Essays on Household Economics and Remittances

by

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ABSTRACT

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Understanding how families make economic decisions about how to allocate scarce household resources is crucial for the development and implementation of effective development policy. This dissertation investigates three specific questions related to this broad area of research. The first chapter demonstrates the importance of information asymmetries in transnational households, where physical distance between family members can make information barriers especially acute. I implement an experiment among 1,300 Salvadoran migrants in Washington, DC and their family members in El Salvador that examines how (1) changing the ability of participants to monitor each other and (2) revealing migrant preferences can affect the sending and spending of remittances. Migrants make an incentivized decision about how much of a cash windfall to keep and how much to send home, and recipients decide how to allocate the spending of a remittance. Migrants remit significantly more when their choice is observed by recipients, and this effect is concentrated among pairs where recipient ability to punish migrants is plausibly high. The results support a model of remittance sending where migrants react strategically to being monitored, but only when recipients can enforce remittance agreements. Recipients make spending choices closer to the migrants' preferences when they are revealed, suggesting that recipients' choices may be inadvertently affected by imperfect information on migrant preferences. Together, these results indicate that information imperfections in families are varied and can affect resource allocation in both strategic and inadvertent ways.

In the second chapter, I examine how the exogenous change in individual income provided by eligibility for the South African government pension can affect decision making in the household. Exploiting the age discontinuity in pension eligibility, I find that eligible females

are 13 to 16 percentage points more likely to be the primary decision makers for expenditures than non-eligible females--rare direct support for bargaining models of the household. There is no corresponding effect for eligible males. Due to labor force withdrawal, male income does not increase at the age of eligibility, providing an explanation for the lack of impacts of male eligibility on decision making. The increase in female decision-making power translates into improved nutritional outcomes for girls and higher levels of durable goods ownership. Because male income does not increase, these findings invite a reconsideration of the common assumption that women make more productive use of cash transfers than men.

The third chapter, written jointly with Diego Aycinena and Dean Yang, returns to transnational households. We study the intersection of two research areas: educational subsidies and migrant remittances. We implement a randomized experiment offering Salvadoran migrants cash 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.

CHAPTER I

Don't Tell on Me: Experimental Evidence of Asymmetric Information in Transnational Households

1.1 Introduction and motivation

Although the implications of asymmetric information have been well documented in the study of important economic institutions such as labor, credit, and insurance markets, theoretical models of intra-household resource allocation have largely assumed perfect information (Chiappori, 1988, 1992; Manser and Brown, 1980; McElroy and Horney, 1981; Lundberg and Pollack, 1993).[1,2] Despite this, a growing body of empirical literature has shown that information asymmetries do exist in households, and further, that household members take strategic advantage of opportunities to use these asymmetries to alter the allocation of resources in the household (Ashraf, 2009; Ashraf, Field, and Lee, 2010; Schaner, 2012).[3] This paper brings the study of how information asymmetries affect intra-household resource allocation to a different setting: transnational households, defined as households composed of international migrants and their family members in the home country, in this case El Salvador. Using experimental methods, I examine the effects of a set of information imperfections on remittance decisions made by both migrants and their family members.

[1] Exceptions include Bloch and Rao (2002) and Chen (2013).
[2] Empirical studies including Lundberg, Pollack, and Wales (1996), Duflo (2000), Duflo (2003), Qian (2008) and Ambler (2012) have supported some of the predictions of these models but do not account for information asymmetries.
[3] In a separate group of empirical studies Udry (1996), Dercon and Krishnan (2000), Goldstein, de Janvry, and Sadoulet (2005), and Dubois and Ligon (2011) show that intra-household resource allocation may be inefficient in some contexts—results that may be indicative of the presence of information asymmetries.

The context of transnational households is significant because migrants and their family members are making financial decisions in a situation where information asymmetries are especially acute. Because of the physical distance separating family members, families with a migrant living away from the household are precisely those where information asymmetries may be the most pronounced. A number of studies have documented the existence of these asymmetries in households with migrants. For example, De Laat (2008) shows that domestic migrants in Kenya spend resources on costly monitoring of their wives. Chen (2006, 2013) finds that in China, wives with migrant husbands exhibit non-cooperative behavior more often for activities that are more difficult to monitor, and Seshan and Yang (2012) find suggestive evidence that Indian migrants underestimate how much their wives at home are saving. However the empirical analysis in these papers is largely observational.[4] This is the first study to causally examine how information asymmetries directly affect behavior, specifically decisions about the sending and spending of remittances.

The importance of understanding how information asymmetries affect decisions in transnational households is heightened by the fact that migrants and their family members are financially linked through the sending of remittances, a large and important financial flow. Global aggregate international remittances to the developing world were $332 billion in 2010, more than any other kind of resource flow with the exception of foreign direct investment (Ratha and Silwal, 2012). In El Salvador specifically, remittances received were 16 percent of GDP in 2010 (Ratha and Silwal, 2012). In 2009, 22 percent of households in El Salvador received remittances from abroad and average monthly remittances were $168 for families that received them, a figure that is almost 50 percent of average monthly household expenditures for remittance recipients (DIGESTYC, 2010). Additionally, the receipt of remittances has been shown to have large, positive impacts on a variety of measures of well being, underscoring their importance as a tool for development (Cox-Edwards and Ureta, 2003; Adams and Page, 2005; Yang and Martinez, 2005; Woodruff and Zentano, 2007; Yang, 2008; Adams and Cuecuecha, 2010). Given the importance of remittances for development, a more complete understanding of how these decisions are made is crucial for policy makers hoping to maximize their economic impact.

[4] In an exception, McKenzie, Gibson, and Stillman (2012) find that potential Tongan migrants underestimate earnings in New Zealand, a fact the authors attribute to under reporting of earnings by current migrants.

This paper addresses two types of information asymmetries that may affect decisions about the sending and spending of remittances. The first are asymmetries that can lead to *strategic* behavior, meaning that migrants and recipients recognize that the asymmetry exists and use it for their benefit. The specific asymmetries considered here are the limited abilities of remittance recipients to observe migrant income and of migrants to observe recipient spending. The second type are those that can have *inadvertent* impacts, defined as asymmetries that unintentionally affect decisions. These asymmetries are represented here as communication barriers that result in recipients having an incomplete understanding of migrant preferences for how the remittances they send should be spent. Communication barriers should be interpreted broadly as any obstacle – social, financial, or logistical – to fully understanding these preferences. I first develop a theoretical framework that derives predictions for how these two types of information asymmetries can affect remittance decisions, and then I test these predictions using experiments conducted with a matched sample of migrants from El Salvador and their family members at home.

The framework views the decisions made by migrants and remittance recipients as being driven both by altruism and contracts (whether implicit or explicit) that dictate how much of their income migrants should send to recipients and what that money should be spent on when received by the recipients. The contracts are enforced through the threat of punishment for noncompliance. I show that under imperfect and incomplete information about migrant income and recipient spending, strategic deviation by both migrants and recipients can be a characteristic feature of these contracts. However, in pairs where the potential for punishment is low, remittances will be mostly motivated by altruism, and these strategic effects will therefore be less important. Additionally, communication barriers, specifically in regards to migrants' preferences over recipients' spending habits, may lead to inadvertent deviation from migrant preferences by recipients.

The experiments explicitly test for both strategic and inadvertent behavior. They were designed to mimic real life decisions about remittances made by migrants and their family members, and by randomly assigning treatment, I am able to causally identify the impacts of the informational conditions being tested. An experiment was first conducted among Salvadoran migrants recruited in the Washington, DC area. The migrants were asked how much of a potential $600 prize they wished to keep and how much they wished to send to a family member in El Salvador. The decision was incentivized, meaning that participants had the chance to win the allocation that they chose. To test whether migrants strategically react to changes in the observability of their income, they were randomly allocated into two treatment groups: those who were told their decision would not be revealed to their family and those who were told that their decision would be revealed.

These family members then participated in a separate experiment. They made an incentivized decision about how to spend a potential $300 remittance prize. To test for strategic reactions to the observability of their spending choices, as in the migrant experiment, half of the recipients were told that their choice would not be revealed to the migrant and the other half were told that their choice would be revealed. In a second, cross-randomized treatment addressing the inadvertent effects of barriers to communication, half of the recipients were informed of the migrant's preferences for how they should spend the money, and the other half were not.

I find that migrants remit $24 more on average out of the possible $600 (an increase of 5 percent over the control group mean of $440 sent) when their decisions are revealed. This effect is concentrated (and larger) in subsamples where the recipient's ability to punish the migrant for deviation is plausibly high. There is no corresponding evidence of strategic behavior in the recipient experiment: recipients who are told their choice will be revealed do not make choices that are more similar to the migrants' preferences than recipients whose choices are not revealed. However, reducing communication costs by revealing migrant preferences to recipients does have an impact, resulting in a 10 percent reduction in the difference between migrant preferences and recipient choices.

This paper is related to a set of field experiments that have examined the effects of offering migrants varying degrees of control over remittances. The idea behind these experiments is that offering control to migrants will mitigate a moral hazard problem in how recipients spend remittances. Ashraf et al. (2011) show that savings levels in bank accounts in El Salvador increase when migrants are given greater control over these accounts. In another experiment among Salvadoran migrants, Torero and Viceisza (2011) find little evidence that migrants send more when they are able to control how remittances are spent, but attribute this to the fact that the control offered by their experiment (vouchers for groceries) was too limiting. Chin et al. (2011) find that the impacts of an experiment that offered migrants assistance in opening bank accounts in the United States are concentrated among migrants who report having no control over how their remittances are spent. This suggests that migrants who are concerned that savings sent to El Salvador will be misused choose to keep those savings in the United States when given the opportunity to do so.

The main limitation of these papers is that while they acknowledge that migrants might have difficulty controlling recipient spending of remittances, they do not consider that information problems might run in both directions. The observational studies documenting information asymmetries in migrant households have also focused on migrant monitoring of recipient behavior (Chen, 2006, 2013; de Laat, 2008). One of the principal contributions of this paper is that it examines the impacts of information asymmetries on *both* sides of the migrant-recipient relationship. In fact, in this experiment, it is only migrants and not recipients who react strategically to whether or not their choices will be monitored. This demonstrates that recipients have important influence in the migrant-recipient relationship, something that has not previously been demonstrated empirically.

This paper also fits into a growing, broader literature on how information asymmetries affect intra-household resource allocation. Ashraf (2009) shows that, in the Philippines, men whose wives are the household financial managers hide income from their wives when that decision is private. When their decision is public, men choose to divert income to committed consumption that cannot be undone. Only when spouses communicate about their choices before they make them do men choose to share the income with their wives. Schaner (2012) finds that spouses are more likely to choose to save in individual (as opposed to joint) savings accounts when they are not well informed about each others' finances.[5]

This study builds on this literature in several ways. First, while these papers have largely focused on just one choice in the resource allocation process (whether or not to share income), the present experiment considers how information asymmetries can affect two different decisions made by families about economic resources. Specifically, in addition to the sharing of income, I also examine how income is spent once it is shared, and acknowledge that decisions may be affected by information asymmetries in both stages. Second, while these studies have focused on

[5] In another experiment in Zambia, women are more likely to take advantage of vouchers for contraception and use concealable forms of contraception when these vouchers are given to them outside of the presence of their husbands, showing that strategic reactions to information asymmetries extend beyond simply the allocation of funds in the household (Ashraf, Field and Lee, 2012).

strategic behavior, I study the effects of different types of information asymmetries, strategic and inadvertent, allowing me to evaluate their relative effects in the same population. Finally, this study documents that information asymmetries can be important outside of the husband-wife pair, the setting that has been the context of almost all the previous experimental work in this area.[6] People in developing countries often transfer resources within extended families (whether within or across households) and decisions about resource allocation consequently are likely to involve people beyond just the husband and wife. The results show that information asymmetries can have important impacts in extended families, but because migrants only react to being monitored when recipient ability to punish them is high, they also indicate that they may not matter in all families where resources are shared.

