American Economic Journal: Applied Economics 2015, 7(2): 1–27
http://dx.doi.org/10.1257/app.20140010

Channeling Remittances to Education: A Field Experiment among Migrants from El Salvador†

By Kate Ambler, Diego Aycinena, and Dean Yang*

We implement a randomized experiment offering Salvadoran migrants matching funds for educational remittances, which are channeled directly to a beneficiary student in El Salvador chosen by the migrant. The matches lead to increased 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 educational investments: for each $1 received by beneficiaries, educational expenditures increase by $3.72. We find no shifting of expenditures away from other students, and no effect on remittances. (*JEL* F24, I21, I22, J13, O15, O19)

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). By contrast, developing country receipts of official foreign development assistance in 2012 amounted to just US$126 billion (Organisation for Economic Co-operation and Development (OECD) 2013). While migrant remittance flows are large in magnitude, they amount to only a minority of the total developed-country earnings of migrant workers from developing countries (Clemens, Montenegro, and Pritchett 2009; Clemens 2011; Yang 2011). The prospect that migrants might be encouraged to send even more remittances, and that these remittances might be better leveraged for the economic development of

*Ambler: Markets, Trade, and Institutions Division, International Food Policy Research Institute, 2033 K St. NW, Washington, DC 20006 (e-mail: k.ambler@cgiar.org); Aycinena: Facultad de Ciencias Económicas, Universidad Francisco Marroquin, 6 Calle final, zona 10 Guatemala, Guatemala 01010 (e-mail: diegoaa@ufm.edu); Yang: Department of Economics and Gerald R. Ford School of Public Policy, University of Michigan, 735 S. State Street, Ann Arbor, MI 48109, National Bureau of Economic Research (NBER), and the Bureau for Research and Economic Analysis of Development (BREAD) (e-mail: deanyang@umich.edu). This paper was previously circulated under the title "Subsidizing Remittances for Education." Jessica Snyder and Kevin Carney, our Innovations for Poverty Action project associates, deserve special thanks for superb work on all aspects of project implementation and data management. We greatly appreciate the support and feedback of Luis Alejos Marroquín, Oriana Bandiera, Ana de Bardi, Paul Dwyer, Daniel Gottschalk, Patricia Guinea de Solorzano, Gabriela Inchauste, Celia Medrano, Moy Pascual de Velasco, Enilson Solano, Eugenia Suay de Castrillo, and seminar participants at Columbia University, the Inter-American Development Bank, Queens College, Central Michigan University, NEUDC 2013, and the NBER Education Program meeting at the Chicago Fed (November 2013). This project would not have been possible without the collaboration of Fundación Empresarial para el Desarrollo Educativo (FEPADE), Viamericas Corporation, and the Salvadoran consulates in Woodbridge, Virginia and Washington, DC. This study was funded by the Inter-American Development Bank (contract C0016-11) and by the University of Michigan's Population Studies Center. Ambler acknowledges support from an NICHD training grant to the Population Studies Center at the University of Michigan (T32 HD007339).

†Go to http://dx.doi.org/10.1257/app.20140010 to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

migrant-origin countries, has led to substantial interest in academic and policy circles in development policies related to migrant remittances.[1]

A type of remittance-related program that has generated particular interest is a matching program to stimulate the use of remittances for investment in migrant-origin countries. Such programs have been implemented by home country governments, but to date have not been rigorously evaluated. For example, the Mexican "Tres por Uno" ("Three for One") program encourages Mexican migrants abroad to invest in their communities of origin. Each dollar invested by migrants is matched by $3 from the Mexican government. Migrants have contributed an average of $15 million annually since the program began (Hazán 2013). Additionally, there has been particular interest in enhancing the positive impacts of remittances on education. Existing research provides evidence of positive impacts of migration and remittances on educational outcomes in migrant-origin countries (Cox Edwards and Ureta 2003; Yang 2008; Theoharides 2014).

In this paper, we study a novel program that seeks to stimulate migrant remittances for educational purposes by providing subsidies in the form of matching funds. The program's target population is migrants from El Salvador and households in the home country that are connected to these migrants. We designed and offered migrants a new product, named "EduRemesa," which allowed migrants to channel funds towards the education of a student of their choice in El Salvador for the 2012 school year.[2] EduRemesa beneficiary students in El Salvador received an ATM card in their name, providing access to the funds, and were told that the funds were for expenditures related to their own education.

We conducted a randomized controlled trial to measure take-up and impacts of the EduRemesa at various levels of matching funds. We randomly assigned migrants (recruited in metro Washington, DC) to a control group or one of a number of treatment conditions which varied in the degree to which our research project matched 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. In a third treatment group ("no match"), migrants were simply offered the EduRemesa product without matching funds.

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, a "target" student whom they would be likely to fund *if offered the EduRemesa product*. Our measurement of impacts in El Salvador relies on surveys of these target students, and of a knowledgeable adult in the student's household.

Our first key finding is that take-up of the EduRemesa was monotonically related to the match level. Eighteen and a half percent of migrants in the 3:1 match

[1] Policy-oriented publications include Pew Hispanic Center (2002); Terry and Wilson (2005); World Bank (2006); and Fajnzylber and López (2007). Yang (2011) reviews recent research on the economics of migrant remittances.

[2] "Remesa" is the Spanish word for "remittance." The US dollar is the national currency of El Salvador.

treatment executed at least one EduRemesa transaction, compared to 6.9 percent in the 1:1 match group and exactly zero in the no match group. A total of 15.1 percent and 6.0 percent of migrants with the 3:1 and 1:1 matches, respectively, sent an EduRemesa to their target student.

These results indicate a high elasticity of demand for channeling remittances towards education with respect to the match rate. The finding of zero demand for the unsubsidized ("no match") EduRemesa contrasts with other studies that have found that migrants seek greater control over how their remittances are used. Our results may reveal that there is no "pure" or unsubsidized demand for control over the use of remittances for education in this context. However, it is also possible that take-up without the matching funds was dampened by nontrivial transaction costs of using the EduRemesa, as well as liquidity constraints on the part of migrants who had to pay the entire EduRemesa amount up front.

In addition, we find 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 matching funds. 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. 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). In addition, the 3:1 match leads to a higher likelihood of attending private school and to lower labor supply on the part of target students. 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 clearly a theoretical possibility, simply representing the case where education is a normal good while "all other goods" are collectively inferior goods.[3]

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, we can say that these crowded-in funds did not come from additional remittances sent by the migrant, since we find no 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.

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). Several papers find 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

[3]Crowd-in becomes more likely (and can be large in magnitude) if increasing consumption of education requires a discrete increase in expenditure. 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. 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. Our results are consistent with this possibility, in that the match 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.

child nutrition programs. Shi (2012) documents a flypaper effect in the context of a change in school fees in rural China. Conversely, 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 Colombian private school vouchers comes closest to finding crowd-in in response to a subsidy, 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.[4] 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.[5]

Our work is also related to the literature on cash transfers and education.[6] While existing studies have not examined impacts on education expenditures,[7] our results are reminiscent of certain findings in that literature. Baird, McIntosh, and Özler (2011) and Edmonds and Schady (2012) find large effects of unconditional cash transfers on school attendance, implying substantial elasticities of attendance with respect to income. Angelucci et al. (2010) find that the Mexican conditional cash transfer program increased secondary school enrollment only when eligible secondary school students had eligible primary school students in their family network. Transfers to households with a secondary school student appear to have crowded in transfers from other eligible households for secondary students' expenditures.

