	[35.01]
No match	-54.15	77.96*	-8.630	-6.616	-19.54**	25.77	-52.50	-23.94	-46.65
	[153.1]	[41.43]	[8.620]	[7.730]	[8.708]	[56.20]	[65.47]	[34.20]	[29.06]
<i>P-values for tests of equality of coefficients</i>									
3:1 = 1:1	0.236	0.399	0.199	0.045	0.528	0.794	0.208	0.712	0.784
3:1 = No match	0.087	0.267	0.753	0.038	0.053	0.261	0.112	0.508	0.676
1:1 = No match	0.473	0.740	0.342	0.783	0.110	0.463	0.652	0.771	0.977
3:1 = 1:1 = No match	0.226	0.529	0.408	0.051	0.098	0.522	0.265	0.802	0.912
Observations	728	728	728	728	728	728	728	728	728
R-squared	0.041	0.053	0.033	0.038	0.037	0.059	0.034	0.035	0.05
Control group mean	2132	251.3	90.78	57.91	86.99	423.6	812.7	310.4	98.31
<b>Panel 2: Female target students</b>									
3:1 match	534.0**	290.0***	12.14	19.13	2.312	141.5*	138.8	-10.51	-59.34
	[262.0]	[85.83]	[15.49]	[12.74]	[16.25]	[82.41]	[115.4]	[55.73]	[40.02]
1:1 match	-165.0	95.03	-13.95	-17.82*	-15.74	41.25	-75.90	-84.77*	-93.09
	[250.3]	[67.34]	[17.56]	[10.57]	[15.64]	[89.52]	[111.3]	[45.26]	[57.80]
No match	-314.2	92.75	-8.863	-14.30	-26.32*	-30.98	-163.6*	-76.56	-86.29*
	[239.5]	[68.63]	[13.86]	[11.28]	[13.51]	[65.11]	[95.63]	[55.42]	[50.26]
<i>P-values for tests of equality of coefficients</i>									
3:1 = 1:1	0.017	0.063	0.187	0.004	0.294	0.312	0.116	0.189	0.350
3:1 = No match	0.007	0.063	0.193	0.017	0.099	0.069	0.017	0.331	0.377
1:1 = No match	0.556	0.977	0.789	0.773	0.476	0.433	0.453	0.893	0.817
3:1 = 1:1 = No match	0.018	0.135	0.306	0.010	0.255	0.191	0.057	0.400	0.599
Observations	387	387	387	387	387	387	387	387	387
R-squared	0.105	0.11	0.053	0.084	0.07	0.079	0.102	0.086	0.107
Control group mean	2233	228	86.71	58.9	92.23	453	845.3	352.6	116.3
<b>Panel 3: Male target students</b>									
3:1 match	8.040	-16.38	-28.82**	-0.164	-1.276	69.57	0.472	5.160	-20.51
	[224.8]	[64.61]	[14.15]	[10.74]	[15.64]	[68.61]	[92.67]	[50.31]	[33.61]
1:1 match	284.4	75.85	-32.58**	4.158	6.924	140.1	30.92	61.77	-2.746
	[276.5]	[72.15]	[12.87]	[11.26]	[14.29]	[114.8]	[99.30]	[58.94]	[62.10]
No match	2.470	30.61	-18.38	-3.262	-16.19	51.01	-14.86	-9.109	-17.35
	[234.8]	[68.07]	[15.04]	[10.63]	[13.54]	[92.57]	[100.5]	[47.16]	[36.73]
<i>P-values for tests of equality of coefficients</i>									
3:1 = 1:1	0.37	0.296	0.802	0.756	0.634	0.576	0.782	0.426	0.745
3:1 = No match	0.985	0.544	0.527	0.796	0.361	0.851	0.902	0.822	0.906
1:1 = No match	0.37	0.608	0.372	0.611	0.126	0.534	0.676	0.342	0.781
3:1 = 1:1 = No match	0.598	0.569	0.663	0.878	0.293	0.814	0.907	0.614	0.948
Observations	341	341	341	341	341	341	341	341	341
R-squared	0.078	0.064	0.068	0.061	0.052	0.105	0.043	0.086	0.101
Control group mean	2000	281.8	96.09	56.62	80.14	385.3	770	255.2	74.85

Notes: Robust standard errors clustered at the level of the day and location of the baseline survey in brackets. Sample is all migrant-student pairs with completed E Salvador follow-up surveys. All regressions include stratification cell fixed effects for the week and location of the baseline survey.

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 8: Instrumental variables regressions**

	(1)	(2)	(3)
	Full El Salvador follow-up sample	Female target students	Male target students
<b><i>Panel 1: First stage: Dependent variable is total target student EduRemesa funds</i></b>			
3:1 match	85.34*** [16.08]	108.5*** [27.90]	51.70*** [18.37]
F-statistic on first stage	28.17	15.12	7.92
Observations	425	228	197
<b><i>Panel 2: IV: Dependent variable is total target student annualized education expenditures</i></b>			
Total target student EduRemesa funds	3.720** [1.647]	4.989** [2.035]	1.730 [3.424]
P-value for equality of coefficient to 1	0.099	0.050	0.831
Observations	425	228	197
<b><i>Panel 3: IV: Dependent variable is estimated target student annualized earnings</i></b>			
Total target student EduRemesa funds	-1.661*** [0.582]	-0.861** [0.429]	-3.080 [2.004]
Observations	425	228	197
<b><i>Panel 4: IV: Dependent variable is target student expenditures minus estimated earnings</i></b>			
Total target student EduRemesa funds	5.381*** [1.946]	5.850*** [2.257]	4.811 [4.389]
P-value for equality of coefficient to 1	0.024	0.032	0.385
Observations	425	228	197

Notes: Robust standard errors clustered at the level of the day and location of the baseline survey in brackets. Sample is all migrant-student pairs with completed El Salvador follow-up surveys in the control group and 3:1 match treatment group. Treatment indicator for the 3:1 match treatment is used to instrument for EduRemesa funds in panels 2, 3, and 4. All regressions include stratification cell fixed effects for the week and location of the baseline survey.

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 9: Target student education outcomes**

	(1)	(2)	(3)	(4)
	Target student is in school	Target student is in any private school	Target student is in parochial school	Target student is in other private school
<b>Panel 1: All target students</b>				
3:1 match	0.0309 [0.0398]	0.109** [0.0430]	0.0288 [0.0375]	0.0803** [0.0350]
1:1 match	-0.0210 [0.0381]	0.0498 [0.0419]	-0.0172 [0.0368]	0.0671* [0.0346]
No match	0.0182 [0.0440]	0.0910** [0.0460]	0.0298 [0.0359]	0.0612 [0.0379]
<i>P-values for tests of equality of coefficients</i>				
3:1 = 1:1	0.244	0.247	0.339	0.780
3:1 = No match	0.819	0.766	0.984	0.705
1:1 = No match	0.426	0.413	0.283	0.898
3:1 = 1:1 = No match	0.453	0.448	0.486	0.928
Observations	728	728	728	728
R-squared	0.048	0.042	0.031	0.044
Control group mean	0.741	0.267	0.159	0.107
<b>Panel 2: Female target students</b>				
3:1 match	0.0836 [0.0599]	0.183*** [0.0619]	0.0334 [0.0508]	0.150*** [0.0571]
1:1 match	-0.0166 [0.0691]	0.119* [0.0643]	0.0408 [0.0546]	0.0781 [0.0507]
No match	-0.00889 [0.0628]	0.0623 [0.0640]	-0.0145 [0.0476]	0.0768 [0.0555]
<i>P-values for tests of equality of coefficients</i>				
3:1 = 1:1	0.189	0.430	0.913	0.322
3:1 = No match	0.220	0.127	0.441	0.307
1:1 = No match	0.920	0.457	0.337	0.985
3:1 = 1:1 = No match	0.335	0.311	0.568	0.526
Observations	387	387	387	387
R-squared	0.082	0.085	0.062	0.087
Control group mean	0.739	0.261	0.157	0.105
<b>Panel 3: Male target students</b>				
3:1 match	-0.0595 [0.0681]	0.00546 [0.0636]	0.0157 [0.0555]	-0.0102 [0.0488]
1:1 match	-0.0536 [0.0587]	-0.0383 [0.0644]	-0.0796 [0.0495]	0.0413 [0.0578]
No match	0.0115 [0.0709]	0.0897 [0.0739]	0.0683 [0.0627]	0.0214 [0.0535]
<i>P-values for tests of equality of coefficients</i>				
3:1 = 1:1	0.934	0.526	0.119	0.429
3:1 = No match	0.385	0.308	0.472	0.591
1:1 = No match	0.397	0.108	0.0211	0.766
3:1 = 1:1 = No match	0.628	0.274	0.0473	0.712
Observations	341	341	341	341
R-squared	0.109	0.061	0.045	0.089
Control group mean	0.744	0.274	0.162	0.111

Notes: Robust standard errors clustered at the level of the day and location of the baseline survey in brackets. Sample is all migrant-student pairs with completed El Salvador follow-up surveys. All regressions include stratification cell fixed effects for the week and location of the baseline survey.