The paper proceeds as follows. Section II describes a framework for understanding how both the probability of being monitored and communication costs may impact decisions about remittances. Section III explains the experiment. Section IV describes the data and the empirical strategy. Section V presents the results, and Section VI concludes.

1.2 Theoretical framework

In this section I develop a simple model to frame my experimental results that shows how information asymmetries can lead to strategic behavior that affects both migrant sending and recipient spending of remittances. I achieve this by modeling both decisions as contingent contracts with an altruistic component between the migrant and the recipient. The structure of the model is similar to Chen's (2013) description of how male migrants in China monitor their wives' behavior. Specifically, Chen shows that when a migrant has imperfect information about his wife's actions and, further, incomplete information about her preferences, the contingent contract offered to the wife by the migrant may not be incentive compatible in all circumstances. I adapt a simplified version of this framework to describe the outcomes considered in this paper.

1.2.1 Migrant remittance decision

I characterize migrants' decisions to remit as being determined by both their altruism for their families at home and contingent contracts with those same families, where the families

[6] The dynamics of transfer arrangements in extended family networks has been studied (Foster and Rosenzweig, 2001) but little is known about how information imperfections affect behavior in these arrangements. Exceptions include Jakiela and Ozier (2012) who find that women in Kenya sacrifice investment returns in order to keep income secret from family members outside their household and di Falco and Bulte (2012) show that larger kin networks lead to higher levels of saving in non-shareable assets.

compel the migrants to send remittances through the threat of potential punishment.[7] An extensive literature exists on the motivations of migrants to send remittances. Commonly cited motives include altruism, payments for services provided by the family, loan repayment, repayments of other investments made by the family such as education, desire to return, and insurance (see Rapoport and Docquier, 2006 for a review). These motives may operate simultaneously, and while there is empirical evidence to support the existence of them all, the literature has been less successful in defining their relative importance. The purpose of this framework is to model the remittance decision in a way that allows both for motivations that may be affected by strategic behavior and those that will not be. Although this model is not specific about the exact motivations for the remittances sent by migrants, the idea of a remittance contract enforced through the threat of a punishment cost encompasses most possible motivations previously examined in the literature. The clear exception is altruistically motivated remittances which will enter separately in this framework.

The potential punishment that enforces the contingent contract will be represented as a utility cost to the migrant and can take several forms. One example of such a cost is substandard care for or attention to people (children or elderly relatives) or possessions (land, livestock or new investments) left by the migrant in the care of his family. Another is social sanctions against the migrant: many migrants come from areas with high rates of migration where there are strong social norms and expectations regarding the amount of money that migrants send home. Particularly for migrants who wish to return home one day, a damaged reputation may be seen as quite costly. Finally, migrants refusing to send home as much money as their families expect may damage their relationships with their families, relationships that migrants with tenuous positions in foreign countries may view as important.[8] Many of these potential punishments are related to the social closeness of migrants and recipients and, indeed, in a qualitative study of Ghanian migrants in the Netherlands, Mazzucato (2009) emphasizes the importance of the social

[7] The incorporation of these two motives together in one framework is drawn from Lucas and Stark's (1985) suggestion of a model of remittance sending that includes both altruism and migrant self-interest.

[8] A similar description of enforcement mechanisms can be found in Rapoport and Docquier (2006). Brown (1997), Hoddinott (1994), Lucas and Stark (1985), and Poirine (1997) all describe remittance contracts enforced through one or more of the discussed mechanisms. Additionally, in studies of dictator games within social networks Leider, et al. (2009) and Ligon and Schecter (2012) document the importance of the expectation of reciprocity in motivating giving.

proximity of migrants and recipients for the effective enforcement of remittance agreements.[9]

Description of the model

The model is constructed as a game with two types of players, migrants who send remittances and members of their families who receive those remittances. Migrants and recipients both get utility from consumption, which is defined for migrants as migrant income (I) minus remittances sent to the recipient (r), and for recipients as recipient income (Y) plus remittances received from the migrant (r). Because they are altruistic, migrants additionally derive utility from the consumption of recipients.[10] Migrant utility is then defined as

$U^M = u^M(I - r) + \gamma u^R(Y + r)$

and recipient utility as

$U^R = u^R(Y + r)$.

For both $u^M$and u^R, $u' > 0$ and $u'' < 0$. γ is the migrant's altruism parameter and is between zero and one.[11] In every period migrants earn either low income (I^L) or high income (I^H) where $I^H > I^L$.[12] The recipient strategy is to offer migrants a contingent contract that specifies the remittance amounts that should be sent for each income level. Migrants then decide whether to comply with this contract or deviate from it. Migrants who deviate (and whose deviation is discovered by the recipient) will suffer a utility cost (C^M) imposed by the recipient. This cost is assumed to be exogenous to the model, but will vary across migrant and recipient pairs. Migrants and recipients know each other's preferences and the value of C^M.

First consider the case where migrant income is fully observable to both migrants and recipients. Migrant payoffs are as follows where r^{c^i} is the size of the remittance sent when the migrant complies and $r^{d^i}$is the remittance sent when the migrant deviates. i is equal to L or H for the low and high income states:

Comply: $U^M = u^M\left(I^i - r^{c^i}\right) + \gamma u^R\left(Y + r^{c^i}\right)$

[9] Additionally, in focus groups done prior to the start of the project, migrants repeatedly cited high levels of pressure from family members as a key reason why they sent remittances home. Relatedly, in their work on Tongan migration to New Zealand, McKenzie, Gibson, and Stillman (2012) find that suggestive evidence that migrants underreport earnings to avoid pressures to remit from family members.

[10] For simplicity the framework does not include recipient altruism toward the migrant.

[11] Migrant altruism has been modeled in similar ways in Lucas and Stark (1985), Stark (1995), and Rapoport and Docquier (2006).

[12] Although variation in recipient income can affect migrant remittance decisions (as in Lucas and Stark, 1985) for the purposes of this paper, I assume Y to be fixed and low relative to migrant income.

Deviate: $U^M = u^M\left(I^i - r^{d^i}\right) + \gamma u^R\left(Y + r^{d^i}\right) - C^M$

The optimal values of r^{c^i} and r^{d^i} are solved for using backward induction. First, given I and γ, migrants choose r^{d^i} to maximize their payoffs when deviating such that:

$$u^{M\prime}\left(I^i - r^{d^i}\right) = \gamma u^{R\prime}\left(Y + r^{d^i}\right)$$

This first order condition implies that migrants set the marginal cost of remittances equal to their marginal benefit. Any further increase in remittances will therefore incur a higher cost than benefit for the migrant and lead to a net loss in utility.[13]

In order to induce migrant cooperation, recipients will set r^{c^i} at a level that is incentive compatible for migrants. In other words, the utility that the migrants get from complying with the contingent contract offered by the recipients must be greater than or equal to the utility they would gain from deviating and being punished. Because recipients wish to receive as much in remittances as possible, the incentive compatibility constraint will bind, and r^{c^i} will be set such that:

$$u^M\left(I^i - r^{c^i}\right) + \gamma u^R\left(Y + r^{c^i}\right) = u^M\left(I^i - r^{d^i}\right) + \gamma u^R\left(Y + r^{d^i}\right) - C^M$$

Because the contract is incentive compatible the migrant will always comply. This condition implies that when C^M is greater than zero the migrant will always send more than the voluntary optimum ($r^{c^i} > r^{d^i}$). If $C^M = 0$, then $r^{c^i} = r^{d^i}$ and the entire remittance payment is motivated by altruism. It is also important to note that r^{c^i} rises with C^M. The higher C^M, the more power recipients have to compel outcomes that are advantageous for them, namely higher remittance payments.

Asymmetric information

Now consider the more realistic case where recipients have imperfect information about migrant income. At the time of the remittance the only information about migrant income that recipients have is what they are told by migrants. However, after the remittance is sent, with probability p recipients will receive accurate information about migrant income, informing them

[13] I assume that conditions hold for r^{d^i} to be non negative. For example, assuming that both $u^M()$ and $u^R()$ are equal to $ln()$, $r^{d^i} \geq 0$ if $\gamma I^i - Y \geq 0$.

of whether the migrants earned I^H or I^L.[14] Recipients who do not receive this information continue to believe what the migrants have told them about their income. This gives migrants who have earned I^H the opportunity to deviate without being discovered by claiming they earned I^L and sending the contracted amount for the lower income level (r^{c^L}).[15] With probability $1-p$ the recipients will not discover the true income level, and the migrants will not have to pay C^M. For migrants who deviate in this way, p is the probability that that deviation will be detected.

Furthermore, p is not constant and can vary across time for each migrant. In every period the migrants know what p is, however recipients have incomplete information about p, knowing only the distribution of its possible values. Assume that p can be either low (p^l) or high (p^h) and that the recipient believes that $p = p^h$ with probability k.[16] The payoffs for migrants earning I^H are now:

Comply: $U^M = u^M\left(I^H - r^{c^H}\right) + \gamma u^R\left(Y + r^{c^H}\right)$

Deviate: $U^M = u^M\left(I^H - r^{c^L}\right) + \gamma u^R\left(Y + r^{c^L}\right) - p^i C^M$

When deviating the migrant will send r^{c^L} because that is the only possible method of deceiving the recipient and avoiding punishment.[17]

As in the case of observable income, recipients must set contracts that are incentive compatible for the migrants. This incentive compatibility constraint will vary by the probability that deviation will be detected.

Periods when $p = p^l$*:*

$$u^M\left(I^H - r^{c^L}\right) + \gamma u^R\left(Y + r^{c^L}\right) - p^l C^M \leq u^M\left(I^H - r^{c^H}\right) + \gamma u^R\left(Y + r^{c^H}\right)$$

Periods when $p = p^h$:

$$u^M\left(I^H - r^{c^L}\right) + \gamma u^R\left(Y + r^{c^L}\right) - p^h C^M \leq u^M\left(I^H - r^{c^H}\right) + \gamma u^R\left(Y + r^{c^H}\right)$$

Because $p^l < p^h$, r^{c^H} must be lower in periods when $p = p^l$ than in periods when $p = p^h$ in

[14] For example, imagine a situation where a migrant earns I^H because he finds some extra temporary work. The recipient may hear about this work from another relative or family friend living in the same community as the migrant in the United States.

[15] Note that nothing has changed for migrants earning I^L as the imperfect information does not afford them any more attractive deviation possibilities.

[16] Continuing with the example where a migrant earns I^H because he finds some extra temporary work, p may be high if another migrant from the migrant's home village has the same job and can relay this information to family members.

[17] Migrants could also deviate by sending r^{d^H} and paying C^M for sure. It is possible that the utility of this strategy is greater than the expected utility of sending r^{c^L}. This would lead to an incentive compatible contract unaffected by information asymmetries and therefore will not be considered here.

order to satisfy the migrant's incentive compatibility constraint. Given that recipients do not know the value of p, they must satisfy the constraint for p^l in order to ensure participation in all periods. The constraint for low probability of detection periods will bind, but the constraint for high probability of detection periods will not.

However, depending on the values of p^l, p^h and k, recipients have another option. They can offer a contract that binds on the high probability of detection period's incentive compatibility constraint but which is not incentive compatible in the low periods. The intuition is that recipients might have to lower the contracted amount (r^{c^H}) so much to induce cooperation in all periods that they would be better off receiving a higher amount in only the high probability of detection periods, than the lower amount in all periods. If recipients offer the contract that is incentive compatible for all values of p, then they will receive the amount that satisfies the constraint for p^l in every period, $r^{c^H p^l}$. If they offer a contract that is incentive compatible only for p^h then when $p = p^l$ migrants will deviate and the recipients will receive r^{c^L}. However, when $p = p^h$ the recipients will receive the higher amount that satisfies the incentive compatibility constraint for $p^h(r^{c^H p^h})$ meaning that they will receive $(1-k)r^{c^L} + k\, r^{c^H p^h}$in expectation. Therefore, the recipient will offer the contract that is not incentive compatible for all types when:

$$r^{c^H p^l} < (1-k)r^{c^L} + k\, r^{c^H p^h}$$

This framework describes a situation in which the optimal contract between migrants and recipients is not incentive compatible in all situations. This results in migrants acting differently depending on the probability that their income will be observed by the recipient. However, this will only happen when $C^M$is positive; if recipients do not have the power to punish the migrant, then the entire remittance is driven by altruism and is not affected by variation in recipient ability to monitor migrant income. This can easily be seen in the migrant's incentive compatibility constraints: when $C^M = 0$, p^i vanishes and $r^{c^i} = r^{d^i}$.