This paper is organized as follows. Section I describes the context and field experimental design. Section II provides an overview of the data and sample summary statistics. Section III presents the empirical results, and Section IV concludes and discusses policy implications.

I. 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. 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 percent and 32 percent respectively (Fundación Salvadoreña para el Desarrollo

[4] 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 3, Table 6), 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, Table 6).

[5] 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.

[6] Conditional cash transfer 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, Sengupta, and Todd (2005); Barrera-Osorio et al. (2011); Baird, McIntosh, and Özler (2011); and Glewwe and Kassouf (2012). Benhassine et al. (2013) show that labeling a cash transfer as intended for education can have similar positive effects on school participation as imposing conditionality. See Fiszbein et al. (2009) for a review.

[7] 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).

Económico y Social (FUSADES) 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 costs, 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. Online Appendix Table 1 shows figures from the 2010 *Encuesta de Hogares de Propositos Multiples* (EHPM), an annual, nationally representative, household survey in El Salvador. Eighty-nine percent of primary students and 79 percent of secondary students attend public schools. At the tertiary level, private institutions are much more important, with 60 percent of enrolled students attending a private institution. There are significant cost differences between attending public and private institutions. At both the secondary and tertiary level, average annual expenditures are roughly two-thirds higher in private than in public schools ($2,214 compared to $1,442 for secondary schools and $2,834 compared to $1,868 for tertiary schools).[8]

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. Baseline field work ran from November 2011 to early February 2012, overlapping with the period between the end of the 2011 school year and the start of the 2012 school year.[9] 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 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.[10] 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. Thus, 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.

[8] These figures are calculated using the education expenditure data for the control group only, collected during the follow-up survey for this project (to be described below).

[9] Public schools in El Salvador began the school year on January 23, 2012.

[10] Of those migrants approached, 24 percent participated. Of those who did not participate, 77 percent did not know an eligible student, 14 percent refused, 7 percent were not from El Salvador, and 2 percent did not participate for other reasons.

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, 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.[11] This was done at the beginning of the baseline survey, before treatment status was revealed, and so rules out differential selection of target students on the basis of treatment status. We refer to this student as the "target student" and to the student's household as the "target household." The rest of the baseline survey included questions about demographics, remittances, and the target student and household. Immediately following the baseline survey, project staff implemented the randomized treatments.[12]

Follow-up surveys were then 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. We also collected information about the target household via phone surveys, separately interviewing the target student and a knowledgeable adult in the household. Students provided information about their education and labor supply, while the adults provided information related to the education of other students in the household. We use 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),[13] an educational nongovernmental organization (NGO) in El Salvador, to develop the EduRemesa. Migrant participants 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 matching 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. On each day and at each 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: receiving no match offer, a 1:1 match offer, or a 3:1 match offer. This randomization was done at the individual

[11] Target students were not required to be currently enrolled in school.

[12] 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.

[13] In English, "Business Foundation for Educational Development."

level and was stratified within sequentially-numbered groups of six surveys. 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 online Appendix Figure 1. The following is a description of the information provided to the different groups.[14]

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 sending remittances directly to students in monthly installments. Project staff reviewed the handout with the migrant and gave it to the migrant to take home. The control group was provided with this information 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.

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

Migrants in this group were provided with the same information and handout given to the migrants in the control group. Following that discussion, migrants were then introduced to the EduRemesa. 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.[15] As part of the project, migrants were exempted from paying FEPADE's administrative fees, and they received a coupon with the informational pamphlet that informed them of this.[16] Migrants who took up the EduRemesa chose the beneficiary student and beneficiaries received 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. 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.

Treatment group 2: EduRemesa with a 1:1 match

Migrants in this group received the same information as migrants in treatment 1, but their coupon informed them that in addition to not having to pay the

[14] Copies of the materials provided to study participants can be found in online Appendix A.

[15] The choice of these fixed amounts was motivated by the amounts offered in the existing scholarship program run by FEPADE. FEPADE designs their scholarships with specific costs in mind and lacks the administrative capacity to implement a larger number of choices. Additionally, it was thought that these discrete options provided a simpler choice for the migrant participants. Ultimately, these concerns outweighed the analytical benefits of allowing migrants free choice for the amount of the EduRemesa.

[16] FEPADE typically charges administrative fees of 15 percent of the total amount for their scholarship programs. All migrants, even those in the no match group, were exempted from this administrative fee (the fees were paid with project funds).

administrative fees, they were being offered a 1:1 match on every dollar 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

This treatment was identical to treatment 2, except that the match rate was 3:1. In order to send a $300 EduRemesa, migrants would have to pay only $75 and the project would provide $225. A description of the amount to be sent by the migrant for each treatment and EduRemesa amount is in online Appendix Table 2.

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. Migrants who indicated that they were interested in sending an EduRemesa filled out a short application indicating the identity of the student beneficiary and were contacted by phone several days later to further discuss their interest and answer any questions. Project staff continued to follow up with all participants until they indicated that they were no longer interested. Migrants also had contact information for staff in the United States and FEPADE in El Salvador.

Migrants who decided to take-up the EduRemesa did so by sending the desired amount directly to FEPADE through a money transfer company, Viamericas Corporation, our other collaborating organization. Once FEPADE had received the funds, they contacted the beneficiary student to request a copy of the student's identification card needed to issue their ATM card. Once received, the student went to FEPADE's offices in San Salvador or a regional office in San Miguel to complete the paperwork. Students and their guardians were reimbursed for travel expenses. Students were required to sign a letter acknowledging the amount of their EduRemesa and the 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.[17]

II. Sample, Balance Tests, and Attrition

Study participants are migrants from El Salvador recruited in the Washington, DC area, and the target students identified by the migrants. Although migrants could send EduRemesas to any student they wished, all impacts will be measured on the sample of target students in both the control and treatment groups. Our "full" sample consists of 991 migrants interviewed at baseline. Of these, 728 target households (the "El Salvador follow-up" sample) completed the El Salvador follow-up survey (73 percent completion). Because the main outcomes of interest are collected in the El Salvador follow-up survey, this will be our main sample of interest. We also examine some impacts using the sample of 735 migrants who completed the migrant follow-up survey (the "migrant follow-up sample," 74 percent completion).[18]

[17] We also implemented a treatment to test the impact of offering a monitoring mechanism to migrants by giving some migrants the ability to receive a report of student grades after each grading period. This treatment and analysis of its impacts are described in online Appendix C.

[18] 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.

Amounts for 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. Target student education expenditures are reported by the target student. When the target student's report is missing, the adult's report is used, and in the few cases where both are missing the expenditures are imputed. For the total expenditures category, 4.0 percent of observations include an adult report for at least one of the categories and 0.8 percent include an imputed value for at least one of the categories. Remittance amounts are reported by migrants, and imputed when missing. A total of 19.6 percent of observations for overall total remittances are imputed. The substance of the results does not change when dropping imputed observations. Further information about the variable construction for all variables and imputation procedures can be found in online Appendix B.