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 10: Target student labor force outcomes**

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Dependent variables refer to work currently being done by the target student</i>					
	Any work	Average hours per week any work	Paid work	Average hours per week paid work	Unpaid work	Average hours per week unpaid work
<b>Panel 1: All target students</b>						
3:1 match	-0.139*** [0.0402]	-4.365*** [1.048]	-0.0718* [0.0369]	-2.928*** [0.936]	-0.0830*** [0.0308]	-1.436*** [0.468]
1:1 match	-0.0751* [0.0412]	-3.204*** [1.095]	-0.0543 [0.0346]	-1.780* [0.968]	-0.0435 [0.0325]	-1.425*** [0.431]
No match	0.00897 [0.0445]	-0.386 [1.323]	-0.0147 [0.0371]	-0.138 [1.223]	0.00231 [0.0352]	-0.248 [0.559]
<i>P-values for tests of equality of coefficients</i>						
3:1 = 1:1	0.187	0.251	0.663	0.230	0.267	0.974
3:1 = No match	0.006	0.003	0.163	0.022	0.021	0.010
1:1 = No match	0.091	0.017	0.290	0.148	0.241	0.015
3:1 = 1:1 = No match	0.023	0.009	0.340	0.071	0.067	0.025
Observations	728	728	728	728	728	728
R-squared	0.041	0.056	0.032	0.048	0.041	0.059
Control group mean	0.326	6.778	0.196	4.426	0.17	2.352
<b>Panel 2: Female target students</b>						
3:1 match	-0.157*** [0.0481]	-3.260*** [1.155]	-0.110*** [0.0406]	-2.277** [0.899]	-0.0706* [0.0370]	-0.983* [0.521]
1:1 match	-0.0817 [0.0528]	-3.275*** [1.045]	-0.0902** [0.0397]	-2.458*** [0.871]	-0.0305 [0.0381]	-0.817 [0.550]
No match	0.00582 [0.0554]	1.371 [1.683]	-0.0705 [0.0430]	0.652 [1.553]	0.0535 [0.0411]	0.718 [0.678]
<i>P-values for tests of equality of coefficients</i>						
3:1 = 1:1	0.183	0.985	0.638	0.744	0.382	0.757
3:1 = No match	0.007	0.004	0.360	0.050	0.012	0.017
1:1 = No match	0.164	0.003	0.638	0.043	0.110	0.054
3:1 = 1:1 = No match	0.027	0.009	0.656	0.125	0.040	0.055
Observations	387	387	387	387	387	387
R-squared	0.103	0.099	0.053	0.07	0.092	0.087
Control group mean	0.275	5.19	0.17	3.353	0.15	1.837
<b>Panel 3: Male target students</b>						
3:1 match	-0.116* [0.0701]	-5.144*** [1.866]	-0.0216 [0.0680]	-3.103* [1.838]	-0.111** [0.0467]	-2.042*** [0.774]
1:1 match	-0.0441 [0.0666]	-2.555 [2.028]	-0.00319 [0.0625]	-0.840 [1.955]	-0.0434 [0.0515]	-1.714** [0.660]
No match	0.0310 [0.0681]	-1.852 [2.332]	0.0571 [0.0661]	-0.682 [2.245]	-0.0454 [0.0592]	-1.169 [0.968]
<i>P-values for tests of equality of coefficients</i>						
3:1 = 1:1	0.373	0.176	0.811	0.249	0.206	0.512
3:1 = No match	0.111	0.112	0.303	0.207	0.272	0.322
1:1 = No match	0.263	0.766	0.362	0.945	0.975	0.516
3:1 = 1:1 = No match	0.264	0.184	0.512	0.326	0.351	0.582
Observations	341	341	341	341	341	341
R-squared	0.079	0.096	0.083	0.095	0.077	0.091
Control group mean	0.393	8.855	0.231	5.829	0.197	3.026

Notes: Robust standard errors clustered at the level of the day and location of the baseline survey in brackets. Sample is all migrant-student pairs with completed El Salvador follow-up surveys. All regressions include stratification cell fixed effects for the week and location of the baseline survey.

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 11: Remittances sent by migrant**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<i>Dependent variable is migrant report of remittances sent since January 1, 2012</i>									
<i>Full migrant follow-up sample</i>			<i>Trimmed top 1% of each column</i>			<i>Inverse hyperbolic sine transformation</i>			
	Remittances to target household	Remittances to other households	Overall total	Remittances to target household	Remittances to other households	Overall total	Remittances to target household	Remittances to other households	Overall total
<b>Panel 1: All target students</b>									
3:1 match	-167.9 [192.2]	-74.69 [70.59]	-242.6 [208.7]	-2.336 [160.1]	-71.33 [48.14]	-49.84 [162.4]	-0.124 [0.333]	-0.252 [0.292]	-0.296 [0.280]
1:1 match	-365.1** [180.7]	-63.63 [66.62]	-428.8** [189.9]	-153.1 [152.4]	29.36 [60.67]	-128.5 [160.5]	-0.441 [0.410]	0.132 [0.330]	-0.424 [0.333]
No match	-482.9*** [165.6]	-141.9** [54.85]	-624.8*** [175.6]	-213.1 [136.5]	-60.65 [49.59]	-316.6** [138.9]	-0.271 [0.323]	-0.171 [0.302]	-0.456 [0.275]
<i>P-values for tests of equality of coefficients</i>									
3:1 = 1:1	0.284	0.900	0.364	0.362	0.130	0.654	0.475	0.289	0.724
3:1 = No match	0.052	0.394	0.036	0.152	0.853	0.095	0.674	0.826	0.618
1:1 = No match	0.370	0.252	0.166	0.623	0.186	0.182	0.664	0.407	0.924
3:1 = 1:1 = No match	0.135	0.446	0.069	0.354	0.284	0.173	0.773	0.535	0.879
Observations	735	735	735	727	727	727	735	735	735
R-squared	0.053	0.037	0.048	0.061	0.04	0.057	0.031	0.03	0.032
Control group mean	1449	363	1812	1206	278.1	1537	6.126	1.973	6.839
<b>Panel 2: Female target students</b>									
3:1 match	-59.45 [301.2]	-20.15 [107.6]	-79.60 [321.8]	-12.05 [224.3]	-134.0** [64.18]	-60.68 [243.4]	0.0210 [0.423]	-0.632 [0.392]	-0.226 [0.369]
1:1 match	-347.3 [242.3]	-1.052 [88.84]	-348.4 [257.0]	-120.3 [206.7]	32.48 [85.13]	-114.5 [226.4]	-0.508 [0.538]	0.128 [0.431]	-0.600 [0.490]
No match	-446.5** [210.4]	-59.51 [74.55]	-506.0** [223.8]	-213.8 [172.8]	-30.57 [70.27]	-262.9 [191.2]	-0.325 [0.416]	0.106 [0.393]	-0.536 [0.384]
<i>P-values for tests of equality of coefficients</i>									
3:1 = 1:1	0.317	0.889	0.395	0.664	0.086	0.827	0.346	0.157	0.449
3:1 = No match	0.151	0.751	0.179	0.318	0.223	0.413	0.437	0.159	0.475
1:1 = No match	0.580	0.550	0.458	0.587	0.518	0.469	0.736	0.964	0.900
3:1 = 1:1 = No match	0.346	0.824	0.385	0.554	0.196	0.654	0.588	0.278	0.681
Observations	401	401	401	397	398	397	401	401	401
R-squared	0.05	0.066	0.052	0.054	0.095	0.072	0.066	0.091	0.068
Control group mean	1415	320.6	1736	1225	298.8	1550	6.089	2.048	6.868
<b>Panel 3: Male target students</b>									
3:1 match	-298.4 [239.5]	-130.3 [103.3]	-428.8 [259.6]	3.236 [193.7]	9.058 [75.67]	-24.91 [195.0]	-0.346 [0.453]	0.0502 [0.474]	-0.331 [0.381]
1:1 match	-390.1 [254.6]	-77.67 [102.4]	-467.8* [264.8]	-173.0 [191.0]	86.16 [84.79]	-50.97 [216.8]	-0.329 [0.541]	0.324 [0.483]	-0.126 [0.449]
No match	-528.9** [238.9]	-211.9*** [79.59]	-740.8*** [239.2]	-151.1 [180.2]	-94.41 [62.21]	-309.2* [174.6]	-0.246 [0.470]	-0.596 [0.458]	-0.368 [0.410]
<i>P-values for tests of equality of coefficients</i>									
3:1 = 1:1	0.687	0.638	0.872	0.377	0.454	0.913	0.977	0.612	0.701
3:1 = No match	0.310	0.315	0.140	0.484	0.146	0.159	0.853	0.248	0.939
1:1 = No match	0.566	0.134	0.247	0.915	0.039	0.268	0.888	0.071	0.634
3:1 = 1:1 = No match	0.595	0.234	0.280	0.649	0.062	0.310	0.980	0.186	0.884
Observations	334	334	334	330	329	330	334	334	334
R-squared	0.102	0.097	0.095	0.121	0.099	0.108	0.048	0.065	0.052
Control group mean	1493	419.7	1913	1180	249.7	1519	6.175	1.873	6.802

Notes: Robust standard errors clustered at the level of the day and location of the baseline survey in brackets. Sample is all migrant-student pairs with completed migrant follow-up surveys. All regressions include stratification cell fixed effects for the week and location of the baseline survey.

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

## **Appendix A: Variable definitions**

Data used in this paper came from two surveys. Baseline surveys were conducted with migrants between early November 2011 and early February 2012. Follow-up surveys were conducted by phone with migrants and the target household in El Salvador (both the target student and a responsible adult) from mid July 2012 to late October 2012. We also use administrative data from the EduRemesa project. Because El Salvador uses the US dollar as its official currency, all monetary figures are in US dollars. Following are descriptions of all variables used for baseline summary statistics and dependent variables in regressions.

### ***Variables from baseline survey***

*Migrant is female* is equal to one if migrant is female and zero if migrant is male.

*Migrant age* is migrant's age in years, calculated from reported date of birth.

*Migrant is married* is equal to one if migrant reports being married or cohabiting and zero otherwise. It is derived from asking for the migrant's civil status.

*Migrant household size in the US* is the total number of persons (including the migrant) living in the migrant's home in the United States.

*Migrant annual remittances to target household* is the total amount sent by the migrant to the target household in the 12 months preceding the survey. This equals the frequency of regular remittance transactions over the past 12 months multiplied by the average amount per regular remittance transaction, plus the total amounts reported to have been sent for special occasions in various categories.

*Migrant annual remittances to other households* is the total amount sent by the migrant to households that are not the target household in the 12 months preceding the survey. This equals the frequency of regular remittance transactions over the past 12 months multiplied by the average amount per regular remittance transaction for each household, plus the total amounts reported to have been sent for special occasions in various categories.

*Target student is female* is equal to one if the migrant reports the target student is female and zero if migrant reports the target student is male.

*Target student age* is the migrant's report of the target student's age.

*Target student is migrant's child* is equal to one if the migrant reports the target student is his/her child and zero if a different relationship is reported. It is derived from a question that asks the migrant to describe his/her relationship with the target student.

*Target student is migrant's sibling* is equal to one if the migrant reports the target student is his/her sibling and zero if a different relationship is reported. It is derived from a question that asks the migrant to describe his/her relationship with the target student.