In summary, the model results in the following predictions regarding the migrant's remittance sending behavior:

Prediction 1: When the probability that recipients will observe migrant income is low, migrants earning high income may strategically take advantage of recipients' imperfect and incomplete knowledge of their income to send less money home.

Prediction 2: In pairs where the recipient ability to punish migrants is low, migrants' motivations for sending remittances are dominated by altruism, and these altruistic remittances are not affected by the probability that migrant income will be observed.

1.2.2 Recipient spending decision

I now consider the recipient's decision about how to spend remittances in a separate framework that can be developed in a parallel way. The decision that recipients make is modeled as the extent to which they follow the migrant's preferences for that spending decision. Recipients get utility from spending the remittance money on the things that they prefer, but they are also altruistic in that they get utility from spending remittances according to the migrants' wishes.[18] Although recipient altruism is modeled here as the recipient getting utility from the migrant's utility, the concept could also include recipients who follow migrant preferences simply because they want to. For example, they may value migrant advice on household budgeting and investment.

Migrants offer recipients a contingent contract specifying the extent to which remittances should be spent according to migrant preferences. Recipients then decide whether to comply with or deviate from that contract. With probability q migrants will learn how the recipients spent the remittance; otherwise they will only know what they are told by recipients (and believe that to be true).[19] Recipients who deviate and are discovered by the migrant will pay a utility cost C^R, which is the punishment that the migrant can impose on the recipient. Potential punishments in this case include withholding of future remittances, social sanctions (to the extent that the migrant can impose them from a distance), and familial discord. The size of the punishment (C^R) need not be equal to the punishment the recipient can use against the migrant (C^M), meaning that one may well have greater influence than the other.

d is what recipients would consume if they followed only their own preferences and b^c and b^d are the extent to which the recipients follow migrant preferences when they comply with the contract (b^c) and when they deviate from it (b^d).[20] α is the recipient's altruism parameter.

[18] For simplicity of exposition I ignore a third category of consumption: expenditures on which the migrant and recipient agree. Incorporation of this category does not change the qualitative predictions of the model.

[19] Migrants could find out about recipient spending behavior by, for example, communicating with other family members in El Salvador that may have knowledge of what the recipient has done.

[20] For example, imagine that a migrant sends a $200 remittance for which the migrant wants $100 to be spent on food and $100 to be spent on education. The recipient wants to spend $200 on home improvements. If the recipient actually spends $100 on food and $100 on home improvements then the recipient has followed the migrant's preferences on $100 of the $200 remittance.

Recipient payoffs can be expressed as follows:

Comply: $U^R = u^R(d - b^c) + \alpha u^M(b^c)$

Deviate: $U^R = u^R(d - b^d) + \alpha u^M(b^d) - q^i C^R$

The probability of detection when deviating (q^i) can be either low or high and varies across time. It is known to recipients, but migrants know only its distribution. As in the migrant remittance decision this leads to a situation where migrants may offer contracts that are incentive compatible only when the probability of detection is high.

Therefore, the framework results in the following predictions for recipient remittance spending behavior:

Prediction 1: When the probability that migrants will observe recipient spending is low, recipients may strategically take advantage of migrants' imperfect and incomplete knowledge of their spending to spend less according to migrant preferences and more according to their own preferences.

Prediction 2: In pairs where the migrant ability to punish recipients is low, recipients' motivations for spending remittances according to the migrants' preferences are dominated by altruism, and this altruistic spending is not affected by the probability that recipient spending will be observed.

The recipient choice is further complicated by the fact that barriers to communication may result in confusion on the recipient's part over what the migrant's preferences actually are and consequently in inadvertent (as opposed to strategic) deviation from those preferences. I will refer to these barriers as communication costs, but the concept is broader than just the cost of a telephone call. With distance, specificity about preferences may become difficult, migrants may feel uncomfortable expressing what they want, and recipients may sometimes have to make decisions without time to directly consult with migrants. Family members may also incorrectly assume that they know what the migrant would prefer. If these communication costs do play a role, decreasing them by making migrant preferences clearer could increase b, leading to the following prediction:

Prediction 3: Improved information about migrant preferences will increase the extent to which recipients follow those preferences.

The main point of this discussion is that strategic deviation can be a feature of the optimal contracts between migrants and their family members. The extent to which deviation is important will depend on the distribution of the probability of detection and the size of the punishments that can be inflicted. At the same time, communication costs can lead to inadvertent deviation when recipients make remittance spending decisions.

1.3 Project design

Testing for the effects of information asymmetries in the choices made by migrants and their family members is difficult for several reasons. First, both the observability of migrant income and recipient spending and the extent of communication costs may be correlated with unobserved characteristics of the migrant-recipient pair, making it difficult to causally identify the impacts of these information asymmetries. Second, precisely measuring any of these (observability of income and spending and communication costs) is difficult in a standard survey context.[21] Finally, capturing reliable information about the behavior of both migrants and their family members is logistically complicated. I implemented a randomized experiment to test the predictions of the framework discussed in the previous section that solves these problems. This experiment is conducted within the context of survey work for a separate field experiment on remittances and education among Salvadoran migrants in Washington, DC and their families in El Salvador.[22] Specifically, I exploit an unusual feature of this data collection exercise; it involves surveys with matched pairs of migrants and family members, allowing me to investigate the preferences and choices of both. In the experiment, I randomly vary (1) whether migrant income and recipient spending are observed and (2) the size of communication costs, allowing me to identify the causal impacts of both of these factors on migrant and recipient remittance behavior. Demographic survey data is used to explore how impacts vary by punishment ability.

Migrants were recruited in the Washington, DC metro area, at the two area locations of the Salvadoran consulate[23] and were interviewed while they were waiting for consular services.[24] The migrant survey was conducted between late September 2011 and late February 2012. Surveyors in the consulate approached migrants and invited them to participate. Because the

[21] For example, directly asking migrants whether they hide income from recipients may not yield truthful responses. Additionally, that question would not identify migrants who could hide income but choose not to.
[22] "Subsidizing Remittances for Education: A Field Experiment Among Migrants from El Salvador," (with Diego Aycinena and Dean Yang).
[23] 96% of migrants interviewed live in Washington, DC, Maryland or Virginia. The others live in states served by these consulate locations.
[24] The most common reason to go to the consulate is passport renewal, but other services include renewal of temporary protected status (TPS), registry of births and deaths, and notarization of documents.

focus of the companion experiment was remittances and education, participants were required to have a high school or college-aged relative in El Salvador.[25] Those who qualified and agreed to participate were administered a baseline survey followed by the randomized offer of a product designed to facilitate the sending of remittances for education to El Salvador.[26] The experiment described in this paper was conducted at the end of the survey but before the randomized marketing treatment.

Over the course of the survey migrants identified a high school or college aged student in El Salvador whom they were interested in supporting.[27] Interviews were subsequently conducted with a member of the household of that student. If the student was 18 years of age or older the student was to be interviewed, and for those students under 18 a guardian was identified to be interviewed. If the indicated person was not available, an alternative adult in the household was interviewed instead. Of the surveys completed, 45 percent were done with the student, 40 percent with the student's guardian, and 15 percent with another adult in the household. The El Salvador survey was conducted by phone in the days following the migrant survey in the United States; the median number of days between the US and El Salvador survey was eight. The El Salvador surveys concluded in mid March 2012, roughly two weeks after the conclusion of field work in the United States. 82 percent of families in El Salvador completed the survey. The experiment in the El Salvador survey was also conducted at the end of the survey. Figure 1.1 describes the phases of the project in the order that they occurred for each pair of participants.

1.3.1 Migrant experiment

The migrant experiment consisted of an incentivized remittance sending decision. Migrants were told that they were being given the chance to win $600 and would have to decide how much of the prize to keep for themselves and how much to send to their family member in El Salvador. Migrants could split the $600 as they wished, but were restricted to using $100 intervals for simplicity.[28] The prize was awarded through a lottery.[29] Although budgetary

[25] 24% of migrants approached participated. Of those that did not participate, 77% did not know an eligible student in El Salvador, 14% refused, 7% were not from El Salvador, and 2% had other reasons.

[26] This was a randomized intervention and migrants received offers of different versions of the product depending on their assigned treatment group. Migrants in a control group received only information and no product offer.

[27] Although the migrants were not required to select a family member as the student, in practice 97% did.

[28] In pilot surveys where migrants were not limited to $100 intervals, almost all chose to split the money in $100 intervals.

[29] Two prizes were awarded. If asked, surveyors told migrants the number of prizes and the date of the drawing. The first prize was awarded midway through survey work and the second when survey work had concluded. Migrants were eligible for only one drawing.

restrictions did not allow for all participants to win the prize, the use of the lottery incentivized participants to treat this as a real decision.[30] In the Ashraf et al. (2011) study of a similar population of Salvadoran migrants, migrant median monthly income was $2,080. Consequently, $600 represents a significant increase in monthly income. The question text can be found in Appendix A. Migrants were randomly allocated into two groups: those who were told that their choice would be revealed to their family member, and those who were told that their choice would not be revealed. In all cases the family member referred to in the question was the person to be surveyed in El Salvador: the student if the student was 18 or over, or the student's guardian if the student was under 18. A description of the treatments is presented in Figure 1.2.

By offering migrants the chance to win $600 in extra income, this experiment essentially places migrants in the high income state discussed in the framework and randomly varies the probability that that extra income will be observed.[31] This allows for an explicit test of whether, as predicted by the model, migrants are more likely to deviate from their agreements with family members and send less money home when the probability that that deviation will be detected is low. Because changes in the probability of detection essentially vary the ability of the recipient to monitor the migrant's actions, I refer to this treatment as the migrant monitoring treatment. Viewing the experiment in the context of the model leads to the following hypothesis:

Hypothesis 1:

Migrants in the treatment group where the migrant choice is revealed to recipients (i.e. where the probability of detection is one) should send more than migrants whose choice is not revealed. However, this effect should vary by migrant-recipient pair. In pairs where recipients cannot threaten strong punishments, migrants are not affected by the probability of detection and therefore there will be no impact of the monitoring treatment when they make the decision about how much money to keep and how much to send to the recipient. Their entire remittance will be motivated by altruism. Migrant responses from the baseline survey can be used to proxy for the

[30] Laury (2005) conducts a laboratory experiment in which respondents are shown to make the same choices when payoffs are random as when payoffs are guaranteed.

[31] The design of the experiment assigns $p^h = 1$. p^l is equal to the baseline recipient ability to observe the migrant's windfall in the absence of the experiment. Use of extreme values does not alter the predictions of the model, although information asymmetries are more likely to be important the greater the difference between p^l and p^h. This affects the external validity of the results if the probability that income will be observed is unlikely to be close to one. However, given that the networks within which migrants in the United States live and work are often closely related to their home country networks (Munshi, 2003), instances when the probability that income will be observed is quite high are likely.

recipient ability to punish migrants.

1.3.2 Recipient experiment

The recipient experiment consisted of an incentivized remittance spending decision. The respondents in the El Salvador phone survey were told that because their family member in the United States participated in the study, they now had the chance to win a remittance worth $300. They had to decide what to spend the remittance on and were asked to split the $300 in any way they wished among four spending categories: restaurant meals, education, daily expenses, and health expenses. Recipient choices were limited to four categories for simplicity of implementation in the context of a phone survey. If among the winners, recipients would receive exactly the allocations that they requested.[32] Prizes were awarded in kind. The median monthly remittance in the Ashraf et al. (2011) study was $325, so a $300 remittance is a standard amount for many recipients. The question text can be found in Appendix B. Two separate treatments were administered to recipients, the recipient monitoring treatment and the recipient communication treatment.