Online Appendix Table 3A provides baseline summary statistics for the El Salvador follow-up sample. 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.[19] 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. Ninety-two percent of target students are in school at baseline. Online Appendix Tables 3B and 3C provide summary statistics for the full sample and the migrant follow-up sample respectively. No meaningful differences are apparent across the three samples at baseline. Additionally, because migrant participants were recruited at the Salvadoran consulate, a relevant concern is whether or not they are similar to the greater population of Salvadoran migrants. In online Appendix Table 4 we compare the migrants in our sample to Salvadoran-born, non-US citizens living in the Washington, DC metro area in the 2008–2010 American Community Survey (ACS) three year sample. Across a limited number of basic characteristics, the migrants in our sample are comparable to the migrants in the ACS, with the exception that the migrants in our sample have been in the United States for slightly less time on average (11.19 years compared to 12.93 years).

Because this is a randomized experiment, it is important to confirm that the randomization was successful in creating balanced treatment groups. Table 1 examines balance across the treatment groups in the El Salvador follow-up sample using the variables reported in online Appendix Table 3. Online Appendix Tables 5A and 5B examine balance in the full and migrant follow-up samples. The first four columns 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

[19] At baseline, 86 percent of migrants report sending nonzero remittances to the target household during the past year.

Table 1—Baseline Balance

	Means				p-values:	
	Control	No match	1:1 match	3:1 match	$C = NM = 1{:}1 = 3{:}1$	Obs.
Migrant is female	0.47	0.49	0.53	0.53	0.239	728
Migrant age	36.76	36.84	36.83	37.16	0.995	709
Migrant is married	0.60	0.55	0.68	0.59	0.168	724
Migrant hh size in United States	4.55	4.50	4.41	4.39	0.705	728
Migrant years of education	9.14	8.78	8.74	9.80	0.207	717
Migrant years in United States	10.90	11.24	11.09	11.88	0.492	726
Migrant annual remittance to target hh (USD)	2,964	2,582	2,408	2,556	0.586	713
Migrant annual remittances to other hhs (USD)	1,248	1,054	1,031	1,342	0.515	721
Target student is female	0.57	0.55	0.50	0.48	0.281	728
Target student age	18.34	18.44	18.68	18.69	0.524	713
Target student is migrant's						
...child	0.27	0.22	0.27	0.26	0.515	727
...sibling	0.23	0.31	0.22	0.25	0.147	727
...niece/nephew	0.30	0.33	0.39	0.33	0.233	727
...cousin	0.12	0.12	0.08	0.09	0.427	727
Target student is in school	0.92	0.90	0.93	0.94	0.562	728
Target student years of education	11.79	11.51	12.04	11.91	0.337	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. All money amounts are in US dollars.

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.[20]

Given the attrition from the full sample to the follow-up samples it is also important to test whether this attrition is related to treatment. Online Appendix Table 6 presents regression estimates of whether survey completion varies in each of the three treatment groups compared to the control group. The table also reports the p-values from tests of the equality of survey completion between the different treatment groups. The dependent variables are completion of the El Salvador follow-up, the migrant follow-up, and both surveys in columns 1, 2, and 3 respectively. The results show that attrition is not related to treatment status.

III. 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:

$$(1)\quad outcome_{ijt} = \beta_0 + \beta_1 3:1\,match_{ijt} + \beta_2 1:1 match_{ijt} + \beta_3 nomatch_{ijt} + \delta_{jt} + \varepsilon_{ijt},$$

[20] Pairwise comparisons of the means in each treatment group also result in few p-values that are below 0.10 or 0.05.

Table 2—Summary of EduRemesa Take-Up

	No match	1:1 match	3:1 match	Total
Panel A. Characteristics of EduRemesas sent by treatment group				
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 B. Average characteristics of EduRemesas conditional on take-up				
Number of EduRemesas sent		1.20	1.29	1.27
Total EduRemesa amount sent by migrant		\$332	\$180	\$217
Total EduRemesa amount sent by migrant plus matching funds		\$690	\$719	\$712
EduRemesa amount sent by migrant to target student		\$270	\$116	\$154
EduRemesa amount sent by migrant to target student plus matching funds		\$540	\$465	\$483

Notes: Data comes from EduRemesa's administrative data. Sample is all migrant-student pairs interviewed at baseline. All money amounts are in US dollars.

where i indexes each migrant-target student pair, j the location of the initial interaction with the migrant, and t the week of the initial interaction. β_1, β_2, and β_3 are the average difference between an 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. δ_{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). The main regressions have 125 day-by-location clusters.

B. *Take-Up*

The first step in our analysis is to examine the take-up of the EduRemesa and how that take-up differs by treatment. All take-up related variables come from the EduRemesa administrative data, provided by both Viamericas and FEPADE. Panel A of Table 2 describes the basic characteristics of the EduRemesas sent. Fifty-two EduRemesas were sent by 41 migrants. Eighty-five percent of migrants who sent an EduRemesa (35 out of 41) sent one to the target student they named at baseline. Sixty-seven percent of EduRemesa recipients were target students (35 out of 52) and most EduRemesas sent to nontarget students were from migrants who sent more than one EduRemesa. Forty 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 B of Table 2 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 to target student beneficiaries. Online Appendix Table 7 shows the number of EduRemesas sent by amount of the

Table 3—Take-Up of EduRemesa by Treatment

	EduRemesa sent (1)	Number of EduRemesas sent (2)	Total EduRemesa amount sent by migrant (3)	Total EduRemesa amount sent by migrant plus matching funds (4)	EduRemesa sent to target student (5)	Total EduRemesa amount sent by migrant to target student (6)	Total EduRemesa amount sent by migrant to target student plus matching funds (7)
3:1 match	0.185***	0.248***	35.09***	139.8***	0.151***	21.61***	85.51***
	[0.0332]	[0.0492]	[6.984]	[27.47]	[0.0291]	[4.236]	[16.25]
1:1 match	0.0686***	0.0841***	23.14***	49.63***	0.0600***	18.49***	37.15***
	[0.0201]	[0.0256]	[7.107]	[15.29]	[0.0190]	[5.934]	[12.18]
No match	−0.000367	0.00532	1.184	4.544	−0.000529	0.559	1.311
	[0.00985]	[0.0129]	[2.445]	[7.153]	[0.00931]	[1.879]	[4.991]
p-values for tests of equality of coefficients							
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^2	0.133	0.114	0.080	0.102	0.114	0.075	0.097
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. There are 125 day × location clusters in each regression. 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. All money amounts are in US dollars.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

EduRemesa. Migrants take advantage of the match offer by usually choosing to send the larger available amount. Twenty-eight of the 34 EduRemesas sent for secondary schooling were for \$500, and 13 of the 18 sent for tertiary schooling were for \$800.

Table 3 estimates the impact of the treatments on take-up using equation (1). The results shown in Table 3 are obtained using the El Salvador follow-up sample and the results of the same analyses in the full and migrant follow-up samples are shown in online Appendix Tables 8A and 8B. Take-up in both the control group and the no match 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 3:1 match is much more effective. Column 1 examines whether a particular migrant sent any EduRemesa, and column 2 examines the total number of EduRemesas sent. 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 matching funds) sent from the 1:1 group and \$140 sent from the 3:1 group (column 4).

Columns 5, 6, and 7 examine EduRemesas sent to target students. The 1:1 match increased the likelihood that an EduRemesa was sent to the target student by 6.0 percentage points; the same figure for the 3:1 match 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 receipts by target students of \$37 and \$86 (column 7).