*Target student is migrant's niece/nephew* is equal to one if the migrant reports the target student is his/her niece/nephew and zero if a different relationship is reported. It is derived from a question that asks the migrant to describe his/her relationship with the target student.

*Target student is migrant's cousin* is equal to one if the migrant reports the target student is his/her cousin and zero if a different relationship is reported. It is derived from a question that asks the migrant to describe his/her relationship with the target student.

*Target student is in school* is equal to one if the migrant reports that the target student currently attends school and zero if the migrant reports that the target student does not currently attend school.

*Target student years of education* is the target student's total number of years of education reported by the migrant. It is the total number of years completed for those students not currently in school and includes the current year for those still in school. It is derived from questions about current level of schooling and number of years within that level.

#### ***Variables from EduRemesa administrative data***

*EduRemesa sent* is equal to one if the migrant sent at least one EduRemesa to any student and zero otherwise.

*Number of EduRemesas sent* is the total number of EduRemesas sent by each migrant.

*Total amount sent by migrant* is the total dollar amount contributed by each migrant to EduRemesas, summing across all EduRemesas sent by each migrant.

*Total amount sent by migrant plus subsidy* is the total dollar amount contributed by each migrant to EduRemesas plus the project subsidy, summing across all EduRemesas sent by each migrant.

*EduRemesa sent to target student* is equal to one if the migrant sent an EduRemesa to his/her designated target student and zero otherwise.

*Total amount sent by migrant to target student* is the total dollar amount contributed by each migrant to EduRemesas for his/her target student.

*Total amount sent by migrant plus subsidy to target student* is the total dollar amount contributed by each migrant to EduRemesas for his/her target student plus the project subsidy.

#### ***Variables from the El Salvador follow-up survey***

Target student expenditures on education:

Spending on all categories is asked with reference to the period since January 1, 2012 and then annualized in the manner described below for each category. For all categories both target students and the responsible adult were asked if there were expenditures in each category. If yes, they were asked how much was spent. The student report is given priority and the responsible adult report is used when the student report is missing. If both are missing, the value is imputed to allow for consistent sample size. Imputations were performed by regressing expenditure in each category on student age, gender, whether student is in school, the type of school, education level, and number of people 22 and under in the student's household using the control group. The data comes from the student reports in El Salvador follow-up survey. This regression is then used to predict values for the missing values in each expenditure category.

*Target student expenditure on:*

*Tuition* is the annual amount spent on tuition for the target student. It is sum of two categories: annual tuition paid in a lump sum at the beginning of the school and monthly tuition paid every month. Monthly tuition report is multiplied by ten (for ten month school year) to arrive at annual figure.

Student report: 99.2%

Adult report: 0.7%

Imputed value: 0.1%

*School supplies* is the annual amount on school supplies for the target student.

Student report: 97.7%

Adult report: 2.2%

Imputed value: 0.1%

*Uniforms* is the annual amount spent on school uniforms for the target student.

Student report: 99.2%

Adult report: 0.7%

Imputed value: 0.1%

*Books* is the annual amount spent on school books for the target student.

Student report: 98.6%

Adult report: 1.0%

Imputed value: 0.4%

*Transport* is the annual amount spent on transportation to and from school for the target student. It is reported as a weekly expenditure and multiplied by 43 for a 10 month school year.

Student report: 99.7%

Adult report: 0.3%

Imputed value: 0.0%



*Food* is the annual amount spent by the target student for food purchased while at school. It is reported as a weekly expenditure and multiplied by 43 for a 10 month school year.

Student report: 99.9%

Adult report: 0.1%

Imputed value: 0.0%

*Computer use* is the annual amount spent by the target student for computer use related to school work. It is reported as a weekly expenditure and multiplied by 43 for a 10 month school year.

Student report: 99.4%

Adult report: 0.5%

Imputed value: 0.1%

*Other* are expenditures that do not fit into any category. These are reported in the frequency of the respondent's choice and multiplied by the appropriate number to annualize for the 10 month school year.

Student report: 99.9%

Adult report: 0.1%

Imputed value: 0.0%

*Total target student education expenditures* is the sum of all the preceding target student education expenditure variables.

All categories are student report: 95.4%

At least one adult report: 4.0%

At least one imputed value: 0.8%

#### Total household expenditures on education:

Spending on all categories is asked with reference to the period since January 1, 2012 and then annualized in the manner described below for each category. For all categories amounts are the target student amount described above plus the amount spent on each additional child in the household in that expenditure category. The additional student reports come from the responsible adult. For each category and for each additional child the responsible adult was asked if there were expenditures in each category. If yes, they are asked how much was spent. If report is missing, the value is imputed to allow for consistent sample size. Imputations were performed by regressing expenditure in each category on additional student age, gender, whether student is in school, the type of school, education level, and number of people 22 and under in the student's household using the control group. The data comes from the adult reports in El Salvador follow-up survey. This regression is then used to predict values for the missing values in each expenditure category.

*Total household expenditure on:*

*Tuition* is the annual amount spent on tuition. It is sum of two categories: annual tuition paid in a lump sum at the beginning of the school and monthly tuition paid every month. Monthly tuition report is multiplied by ten (for ten month school year) to arrive at annual figure.

At least one imputed value: 0.8%

*School supplies* is the annual amount on school supplies.

At least one imputed value: 1.1%

*Uniforms* is the annual amount spent on school uniforms.

At least one imputed value: 0.3%

*Books* is the annual amount spent on school books.

At least one imputed value: 1.9%

*Transport* is the annual amount spent on transportation to and from school. It is reported as a weekly expenditure and multiplied by 43 for a 10 month school year.

At least one imputed value: 0.1%

*Food* is the annual amount spent on food purchased while at school. It is reported as a weekly expenditure and multiplied by 43 for a 10 month school year.

At least one imputed value: 0.2%

*Computer use* is the annual amount spent on computer use related to school work. It is reported as a weekly expenditure and multiplied by 43 for a 10 month school year.

At least one imputed value: 0.5%

*Other* are expenditures that do not fit into any category. These are reported in the frequency of the respondent's choice and multiplied by the appropriate number to annualize for the 10 month school year.

At least one imputed value: 1.3%

*Total household education expenditures* is the sum of all the preceding household education expenditure variables.

At least one imputed value: 4.5%

Target student education outcomes:

*Target student is in school* is equal to one if the target student reports he/she is currently attending school and zero if he/she reports that he/she is not.

*Target student is in any private school* is equal to one if the target student reports that he/she attends either parochial school or non-parochial private school. It is equal to zero if target student reports attending public school or the target student is not currently in school.

*Target student is in parochial school* is equal to one if the target student reports that he/she attends parochial school. It is equal to zero if target student reports attending non-parochial private school, public school, or the target student is not currently in school.

*Target student is in other private school* is equal to one if the target student reports that he/she attends a non-parochial private school. It is equal to zero if target student reports attending parochial private school, public school, or the target student is not currently in school.

Target student labor force outcomes:

*Paid work* is equal to one if the target student reports currently spending time working at a job where he/she receives pay and zero otherwise.

*Average hours per week paid work* is the number of weekly hours the target student reports spending on average at the job(s) where he/she receives pay. It is equal to zero for target students who said they did not perform paid work.

*Unpaid work* is equal to one if the target student reports currently spending time working at a job where he/she does not receive pay and zero otherwise.

*Average hours per week unpaid work* is the number of weekly hours the target student reports spending on average at the job(s) where he/she does not receive pay. It is equal to zero for target students who said they did not perform unpaid work.

*Any work* is equal to one if the target student reports doing any work and zero otherwise. It is derived from responses to *paid work* and *unpaid work*.

*Average hours per week any work* is the number of weekly hours the target student reports spending on average at any job. It is the sum of *average hours per week paid work* and *average hours per week unpaid work*.

***Variables from the migrant follow-up survey***

Remittances sent by migrant:

All remittance variables refer to the total amount sent by the migrant since January 1, 2012. For each category (regular and special occasion remittances to the target household and other households) missing values are imputed to ensure consistent sample size. Imputations are done by regressing the amount in each category on migrant age, migrant gender, years the migrant has been in the US, annual regular and special occasion remittances to the target household and other households, migrant years of education, an indicator variable for whether or not the migrant's spouse is in the US, the number of children the migrant has living in the US, and an indicator variable for whether or not the migrant has a child under 23 living in El Salvador using the control group. The data comes from the baseline survey. This regression is then used to predict values for the missing values.

*Remittances to target household* is the total amount sent by the migrant to the target household since January 1, 2012. This equals the number of regular remittances sent since January 1, 2012 multiplied by the average amount of each remittance, plus the total amounts reported to have been sent for special occasions in various categories since January 1, 2012. This figure *does not* include any funds that may have been sent as an EduRemesa.

Imputed value: 16.2%

*Remittance to other households* is the total amount sent by the migrant to households that are not the target household since January 1, 2012. This equals the number of regular remittances sent to other households since January 1, 2012 multiplied by the average amount per regular remittance for each household, plus the total amounts reported to have been sent for special occasions in various categories. This figure *does not* include any funds that may have been sent as an EduRemesa.

Imputed value: 4.6%

*Overall total* is the sum of remittances to the target household and to other household.

Imputed value: 19.6%

*Inverse hyperbolic sine transformation of remittance variables* is the three above remittance variables transformed as follows:  $\log(y_i + (y_i^2 + 1)^{1/2})$ .

**Appendix Table 1: Type of school by school level, current students, El Salvador**

	Primary	Secondary	Tertiary
Public	89.06%	78.90%	38.92%
Private	10.94%	21.10%	61.08%
<i>Parochial</i>	4.34%	5.31%	5.76%
<i>Other private</i>	6.60%	15.78%	55.32%

Notes: Source is El Salvador Encuesta de Hogares de Propósitos Múltiples 2010.

**Appendix Table 2: Average Annual Education Expenditures (USD), current students**

	<i>Secondary</i>		<i>Tertiary</i>	
	Public	Private	Public	Private
Total	1442	2214	1868	2834
Tuition	6	499	177	702
Supplies	80	107	59	97
Uniforms	76	71	7	11
Texts	63	81	94	97
Shoes	280	288	541	573
Transport	571	548	645	778
Food	342	284	266	292
Other	25	337	79	283

Notes: Source is reports on target student expenditure in the control group.