Recipient monitoring treatment:

In a parallel treatment to the migrant monitoring treatment, recipients were randomly allocated into two groups: those who were told that their choice would be revealed to the migrant, and those who were told that their choice would not be revealed to the migrant. This treatment randomly varied the probability that recipient spending would be observed and is an explicit test of the model's prediction that recipients are more likely to strategically deviate when the probability of detection is low.

Recipient communication treatment:

During the US survey, migrants were told about the lottery for recipients and asked what their preferences were for how the recipients would spend the money. Again, recipients were randomly allocated into two groups: those for whom the migrant's preferences were revealed and those for whom the migrant's preferences were not revealed. Making these preferences clear is a proxy for improving communication, and this treatment is therefore a test of whether or not communication costs can lead to inadvertent deviation from migrant preferences by the recipient.

The two recipient treatments were cross randomized, also allowing for the analysis of

[32] Four prizes were awarded. If asked, surveyors told recipients the number of prizes and the date of the drawing. Two prizes were awarded midway through survey work and the other two when survey work had concluded. Recipients were eligible for only one drawing.

their interaction. They are depicted in Figure 1.3. Viewing the recipient experiment in the context of the model results in the following hypotheses for recipient behavior:

Hypothesis 2:

Recipients in the treatment group where their choices are revealed to the migrant (i.e. where the probability of detection is one) should make choices that are closer to the migrants' preferences than recipients whose choice is not revealed. This effect should not be evident in pairs where the migrant cannot threaten a strong punishment. In these cases the extent to which the recipient complies with the migrant preferences will depend wholly on altruism.

Hypothesis 3:

Revealing migrant preferences to the recipient should decrease the difference between the recipients' choices and the migrants' preferences when communication problems exist. This effect will not necessarily depend on the potential punishment because communication issues may affect compliance with migrant preferences compelled by the migrant as well as altruistic compliance by the recipient.

1.3.3 Experiment logistics

In order for the experiment to work as intended, respondents must have believed that the threat of revealing their choices to their family members was credible. Because the interviewer collected contact information for the recipient families from the migrants and allowed the migrants to use a project phone during the interview to call their family members and tell them about the study, migrants were aware that their family members could indeed be contacted. Similarly, because recipients being interviewed knew that they had been contacted through the migrant, they also knew that their migrant family members could be contacted. Although it has no impact on the results of the experiment, for all respondents in the "choice revealed" treatment groups of the monitoring treatments, an effort was made to inform their family member of the choice made by the participant. After both the migrant and recipient survey had been completed, text messages were sent to the appropriate participants informing them of the choice of their family members. Participants without cellular phones received a phone call from a project staff member with the information.

The randomization in this study was performed at the participant level. Surveys were pre-assigned treatment status before being sent into the field and migrant and recipient treatments were randomized separately. Because remittance behavior can vary by season it was important to ensure that treatments were balanced over time.[33] I achieved this by stratifying the randomization for all treatments within groups of 16 surveys and by the treatment offered in the companion experiment. The recipient treatments were additionally stratified by the migrant treatment. Because the experiment was conducted in conjunction with the baseline survey it was not possible to stratify on individual baseline characteristics.

1.3.4 Threats to interpretation

Although the experimental methodology used in this paper allows for the causal identification of the effects of information asymmetries that are otherwise difficult to isolate, there are several aspects of the experimental design that could lead to arguments that participants' behavior in the experiment is not the same as it would be in their day-to-day lives. The first of these is that the experiments, particularly the migrant experiment, ask participants to make decisions about windfall income that is given to them rather than earned, and that migrants may be more generous with this income than they would be with other income. There are several responses to this. First, although it is true that the income in the experiment is transitory and not permanent, many of the migrants in this study work in jobs where income is highly variable from month to month, making transitory vs. permanent income a less important distinction. Second, studies that have examined earned vs. unearned windfall income have found that people are more generous with unearned winnings, but that the effect is small (Jakiela, 2009). Finally, the focus of the paper is not on the total amount sent by the migrants, but on the effect of the monitoring treatment on the amount sent. The issues discussed here should apply equally to each treatment group. If anything, if migrants in the choice not revealed group are more able to keep the funds for themselves, the impact of the monitoring treatment should increase as migrants feel more ownership over the winnings, meaning that the results in this paper can be considered a lower bound on the true effect.

A second potential issue is that in both the migrant and recipient experiment prizes were awarded by lottery, meaning that the expected value of the prize for each participant is much lower than the value of the actual prize. There is a concern that participants may be more generous or less likely to make decisions that may upset their partner because they know they are unlikely to win the lottery prize. Although little research has been done into how experimental

[33] In particular, the time period of the study included December, the most popular month for remittance sending due to Christmas.

subjects react to lottery prizes, some evidence does exist. Laury (2005) conducts a laboratory experiment in which respondents are shown to make the same choices when payoffs are random as when payoffs are guaranteed. Additionally, again because the questions in this paper relate to the differences between the treatment groups, if the lottery does impact participants' decisions, we can again consider the estimated effect to be a lower bound on the true effect.

The final issue is that of the fungibility of choices made during the experiment. Both migrants and recipients could potentially undo their choices during the experiment through their actions afterwards. Migrants could choose to not send a remittance that they would have sent otherwise, and recipients could comply with migrant wishes during the experiment and then make purchases later that the migrant would not agree with. Although it is possible that some of this behavior is occurring, it is again not necessarily important for the interpretation of the impacts of the treatment. If the results show differences between the two treatments then that is evidence that people are reacting to variations in information.[34]

1.4 Data and estimation strategy

1.4.1 Data

The migrant baseline survey collected extensive information on migrant and recipient demographics and characteristics of migrant family relationships both in the United States and in El Salvador. It contained detailed information on remittances sent by the migrant to the recipient household and to other households and a set of questions to assess the quality of the migrant's relationship with the recipient household and the migrant's involvement in household affairs. The recipient survey, administered by phone, was shorter and contained demographic information and some limited questions on remittances received from the migrant.[35]

Table 1.1 shows summary statistics from both the migrant and the recipient surveys. 1,581 migrant surveys were performed and, of those migrant surveys, 1,298 recipient surveys were successfully completed. This is a completion rate of 82 percent. For the migrant survey, summary statistics are shown both for the full sample and the sample with completed recipient surveys. No meaningful differences are evident between the two samples; therefore I limit the

[34] Even without evidence of differences between treatments, if participants wish to do something that their partner would disagree with it makes sense for them to take advantage of the experiment to do so, when the probability of keeping that action secret is high.

[35] It also contained an extensive module on the education of children in the household.

analysis sample to the 1,298 migrant-recipient pairs with completed El Salvador surveys.[36] This allows me to examine the behavior of migrants and recipients in the exact same sample. Importantly, results from the migrant experiment do not change significantly between the two samples. Additionally, I show that attrition from the full sample of migrant surveys to the sample of completed recipient surveys is not related to treatment status (Tables 1.2 and 1.3 below).

In the final analysis sample, the treatment breakdown is as follows. For the migrant monitoring treatment there are 648 migrants in the "migrant choice not revealed to recipient" group and 650 in the "migrant choice revealed to recipient" group. In the recipient monitoring treatment there are 638 people in the "recipient choice not revealed to migrant" group and 660 in the "recipient choice revealed to migrant" group. For the recipient communication treatment there are 641 people in the "migrant preference not revealed to recipient" group and 657 in the "migrant preference revealed to recipient" group. These breakdowns into treatment groups can be seen in Figure 1.2 (migrant experiment) and Figure 1.3 (recipient experiment).

The migrants are half male and half female with an average age of 38. Importantly, 85 percent have sent remittances to the recipient household in the last 12 months, indicating that most pairs in the sample have an established remittance relationship. Average annual remittances to the recipient household (reported by the migrant) are $2,629.[37] Average annual remittances to other households in El Salvador are $1,059. The $1,600 difference between average remittances to the recipient household and average remittances to other households suggests that in most cases the recipient household is the migrant's primary remittance recipient. The mean number of years in the United States is 11, so the sample is composed largely of migrants who are established in the United States. 32 percent of migrants report having a son or daughter aged 22 or under in El Salvador and 69 percent report communicating with the recipient household at least weekly. The sample is also low income; half of the migrants report earning $400 a week or less.[38] Because of the structure of the project, the interviewed recipients are either the student identified by the migrant (45 percent) or the student's guardian if the student is under 18 (40 percent). The remaining 15 percent of interviews were done with a different adult in the

[36] Additionally 10 observations are lost because respondents did not answer the questions that made up the experiment.

[37] Remittance data on the recipient survey was collected by asking the migrant for the average value of remittances sent and the frequency of those remittances. The migrant was additionally asked to report the annual amounts of remittances sent for special occasions or emergencies.

[38] Respondents were asked to classify the combined income of them and their co-resident spouses into one of four categories: $400 weekly or less, $401 - $600 weekly, $601 to $800 weekly, $801 or more weekly.

household if the indicated student or guardian could not be reached. The recipient sample is heavily female (68 percent) because identified student guardians tend to be female.

Because migrants were recruited in the Salvadoran consulate and screened into the study on the basis of having a young adult relative in El Salvador, a concern may be that the respondents are not representative of the larger migrant community and that the results are therefore not indicative of what might be found in a more representative sample. In Table 1.A.1, I compare characteristics of the migrants from the baseline survey (gender, age, time in the US, household size and education) to migrants in the 2008-2010 American Community Survey (ACS). I restrict the ACS sample to non-US citizens aged 18 to 65 who live in the Washington, DC metro area who are either Salvadoran born or Hispanic. The study participants are quite similar to the ACS samples, in particular to the Salvadoran born sample, suggesting that study participants are not overly different from the greater migrant population.

Table 1.1 also provides suggestive evidence that information asymmetries may be important in these transnational households. Because monitoring and communication costs should be much less important when migrants and recipients agree about how remittances should be spent, I examine whether migrants and recipients have different preferences. During the baseline surveys, both were asked to list the three most important budget priorities for the recipient household from a set list of seven categories: food and other basic expenses, health, education, savings, entertainment, home improvement and transport. Despite significant bunching of responses in the first three categories (food, health and education), only 48 percent of pairs report the same three priorities, suggesting that migrant and recipient preferences for the spending of remittances do differ to some extent. I also check whether communication costs are likely to be important by testing the migrant's knowledge of the recipient household. Only 24 percent of migrants could correctly estimate the student's GPA as reported by the recipient, and only 43 percent could correctly report the mode of transport a student uses to get to school.[39] Although this is not the same thing as recipients not understanding how migrants want them to spend remittances, it is evidence that knowledge does not necessarily flow freely between countries.

[39] The questions about student GPA and transport to school were only asked when the student is reported to be in school. Migrants were asked to report the student's GPA within a 2 point (out of 10) range while recipients reported an exact number. The migrant was said to have correctly reported the GPA if the recipient's response was within the range the migrant indicated.

The random assignment of the treatments in this experiment allows for the causal identification of their impacts. Randomization should provide treatment groups that are the same on average so that any difference between the groups can be attributed to the treatment and not some pre-existing difference between groups. Tables 1.2 and 1.3 test whether the treatment groups are balanced on observed characteristics from the baseline survey for the treatment groups for the migrant experiment and the recipient experiments respectively. In Table 1.2 the means for both treatment groups in the migrant monitoring treatment are presented in the first two columns and the p-value of the hypothesis test of whether or not those means are equal is in the third column. Overall the treatment groups are well balanced: only two of 34 differences are significantly different from zero at the 10 percent level. Table 1.3 shows the means by treatment group for the two recipient treatments and p-values for differences in those means. Again the groups are well balanced, only three of the 34 p-values for the recipient monitoring treatment and one of the 34 p-values for the recipient communication treatment are less than 0.10. Some differences between treatment groups may occur by chance, and these few small differences are not cause for concern. However, to allay any concerns of an unbalanced sample affecting results, I include regression specifications with control variables.