The take-up results indicate zero demand for the EduRemesa without subsidy via matching funds. The unsubsidized EduRemesa could have been attractive to migrants if they had stronger preferences for educational expenditures than their family members. The existing evidence is mixed on whether migrants desire control over how remittances are used. Ashraf et al. (2015) find that migrants demand control over savings in the home country, but Torero and Viceisza (2013) do not find the same for control over grocery spending (both these studies were also conducted among migrants from El Salvador in the DC metro area). De Arcangelis et al. (2014) find, in an artefactual field experiment in Italy, that Filipino migrants share more resources with family members in the home country when they can label them as intended for education (a "soft" commitment), but offering a "hard" commitment (channeling funds directly to schools) on top of the labeling leads to a much smaller additional increase in remittances.

Zero take-up of the EduRemesa without the matching funds suggests that, in this context, migrants do not seek greater control over remittance uses, at least when it comes to education. However, other aspects of the specific mechanism offered may have contributed to zero take-up. In particular, the fact that migrants had to furnish the entire amount of the EduRemesa up front was probably detrimental to take-up (given this is a relatively low-income and likely liquidity-constrained population). It is also possible that migrants did not believe that the product would provide the level of control they desired, since funds were not actually channeled directly to schools or educational expenditures. Other transaction costs such as the need to collect documentation and the opportunity cost of the time spent traveling to the disbursement site may also have impeded take-up.

Despite zero take-up in the no match group, take-up increases monotonically with the match level and suggests a high elasticity of demand for the EduRemesa with respect to the match rate. This finding is consistent with Duflo et al. (2006), who found very low take-up of a savings program (Individual Retirement Accounts, or IRAs) without a match and participation that increased in the match level when a match was offered.[21]

We interpret this result to indicate that the "demand for commitment," in this context, is elastic with respect to subsidies. Our results are consistent with migrants having a demand for channeling remittances to education, but having no demand for the unsubsidized EduRemesa because of transaction costs or liquidity constraints, or because they perceive that the degree of commitment provided by the EduRemesa is

[21] It contrasts however, with work on charitable donations that finds that take-up does not increase with the match rate (Karlan and List 2007).

Table 4—Target Student Education Expenditures

	Dependent variable: Annualized target student expenditure (USD) on							
	Total (1)	Tuition (2)	School supplies (3)	Uniforms (4)	Books (5)	Transport (6)	Food (7)	Computer use (8)
3:1 match	301.5**	105.8***	−3.343	6.962	7.323	76.67**	143.5**	0.0542
	[125.5]	[32.52]	[7.791]	[6.069]	[7.797]	[37.81]	[57.33]	[26.29]
1:1 match	74.97	83.38**	−11.28	−8.662*	5.047	35.85	48.37	−29.75
	[117.0]	[32.89]	[7.079]	[4.784]	[7.913]	[41.41]	[51.78]	[25.04]
No match	19.32	66.58*	−1.105	−7.527	−11.26*	1.060	35.94	−20.00
	[111.5]	[34.93]	[7.508]	[4.815]	[5.802]	[31.04]	[47.20]	[25.29]
p-values for tests of equality of coefficients								
3:1 = 1:1	0.102	0.603	0.338	0.007	0.830	0.391	0.123	0.302
3:1 = No match	0.060	0.405	0.818	0.010	0.029	0.075	0.102	0.502
1:1 = No match	0.675	0.691	0.270	0.811	0.053	0.406	0.840	0.765
3:1 = 1:1 = No match	0.136	0.705	0.459	0.014	0.029	0.200	0.191	0.560
Observations	728	728	728	728	728	728	728	728
R^2	0.033	0.052	0.032	0.052	0.033	0.042	0.045	0.037
Control group mean	1,358	187	60	36	55	270	443	218

Notes: Robust standard errors clustered at the level of the day and location of the baseline survey in brackets. There are 125 day × location clusters in each regression. 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. All money amounts are in US dollars. Total expenditures also include an "other" category that is not shown.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

limited. The matching funds overcome these costs, leading to positive take-up that monotonically increases in the match rate.[22]

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 adults in their households allow us to answer this question.

Table 4 reports impacts on target student education expenditures. Column 1 examines total annualized expenditures on the target student's education and columns 2 through 8 examine expenditures by category. The main result 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 the control group. As 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

[22] Another possibility, of course, is that migrants have no demand for commitment, are simply "gaming" the offer to obtain the matching funds, and do not intend to use the funds for education. As it turns out, this appears not to be the case: in Section IIID below, we show that the EduRemesa treatment does lead to increases in educational expenditures.

AQ1

Table 5—Total Household Education Expenditures

	Dependent variable: Annualized household expenditure (USD) on								
	Total (1)	Tuition (2)	School supplies (3)	Uniforms (4)	Books (5)	Transport (6)	Food (7)	Computer use (8)	Other (9)
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]
p-values for tests of equality of coefficients									
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^2	0.041	0.053	0.033	0.038	0.037	0.059	0.034	0.035	0.050
Control group mean	2,132	251	91	58	87	424	813	310	98

Notes: Robust standard errors clustered at the level of the day and location of the baseline survey in brackets. There are 125 day × location clusters in each regression. 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. All money amounts are in US dollars.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

in tuition ($106), transportation ($77), and food ($143). The only statistically significant increase in the 1:1 match group is for tuition ($83). Despite zero take-up in the no match group, tuition expenditures increase by $67, but this does not translate to an increase in overall education expenditures.[23]

In order to fully understand how the EduRemesa is affecting resources allocated towards education it is instructive to examine total household education expenditures. If household expenditures go up by less than target student expenditures, then the increases documented in Table 4 may be due to shifting of resources away from other students 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.[24] The results are presented in Table 5. The setup of the table is parallel to Table 4, but all the outcomes are for household-level expenditures.

The results mirror those for target student education expenditures. Expenditures increase overall and this increase is driven by increases in tuition, transportation,

[23] These results are shown graphically in online Appendix Figure 2, which plots the cumulative distribution of total expenditures for the control group and the three treatment groups. 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.

[24] The expenditures on these other students were reported by the adult interviewed in the target household and imputed when missing to maintain a consistent sample. Four and a half percent of observations in the total expenditures category include an imputed value for at least one of the categories. Further details are described in online Appendix B and all results are robust to the exclusion of imputed values.

Table 6—Instrumental Variables Regressions

	Dependent variable:		
	Total target student annualized education expenditures (1)	Estimated target student annualized earnings (2)	Target student education expenditures minus annualized earnings (3)
Total EduRemesa funds received by target student (migrant funds plus matching funds)	3.720**	−1.661***	5.381***
	[1.647]	[0.582]	[1.946]
p-value for equality of coefficient to 1	0.099		0.024
Observations	425	425	425

Notes: Robust standard errors clustered at the level of the day and location of the baseline survey in brackets. There are 117 day × location clusters in each regression. 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. The first stage coefficient is 85.34 and the F-statistic is 28.17. All money amounts are in US dollars.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

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

Table 4 shows that the 3:1 match treatment increases total target student education expenditures by $301, which should be compared to average target student EduRemesa funds received of $85 (Table 3, column 7). It appears that education spending increases by the total amount of the EduRemesa, and, additionally 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, column 1 of Table 6 reports the results of an 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 just 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. The first stage coefficient is 85.34 and the F-statistic is 28.17, indicating that the instrument is strong according to the Stock and Yogo (2005) thresholds.