**Appendix Table 3A: Baseline summary statistics: El Salvador follow-up sample**

<b>Variable</b>	<b>Mean</b>	<b>Std. Dev.</b>	<b>Min</b>	<b>10th pct.</b>	<b>Median</b>	<b>90th pct.</b>	<b>Max</b>	<b>N</b>
<b><i>Panel 1: Migrant-student pairs with female target student</i></b>								
Migrant is female	0.49	0.50	0	0	0	1	1	387
Migrant age	36.92	9.25	15	26	36	50	74	375
Migrant is married	0.61	0.49	0	0	1	1	1	385
Migrant hh size in US	4.58	2.13	1	2	4	7	13	387
Migrant years of education	9.33	4.72	0	0	10	14	20	384
Migrant years in US	11.51	6.61	0	5	11	22	38	386
Migrant annual remittance to target hh	2,766	3,542	0	0	1,800	7,200	26,900	380
Migrant annual remittances to other hhs	1,166	1,876	0	0	100	3,800	11,500	385
Target student is female	1.00	0.00	1	1	1	1	1	387
Target student age	18.59	3.34	11	15	18	23	35	380
<i>Target student is migrant's...</i>								
...child	0.26	0.44	0	0	0	1	1	387
...sibling	0.27	0.44	0	0	0	1	1	387
...niece/nephew	0.34	0.48	0	0	0	1	1	387
...cousin	0.09	0.28	0	0	0	0	1	387
Target student is in school	0.92	0.27	0	1	1	1	1	387
Target student years of education	11.89	2.10	9	9	12	15	19	357
<b><i>Panel 2: Migrant-student pairs with male target student</i></b>								
Migrant is female	0.50	0.50	0	0	1	1	1	341
Migrant age	36.83	9.64	18	25	36	49	71	334
Migrant is married	0.59	0.49	0	0	1	1	1	339
Migrant hh size in US	4.36	2.05	1	2	4	7	13	341
Migrant years of education	8.88	4.58	0	2	9	14	21	333
Migrant years in US	10.89	6.07	0	5	10	21	37	340
Migrant annual remittance to target hh	2,590	3,373	0	0	1,500	6,750	31,620	333
Migrant annual remittances to other hhs	1,200	2,140	0	0	0	3,600	15,600	336
Target student is female	0.00	0.00	0	0	0	0	0	341
Target student age	18.40	3.03	14	15	18	22	38	333
<i>Target student is migrant's...</i>								
...child	0.26	0.44	0	0	0	1	1	340
...sibling	0.23	0.42	0	0	0	1	1	340
...niece/nephew	0.32	0.47	0	0	0	1	1	340
...cousin	0.13	0.33	0	0	0	1	1	340
Target student is in school	0.93	0.26	0	1	1	1	1	341
Target student years of education	11.72	2.27	8	9	12	15	24	321

Notes: Sample is all migrant-student pairs with completed El Salvador follow-up surveys. Variables all come from migrant baseline survey. Sample size varies slightly with missing values for each variable.

**Appendix Table 3B: Baseline summary statistics: Full sample**

<b>Variable</b>	<b>Mean</b>	<b>Std. Dev.</b>	<b>Min</b>	<b>10th pct.</b>	<b>Median</b>	<b>90th pct.</b>	<b>Max</b>	<b>N</b>
<b><i>Panel 1: All migrant-student pairs</i></b>								
Migrant is female	0.49	0.50	0	0	0	1	1	991
Migrant age	36.79	9.52	15	25	36	49	74	963
Migrant is married	0.60	0.49	0	0	1	1	1	986
Migrant hh size in US	4.39	2.03	1	2	4	7	13	990
Migrant years of education	9.22	4.63	0	1	9	14	21	976
Migrant years in US	11.22	6.34	0	5	10	21	38	987
Migrant annual remittance to target hh	2,658	3,344	0	0	1700	6950	31620	973
Migrant annual remittances to other hhs	1,116	1,907	0	0	0	3600	15600	983
Target student is female	0.53	0.50	0	0	1	1	1	991
Target student age	18.57	3.40	11	15	18	23	40	967
<i>Target student is migrant's...</i>								
...child	0.26	0.44	0	0	0	1	1	989
...sibling	0.23	0.42	0	0	0	1	1	989
...niece/nephew	0.33	0.47	0	0	0	1	1	989
...cousin	0.11	0.32	0	0	0	1	1	989
Target student is in school	0.92	0.27	0	1	1	1	1	990
Target student years of education	11.79	2.15	8	9	12	14	24	913
<b><i>Panel 2: Migrant-student pairs with female target student</i></b>								
Migrant is female	0.48	0.50	0	0	0	1	1	522
Migrant age	37.07	9.52	15	26	36	50	74	508
Migrant is married	0.62	0.49	0	0	1	1	1	519
Migrant hh size in US	4.45	2.05	1	2	4	7	13	521
Migrant years of education	9.35	4.66	0	0	10	14	20	517
Migrant years in US	11.49	6.54	0	4	11	22	38	520
Migrant annual remittance to target hh	2,694	3,394	0	0	1690	7200	26900	513
Migrant annual remittances to other hhs	1,122	1,800	0	0	0	3600	11500	519
Target student is female	1.00	0.00	1	1	1	1	1	522
Target student age	18.69	3.61	11	15	18	23	36	511
<i>Target student is migrant's...</i>								
...child	0.26	0.44	0	0	0	1	1	521
...sibling	0.24	0.43	0	0	0	1	1	521
...niece/nephew	0.35	0.48	0	0	0	1	1	521
...cousin	0.10	0.29	0	0	0	0	1	521
Target student is in school	0.91	0.29	0	1	1	1	1	521
Target student years of education	11.90	2.09	9	9	12	15	19	476
<b><i>Panel 3: Migrant-student pairs with male target student</i></b>								
Migrant is female	0.51	0.50	0	0	1	1	1	469
Migrant age	36.48	9.52	18	25	36	48	71	455
Migrant is married	0.59	0.49	0	0	1	1	1	467
Migrant hh size in US	4.32	2.02	1	2	4	7	13	469
Migrant years of education	9.07	4.61	0	2	9	15	21	459
Migrant years in US	10.93	6.10	0	5	10	21	37	467
Migrant annual remittance to target hh	2,619	3,290	0	0	1700	6725	31620	460
Migrant annual remittances to other hhs	1,110	2,023	0	0	0	3500	15600	464
Target student is female	0.00	0.00	0	0	0	0	0	469
Target student age	18.42	3.14	13	15	18	22	40	456
<i>Target student is migrant's...</i>								
...child	0.26	0.44	0	0	0	1	1	468
...sibling	0.23	0.42	0	0	0	1	1	468
...niece/nephew	0.32	0.47	0	0	0	1	1	468
...cousin	0.13	0.34	0	0	0	1	1	468
Target student is in school	0.93	0.26	0	1	1	1	1	469
Target student years of education	11.68	2.21	8	9	11	14	24	437

Notes: Sample is all migrant-student pairs interviewed at baseline. Variables all come from migrant baseline survey. Sample size varies slightly with missing values for each variable.



**Appendix Table 3C: Baseline summary statistics: Migrant follow-up sample**

Variable	Mean	Std. Dev.	Min	10th pct.	Median	90th pct.	Max	N
<b>Panel 1: All migrant-student pairs</b>								
Migrant is female	0.50	0.50	0	0	0	1	1	735
Migrant age	37.28	9.56	17	26	36	50	74	717
Migrant is married	0.62	0.49	0	0	1	1	1	733
Migrant hh size in US	4.52	2.09	1	2	4	7	13	735
Migrant years of education	9.07	4.69	0	0	9	14	21	724
Migrant years in US	11.08	6.34	0	5	10	21	38	733
Migrant annual remittance to target hh	2,765	3,413	0	0	1,800	7,200	31,620	724
Migrant annual remittances to other hhs	1,189	2,048	0	0	0	3,675	15,600	730
Target student is female	0.55	0.50	0	0	1	1	1	735
Target student age	18.51	3.40	11	15	18	23	40	724
<i>Target student is migrant's...</i>								
...child	0.28	0.45	0	0	0	1	1	735
...sibling	0.23	0.42	0	0	0	1	1	735
...niece/nephew	0.33	0.47	0	0	0	1	1	735
...cousin	0.09	0.29	0	0	0	0	1	735
Target student is in school	0.92	0.28	0	1	1	1	1	735
Target student years of education	11.74	2.18	8	9	12	14	24	683
<b>Panel 2: Migrant-student pairs with female target student</b>								
Migrant is female	0.49	0.50	0	0	0	1	1	401
Migrant age	37.41	9.53	17	26	36.5	50.5	74	390
Migrant is married	0.64	0.48	0	0	1	1	1	400
Migrant hh size in US	4.57	2.08	1	2	4	7	13	401
Migrant years of education	9.21	4.72	0	0	10	14	20	398
Migrant years in US	11.42	6.53	0	4	11	22	38	400
Migrant annual remittance to target hh	2,871	3,485	0	0	1,800	7,380	26,900	394
Migrant annual remittances to other hhs	1,193	1,913	0	0	165	3,800	11,500	399
Target student is female	1.00	0.00	1	1	1	1	1	401
Target student age	18.60	3.57	11	15	18	23	35	396
<i>Target student is migrant's...</i>								
...child	0.30	0.46	0	0	0	1	1	401
...sibling	0.23	0.42	0	0	0	1	1	401
...niece/nephew	0.33	0.47	0	0	0	1	1	401
...cousin	0.08	0.28	0	0	0	0	1	401
Target student is in school	0.91	0.29	0	1	1	1	1	401
Target student years of education	11.86	2.14	9	9	12	15	19	366
<b>Panel 3: Migrant-student pairs with male target student</b>								
Migrant is female	0.51	0.50	0	0	1	1	1	334
Migrant age	37.12	9.61	18	25	36	50	71	327
Migrant is married	0.59	0.49	0	0	1	1	1	333
Migrant hh size in US	4.46	2.10	1	2	4	7	13	334
Migrant years of education	8.89	4.66	0	1	9	14	21	326
Migrant years in US	10.66	6.08	0	5	9	20	37	333
Migrant annual remittance to target hh	2,638	3,326	0	0	1,800	6,725	31,620	330
Migrant annual remittances to other hhs	1,184	2,203	0	0	0	3,650	15,600	331
Target student is female	0.00	0.00	0	0	0	0	0	334
Target student age	18.40	3.20	14	15	17	22	40	328
<i>Target student is migrant's...</i>								
...child	0.27	0.44	0	0	0	1	1	334
...sibling	0.22	0.42	0	0	0	1	1	334
...niece/nephew	0.34	0.47	0	0	0	1	1	334
...cousin	0.10	0.31	0	0	0	1	1	334
Target student is in school	0.93	0.25	0	1	1	1	1	334
Target student years of education	11.60	2.22	8	9	11	14	24	317

Notes: Sample is all migrant-student pairs with completed migrant follow-up surveys. Variables all come from migrant baseline survey. Sample size varies slightly with missing values for each variable.