The first row of both Tables 1.2 and 1.3 also test whether attrition from the full sample of migrants to the estimation sample of migrant-recipient pairs with completed recipient surveys is related to treatment. Attrition is not significantly related to treatment for migrants or recipients.

1.4.2 Estimation strategy

Migrant experiment

The results of the migrant experiment can be analyzed by estimating the following regression using ordinary least squares:

$$Remit_i = \delta + \alpha ChoiceRevealed_i + X_i'\gamma + \varepsilon_i \tag{1}$$

where $Remit_i$ is the dependent variable indicating the amount that the migrant chose to send to the recipient, or, alternatively, an indicator for whether or not the migrant chose to send all $600. $ChoiceRevealed_i$ is the treatment indicator for the monitoring treatment, and it is equal to one when the migrant's choice is revealed to the recipient. The coefficient α is the average difference between how much migrants choose to send when their decisions are not revealed and when they are. If α is positive, migrants send more money to the recipients when $ChoiceRevealed_i$ equals one. X_i is a vector of control variables that includes migrant age, gender, education, household size, years in the United States, remittances to recipient household, and other migrant background characteristics. It also includes fixed effects for randomization stratification group. Because treatment is randomly determined, the inclusion of control variables is not necessary for casual inference, but I will show specifications with and without the controls to show that they do not affect the results. ε_i is the error term, which I adjust for heteroskedasticity.

Recipient experiment

Unless average migrant preferences and average recipient preferences are different, regressions examining the impact of the treatment on the amounts allocated to the four different categories by the recipients will be uninformative. However, because the US survey collected the migrant's preferences over the recipient's choices for all participants, it is possible to examine the exact parameter described in the model guiding the experiment: the extent to which the recipient's choices match the migrant's preferences. I operationalize this concept as the absolute value of the difference between the recipient's choice and the migrant's preference in each of the four categories. I also create a summary measure across the four categories by summing the four difference variables and dividing by two to scale the total to 300. I refer to this as the total difference, and it is the primary dependent variable of interest. It is a measure of the number of dollars out of the 300 on which the migrant and recipient match. For example, a total difference of 100 would mean the recipient's choices matched the migrant's preferences on 200 of the 300 dollars, but that they allocated the remaining 100 dollars to different categories.

The results of the recipient experiment can be analyzed by estimating the following regression:

$$Difference_i = \varphi + \beta_1 ChoiceRevealed_i + \beta_2 PreferenceRevealed_i + Z_i{}'\theta + \mu_i \qquad (2)$$

where $Difference_i$ is the difference between migrant preferences and recipient choices in each of the four spending categories or the total difference. $ChoiceRevealed_i$ is the treatment indicator for the recipient monitoring treatment and is equal to one when the recipient's choice is revealed to the migrant. $PreferenceRevealed_i$ is the treatment indicator for the communication treatment and is equal to one when the migrant's preferences are revealed to the recipient before the recipient decides how to allocate the remittance funds. The coefficient β_1 is the average difference in the difference between migrant preferences and recipient choices when the recipient choice is not revealed as compared to when it is revealed. Similarly, β_2 is the average difference in the difference between migrant preferences and recipient choices when the migrant's preferences are revealed to the recipient as compared to when they are not revealed. If, as predicted, revealing the recipients' choices to the migrants and communicating the migrants' preferences to the recipients causes the recipients to make choices more similar to the migrants' preferences, then the difference variable will be smaller in the "choice revealed" and "preference revealed" treatment groups, and β_1 and β_2 should be negative. Z_i includes the same variables as X_i in the migrant experiment as well as recipient gender, age, education, household size and the number of days between the migrant and recipient survey. Fixed effects for randomization stratification variables (survey group and migrant treatment) are also included. μ_i is the error term, which I adjust for heteroskedasticity.

I also examine the interactions of the two treatments. It is possible, for example, that any impacts of the monitoring treatment could be amplified by revealing migrant preferences. Therefore, I also estimate an alternative specification with indicators for each unique combination of the monitoring and communication treatments.

1.5 Results

1.5.1 Migrant experiment

I first analyze the results of the migrant experiment in which migrants make an incentivized decision over how much of a potential \$600 windfall to send to the recipient and how much to keep. Figure 1.4 shows the distribution of the amount sent by migrants, separately by treatment group. Because the experimental protocol limited migrants to splitting the money in 100 dollar increments, the distributions are discrete. The first observation to be made from this figure is that the migrants send large amounts: over half of the migrants in both treatment groups choose to send the entire \$600 to the recipient. The other smaller spike in both distributions is at \$300 where migrants decide to split the money equally between themselves and the recipient. Despite the fact that the two distributions follow the same basic shape, differences are evident. Specifically, the spike at sending everything is smaller when choices are not revealed (53 percent versus 58 percent) and the percent of migrants choosing to send \$400 and less is higher (44 percent versus 38 percent). The difference between the two treatment groups is visually clearer in Figure 1.5 which graphs the cumulative distributions of the amount sent by the migrant by treatment group. The spikes at \$300 and \$600 are clearly apparent, and it is also easy to see that the distribution of the choices in the "choice revealed" treatment group is always below the distribution of choices in the “choice not revealed” group.

The fact that almost all migrants in the “choice not revealed” treatment group choose to send something is consistent with the model presented in Section II, where migrants who deviate when the probability of detection is low still send positive amounts in remittances. Additionally, given that the experimentally induced “low” probability that recipients will observe migrant income is small, the fact that most migrants in this group choose to send the entire $600 is suggestive that the altruistic component of remittances is high.[40] However, the differences between the two distributions are evidence that information asymmetries also seem to play a role. Migrants whose choices are not revealed are choosing to send less home.

These results are formalized in Table 1.4 using a regression framework that estimates regression equation 1 from Section IV of this paper. Columns 1, 2, and 3 show results for amount sent by the migrant and columns 4, 5, and 6 for an indicator variable indicating whether or not the migrant sent everything. Columns 1 and 4 are a simple regression of the dependent variable on treatment status, columns 2 and 5 include stratification cell fixed effects, and columns 3 and 6 further add the demographic control variables.[41] The results are robust to the exclusion of control variables, although the impact of treatment on the migrant sending everything is no longer significant when controls for stratification cell are included because the magnitude of the coefficient drops slightly. Migrants send $20 to $24 more when their choice will be revealed, which represents a 5 percent increase over the “not revealed” group mean. Additionally, migrants are 4 to 5 percentage points more likely to send everything when their decision will be revealed to the recipient, but the coefficient is not quite statistically significant at standard levels when control variables are added.

Table 1.4 also reports the coefficients on the demographic control variables included in columns 3 and 6. Five characteristics predict the migrant’s choice. Female migrants send on average $26 less than male migrants. Although women keep more on average than men, the effect of the treatment does not vary by gender (results not shown, available from the author on

[40] The amount sent by the “not revealed” treatment group is not necessarily completely due to altruism. Recall that, according to the framework, migrants must send an amount that makes their deviation credible. Additionally, migrants may be reacting to a certain level of baseline recipient monitoring that exists outside of the experimental construct.

[41] Control variables are migrant gender, age, years of education, household size, years in the US, whether the migrant lives with his/her spouse, whether the migrant has a child 22 or under in El Salvador, whether the recipient is the migrant’s close relative, whether the migrant is in the lowest income bracket, migrant’s annual remittances to the recipient household and whether the migrant communicates with the recipient household weekly.

request). Migrants who have been in the United States for longer send more, although the effect is small. Migrants who live with their spouses send $29 less than those who do not. This is likely because they have greater financial obligations in the United States and are more likely to have their immediate family with them in the United States. Migrants in the lowest income bracket are estimated to send $22 less on average than those in the other income brackets. Finally, total annual remittances sent are positively correlated with amount sent in the experiment. The coefficient is small, but the relationship suggests that migrant behavior in the experiment is related to real world migrant behavior.

The results in Table 1.4 show that information asymmetries can affect migrants making remittance decisions, and that at least some migrants take strategic advantage of a situation where the probability that their income will be observed is very low. The size of the effect (a $20 increase in amount sent when the migrant's choice is revealed) is not large, but it is similar to the size of the documented correlations with the demographic variables in column 3 of Table 1.4. The size of the effect is also comparable to those in experimental studies in families (Hoel, 2012) and social networks (Leider et al., 2009; Ligon and Schechter, 2012) that study the effects of making choices in dictator games known to the recipient. For example, Ligon and Schechter find that 91 percent of sharing in their experiment is related to altruistic motives. However, they also find that strategic behavior in their games predicts real-world strategic behavior, while altruistic behavior in the games does not predict any real-world activity. This suggests that strategic behavior may in fact be even more important outside of the experimental context than within it.[42]

Information from the baseline survey allows for further investigation of the mechanics of this result. Specifically, the model presented in Section II predicts that information asymmetries are only important in pairs where the recipients are inducing migrants to send remittances above what they would have sent altruistically through the threat of punishment. If recipients cannot threaten punishment then no differences between treatment groups should be observed.

Several variables from the baseline survey can plausibly be thought to proxy for punishment costs described in Section II. I examine how the treatment effect varies by these variables. I do not have a perfect measure of these potential punishment costs (and certainly one

[42] The effect size can also be compared to other studies with experimental designs that are not as similar. Jakiela and Ozier (2012) estimate a 4 percent kin tax on income in an experiment where participants sacrifice returns on income in order to keep it secret. Goldberg (2011) estimates a 7 percent sharing tax on income in an experiment where she compares the spending of the winners of public lotteries to the winners of private lotteries.

would be hard to obtain), but by showing a consistent pattern with all five of these variables the argument that ability to punish is important is convincing. The five variables, the predicted relationship with punishment ability, and the rationale for choosing them are described below.

- *Migrant years in the United States* (negative correlation with punishment ability): A migrant's reputation at home is important for migrants who wish to return, and the probability of return may decline with length of time in the United States. With time it is also more likely that the migrant has paid off any debts related to his initial migration costs. The median number of years in the United States is 10.
- *Migrant has a child 22 or under in El Salvador* (positive correlation with punishment ability): Migrants who have left a non-adult child in El Salvador may have left that child in the care of the recipient. The possibility of child care that does not meet the migrant's preferences could be a powerful tool to compel migrants to send more money home. 34 percent of migrants have a son or daughter aged 22 or under in El Salvador. [43]
- *Migrant and the recipient are closely related* (positive correlation with punishment ability): This is defined as spouses or parent and child. Being closely related can mean both that migrants have entrusted recipients with the care of things that are important to them and that positive relationships with the recipients are valuable to the migrants. 31 percent of migrants and recipients are closely related.
- *Migrant communicates with recipient household at least once a week* (positive correlation with punishment ability): Frequent communication is a sign that migrants value their relationships with recipients. 71 percent of migrants report communicating at least weekly with the recipient household.
- *Remittances sent by migrant to recipient household* (positive correlation with punishment ability): Because remittance relationships where recipients induce migrants to send money result in higher remittance payments, higher remittances may indicate high punishment costs. The median annual remittance total to the recipient household reported by the migrant is $1,800.

Although these variables are all plausible proxies for punishment costs, given that they

[43] The 22 and under cutoff is used because it was available on the survey which measured the number of young relatives up to college age the migrant had in El Salvador. The structure of the question does not allow me to identify whether the child is in the recipient's home or not.

generally indicate a stronger or closer relationship between migrant and recipient it could be argued that they may also be proxies for higher levels of altruism. It is true that in general punishment costs and altruism may be correlated, but as described in the model, altruistic remittances should not be affected by variations in monitoring. In other words, if these variables were proxies for only altruism and not punishment ability, the treatment effect of monitoring in the high altruism sub-groups should not be higher than in low-altruism sub-groups. Additionally, the mean amount sent in the treatment group where decisions are not revealed is in every case *lower* in the high punishment cost sub-groups than in the low punishment cost sub-groups. Because payments in this "choice not revealed" treatment group should be largely motivated by altruism, this is evidence that the variables chosen are representing more than just higher levels of altruism.