The estimated coefficient reveals the impact of each dollar of EduRemesa funds on target student educational expenditures. It can be interpreted as a test of crowd-out versus crowd-in: a coefficient statistically significantly smaller than 1 would reveal crowd-out, while a coefficient statistically significantly larger than 1 would indicate crowd-in. The coefficient is 3.72, indicating that each dollar of EduRemesa funds leads to an increase of \$3.72 in target student education expenditures. This estimate is statistically significantly different from unity at the 10 percent level. Because this coefficient exceeds 1, we refer to its coefficients as a "crowd-in ratio."[25]

One caveat to the results that we present here is that all of the school expenditure data is self-reported, leaving open concerns about experimenter demand effects (reporting bias that is differential by treatment group). Target students receiving the EduRemesa knew that they were receiving a transfer that was meant to be used for education and may have overstated their educational expenditures as a result. Unfortunately, it was infeasible for us to collect administrative data on expenditures from the wide variety of public and private schools attended by the students and the diverse categories in which expenditures were made. Therefore, we cannot conclusively rule out the possibility that reporting bias contributes to our large estimated effect sizes.

We can point to some evidence that reporting bias is not likely to be the only or the principal driver of our results. First, the increases in expenditures are concentrated in tuition, transport, and food. If target students were inflating their expenditures, it is not clear why they would limit themselves to those three categories, instead of increasing all their reports, or concentrating on categories more directly linked to education such as books. Additionally, though the EduRemesa was linked to the target student, it is possible that if reporting bias was severe, it would spill over into reports of expenditures on other children. We find no evidence of this, as the impacts on household expenditures (Table 5) are similar to those on target student expenditures (Table 4). However, despite these arguments, absent administrative data we cannot be definitive about the exact magnitude of the impact on expenditures.[26]

D. *Impact on Other Target Student Outcomes*

Given the finding of crowd-in it is interesting to examine other elements of the target students' behavior that may be consistent with these increases in expenditure. For example, changes in school enrollment decisions could necessitate large increases in spending that are not fully covered by the EduRemesa. In Table 7, we turn to the impacts on school enrollment and type of school. Column 1 examines whether or not the target student is in school at follow-up and columns 2 through 4 examine whether the target student is in any private school, parochial school, or nonparochial private school (the latter two are subcategories of private schools).

[25] It should be noted that this crowd-in ratio is estimated using the limited number of discrete amounts offered by the EduRemesa product. One limitation of our design is that we cannot extrapolate what this crowd-in ratio may have been outside of the amounts that we offer as part of this experiment. Larger EduRemesas for example may actually decrease crowd-in as more expenditures are covered by the EduRemesa itself.

[26] Another possible interpretation of our expenditure results is that EduRemesa treatment acted to make EduRemesa recipients simply more aware of education related costs and the differences between treatment and control are due to improved reporting by the recipient target students. This explanation would require that target students in the control group are systematically and significantly underreporting (and not overreporting) expenditures. While we cannot be sure that this is not happening, we see no compelling reason why underreporting would be the norm.

Table 7—Target Student Education Outcomes

	Target student is in school (1)	Target student is in any private school (2)	Target student is in parochial school (3)	Target student is in other private school (4)
3:1 match	0.0309	0.109**	0.0288	0.0803**
	[0.0398]	[0.0430]	[0.0375]	[0.0350]
1:1 match	−0.0210	0.0498	−0.0172	0.0671*
	[0.0381]	[0.0419]	[0.0368]	[0.0346]
No match	0.0182	0.0910**	0.0298	0.0612
	[0.0440]	[0.0460]	[0.0359]	[0.0379]
p-values for tests of equality of coefficients				
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^2	0.048	0.042	0.031	0.044
Control group mean	0.74	0.27	0.16	0.11

Notes: Robust standard errors clustered at the level of the day and location of the baseline survey in brackets. There are 125 day × location clusters in each regression. 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.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

The treatments do not have statistically significant effects on school enrollment. There is, however, a large impact on the probability that the target student is attending private school. Target students in the 3:1 match group are 11 percentage points more likely to be in private school. This is a large increase relative to the control group private school attendance rate of 27 percent. These increases in private school attendance concord with the increases in expenditure on tuition and other expenditures discussed above. However, given that private school attendance also increases in the no match group by a similar amount, it is not clear how large a part the private school effect plays in increasing expenditures.

The concerns related to reporting bias discussed for the expenditure results are also relevant for attendance in private school, particularly given the large size of the effect relative to the control group. In this case we have access to some limited administrative data to verify our survey reports. As a condition of receiving the EduRemesa, all recipients were required to provide FEPADE with proof of enrollment in school before transfers could begin, and FEPADE recorded whether this school was public or private. In the sample of target students in the El Salvador follow-up sample who received an EduRemesa, the data indicates that 53 percent and 50 percent of target students are enrolled in private school, reported in the survey data and the FEPADE data respectively.[27] These reports are very close, and where they do not agree, the disagreement goes in both directions. It is not only that

[27] This is in contrast to a 27 percent private school enrollment rate in the control group. Of course, we do not know the counterfactual enrollment rate for those in the control group who would have received an EduRemesa had they been offered it.

Table 8—Target Student Labor Supply Outcomes

	Dependent variables refer to work currently being done by the target student					
	Any work (1)	Average hours per week any work (2)	Paid work (3)	Average hours per week paid work (4)	Unpaid work (5)	Average hours per week unpaid work (6)
3:1 match	−0.139***	−4.365***	−0.0718*	−2.928***	−0.0830***	−1.436***
	[0.0402]	[1.048]	[0.0369]	[0.936]	[0.0308]	[0.468]
1:1 match	−0.0751*	−3.204***	−0.0543	−1.780*	−0.0435	−1.425***
	[0.0412]	[1.095]	[0.0346]	[0.968]	[0.0325]	[0.431]
No match	0.00897	−0.386	−0.0147	−0.138	0.00231	−0.248
	[0.0445]	[1.323]	[0.0371]	[1.223]	[0.0352]	[0.559]
p-values for tests of equality of coefficients						
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^2	0.041	0.056	0.032	0.048	0.041	0.059
Control group mean	0.33	6.78	0.20	4.43	0.17	2.35

Notes: Robust standard errors clustered at the level of the day and location of the baseline survey in brackets. There are 125 day × location clusters in each regression. 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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

students who are actually in public school report that they are enrolled in private school. Although this is only a small amount of administrative data and its analysis is not probative, it does not suggest a large amount of reporting bias.

We also examine the impact of the treatments on target students' labor supply. Because the EduRemesa had 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. Increased attendance at private schools may have also required target students to dedicate more time to their studies, reducing their ability to work. However, it is also possible that target students would have had to increase their labor supply, given the large crowd-in of expenditures. 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) in Table 8. 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 matches had a significant effect on target student labor supply. Students in the 3:1 match group are 14 percentage points less likely to work and work an average of 4.4 fewer hours per week than students in the control group. Students in the 1:1 match group are 7.5 percentage points less likely to work and work 3.2 fewer hours per week. These are large effects: a 64 percent reduction in the 3:1 match group compared to the control group. This is evidence of effects on both the extensive and intensive margins. 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 long hours.[28]

These large reductions in labor supply for 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. To examine this, in column 2 of Table 6 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 Propositos Multiples* by the number of annual paid hours worked by the target student. This approximation suggests that for every dollar received, target students reduce their earnings by \$1.66.