**Appendix Table 4A: Baseline balance: El Salvador follow-up sample**

	<i>Means</i>				<i>P-values</i>				<i>N</i>
	Control	No match	1:1 match	3:1 match	C = NM = 1:1 = 3:1	C = NM	C = 1:1	C = 3:1	
<b><i>Panel 1: Migrant-student pairs with female target student</i></b>									
Migrant is female	0.48	0.46	0.52	0.52	0.683	0.622	0.371	0.320	387
Migrant age	36.88	37.21	37.42	36.18	0.619	0.534	0.542	0.638	375
Migrant is married	0.61	0.55	0.68	0.61	0.479	0.205	0.588	0.836	385
Migrant hh size in US	4.63	4.54	4.71	4.41	0.693	0.821	0.691	0.236	387
Migrant years of education	9.29	8.99	8.91	10.21	0.287	0.542	0.450	0.197	384
Migrant years in US	11.24	11.74	11.19	12.15	0.794	0.383	0.960	0.677	386
Migrant annual remittance to target hh	3,046	2,243	2,580	2,955	0.458	0.126	0.506	0.865	380
Migrant annual remittances to other hhs	1,110	1,114	1,193	1,315	0.757	0.781	0.503	0.368	385
Target student age	18.42	18.21	18.58	19.35	0.091	0.755	0.535	0.036	380
<i>Target student is migrant's...</i>									
...child	0.25	0.18	0.35	0.25	0.070	0.108	0.213	0.913	387
...sibling	0.25	0.30	0.21	0.31	0.464	0.254	0.847	0.257	387
...niece/nephew	0.35	0.40	0.33	0.28	0.368	0.257	0.862	0.278	387
...cousin	0.08	0.08	0.08	0.09	0.982	0.934	0.724	0.995	387
Target student is in school	0.93	0.90	0.91	0.93	0.906	0.569	0.523	0.770	387
Target student years of education	11.79	11.63	11.88	12.37	0.112	0.514	0.827	0.065	357
<b><i>Panel 2: Migrant-student pairs with male target student</i></b>									
Migrant is female	0.44	0.52	0.55	0.54	0.391	0.457	0.138	0.219	341
Migrant age	36.60	36.40	36.22	38.06	0.705	0.895	0.554	0.452	334
Migrant is married	0.58	0.54	0.68	0.58	0.495	0.519	0.240	0.725	339
Migrant hh size in US	4.45	4.45	4.11	4.38	0.609	0.759	0.313	0.959	341
Migrant years of education	8.93	8.53	8.57	9.40	0.938	0.740	0.974	0.715	333
Migrant years in US	10.44	10.63	11.00	11.64	0.319	0.847	0.380	0.092	340
Migrant annual remittance to target hh	2,856	3,003	2,235	2,187	0.223	0.813	0.150	0.200	333
Migrant annual remittances to other hhs	1,434	980	867	1,368	0.310	0.197	0.121	0.830	336
Target student age	18.22	18.71	18.79	18.05	0.106	0.047	0.098	0.806	333
<i>Target student is migrant's...</i>									
...child	0.28	0.28	0.19	0.26	0.193	0.774	0.032	0.534	340
...sibling	0.21	0.32	0.23	0.20	0.443	0.123	0.706	0.851	340
...niece/nephew	0.25	0.23	0.45	0.38	0.006	0.746	0.002	0.078	340
...cousin	0.16	0.16	0.08	0.09	0.175	0.556	0.233	0.209	340
Target student is in school	0.91	0.90	0.96	0.95	0.174	0.284	0.303	0.413	341
Target student years of education	11.80	11.34	12.19	11.48	0.167	0.326	0.139	0.471	321

Notes: Sample is all migrant-student pairs with completed El Salvador follow-up surveys. Variables all come from migrant baseline survey. Sample size varies slightly with missing values for each variable. P-values come from regressions of each baseline variable on the treatment variables, including stratification cell fixed effects for week and location of baseline survey, with standard errors clustered at the level of the day and location of the baseline survey.

**Appendix Table 4B: Baseline balance: Full sample**

	<i>Means</i>				<i>C = NM = 1:1 = 3:1</i>	<i>P-values</i>			<i>N</i>
	Control	No match	1:1 match	3:1 match		<i>C = NM</i>	<i>C = 1:1</i>	<i>C = 3:1</i>	
<b><i>Panel 1: All migrant-student pairs</i></b>									
Migrant is female	0.47	0.48	0.51	0.54	0.233	0.651	0.273	0.052	991
Migrant age	36.63	36.42	36.67	37.53	0.665	0.789	0.678	0.298	963
Migrant is married	0.58	0.59	0.65	0.61	0.535	0.849	0.208	0.552	986
Migrant hh size in US	4.48	4.47	4.37	4.18	0.466	0.810	0.469	0.246	990
Migrant years of education	9.32	9.11	9.21	9.16	0.970	0.714	0.886	0.648	976
Migrant years in US	10.87	11.13	10.97	12.15	0.147	0.575	0.804	0.028	987
Migrant annual remittance to target hh	2,838	2,419	2,520	2,717	0.372	0.150	0.263	0.763	973
Migrant annual remittances to other hhs	1,223	1,021	996	1,147	0.635	0.320	0.269	0.748	983
Target student is female	0.56	0.56	0.50	0.46	0.038	0.830	0.142	0.033	991
Target student age	18.48	18.65	18.65	18.55	0.693	0.313	0.352	0.409	967
<i>Target student is migrant's...</i>									
...child	0.25	0.23	0.27	0.29	0.481	0.295	0.842	0.611	989
...sibling	0.23	0.28	0.21	0.23	0.227	0.072	0.608	0.630	989
...niece/nephew	0.32	0.33	0.37	0.32	0.520	0.548	0.150	0.760	989
...cousin	0.12	0.13	0.10	0.09	0.446	0.543	0.579	0.334	989
Target student is in school	0.91	0.90	0.93	0.94	0.434	0.280	0.680	0.472	990
Target student years of education	11.80	11.47	11.98	11.92	0.101	0.181	0.210	0.244	913
<b><i>Panel 2: Migrant-student pairs with female target student</i></b>									
Migrant is female	0.46	0.45	0.51	0.53	0.568	0.731	0.308	0.226	522
Migrant age	37.32	36.89	36.98	36.85	0.993	0.907	0.785	0.990	508
Migrant is married	0.61	0.58	0.66	0.63	0.715	0.367	0.644	0.864	519
Migrant hh size in US	4.53	4.46	4.63	4.11	0.246	0.783	0.935	0.058	521
Migrant years of education	9.39	9.18	9.31	9.53	0.930	0.646	0.684	0.866	517
Migrant years in US	11.33	11.53	10.69	12.60	0.183	0.709	0.347	0.227	520
Migrant annual remittance to target hh	2,765	2,164	2,775	3,093	0.172	0.106	0.938	0.418	513
Migrant annual remittances to other hhs	1,114	1,135	1,114	1,132	0.973	0.806	0.746	0.724	519
Target student age	18.63	18.63	18.70	18.90	0.779	0.990	0.640	0.355	511
<i>Target student is migrant's...</i>									
...child	0.25	0.20	0.34	0.28	0.107	0.239	0.127	0.657	521
...sibling	0.22	0.26	0.20	0.27	0.387	0.214	0.776	0.242	521
...niece/nephew	0.37	0.40	0.31	0.29	0.313	0.483	0.339	0.262	521
...cousin	0.08	0.11	0.11	0.09	0.825	0.505	0.748	0.787	521
Target student is in school	0.91	0.91	0.89	0.93	0.781	0.958	0.468	0.715	521
Target student years of education	11.89	11.54	11.97	12.27	0.018	0.138	0.772	0.065	476
<b><i>Panel 3: Migrant-student pairs with male target student</i></b>									
Migrant is female	0.48	0.53	0.50	0.55	0.853	0.541	0.716	0.399	469
Migrant age	35.76	35.82	36.35	38.14	0.288	0.687	0.866	0.105	455
Migrant is married	0.55	0.60	0.63	0.60	0.524	0.493	0.165	0.291	467
Migrant hh size in US	4.41	4.48	4.11	4.25	0.516	0.587	0.289	0.912	469
Migrant years of education	9.24	9.01	9.12	8.84	0.907	0.961	0.898	0.522	459
Migrant years in US	10.28	10.62	11.25	11.77	0.125	0.642	0.203	0.028	467
Migrant annual remittance to target hh	2,932	2,750	2,270	2,399	0.311	0.746	0.094	0.194	460
Migrant annual remittances to other hhs	1,366	874	878	1,159	0.292	0.099	0.117	0.402	464
Target student age	18.29	18.68	18.60	18.23	0.327	0.081	0.251	0.471	456
<i>Target student is migrant's...</i>									
...child	0.25	0.27	0.20	0.30	0.408	0.749	0.135	0.795	468
...sibling	0.23	0.29	0.23	0.20	0.461	0.166	0.934	0.817	468
...niece/nephew	0.25	0.25	0.43	0.35		0.943	0.002	0.090	468
...cousin	0.17	0.15	0.10	0.09	0.176	0.659	0.158	0.127	468
Target student is in school	0.92	0.88	0.97	0.95	0.027	0.048	0.171	0.795	469
Target student years of education	11.68	11.39	12.00	11.61	0.470	0.649	0.178	0.826	437

Notes: Sample is all migrant-student pairs interviewed at baseline. Variables all come from migrant baseline survey. Sample size varies slightly with missing values for each variable. P-values come from regressions of each baseline variable on the treatment variables, including stratification cell fixed effects for week and location of baseline survey, with standard errors clustered at the level of the day and location of the baseline survey.