Table 1.5 presents regression results by subsamples of these variables. For the continuous variables (years in the US and remittances) the sample is split at the sample median, and for the binary variables (child in El Salvador, close relationship, and weekly communication) the sample is split according to the two values of the variable. Panel 1 presents regressions without any control variables and Panel 2 presents regressions with stratification group fixed effects. The results are striking in that for each of these variables, the treatment effects are almost entirely concentrated in the subsample where punishment costs should be higher (columns 2, 4, 6, 8 and 10). These treatment effects are larger and more precisely estimated than in the full sample: depending on the subsample, coefficients range from 32 to 56 more dollars sent when the choice is revealed. These numbers are about 7 to 13 percent of the average amount sent in the "choice not revealed" group. In the subsamples where punishment costs should be low (columns 1, 3, 5, 7, and 9) the coefficients are all small and do not approach statistical significance. The table also reports the p-values on the test for equality of the treatment effects in the two subsamples for each of the five proxy variables. Two of the five coefficient pairs in both panels are statistically significantly different from each other. An alternate specification that utilizes the first principal component of the five proxy variables as a summary measure yields similar results (Table 1.A.2). These results are consistent with the model's prediction that when punishment costs are low, variation in the observability of migrant income will not affect migrant remittance decisions.

An alternative explanation of the results in the migrant experiment is that migrants, instead of being motivated by a remittance contract with the recipient, simply care about being perceived as altruistic and utilize the “choice revealed” treatment to signal that altruism to their family. This concept of signaling altruism was developed by Bénabou and Tirole (2006). I cannot definitively rule this out; however, several factors suggest that it is unlikely. For example, the strong patterns of heterogeneity by sub-group are directly connected to the theoretical framework presented here, but it is not obvious how they would relate to a story about signaling. To be consistent with the sub-group results, the variables that describe recipient punishment ability would also have to represent groups to which migrants cared about appearing altruistic. However, across all sub-groups, the migrants’ allocations in the “not revealed” treatment group are high, suggesting that migrants are altruistic to all recipients. If the signaling story were true it would then have to be the case that true altruism and the desire to signal altruistic behavior were not at all correlated. Additionally, as noted in Table 1.5, the effect of revealing the migrant’s choice is concentrated in pairs where actual remittances to the recipient are above the sample median. Given that these are migrants who would have repeatedly signaled their generosity already, it is not clear why migrants who send fewer remittances overall would not take advantage of this low cost opportunity to do so. [44]

1.5.2 Recipient experiment

Analysis of the migrant experiment found that migrants react strategically to variations in the ability of recipients to monitor their income. Previous literature examining information asymmetries in remittance behavior has suggested that migrant monitoring of recipients should also be important (de Laat, 2008; Ashraf et al., 2011; Chen, 2013). To look for these effects in the context of this experiment, I now turn to analysis of the recipient experiment in which recipients allocated a potential $300 remittance prize among four different spending categories. Mean amounts allocated to different spending categories by recipients and migrants are presented in Table 1.6. The first two columns show the mean amounts allocated by recipients broken down by the recipient monitoring treatment and columns 3 and 4 show mean recipient allocations by the recipient communication treatment. The fifth column shows the means of the preferences reported by the migrant.[45] Across both recipients and migrants education is the most

[44] Although the context is different, in an experiment studying the social networks of Harvard students, Leider, et al. (2009) are able to definitely rule out the signaling explanation for non-anonymous giving in favor of one based on reciprocity and future interactions.

[45] Recall that migrant preferences for the recipient’s decision were solicited from all migrants.

popular choice.[46] Daily expenses are the next most popular category, followed closely by health and finally restaurant meals. As discussed previously, unless clear differences between migrant and recipient preferences are evident in the population on average, an analysis of the impact of treatment on amounts allocated to different categories will not be interesting. Although migrants allocate less to education than recipients and more to daily expenses, health expenses and restaurant meals, regressions of treatment on the raw amounts recipients choose to allocate to the different categories do not reveal any interesting patterns (shown in Table 1.A.3).

A more revealing analysis utilizes the data collected from both the migrant and the recipient to analyze how the treatments affect the pair-level differences between their choices. Table 1.7 displays the mean differences by recipient treatment. The results are displayed separately for the monitoring treatment and the communication treatment; the means from the monitoring treatment and p-values testing the equality of those means are in the first three columns and the corresponding information for the communication treatment is in the last three columns. Means of the differences for the four spending categories as well as the mean total difference are shown. For both treatments the prediction is that the difference will be smaller in the "revealed" treatment group. When the probability spending choices will be observed is high or when recipients are well informed about migrant preferences, recipients should more greatly adhere to those preferences.

This prediction is not borne out for the monitoring treatment. For all spending categories and the total difference, the means across the two treatment groups are essentially equal. However, differences are evident for the communication treatment. In all categories the difference between recipient choices and migrant preferences is smaller when the migrants' preferences are revealed than when they are not. Although of the spending categories only the difference for education is significant, importantly so is the total difference, implying that migrants and recipients are getting closer together overall. The $14 reduction in the total difference is driven by the difference in education spending with the corresponding reductions in the differences in other categories being split between daily and health expenses and, to a lesser

[46] The preference for education may be partly due to the fact that participants answered this question at the conclusion of a survey that was rather heavily focused on questions about education, meaning that they may have been primed to consider education. This is not necessarily a problem as there is no reason to believe that either migrants or recipients were more primed than the other.

extent, spending on restaurant meals.[47]

Table 1.8 shows these results in regression format and adds control variables. Panel 1 shows the results from estimating the regression equation that estimates the effect of each treatment separately and panel 2 presents the results of estimating an alternate specification that considers the separate effects of the four distinct treatment combinations. The "recipient choice **not revealed** to migrant, migrant preference **not revealed** to recipient" group is the omitted category. The dependent variables in columns 1 through 4 are the migrant-recipient differences in restaurant spending, education spending, spending on daily expenses, and health spending respectively. The dependent variable in columns 5, 6, and 7 is the total migrant-recipient difference. Column 6 adds stratification cell fixed effects and column 7 additionally adds demographic control variables. The control variables are the same as those presented in Table 1.4 with the addition of recipient gender, age, years of education and household size and a control for number of days between the migrant and the recipient survey.

The results in panel 1 replicate the results from Table 1.7 almost exactly. Controlling for the other treatment does not change either estimate. In addition, the results are robust to the addition of all control variables (results for individual spending categories not shown but available upon request). The results in panel 2 show that the same conclusion is drawn when considering the separate impacts of the four groups. Focusing on the total difference results in columns 5 and 6, the coefficients on the "recipient choice **not revealed** to migrant, migrant preference **revealed** to recipient" and "recipient choice **revealed** to migrant, migrant preference **revealed** to recipient" groups are both negative and significant, meaning that the total difference in these groups is smaller than in the omitted category. These are the two groups where migrant preferences are revealed to the recipient and the estimated coefficients are quite similar in magnitude to the coefficient on the communication treatment in panel 1. The coefficient on the remaining group ("recipient choice **revealed** to migrant, migrant preference **not revealed** to recipient") is small and statistically indistinguishable from zero. Essentially the specification that considers the separate effects of the interacted treatment groups shows the same pattern as the specification that considers the treatments separately. Revealing the migrant's preferences lessens the total difference between recipient choices and migrant preferences by $14 or

[47] Mechanically the sum of the differences between the "not revealed" and "revealed" groups over the four categories must be equal to twice the difference in the total difference.

approximately ten percent of the “recipient choice **not revealed** to migrant, migrant preference **not revealed** to recipient” mean. The monitoring treatment has no effect.

The impact of the communication treatment suggests that migrant preferences do matter to recipients and that some deviation from those preferences may be inadvertent. However, the lack of impact of the monitoring treatment further implies that recipients do not react strategically to changes in the probability of detection. The model presented in Section II proposes an explanation for why recipients may not take advantage of the opportunity to hide their spending choices from migrants. Migrants simply may have limited ability to punish the recipients for not following their preferences. While the results in the migrant experiment are that recipient ability to punish varies across recipients, these results from the recipient experiment suggest that, in this context, migrant ability to punish is low across the population. In practice, this would result in a situation where the migrants have very little power to compel recipients to spend the remittances as they wish.

Although a limited ability to punish is the explanation for the lack of effect of the monitoring treatment that is suggested by the model, it is important to consider other possible explanations. The first alternative explanation is that migrant monitoring of recipients is essentially perfect and that recipients know that their choices will be discovered if they win. However, given that, as reported in Table 1.1, only 24 percent of migrants could correctly report student GPA and 43 percent correctly report how students travel to school, it does not seem plausible that existing monitoring is good enough across the board as to render the experimental variation irrelevant. A second explanation is that migrants and recipients have the same preferences for spending, and therefore they make the same choices regardless of punishment ability. Certainly this may be true for some families, but if it were true for most, there should not be an impact of the recipient communication treatment. Additionally, only 48 percent of migrant-recipient pairs report the same three budget priorities (Table 1.1), further evidence that there is heterogeneity in preferences within families.

For completeness, it is instructive to examine how the impacts of the recipient treatments may vary by sub-group. For symmetry, I present the results of the recipient experiments broken down by the same sub-groups used for analysis in the migrant experiment. Although the ways in which the migrant may punish the recipient are less obvious, the variables that indicate valuable family relationships should be important, as well as total remittances, given that the threat of withholding remittances may be one of the migrant's most valuable tools.

Table 1.9 shows the results of the recipient experiments by subsample. This table focuses only on the total migrant-recipient difference. Panel 1 presents regressions without control variables and Panel 2 presents regressions with stratification group fixed effects. In contrast to the migrant experiment the subsample analysis reveals no consistent patterns. Other than two positive coefficients which may be due to chance, there continues to be no significant impact of the monitoring treatment, a result that is not surprising given how close to zero the coefficients in the full sample analysis were. The results of the communication treatment are also fairly consistent across subsamples. Given that there are no impacts of the monitoring treatment, this suggests that any changes due to increased information about migrant preferences will happen because of the recipient desire to follow the preferences of the migrant, and this does not necessarily vary with the migrant's ability to punish. The stronger impacts of the communication treatment in cases where the migrant has a child 22 or under in El Salvador and where the migrant communicates with the recipient household at least weekly could simply suggest that recipients in those groups have a greater desire to follow the migrant's preferences than other recipients.

A potential criticism of the results in the communication experiment is that recipients respond to the information about migrant preferences not because they necessarily want to follow the migrants' preferences, but because they are reacting to being given a suggested allocation for the choice they are making. In other words, recipients may have reacted in the same way even if the suggested preferences were attributed to someone besides the migrant. I can address this concern by examining heterogeneity in the effects of the communication treatment by proxies for the quality of information in the relationship. Specifically, in Table 1.A.4, I estimate regression equation 2 separately by whether or not the migrant can correctly report the student's GPA and mode of transport to school. Although these variables are not direct representations of recipient knowledge of migrant preferences, they are likely to be indicative of low information quality in general. If recipients are reacting to a lack of knowledge of the migrants' preferences then the effects of the communication treatment should be concentrated among pairs where these variables suggest that information quality is low. I find that this is indeed the case: effects of the communication treatment are evident only where migrants do not know students' GPAs or modes of transportation to school.

1.5.3 Discussion

Economic studies of information asymmetries in households with migrants have until now focused on migrant monitoring of recipient behavior (Chen, 2012; de Laat, 2008) and the impacts of offering migrants greater control over how remittances are spent (Ashraf et al., 2011; Chin et al., 2011; Torero and Viceisza, 2011). This is the first study that explicitly looks at the effect of information asymmetries on *both* sides of the remittance relationship – the migrant sending of remittances as well as the recipient spending of those remittances. Despite the previous emphasis on migrant monitoring, the results of the two monitoring treatments presented in this paper are that, in this context, it is only migrants, and not recipients, who strategically react to variations in the probability that their actions will be monitored.