Finally, we can examine the household's contribution to the target student's educational expenditures, net of the target student's earnings. This is shown in column 3 of Table 6 where the dependent variable is total target student education expenditures minus target student estimated earnings. Using the same instrumentation strategy with the addition of the foregone earnings, we find a large crowd-in ratio of 5.38. 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 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.

E. *Impact on Remittances*

Having examined how the EduRemesa treatments directly impacted outcomes related to education, it is instructive to consider impacts on remittances sent by the migrant. These remittances were reported by the migrant in the migrant follow-up survey. The survey questions used to calculate total remittances explicitly instructed migrants not to include any funds that were remitted as part of an EduRemesa. Therefore, we analyze the impact of the EduRemesa treatments on all remittances sent by the migrant, except for the funds sent as an EduRemesa.

This analysis is presented in Table 9. 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) and to other households in El Salvador (column 2).[29] Because of several outliers in the remittance data, we also show results that trim the top 1 percent of values (columns 3 and 4) and results that utilize

[28] Online Appendix Figure 3 shows the cumulative distribution of hours worked by treatment. 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. The intensive margin is evidenced by the longer tails of the no match and control group distributions.

[29] Because the information was reported by the migrant during the migrant follow-up survey, the analysis sample differs slightly from most other analyses in this paper, which are on the El Salvador follow-up survey sample.

Table 9—Non-EduRemesa Remittances Sent by Migrant

	Dependent variable is migrant report of remittances sent since January 1, 2012					
	Full migrant follow-up sample		Trimmed top 1 percent of each column		Inverse hyperbolic sine transformation	
	Remittances to target household	Remittances to other households	Remittances to target household	Remittances to other households	Remittances to target household	Remittances to other households
	(1)	(2)	(3)	(4)	(5)	(6)
3:1 match	−167.9	−74.69	−2.336	−71.33	−0.124	−0.252
	[192.2]	[70.59]	[160.1]	[48.14]	[0.333]	[0.292]
1:1 match	−365.1**	−63.63	−153.1	29.36	−0.441	0.132
	[180.7]	[66.62]	[152.4]	[60.67]	[0.410]	[0.330]
No match	−482.9***	−141.9**	−213.1	−60.65	−0.271	−0.171
	[165.6]	[54.85]	[136.5]	[49.59]	[0.323]	[0.302]
p-values for tests of equality of coefficients						
3:1 = 1:1	0.284	0.900	0.362	0.130	0.475	0.289
3:1 = No match	0.052	0.394	0.152	0.853	0.674	0.826
1:1 = No match	0.370	0.252	0.623	0.186	0.664	0.407
3:1 = 1:1 = No match	0.135	0.446	0.354	0.284	0.773	0.535
Observations	735	735	727	727	735	735
R^2	0.053	0.037	0.061	0.040	0.031	0.030
Control group mean	1449.00	363.00	1206.00	278.10	6.13	1.97

Notes: Robust standard errors clustered at the level of the day and location of the baseline survey in brackets. There are 125 day × location clusters in each regression. 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. Remittance amounts do not include EduRemesa funds. All money amounts are in US dollars.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

the inverse hyperbolic sine transformation of the remittance variable (columns 5 and 6).[30,31]

There is no consistent evidence in Table 9 that the 3:1 match treatment (the treatment with the highest take-up) results in changes in remittances either to the target household or other households. The estimated coefficients are negative, but not statistically significant. An oddity is that in columns 1 and 2 there appear to be negative effects of the 1:1 and no match treatments. However, these effects are not robust to trimming or to the inverse hyperbolic sine transformation.

The imprecision of the estimated treatment effects on remittances makes it difficult to make strong conclusions. The results in Table 9 are suggestive that total remittances are relatively inelastic with respect to the kind of subsidies we offered in this intervention. Because the dependent variables in Table 9 exclude funds sent via

[30] The inverse hyperbolic sine transformation is $\log\left(y_i + \left(y_i^2 + 1\right)^{1/2}\right)$. 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, Magee, and Robb 1988).

[31] All results in previous tables relating to education expenditures (Tables 4 and 5) are robust to trimming of the top 1 percent and the inverse hyperbolic sine transformation.

the EduRemesa, negative point estimates on the treatment indicators are consistent with migrants maintaining a relatively constant total resource flow (remittances plus EduRemesa funds) to El Salvador. Indeed, the negative point estimate on the 3:1 match treatment in Table 9, column 1 (−167.9) is roughly similar in magnitude to the positive point estimate on the 3:1 match treatment in Table 3, column 7 (85.5), which is consistent with migrants reducing remittances to the target household by roughly the match-inclusive EduRemesa amount.

Overall, it appears that the 3:1 match treatment leaves total remittances relatively constant, reduces target student labor supply, and does not reduce educational expenditures on other (nontarget) students in the household. Funds for observed increases in educational expenditures on target students must therefore be coming from other sources, such as noneducation household consumption or savings.

F. *Discussion*, *Additional Analyses*, *and Longer-Term Impacts*

Given the long literature exploring differences in education outcomes by gender in developing countries, we examine whether or not the EduRemesa treatments differentially affected male and female students. This analysis is presented in Table 10, where we examine treatment impacts on key outcomes for female target students in panel A and male target students in panel B.[32] The results show that female target students in the 3:1 match group are more likely than male target students to receive an EduRemesa (18 percent compared to 11 percent) and this translates into larger impacts across most outcome categories for female students. The exception is the labor market outcomes, where the impacts are similar, but this may be due to higher rates of male students working overall. The stronger impacts for female target students are interesting particularly in light of the fact that while a majority of target students are female, they are not overwhelmingly so; 43 percent of migrants choose male target students.

We provide some discussion and other analyses of our results in online Appendix C. We discuss how we rule out marketing effects as the main driver of our results, and also discuss the relative magnitude of the impacts of the 3:1 and the 1:1 matches. We additionally provide analyses showing that an additional treatment that varied whether migrants received official grade reports for the EduRemesa beneficiaries had no statistically identifiable impacts.[33]

In online Appendix D we present the results of a second follow-up survey conducted roughly one year after the first intended to study the longer-term impacts of the EduRemesa program. Due to high attrition (46 percent completion), we do not discuss these results in the main text of the paper. Many of the results follow the same basic patterns, with the exception of the labor supply results which are no

[32] Online Appendix Tables 9 and 10 show attrition and baseline balance by gender. There is no consistent pattern of differential attrition or baseline imbalance when separating the sample by gender. 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. Additionally, there is imbalance between treatment groups for male target students in baseline school enrollment.

[33] In the same sample, using an artefactual field experiment, Ambler (2015) finds that remittance recipients do not alter spending decisions in response to being monitored by the migrant. This suggests that, in this context, recipients are not responsive to migrant monitoring.