**Appendix Table 4C: Baseline balance: Migrant follow-up sample**

	<i>Means</i>				<i>P-values</i>				<i>N</i>
	Control	No match	1:1 match	3:1 match	C = NM = 1:1 = 3:1	C = NM	C = 1:1	C = 3:1	
<b><i>Panel 1: All migrant-student pairs</i></b>									
Migrant is female	0.49	0.47	0.51	0.52	0.662	0.781	0.475	0.306	735
Migrant age	37.14	37.16	36.98	37.87	0.966	0.793	0.831	0.788	717
Migrant is married	0.61	0.60	0.64	0.62	0.937	0.633	0.765	0.981	733
Migrant hh size in US	4.58	4.67	4.45	4.34	0.358	0.332	0.533	0.404	735
Migrant years of education	9.06	9.17	9.04	9.01	0.989	0.813	0.996	0.889	724
Migrant years in US	10.75	11.33	11.12	11.34	0.807	0.383	0.640	0.483	733
Migrant annual remittance to target hh	3,005	2,445	2,670	2,743	0.438	0.108	0.343	0.587	724
Migrant annual remittances to other hhs	1,321	1,007	1,035	1,275	0.489	0.205	0.212	0.910	730
Target student is female	0.57	0.58	0.54	0.48	0.107	0.934	0.336	0.031	735
Target student age	18.44	18.57	18.52	18.57	0.869	0.448	0.585	0.557	724
<i>Target student is migrant's...</i>									
...child	0.27	0.25	0.30	0.33	0.411	0.324	0.573	0.404	735
...sibling	0.23	0.26	0.20	0.21	0.512	0.385	0.344	0.617	735
...niece/nephew	0.32	0.34	0.37	0.31	0.572	0.514	0.210	0.950	735
...cousin	0.09	0.12	0.07	0.09	0.501	0.164	0.762	0.799	735
Target student is in school	0.91	0.88	0.94	0.94	0.247	0.371	0.196	0.218	735
Target student years of education	11.77	11.39	11.92	11.85	0.224	0.194	0.377	0.582	683
<b><i>Panel 2: Migrant-student pairs with female target student</i></b>									
Migrant is female	0.51	0.42	0.49	0.53	0.834	0.591	0.906	0.512	401
Migrant age	37.19	38.10	37.60	36.88	0.769	0.679	0.749	0.592	390
Migrant is married	0.63	0.61	0.64	0.67	0.924	0.536	0.985	0.805	400
Migrant hh size in US	4.61	4.70	4.55	4.37	0.511	0.419	0.792	0.351	401
Migrant years of education	9.18	9.08	9.14	9.49	0.885	0.799	0.967	0.560	398
Migrant years in US	10.88	12.13	11.27	11.81	0.676	0.239	0.818	0.702	400
Migrant annual remittance to target hh	3,161	2,137	3,030	2,974	0.095	0.038	0.954	0.972	394
Migrant annual remittances to other hhs	1,182	1,196	1,164	1,239	0.987	0.948	0.949	0.732	399
Target student age	18.46	18.41	18.64	19.06	0.607	0.790	0.648	0.268	396
<i>Target student is migrant's...</i>									
...child	0.28	0.22	0.39	0.32	0.073	0.136	0.139	0.732	401
...sibling	0.23	0.23	0.18	0.28	0.476	0.734	0.473	0.249	401
...niece/nephew	0.33	0.42	0.29	0.26	0.141	0.147	0.465	0.285	401
...cousin	0.08	0.09	0.09	0.08	0.883	0.479	0.689	0.959	401
Target student is in school	0.91	0.89	0.91	0.91	0.972	0.639	0.866	0.954	401
Target student years of education	11.87	11.49	11.90	12.22	0.095	0.100	0.970	0.243	366
<b><i>Panel 3: Migrant-student pairs with male target student</i></b>									
Migrant is female	0.48	0.55	0.52	0.52	0.873	0.471	0.622	0.614	334
Migrant age	37.07	35.89	36.24	38.76	0.328	0.196	0.493	0.428	327
Migrant is married	0.59	0.58	0.64	0.57	0.755	0.663	0.466	0.992	333
Migrant hh size in US	4.53	4.64	4.34	4.30	0.753	0.556	0.517	0.701	334
Migrant years of education	8.89	9.31	8.91	8.55	0.662	0.330	0.806	0.559	326
Migrant years in US	10.57	10.22	10.94	10.92	0.783	0.512	0.520	0.712	333
Migrant annual remittance to target hh	2,799	2,869	2,247	2,537	0.409	0.960	0.167	0.463	330
Migrant annual remittances to other hhs	1,509	750	880	1,308	0.193	0.059	0.157	0.685	331
Target student age	18.41	18.78	18.38	18.11	0.468	0.125	0.718	0.808	328
<i>Target student is migrant's...</i>									
...child	0.25	0.29	0.20	0.34	0.350	0.777	0.288	0.360	334
...sibling	0.24	0.30	0.23	0.14	0.230	0.438	0.648	0.120	334
...niece/nephew	0.30	0.23	0.48	0.36		0.248	0.008	0.296	334
...cousin	0.11	0.15	0.05	0.10	0.172	0.063	0.347	0.727	334
Target student is in school	0.91	0.88	0.98	0.97	0.018	0.274	0.012	0.095	334
Target student years of education	11.64	11.25	11.95	11.51	0.539	0.594	0.291	0.978	317

Notes: Sample is all migrant-student pairs for completed migrant follow-up surveys. Variables all come from migrant baseline survey. Sample size varies slightly with missing values for each variable. P-values come from regressions of each baseline variable on the treatment variables, including stratification cell fixed effects for week and location of baseline survey, with standard errors clustered at the level of the day and location of the baseline survey.

**Appendix Table 5: Attrition**

	(1)	(2)	(3)
	El Salvador follow-up complete	Migrant follow-up complete	Both follow-ups complete
<b>Panel 1: All target students</b>			
3:1 match	-0.0345 [0.0355]	0.0178 [0.0365]	-0.000549 [0.0426]
1:1 match	-0.0240 [0.0363]	-0.0459 [0.0368]	-0.0577 [0.0422]
No match	-0.0266 [0.0374]	-0.00370 [0.0390]	-0.0464 [0.0468]
<i>P-values for tests of equality of coefficients</i>			
3:1 = 1:1	0.803	0.089	0.184
3:1 = No match	0.871	0.634	0.376
1:1 = No match	0.952	0.302	0.816
3:1 = 1:1 = No match	0.969	0.209	0.397
Observations	991	991	991
R-squared	0.03	0.04	0.022
Control group mean	0.758	0.758	0.614
<b>Panel 2: Female target students</b>			
3:1 match	-0.0160 [0.0512]	0.0221 [0.0490]	0.00986 [0.0583]
1:1 match	-0.0412 [0.0535]	-0.0145 [0.0523]	-0.0425 [0.0630]
No match	-0.0649 [0.0491]	-0.0297 [0.0490]	-0.0882 [0.0555]
<i>P-values for tests of equality of coefficients</i>			
3:1 = 1:1	0.673	0.533	0.454
3:1 = No match	0.436	0.341	0.125
1:1 = No match	0.692	0.807	0.522
3:1 = 1:1 = No match	0.737	0.611	0.306
Observations	522	522	522
R-squared	0.052	0.073	0.049
Control group mean	0.772	0.772	0.624
<b>Panel 3: Male target students</b>			
3:1 match	-0.0651 [0.0513]	0.0155 [0.0445]	-0.0143 [0.0582]
1:1 match	-0.0133 [0.0462]	-0.0881* [0.0516]	-0.0877 [0.0535]
No match	0.00733 [0.0566]	-0.00127 [0.0595]	-0.0245 [0.0688]
<i>P-values for tests of equality of coefficients</i>			
3:1 = 1:1	0.370	0.063	0.187
3:1 = No match	0.308	0.797	0.894
1:1 = No match	0.732	0.217	0.312
3:1 = 1:1 = No match	0.554	0.164	0.301
Observations	469	469	469
R-squared	0.06	0.066	0.043
Control group mean	0.741	0.741	0.601

Notes: Robust standard errors clustered at the level of the day and location of the baseline survey in brackets. Sample is all migrant-student pairs interviewed at baseline. All regressions include stratification cell fixed effects for the week and location of the baseline survey.