This is an important finding not only because it shows that information asymmetries have an important impact on the remittance sending decision, but also because this implies that recipients have important power in the migrant-recipient relationship. Although this influence has been considered in the extensive literature on the motivations to send remittances, it has not previously been rigorously documented empirically. Policymakers who seek to design tools to facilitate the sending of remittances and enhance their impacts[48] should take the role of the recipient in determining remittance amounts into account. Policies that assume that migrants have complete autonomy over the manner in which remittances are sent may fall short of their full potential. Additionally, the analysis in this paper indicates that because migrants are responding to the opportunity to hide income, some of them are already sending home more than they would choose to voluntarily. This suggests that, in particular, programs that seek to increase remittances will face difficulties within this group. Policy makers should also consider the welfare implications of such a policy; the low income status of the migrants in this study suggests the possibility that the extra funds could be more efficiently used by the migrants in the United States.

Additionally, the fact that the monitoring treatment had no effect on the recipient spending decision adds a new angle to the recent work on the impact of control on remittance behavior (Ashraf et al., 2011; Chin et al., 2011; Torero and Viceisza, 2011). To varying degrees, these studies offer migrants direct control over money sent to family members at home. Viewed through the framework presented in this paper, control over remittances improves both the

[48] See Yang (2011) for a discussion.

monitoring and enforcement of remittance spending contracts, but the existing studies are not able to distinguish between the two channels. The results of this study, that migrant monitoring of recipient remittance spending does not seem to matter, suggest that if migrants do indeed desire control (and the literature is mixed on whether or not they do) it may not be due to an inability to monitor the recipients but rather to an inability to effectively punish recipients and therefore compel recipients to spend remittances in a certain way. In the absence of punishment ability, the ability to control would act as the enforcement mechanism in the migrant-recipient contract.

Overall, the findings that information asymmetries can affect both the sending and spending of remittances suggest that interventions or technological innovations that improve communication in transnational households could have important effects on financial decisions made by both migrants and recipients. In particular, the results of the communication experiment imply that for migrants who wish to change the spending behavior of their family members, policies that promote improved communication about spending preferences may be an inexpensive way to achieve a higher level of compliance with their preferences. Although this study only addressed the inadvertent impacts of information asymmetries in the context of preferences for the spending of remittances, it is possible that improving communication could also alleviate other possible inadvertent effects in the migrant-recipient relationship. For example, if recipients do not have a full understanding of migrants' cost of living in the United States, improving that knowledge could lead recipients to expect lower remittance payments.

1.6 Conclusion

This paper analyzes a set of experiments designed to test for the effects of information asymmetries in transnational households. First, an experiment among Salvadoran migrants in the Washington, DC area examines the extent to which the probability that recipients will observe migrant income is a factor in remittance decisions. The migrant's remittance decision is modeled as a combination of money sent for altruistic purposes and money sent because of an agreement with the recipient that is enforced with the threat of punishment. The model shows that variability in recipient ability to monitor migrant income can lead to migrants strategically deviating from this agreement when the probability that their deviation will be detected is low. When choosing how much of a potential prize of $600 to keep and how much of it to send to family in El Salvador, migrants send less to their family when the probability that their family member will be made aware of their choice is low. Consistent with the model, the effects are only present in subsamples of migrants where the cost of the punishment is plausibly high.

A second experiment conducted among the family members of the migrant sample examines the role of migrant monitoring in the decisions remittance recipients make about how to spend the transfers that they receive. The experiment varies whether or not the migrant will be informed of how the recipient chose to allocate a potential prize of $300. A simultaneous, cross-randomized intervention tests whether lowering communication costs by revealing the migrant's specific preferences over the spending decision causes recipients to more closely adhere to these preferences. In contrast to the migrant experiment, recipient decisions are not affected by the monitoring treatment. However, lowering communication costs by revealing migrant preferences does bring recipient choices closer to migrant preferences.

This is the first study to explicitly manipulate information asymmetries and causally identify their impacts on both sides of transnational households. Although previous work in this area has focused on how migrants monitor the actions of recipients or seek to increase control over the remittances they send, this study additionally recognizes that recipients have influence over how much is sent home by the migrant. In fact, in this experimental context recipient influence on migrants is substantially more important than migrant influence on recipients, suggesting that recipients hold important power in the migrant-recipient relationship. The results also suggest that the desire for migrant control over remittances previously noted in the literature (for example, Ashraf et al. 2011) may not be due to the migrants' inability to monitor recipients, but instead to the migrants' inability to compel recipients to spend remittances as the migrant prefers.

Although my results are specific to the context of transnational households, they can also inform the broader literature on household resource allocation. Whereas previous studies have focused only on strategic behavior, I find that both strategic and inadvertent information asymmetries can have important impacts on resource allocation. I also find that different types of decisions, analogous to the sharing and spending of resources, are affected by information imperfections. Finally, I bring the study of information asymmetries outside of the husband-wife pair. The heterogeneity in my results suggests that while the strategic effects of information asymmetries are important, they may not be relevant for all extended family networks where resource sharing is observed. However, even when strategic effects are not present, information imperfections may still have inadvertent impacts on the final allocation of resources.

Figure 1.1: Project timeline

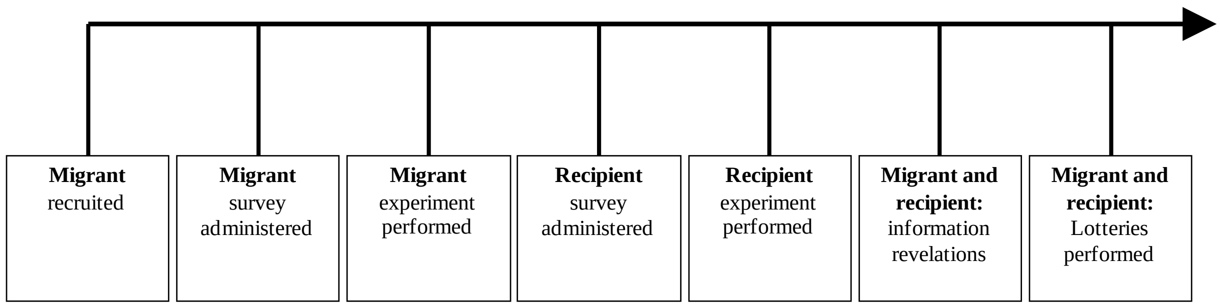


Figure 1.2: Migrant experiment: Treatments

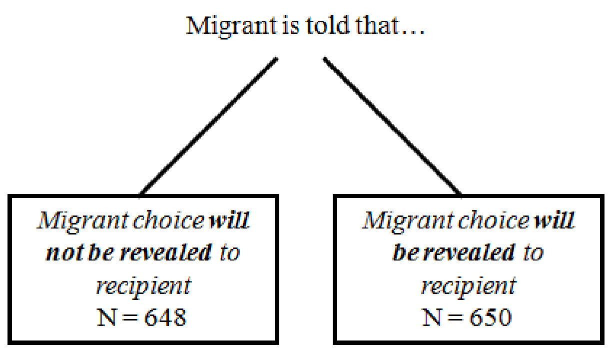
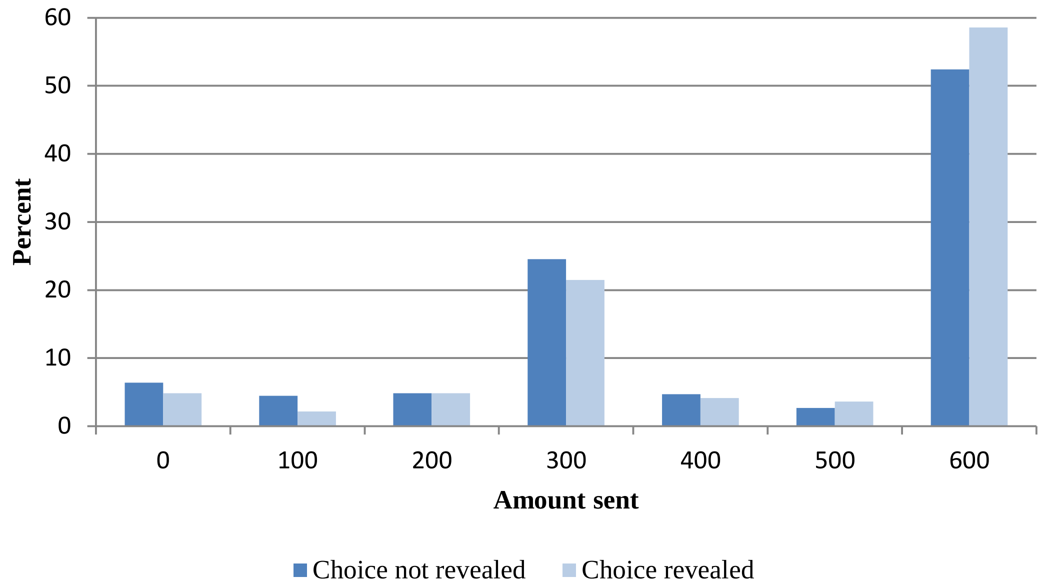


Figure 1.3: Recipient experiment: Treatments

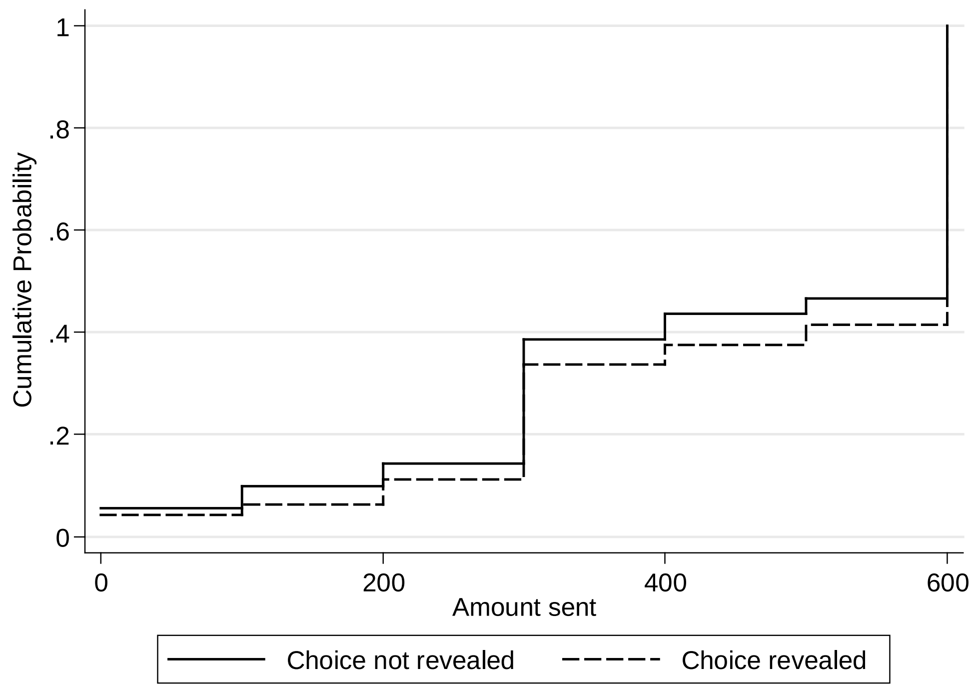
Monitoring treatment		***Communication treatment***		
		Migrant preference ***not revealed*** *to recipient*	*Migrant preference* ***revealed*** *to recipient*	
	Recipient choice ***not revealed*** *to migrant*	N = 314	N = 324	N = 638
	Recipient choice ***revealed*** *to migrant*	N = 327	N = 333	N = 660
		N = 641	N = 657	

Figure 1.4: Distribution of amount sent by migrant by treatment group



Notes: Sample is observations with non-missing values for experiment questions and completed recipient survey. Choice not revealed: N = 648. Choice revealed: N = 650.