Table 10—Results by Target Student Gender

	Take-up		Annualized education expenditures		Education outcomes		Labor supply	
	EduRemesa sent to target student (1)	Total EduRemesa amount sent by migrant to target student plus matching funds (2)	Total target student expenditures (3)	Total household expenditures (4)	Target student is in school (5)	Target student is in any private school (6)	Any work (7)	Average hours per week any work (8)
Panel A. Female target students								
3:1 match	0.178***	108.4***	509.4***	534.0**	0.0836	0.183***	−0.157***	−3.260***
	[0.0464]	[27.21]	[183.8]	[262.0]	[0.0599]	[0.0619]	[0.0481]	[1.155]
1:1 match	0.101***	59.89***	45.60	−165.0	−0.0166	0.119*	−0.0817	−3.275***
	[0.0346]	[21.93]	[185.7]	[250.3]	[0.0691]	[0.0643]	[0.0528]	[1.045]
No match	0.00990	7.475	−55.40	−314.2	−0.00889	0.0623	0.00582	1.371
	[0.0136]	[8.565]	[169.1]	[239.5]	[0.0628]	[0.0640]	[0.0554]	[1.683]
p-values for tests of equality of coefficients								
3:1 = 1:1	0.186	0.165	0.028	0.017	0.189	0.430	0.183	0.985
3:1 = No match	0.001	0.001	0.006	0.007	0.220	0.127	0.007	0.004
1:1 = No match	0.004	0.006	0.596	0.556	0.920	0.457	0.164	0.003
3:1 = 1:1 = No match	0.000	0.000	0.017	0.018	0.335	0.311	0.027	0.009
Observations	387	387	387	387	387	387	387	387
R^2	0.145	0.137	0.103	0.105	0.082	0.085	0.103	0.099
Control group mean	0	0	1,412	2,233	0.74	0.26	0.28	5.19
Panel B. Male target students								
3:1 match	0.115***	55.96***	43.57	8.040	−0.0595	0.00546	−0.116*	−5.144***
	[0.0366]	[18.83]	[186.7]	[224.8]	[0.0681]	[0.0636]	[0.0701]	[1.866]
1:1 match	0.00842	7.756	64.92	284.4	−0.0536	−0.0383	−0.0441	−2.555
	[0.0184]	[10.88]	[195.1]	[276.5]	[0.0587]	[0.0644]	[0.0666]	[2.028]
No match	−0.0129	−7.167	−27.38	2.470	0.0115	0.0897	0.0310	−1.852
	[0.0143]	[7.916]	[189.5]	[234.8]	[0.0709]	[0.0739]	[0.0681]	[2.332]
p-values for tests of equality of coefficients								
3:1 = 1:1	0.023	0.047	0.921	0.370	0.934	0.526	0.373	0.176
3:1 = No match	0.003	0.005	0.724	0.985	0.385	0.308	0.111	0.112
1:1 = No match	0.305	0.214	0.647	0.370	0.397	0.108	0.263	0.766
3:1 = 1:1 = No match	0.007	0.011	0.886	0.598	0.628	0.274	0.264	0.184
Observations	341	341	341	341	341	341	341	341
R^2	0.161	0.146	0.058	0.078	0.109	0.061	0.079	0.096
Control group mean	0	0	1,287	2,000	0.74	0.27	0.39	8.86

Notes: Robust standard errors clustered at the level of the day and location of the baseline survey in brackets. There are 119 day × location clusters in each regression in panel A and 114 day × location clusters in each regression in panel B. Sample is all migrant-student pairs with completed migrant follow-up surveys by gender of the target student. All regressions include stratification cell fixed effects for the week and location of the baseline survey. All money amounts are in US dollars.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

longer present. However, due to the high attrition, small sample sizes do not allow us to draw definitive conclusions.

IV. Conclusions and Policy Implications

These results can help guide policies aimed at increasing the development impact of remittances. They indicate that programs subsidizing education in developing countries can extend the resources available to them via contributions from two additional sources: (i) international migrants, who respond positively to matching grant programs for home-country education, and (ii) beneficiary households themselves, who respond to subsidies by contributing additional resources. Our estimates indicate that each \$1 of donor funds provided for secondary or tertiary education can generate additional contributions amounting to \$0.33 from migrants and \$3.62 from beneficiary households themselves.[34]

Our finding of zero take-up in the no match treatment may also reveal that migrants have no or limited unsubsidized demand for control over remittance recipient expenditures on education. Alternatively, transaction costs in this context may exceed migrant willingness to pay for the services, suggesting that policymakers should seek to reduce the administrative burden of such programs.

Beyond the use of matching funds and the payment mechanism, an additional characteristic of the EduRemesa intervention that differentiates it from other programs is the fact that the beneficiaries are chosen by the migrants. Given the magnitude of the effects of the program, it seems that the migrants were successful in selecting students likely to use the subsidy to make large investments in their education. In other words, although take-up was low, utilizing migrants to screen recipients was effective in directing the program towards students who would benefit. This result suggests that requiring contributions from family members can do more than simply alleviate the financial burden of such educational transfer programs, but can also serve to target those transfer programs towards students who will benefit even with only minimal oversight.

Our results are, of course, limited to the context that we study, and so it is not known whether self-screening by program participants would be as effective in other situations; for example, if the product was offered to a general household population in a developing country without international migrant members. Relatedly, transnational households may respond differently to the offer of an EduRemesa-like product, because they self-selected into migration and may have characteristics also associated with higher demand for long-term household investments (such as education). Therefore, it is important to replicate this study in other populations and contexts to gauge the generalizability of these results.

[34] These figures are implied by the crowd-in ratio of 3.72 (column 1, Table 6): 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).

REFERENCES

Afridi, Farzana. 2010. "Child Welfare Programs and Child Nutrition: Evidence from a Mandated School Meal Program in India." *Journal of Development Economics* 92 (2): 152–65.

Ambler, Kate. 2015. "Don't Tell on Me: Experimental Evidence of Asymmetric Information in Transnational Households." *Journal of Development Economics* (113): 52–69.

Ambler, Kate, Diego Aycinena, and Dean Yang. 2015. "Channeling Remittances to Education: A Field Experiment among Migrants from El Salvador: Dataset." *American Economic Journal: Applied Economics* 7 (2). http://dx.doi.org/10.1257/app.20140010.

Angelucci, Manuela, and Orazio Attanasio. 2009. "Program Effect on Consumption, Low Participation, and Methodological Issues." *Economic Development and Cultural Change* 57 (3): 479–506.

Angelucci, Manuela, and Giacomo De Giorgi. 2009. "Indirect Effects of an Aid Program: How do Cash Transfers Affect Ineligibles' Consumption?" *American Economic Review* 99 (1): 486–508.

Angelucci, Manuela, Giacomo de Giorgi, Marcos Rangel, and Imran Rasul. 2010. "Family Networks and School Enrollment: Evidence from a Randomized Social Experiment." *Journal of Public Economics* 94 (3–4): 197–221.

Angrist, Joshua, Eric Bettinger, Erik Bloom, Elizabeth King, and Michael Kremer. 2002. "Vouchers for Private Schooling in Colombia: Evidence from a Randomized Natural Experiment." *American Economic Review* 92 (5): 1535–58.

Ashraf, Nava, Diego Aycinena, Claudia Martínez A., and Dean Yang. 2015. "Savings in Transnational Households: A Field Experiment Among Migrants from El Salvador." *Review of Economics and Statistics*.

Baird, Sarah, Craig McIntosh, and Berk Özler. 2011. "Cash or Condition? Evidence from a Cash Transfer Experiment." *Quarterly Journal of Economics* 126 (4): 1709–53.

Barrera-Osorio, Felipe, Marianne Bertrand, Leigh Linden, and Francisco Perez-Calle. 2011. "Improving the Design of Conditional Transfer Programs: Evidence from a Randomized Education Experiment in Colombia." *American Economic Journal: Applied Economics* 3 (2): 167–95.

Behrman, Jere R., Piyali Sengupta, and Petra Todd. 2005. "Progressing through PROGRESA: An Impact Assessment of a School Subsidy Experiment in Rural Mexico." *Economic Development and Cultural Change* 54 (1): 237–75.