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Appendix Table 6A: Takeup of EduRemesa by treatment: Full sample**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	EduRemesa sent	Number of EduRemesas sent	Total amount sent by migrant	Total amount sent by migrant plus subsidy	EduRemesa sent to target student	Total amount sent by migrant to target student	Total amount sent by migrant to target student plus subsidy
<b>Panel 1: All target students</b>							
3:1 match	0.145*** [0.0245]	0.188*** [0.0354]	26.24*** [4.997]	105.0*** [19.47]	0.120*** [0.0216]	16.92*** [3.131]	67.25*** [11.96]
1:1 match	0.0520*** [0.0153]	0.0633*** [0.0194]	17.03*** [5.446]	36.20*** [11.62]	0.0443*** [0.0144]	13.55*** [4.508]	26.93*** [9.207]
No match	-0.000802 [0.00735]	0.00213 [0.00988]	0.242 [1.853]	1.838 [5.245]	-0.00235 [0.00704]	-0.130 [1.414]	-0.380 [3.701]
<i>P-values for tests of equality of coefficients</i>							
3:1 = 1:1	0.001	0.002	0.232	0.004	0.005	0.541	0.010
3:1 = No match	0.000	0.000	0.000	0.000	0.000	0.000	0.000
1:1 = No match	0.001	0.004	0.004	0.005	0.003	0.003	0.004
3:1 = 1:1 = No match	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Observations	991	991	991	991	991	991	991
R-squared	0.103	0.089	0.063	0.08	0.091	0.059	0.078
Control group mean	0	0	0	0	0	0	0
<b>Panel 2: Female target students</b>							
3:1 match	0.164*** [0.0364]	0.203*** [0.0471]	30.61*** [7.112]	120.0*** [28.19]	0.164*** [0.0364]	24.99*** [5.461]	97.47*** [21.39]
1:1 match	0.0754*** [0.0267]	0.0792*** [0.0284]	22.29*** [8.296]	46.46*** [18.01]	0.0754*** [0.0267]	21.62*** [8.121]	43.77*** [16.87]
No match	0.00193 [0.0110]	0.00498 [0.0145]	1.683 [2.902]	5.001 [8.999]	0.00193 [0.0110]	1.009 [2.413]	2.305 [6.613]
<i>P-values for tests of equality of coefficients</i>							
3:1 = 1:1	0.058	0.025	0.428	0.024	0.058	0.727	0.055
3:1 = No match	0.000	0.000	0.000	0.000	0.000	0.000	0.000
1:1 = No match	0.006	0.008	0.010	0.014	0.006	0.010	0.011
3:1 = 1:1 = No match	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Observations	522	522	522	522	522	522	522
R-squared	0.128	0.127	0.097	0.116	0.128	0.095	0.121
Control group mean	0	0	0	0	0	0	0
<b>Panel 3: Male target students</b>							
3:1 match	0.121*** [0.0300]	0.168*** [0.0528]	20.77*** [7.012]	86.07*** [27.23]	0.0771*** [0.0249]	8.906*** [3.224]	37.13*** [12.39]
1:1 match	0.0261 [0.0161]	0.0470* [0.0266]	11.21* [6.493]	25.96* [14.80]	0.0106 [0.0126]	4.604 [3.619]	7.761 [7.801]
No match	-0.00714 [0.0122]	-0.00565 [0.0195]	-2.380 [3.617]	-3.963 [10.57]	-0.0112 [0.0116]	-2.322 [2.050]	-6.331 [6.109]
<i>P-values for tests of equality of coefficients</i>							
3:1 = 1:1	0.005	0.041	0.334	0.054	0.032	0.408	0.065
3:1 = No match	0.000	0.001	0.001	0.002	0.003	0.003	0.004
1:1 = No match	0.082	0.113	0.113	0.100	0.192	0.144	0.158
3:1 = 1:1 = No match	0.001	0.003	0.004	0.003	0.009	0.010	0.011
Observations	469	469	469	469	469	469	469
R-squared	0.122	0.098	0.079	0.093	0.111	0.084	0.1
Control group mean	0	0	0	0	0	0	0

Notes: Robust standard errors clustered at the level of the day and location of the baseline survey in brackets. Sample is all migrant-student pairs interviewed at baseline. All regressions include stratification cell fixed effects for the week and location of the baseline survey. Dependent variables are from EduRemesa administrative data.

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Appendix Table 6B: Takeup of EduRemesa by treatment: Migrant follow-up sample**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	EduRemesa sent	Number of EduRemesas sent	Total amount sent by migrant	Total amount sent by migrant plus subsidy	EduRemesa sent to target student	Total amount sent by migrant to target student	Total amount sent by migrant to target student plus subsidy
<b>Panel 1: All target students</b>							
3:1 match	0.163*** [0.0302]	0.221*** [0.0453]	30.41*** [6.643]	122.1*** [25.95]	0.137*** [0.0267]	19.08*** [3.912]	76.12*** [14.85]
1:1 match	0.0718*** [0.0215]	0.0923*** [0.0278]	25.09*** [7.838]	54.58*** [16.90]	0.0611*** [0.0202]	19.25*** [6.371]	37.97*** [13.12]
No match	-0.00184 [0.00997]	0.00417 [0.0137]	1.157 [2.572]	4.503 [7.475]	-0.00414 [0.00990]	0.104 [1.931]	-0.359 [5.275]
<i>P-values for tests of equality of coefficients</i>							
3:1 = 1:1	0.010	0.012	0.596	0.021	0.025	0.981	0.055
3:1 = No match	0.000	0.000	0.000	0.000	0.000	0.000	0.000
1:1 = No match	0.001	0.003	0.003	0.003	0.003	0.002	0.003
3:1 = 1:1 = No match	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Observations	735	735	735	735	735	735	735
R-squared	0.123	0.107	0.079	0.095	0.111	0.077	0.096
Control group mean	0	0	0	0	0	0	0
<b>Panel 2: Female target students</b>							
3:1 match	0.168*** [0.0437]	0.225*** [0.0674]	34.15*** [11.15]	135.1*** [44.10]	0.168*** [0.0437]	25.31*** [6.642]	99.77*** [25.69]
1:1 match	0.0947*** [0.0332]	0.102*** [0.0369]	29.35*** [10.44]	62.64*** [23.84]	0.0947*** [0.0332]	27.78*** [10.01]	56.38*** [20.92]
No match	0.00317 [0.0142]	0.0122 [0.0220]	3.336 [4.418]	12.14 [15.14]	0.00317 [0.0142]	1.423 [3.101]	4.485 [8.821]
<i>P-values for tests of equality of coefficients</i>							
3:1 = 1:1	0.188	0.087	0.726	0.093	0.188	0.832	0.177
3:1 = No match	0.001	0.001	0.001	0.001	0.001	0.000	0.000
1:1 = No match	0.006	0.009	0.009	0.015	0.006	0.007	0.010
3:1 = 1:1 = No match	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Observations	401	401	401	401	401	401	401
R-squared	0.139	0.146	0.118	0.140	0.139	0.109	0.134
Control group mean	0	0	0	0	0	0	0
<b>Panel 3: Male target students</b>							
3:1 match	0.149*** [0.0394]	0.209*** [0.0680]	25.26*** [9.115]	104.8*** [34.86]	0.102*** [0.0332]	11.88*** [4.432]	49.34*** [16.71]
1:1 match	0.0342 [0.0273]	0.0685 [0.0442]	17.21 [10.94]	37.38 [24.61]	0.0116 [0.0221]	6.734 [6.202]	9.088 [13.68]
No match	-0.0189 [0.0205]	-0.0197 [0.0310]	-4.525 [5.819]	-10.60 [16.60]	-0.0258 [0.0200]	-4.349 [3.459]	-13.55 [10.67]
<i>P-values for tests of equality of coefficients</i>							
3:1 = 1:1	0.020	0.081	0.584	0.116	0.048	0.534	0.100
3:1 = No match	0.000	0.002	0.002	0.003	0.004	0.005	0.006
1:1 = No match	0.125	0.119	0.118	0.112	0.204	0.147	0.175
3:1 = 1:1 = No match	0.002	0.007	0.007	0.008	0.014	0.014	0.018
Observations	334	334	334	334	334	334	334
R-squared	0.158	0.127	0.111	0.121	0.158	0.128	0.149
Control group mean	0	0	0	0	0	0	0

Notes: Robust standard errors clustered at the level of the day and location of the baseline survey in brackets. Sample is all migrant-student pairs with completed migrant follow-up surveys. All regressions include stratification cell fixed effects for the week and location of the baseline survey. Dependent variables are from EduRemesa administrative data.

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Appendix Table 7: Takeup of EduRemesa by grades treatment**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	EduRemesa sent	Number of EduRemesas sent	Total amount sent by migrant	Total amount sent by migrant plus subsidy	EduRemesa sent to target student	Total amount sent by migrant to target student	Total amount sent by migrant to target student plus subsidy
<i>Panel 1: All target students</i>							
3:1 match & no grades	0.180*** [0.0444]	0.227*** [0.0670]	31.43*** [8.802]	123.9*** [34.59]	0.138*** [0.0395]	20.83*** [5.768]	81.59*** [22.38]
1:1 match & no grades	0.126*** [0.0315]	0.157*** [0.0414]	44.33*** [12.06]	95.28*** [24.82]	0.111*** [0.0309]	35.10*** [10.00]	71.39*** [20.10]
No match & no grades	0.00794 [0.0123]	0.0139 [0.0171]	2.842 [3.045]	9.908 [9.146]	0.00655 [0.0116]	1.903 [2.197]	5.431 [6.058]
3:1 match & grades	0.190*** [0.0480]	0.269*** [0.0670]	38.55*** [9.677]	155.1*** [39.13]	0.164*** [0.0415]	22.27*** [5.631]	89.16*** [22.64]
1:1 match & grades	0.0100 [0.0140]	0.0101 [0.0163]	1.620 [2.797]	3.453 [7.709]	0.00887 [0.0132]	1.576 [2.489]	2.366 [5.786]
No match & grades	-0.0109 [0.0115]	-0.00508 [0.0146]	-1.044 [2.659]	-1.771 [8.051]	-0.00902 [0.0106]	-1.356 [2.053]	-3.986 [5.686]
<i>P-values for tests of equality of coefficients in no grades and grades treatment:</i>							
3:1 match group	0.876	0.658	0.570	0.537	0.650	0.853	0.810
1:1 match group	0.001	0.001	0.001	0.000	0.002	0.001	0.001
No match group	0.163	0.313	0.193	0.231	0.207	0.114	0.133
Observations	728	728	728	728	728	728	728
R-squared	0.147	0.125	0.107	0.115	0.128	0.106	0.114
Control group mean	0	0	0	0	0	0	0
<i>Panel 2: Female target students</i>							
3:1 match & no grades	0.195*** [0.0605]	0.204*** [0.0621]	33.94*** [10.25]	132.6*** [40.58]	0.195*** [0.0605]	32.38*** [9.991]	126.4*** [39.55]
1:1 match & no grades	0.176*** [0.0514]	0.186*** [0.0519]	54.74*** [17.21]	117.8*** [34.61]	0.176*** [0.0514]	52.98*** [17.23]	110.8*** [34.27]
No match & no grades	0.0236 [0.0165]	0.0362* [0.0201]	7.986* [4.340]	24.38* [13.25]	0.0236 [0.0165]	5.865 [3.804]	15.89 [10.42]
3:1 match & grades	0.160** [0.0693]	0.266** [0.105]	38.04** [14.93]	151.1** [60.94]	0.160** [0.0693]	22.57** [8.709]	89.21** [36.07]
1:1 match & grades	0.0228 [0.0320]	0.0302 [0.0358]	6.016 [6.253]	13.83 [17.17]	0.0228 [0.0320]	4.327 [5.708]	7.079 [14.19]
No match & grades	-0.00589 [0.0189]	-0.00273 [0.0247]	0.475 [4.696]	0.884 [15.13]	-0.00589 [0.0189]	-0.352 [3.875]	-2.427 [11.27]
<i>P-values for tests of equality of coefficients in no grades and grades treatment:</i>							
3:1 match group	0.700	0.598	0.808	0.790	0.700	0.442	0.483
1:1 match group	0.009	0.008	0.005	0.003	0.009	0.006	0.003
No match group	0.176	0.141	0.112	0.118	0.176	0.144	0.153
Observations	387	387	387	387	387	387	387
R-squared	0.169	0.162	0.151	0.150	0.169	0.159	0.165
Control group mean	0	0	0	0	0	0	0
<i>Panel 3: Male target students</i>							
3:1 match & no grades	0.155** [0.0621]	0.245** [0.122]	28.27* [15.18]	111.7* [59.70]	0.0733 [0.0452]	8.218 [5.054]	31.84 [19.25]
1:1 match & no grades	0.0653 [0.0407]	0.115* [0.0670]	31.03* [17.08]	66.76* [37.27]	0.0318 [0.0340]	14.12 [9.705]	24.06 [20.93]
No match & no grades	-0.00163 [0.0160]	-0.00496 [0.0255]	-1.285 [4.636]	-1.062 [13.55]	-0.00811 [0.0165]	-1.326 [2.767]	-3.291 [8.611]
3:1 match & grades	0.202*** [0.0602]	0.248*** [0.0784]	33.34*** [11.37]	140.6*** [44.97]	0.152*** [0.0554]	18.33** [7.589]	77.28** [30.11]
1:1 match & grades	-0.00362 [0.0148]	-0.00733 [0.0238]	-2.090 [4.265]	-3.431 [12.99]	-0.00960 [0.0131]	-1.991 [2.507]	-5.148 [7.165]
No match & grades	-0.0246 [0.0256]	-0.0193 [0.0328]	-5.062 [6.227]	-13.57 [20.87]	-0.0150 [0.0215]	-3.367 [3.809]	-9.938 [12.85]
<i>P-values for tests of equality of coefficients in no grades and grades treatment:</i>							
3:1 match group	0.589	0.981	0.784	0.694	0.278	0.260	0.204
1:1 match group	0.093	0.081	0.072	0.075	0.241	0.124	0.187
No match group	0.400	0.711	0.584	0.593	0.791	0.643	0.664
Observations	341	341	341	341	341	341	341
R-squared	0.186	0.141	0.124	0.139	0.173	0.135	0.162
Control group mean	0	0	0	0	0	0	0