Benhassine, Najy, Florencia Devoto, Esther Duflo, Pascaline Dupas, and Victor Pouliquen. 2013. "Turning a Shove into a Nudge? A 'Labeled Cash Transfer' for Education." National Bureau of Economic Research (NBER) Working Paper 19227.

Burbidge, John B., Lonnie Magee, and A. Leslie Robb. 1988. "Alternative Transformations to Handle Extreme Values of the Dependent Variable." *Journal of the American Statistical Association* 83 (401): 123–27.

Carneiro, Pedro, Andrea Locatelli, Tewolde Ghebremeskel, and Joseph Keating. 2012. "Do Public Health Interventions Crowd Out Private Health Investments? Malaria Control Policies in Eritrea." Centre for Microdata Methods and Practice (CEMMAP) Working Paper CWP12/12.

Clemens, Michael A. 2011. "Economics and Emigration: Trillion-Dollar Bills on the Sidewalk?" *Journal of Economic Perspectives* 25 (3): 83–106.

Clemens, Michael A., Claudio E. Montenegro, and Lant Pritchett. 2009. "The Place Premium: Wage Differences for Identical Workers Across the U.S. Border." Center for Global Development Working Paper 148.

Cox Edwards, Alejandra, and Manuelita Ureta. 2003. "International Migration, Remittances, and Schooling: Evidence from El Salvador." *Journal of Development Economics* 72 (2): 429–61.

Das, Jishnu, Stefan Dercon, James Habyarimana, Pramila Krishnan, Karthik Muralidharan, and Venkatesh Sundararaman. 2013. "School Inputs, Household Substitution, and Test Scores." *American Economic Journal: Applied Economics* 5 (2): 29–57.

De Arcangelis, Giuseppe, Majlinda Joxhe, David McKenzie, Erwin Tiongson, and Dean Yang. 2014. "Directing Remittances to Education with Soft and Hard Commitments: Evidence from a Lab-in-the-field Experiment and New Product Take-up Among Filipino Migrants in Rome." World Bank Development Research Group Working Paper 6896.

Dirección General de Estadística y Censos. 2010. Encuesta de Hogares de Propositos Multiples. http://www.digestyc.gob.sv/index.php/temas/des/ehpm.html (accessed August 24, 2011).

Duflo, Esther, William Gale, Jeffrey Liebman, Peter Orszag, and Emmanuel Saez. 2006. "Saving Incentives for Low- and Middle-Income Families: Evidence from a Field Experiment with H&R Block." *Quarterly Journal of Economics* 121 (4): 1311–46.

Edmonds, Eric V., and Norbert Schady. 2012. "Poverty Alleviation and Child Labor." *American Economic Journal: Economic Policy* 4 (4): 100–124.

Fajnzylber, Pablo, and J. Humberto López. 2007. *Close to Home: The Development Impact of Remittances in Latin America*. World Bank. Washington, DC.

Fiszbein, Ariel, Norbert Schady, Francisco H. G. Ferreira, Margaret Grosh, Niall Kelleher, Pedro Olinto, and Emmanuel Skoufias. 2009. *Conditional Cash Transfers: Reducing Present and Future Poverty*. World Bank Policy Report 47603.

Fundación Salvadoreña para el Desarrollo Económico y Social (FUSADES). 2011. *Tendencias en Educación*. Informe de Coyuntura Social. El Salvador, December.

Glewwe, Paul, and Ana Lucia Kassouf. 2012. "The impact of the *Bolsa Escola/Familia* conditional cash transfer program on enrollment, dropout rates and grade promotion in Brazil." *Journal of Development Economics* 97 (2): 505–17.

Hazán, Miryam. 2013. "Beyond 3x1: Linking Sending and Receiving Societies in the Development Process." *International Migration* 51 (5): 48–60.

Hoddinott, John, and Emmanuel Skoufias. 2004. "The Impact of PROGRESA on Food Consumption." *Economic Development and Cultural Change* 53 (1): 37–61.

Islam, Mahnaz, and John Hoddinott. 2009. "Evidence of Intrahousehold Flypaper Effects from a Nutrition Intervention in Rural Guatemala." *Economic Development and Cultural Change* 57 (2): 215–38.

Jacoby, Hanan G. 2002. "Is There an Intrahousehold 'Flypaper Effect'? Evidence from a School Feeding Programme." *Economic Journal* 112 (476): 196–221.

Karlan, Dean, and John A. List. 2007. "Does Price Matter in Charitable Giving? Evidence from a Large-Scale Natural Field Experiment." *American Economic Review* 97 (5): 1774–93.

Organisation for Economic Co-operation and Development (OECD). 2013. Aid Statistics. http://www.oecd.org/dac/stats/data.htm (accessed June 23, 2013).

Payne, A. Abigail. 2009. "Does Government Funding Change Behavior? An Empirical Analysis of Crowd-Out." *Tax Policy and the Economy*, Vol. 23, edited by Jeffrey R. Brown and James M. Poterba, 159–84. Chicago: University of Chicago Press.

Peltzman, Sam. 1973. "The Effect of Government Subsidies-in-Kind on Private Expenditures: The Case of Higher Education." *Journal of Political Economy* 81 (1): 1–27.

Pew Hispanic Center. 2002. *Billions in Motion: Latino Immigrants, Remittances, and Banking*. University of Southern California Annenberg School for Communication. Washington, DC.

Ruggles, Steven J., Trent Alexander, Katie Genadek, Ronald Goeken, Matthew B. Schroeder, and Matthew Sobek. 2010. "Integrated Public Use Microdata Series: Version 5.0." University of Minnesota. Minneapolis.

Schultz, T. Paul. 2004. "School Subsidies for the Poor: Evaluating the Mexican Progresa Program." *Journal of Development Economics* 74 (1): 199–250.

Shi, Xinzheng. 2012. "Does an Intra-household Flypaper Effect Exist? Evidence from the Educational Fee Reduction Reform in Rural China." *Journal of Development Economics* 99 (2): 459–73.

Stock, James, and Motohiro Yogo. 2005. "Testing for weak instruments in linear IV regression." In *Identification and Inference in Econometric Models: Essays in Honor of Thomas J. Rothenberg*, edited by D. Andrews and J. Stock, 80–108. Cambridge: Cambridge University Press.

Terry, Donald F., and Steven R. Wilson, eds. 2005. *Beyond Small Change: Making Migrant Remittances Count*. Washington, DC: Inter-American Development Bank.

Theoharides, Caroline. 2014. "Manila to Malaysia, Quezon to Qatar: International Migration and the Effects on Origin-Country Human Capital." http://www-personal.umich.edu/~cbtheo/Theoharides_JMP.pdf.

Torero, Máximo, and Angelino Viceisza. 2013. "To Remit or Not to Remit: That is the Question. A Remittance Field Experiment." Unpublished.

World Bank. 2006. *Global Economic Prospects 2006: Economic Implications of Remittances and Migration*. World Bank Report 34320.

World Bank. 2012. "Remittances to developing countries will surpass $400 billion in 2012." *World Bank Migration and Development Brief 19*.

Yang, Dean. 2008. "International Migration, Remittances and Household Investment: Evidence from Philippine Migrants' Exchange Rate Shocks." *Economic Journal* 118 (528): 591–630.

Yang, Dean. 2011. "Migrant Remittances." *Journal of Economic Perspectives* 25 (3): 129–15.