Notes: Robust standard errors clustered at the level of the day and location of the baseline survey in brackets. Sample is all migrant-student pairs with completed El Salvador follow-up surveys. All regressions include stratification cell fixed effects for the week and location of the baseline survey. Dependent variables are from EduRemesa administrative data.

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1



**Appendix Table 8: Target student education expenditures: interactions with grades treatment**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	<i>Dependent variable: Annualized target student expenditure on</i>								
	Total	Tuition	School supplies	Uniforms	Books	Transport	Food	Computer use	Other
<b>Panel 1: All target students</b>									
3:1 match & no grades	347.2** [171.1]	129.4*** [44.70]	-7.346 [9.334]	14.14* [7.494]	3.387 [9.713]	103.7* [53.17]	122.9* [71.78]	23.76 [38.25]	-42.78 [35.56]
1:1 match & no grades	2.681 [131.6]	97.33** [45.66]	-10.08 [9.030]	-5.372 [5.021]	0.968 [8.664]	23.44 [34.50]	0.241 [50.70]	-32.89 [30.07]	-70.96** [33.48]
No match & no grades	7.252 [144.5]	44.10 [45.52]	5.482 [10.37]	-6.593 [6.037]	-15.02** [7.232]	26.30 [39.73]	31.60 [61.11]	-23.15 [34.91]	-55.47* [33.37]
3:1 match & grades	256.7 [155.5]	81.95* [42.05]	0.714 [10.62]	-0.154 [8.128]	11.19 [11.17]	50.39 [46.87]	164.3** [77.21]	-23.43 [31.67]	-28.25 [28.12]
1:1 match & grades	147.6 [162.5]	68.14* [40.78]	-12.26 [9.292]	-12.14* [6.711]	9.203 [11.81]	48.56 [68.43]	97.82 [81.49]	-27.09 [33.22]	-24.64 [45.31]
No match & grades	32.07 [135.4]	89.51* [49.21]	-8.145 [9.239]	-9.059 [5.762]	-6.785 [7.916]	-27.84 [38.37]	43.28 [57.01]	-17.80 [29.97]	-31.09 [30.16]
<i>P-values for tests of equality of coefficients in no grades and grades treatment:</i>									
3:1 match group	0.666	0.413	0.519	0.153	0.579	0.418	0.665	0.308	0.613
1:1 match group	0.427	0.606	0.852	0.332	0.539	0.717	0.273	0.881	0.260
No match group	0.884	0.485	0.283	0.719	0.392	0.255	0.869	0.897	0.392
Observations	728	728	728	728	728	728	728	728	728
R-squared	0.034	0.054	0.033	0.056	0.034	0.044	0.047	0.039	0.052
Control group mean	1358	186.8	60.16	35.94	54.68	270.4	442.9	217.5	89.63
<b>Panel 2: Female target students</b>									
3:1 match & no grades	553.1** [253.3]	248.7*** [71.75]	3.244 [13.54]	20.43 [13.13]	1.299 [13.90]	183.3** [71.18]	173.3 [120.7]	16.08 [57.37]	-93.25** [42.99]
1:1 match & no grades	158.4 [206.2]	162.8** [68.57]	10.46 [19.62]	-8.952 [6.382]	9.519 [16.12]	94.88* [52.16]	30.99 [83.88]	-50.02 [48.68]	-91.33 [63.52]
No match & no grades	43.94 [195.3]	81.13 [66.92]	-2.851 [9.985]	-2.386 [8.381]	-15.54 [10.06]	42.34 [50.85]	74.84 [87.17]	-27.91 [60.09]	-105.7* [62.18]
3:1 match & grades	465.0** [229.5]	152.3* [78.34]	9.378 [15.08]	4.201 [12.80]	14.34 [16.52]	76.53 [64.60]	263.2** [116.6]	-30.34 [48.89]	-24.52 [47.22]
1:1 match & grades	-68.88 [262.3]	34.09 [61.09]	-12.99 [14.92]	-18.80** [7.194]	-0.538 [15.58]	30.90 [107.9]	52.07 [142.7]	-63.09 [50.72]	-90.52 [62.19]
No match & grades	-151.5 [222.0]	50.48 [68.22]	-2.066 [14.85]	-11.89* [6.264]	-9.320 [11.29]	-41.24 [51.80]	11.00 [82.86]	-74.46 [53.18]	-73.99 [48.70]
<i>P-values for tests of equality of coefficients in no grades and grades treatment:</i>									
3:1 match group	0.778	0.336	0.734	0.373	0.519	0.248	0.562	0.499	0.109
1:1 match group	0.427	0.110	0.324	0.216	0.607	0.555	0.895	0.826	0.986
No match group	0.429	0.734	0.963	0.292	0.632	0.189	0.519	0.517	0.515
Observations	387	387	387	387	387	387	387	387	387
R-squared	0.106	0.116	0.050	0.101	0.071	0.107	0.092	0.085	0.111
Control group mean	1412	173.6	56.35	34.59	57.22	279.4	454.7	245.6	110.9
<b>Panel 3: Male target students</b>									
3:1 match & no grades	10.74 [233.3]	-37.79 [63.29]	-23.55* [13.94]	1.890 [9.389]	-2.291 [13.73]	23.45 [61.29]	28.41 [90.38]	16.90 [50.43]	3.732 [44.30]
1:1 match & no grades	-226.0 [193.5]	11.46 [67.39]	-38.27*** [9.137]	-8.213 [8.468]	-9.525 [9.907]	-40.09 [52.22]	-62.84 [75.28]	-18.13 [37.04]	-60.42 [39.78]
No match & no grades	-129.1 [223.6]	1.739 [61.54]	3.325 [17.20]	-13.03 [7.902]	-12.89 [10.58]	-10.84 [65.47]	-46.40 [83.48]	-35.52 [34.02]	-15.48 [36.57]
3:1 match & grades	72.03 [232.2]	25.04 [63.90]	-11.12 [14.28]	-4.316 [9.405]	12.65 [16.31]	16.27 [71.40]	75.33 [90.45]	-17.83 [43.05]	-23.99 [33.15]
1:1 match & grades	328.8 [259.0]	87.63 [68.66]	-20.33 [13.51]	-7.677 [11.49]	21.20 [18.79]	61.11 [81.58]	135.6 [91.11]	10.14 [47.73]	41.14 [98.75]
No match & grades	120.0 [234.5]	129.2 [90.15]	-19.52 [12.35]	-5.874 [9.749]	-8.460 [13.13]	-19.57 [50.48]	37.64 [94.35]	16.65 [48.26]	-10.16 [48.41]
<i>P-values for tests of equality of coefficients in no grades and grades treatment:</i>									
3:1 match group	0.825	0.396	0.465	0.594	0.430	0.931	0.680	0.562	0.500
1:1 match group	0.0343	0.347	0.143	0.966	0.102	0.230	0.0479	0.583	0.292
No match group	0.333	0.187	0.226	0.512	0.749	0.898	0.425	0.292	0.901
Observations	341	341	341	341	341	341	341	341	341
R-squared	0.070	0.076	0.096	0.067	0.072	0.074	0.063	0.069	0.110
Control group mean	1287	204.0	65.15	37.69	51.36	258.7	427.4	180.8	61.88

Notes: Robust standard errors clustered at the level of the day and location of the baseline survey in brackets. Sample is all migrant-student pairs with completed El Salvador follow-up surveys. All regressions include stratification cell fixed effects for the week and location of the baseline survey.

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1