Figure 1.5: Cumulative distribution of amount sent by migrant by treatment group



Notes: Sample is observations with non-missing values for experiment questions and completed recipient survey. Choice not revealed: N = 648. Choice revealed: N = 650.

Table 1.1: Baseline summary statistics

	All Observations			*Observations with completed recipient survey*		
	Mean	SD	N	Mean	SD	N
Baseline variables from migrant survey						
Migrant is female	0.50	0.50	1,581	0.51	0.50	1,298
Migrant age	36.83	9.41	1,538	36.92	9.29	1,264
Migrant can read and write	0.96	0.20	1,554	0.96	0.20	1,275
Migrant's years of education	9.08	4.67	1,560	9.01	4.67	1,282
Migrant's years in the US	11.31	6.38	1,577	11.13	6.27	1,295
Migrant is married	0.62	0.48	1,575	0.63	0.48	1,294
Migrant lives with spouse	0.49	0.50	1,579	0.50	0.50	1,296
Migrant's total number of children	2.28	1.69	1,579	2.34	1.69	1,296
Migrant's number of children in El Salvador	1.01	1.43	1,577	1.07	1.47	1,294
Migrant's number of children in US	1.26	1.32	1,575	1.25	1.29	1,293
Migrant's hh size in US	4.32	1.98	1,581	4.36	1.96	1,298
Migrant has child 22 or under in El Salvador	0.32	0.47	1,581	0.34	0.47	1,298
Recipient is migrant's close relative	0.29	0.45	1,574	0.31	0.46	1,291
Migrant has worked in last 12 months	0.89	0.31	1,581	0.89	0.31	1,298
Migrant in lowest income bracket	0.52	0.50	1,429	0.53	0.50	1,181
Migrant sent remittances to recipient hh	0.85	0.36	1,580	0.87	0.34	1,297
Migrant's annual regular remittances to recipient hh ($)	2,298	2,907	1,565	2,440	2,998	1,283
Migrant's annual irregular remittances to recipient hh ($)	337	706	1,575	344	707	1,293
Migrant's annual total remittances to recipient hh ($)	2,629	3,199	1,563	2,777	3,284	1,281
Migrant's annual total remittances to other hhs ($)	1,097	1,905	1,567	1,123	1,944	1,284
Migrant communicates with recipient hh at least weekly	0.69	0.46	1,578	0.71	0.45	1,295
Baseline variables from recipient survey						
Recipient is target student				0.45	0.50	1,298
Recipient is student's guardian				0.40	0.49	1,298
Recipient is female				0.68	0.47	1,298
Recipient age				34.20	15.84	1,295
Recipient is married				0.36	0.48	1,298
Recipient's years of education				9.37	5.27	1,292
Recipient lives in urban area				0.43	0.50	1,298
Recipient's hh size				4.99	2.04	1,296
Annual remittances received from migrant ($)				1,522	1,916	1,203
Baseline comparison variables						
Migrant and recipient report same hh budget priorities				0.48	0.50	1,231
Migrant and recipient report same student GPA				0.24	0.43	1,041
Migrant and recipient report same student mode of transport				0.43	0.50	1,107

Notes: All observations sample is respondents with non-missing data for questions in the migrant experiment. Completed recipient survey sample additionally conditions on completion of the recipient survey and non-missing migrant and recipient information for questions in the recipient experiment. Number of observations varies slightly with missing values. Recipient is defined as close relative if migrant reports recipient to be spouse, parent or child. Migrants in the lowest income bracket chose $400 or less as the weekly income of themselves plus their co-resident spouses. The other categories were $401-600, $601-800, and $801 and above. Annual regular remittances were collected by asking for the frequency of remittances sent and the average amount sent each time. Annual irregular remittances are remittances sent for special occasions or emergencies. The recipient variables in all cases refer to the person completing the recipient survey. The baseline comparison variables were asked on both surveys and are equal to one if the migrant and recipient responses match. Both respondents were asked to choose the three most important budget priorities for the recipient hh from a list of seven categories. Student refers to the student identified by the migrant during the baseline survey. GPA and mode of transport were only asked when student was reported to be in school.

Table 1.2: Balance tests: Migrant experiment

	Treatment group means: Migrant choice **not revealed** to recipient	Migrant choice **revealed** to recipient	P-value for difference of means: Choice not revealed and choice revealed
Attrition			
Recipient survey completed	0.82	0.83	0.819
Baseline variables from US Survey			
Migrant is female	0.53	0.49	0.165
Migrant age	36.90	36.94	0.941
Migrant can read and write	0.95	0.97	0.150
Migrant's years of education	9.01	9.00	0.966
Migrant's years in the US	10.90	11.37	0.178
Migrant is married	0.61	0.65	0.151
Migrant lives with spouse	0.50	0.50	0.956
Migrant's total number of children	2.34	2.34	0.956
Migrant's number of children in El Salvador	1.03	1.10	0.365
Migrant's number of children in US	1.28	1.22	0.410
Migrant's hh size in US	4.34	4.38	0.720
Migrant has child 22 or under in El Salvador	0.32	0.37	0.059
Recipient is migrant's close relative	0.29	0.33	0.178
Migrant has worked in last 12 months	0.90	0.89	0.943
Migrant in lowest income bracket	0.53	0.53	0.886
Migrant sent remittances to recipient hh	0.87	0.86	0.586
Migrant's annual regular remittances to recipient hh ($)	2,494	2,386	0.520
Migrant's annual irregular remittances to recipient hh ($)	354	334	0.627
Migrant's annual total remittances to recipient hh ($)	2,828	2,726	0.579
Migrant's annual total remittances to other hhs ($)	1,059	1,185	0.245
Migrant communicates with recipient hh at least weekly	0.73	0.69	0.057
Baseline variables from recipient survey			
Recipient is target student	0.45	0.45	0.907
Recipient is student's guardian	0.42	0.38	0.160
Recipient is female	0.69	0.67	0.331
Recipient age	35.09	33.31	0.043
Recipient is married	0.36	0.36	0.941
Recipient's years of education	9.21	9.54	0.285
Recipient lives in urban area	0.43	0.44	0.649
Recipient's hh size	4.90	5.08	0.111
Annual remittances received from migrant ($)	1,491	1,553	0.580
Baseline comparison variables			
Migrant and recipient report same hh budget priorities	0.48	0.48	0.926
Migrant and recipient report same student GPA	0.25	0.24	0.709
Migrant and recipient report same student mode of transport	0.44	0.42	0.573

Notes: Sample is observations with non-missing values for the experiment questions and completed recipient survey. Attrition is measured from sample of all migrants who completed the survey and the migrant experiment to sample with completed recipient survey and recipient experiment. Sample size for each comparison of means varies slightly by missing values for each variable. The percentage of missing values for each variable is also tested for balance across treatment groups with no significant differences. Other notes on variable construction are as in Table 1.1. P-values come from a regression of each variable on treatment, with standard errors adjusted for heteroskedasticity.

Table 1.3: Balance tests: Recipient experiment

	Monitoring treatment			Communication treatment		
	Treatment group			*Treatment group*		
	Recipient choice **not revealed** to migrant	Recipient choice **revealed** to migrant	P-value for difference of means: Choice not revealed and choice revealed	Migrant preference **not revealed** to recipient	Migrant preference **revealed** to recipient	P-value for difference of means: Pref. not revealed and pref. revealed
Attrition						
Recipient survey completed	0.81	0.83	0.315	0.82	0.83	0.730
Baseline variables from US Survey						
Migrant is female	0.52	0.50	0.532	0.49	0.53	0.186
Migrant age	36.56	37.27	0.176	36.90	36.95	0.922
Migrant can read and write	0.95	0.96	0.461	0.96	0.95	0.295
Migrant's years of education	9.02	9.00	0.947	8.97	9.04	0.798
Migrant's years in the US	11.18	11.08	0.774	11.13	11.13	0.993
Migrant is married	0.65	0.61	0.175	0.63	0.63	0.952
Migrant lives with spouse	0.51	0.49	0.543	0.50	0.50	0.957
Migrant's total number of children	2.30	2.38	0.352	2.37	2.31	0.560
Migrant's number of children in El Salvador	1.01	1.12	0.206	1.04	1.09	0.557
Migrant's number of children in US	1.27	1.24	0.725	1.31	1.20	0.105
Migrant's hh size in US	4.43	4.29	0.183	4.43	4.29	0.214
Migrant has child 22 or under in El Salvador	0.33	0.35	0.366	0.34	0.34	0.885
Recipient is migrant's close relative	0.30	0.32	0.539	0.34	0.29	0.059
Migrant has worked in last 12 months	0.89	0.90	0.401	0.89	0.89	0.950
Migrant in lowest income bracket	0.51	0.54	0.229	0.53	0.53	0.934
Migrant sent remittances to recipient hh	0.86	0.88	0.510	0.87	0.87	0.802
Migrant's annual regular remittances to recipient hh ($)	2,435	2,444	0.953	2,315	2,561	0.141
Migrant's annual irregular remittances to recipient hh ($)	382	308	0.062	353	335	0.655
Migrant's annual total remittances to recipient hh ($)	2,802	2,752	0.786	2,648	2,903	0.165
Migrant's annual total remittances to other hhs ($)	1,137	1,110	0.804	1,068	1,177	0.314
Migrant communicates with recipient hh at least weekly	0.73	0.69	0.192	0.70	0.72	0.585
Baseline variables from recipient survey						
Recipient is target student	0.44	0.46	0.402	0.46	0.44	0.495
Recipient is student's guardian	0.42	0.38	0.239	0.39	0.41	0.319
Recipient is female	0.68	0.68	0.998	0.68	0.68	0.726
Recipient age	34.44	33.97	0.589	34.29	34.11	0.835
Recipient is married	0.41	0.32	0.001	0.35	0.38	0.243
Recipient's years of education	9.22	9.53	0.294	9.30	9.45	0.622
Recipient lives in urban area	0.41	0.46	0.061	0.42	0.45	0.312
Recipient's hh size	5.04	4.95	0.471	5.06	4.93	0.271
Annual remittances received from migrant ($)	1,534	1,510	0.825	1,484	1,559	0.497
Baseline comparison variables						
Migrant and recipient report same hh budget priorities	0.46	0.50	0.189	0.47	0.49	0.401
Migrant and recipient report same student GPA	0.25	0.24	0.844	0.24	0.24	0.952
Migrant and recipient report same student mode of transpor	0.41	0.45	0.228	0.43	0.42	0.671

Notes: Sample is observations with non-missing values for the experiment questions and completed recipient survey. Attrition is measured from sample of all migrants who completed the survey and the migrant experiment to sample with completed recipient survey and recipient experiment. Sample size for each comparison of means varies slightly by missing values for each variable. The percentage of missing values for each variable is also tested for balance across treatment groups with no significant differences. Other notes on variable construction are as in Table 1.1. P-values come from a regression of each variable on treatment, with standard errors adjusted for heteroskedasticity.

Table 1.4: Impact of monitoring treatment on migrant remittance decision

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: *Amount sent by migrant*			Dependent variable: *Migrant sent everything*		
Migrant choice **revealed** to recipient	24.03**	20.40**	19.50*	0.0507*	0.0424	0.0352
	[10.35]	[10.27]	[10.26]	[0.0275]	[0.0273]	[0.0272]
Migrant is female			-26.46**			-0.0793***
			[11.09]			[0.0297]
Migrant age			-0.487			0.000173
			[0.741]			[0.00184]
Migrant's years of education			-0.119			0.00251
			[1.225]			[0.00322]
Migrant's years in the US			1.968*			0.00625**
			[1.071]			[0.00277]
Migrant lives with spouse			-28.75**			-0.0586*
			[11.83]			[0.0312]
Migrant's hh size in US			1.293			0.00404
			[2.800]			[0.00766]
Migrant has child 22 or under in ES			0.984			0.0379
			[12.41]			[0.0331]
Recipient is migrant's close relative			-0.675			-0.00535
			[12.74]			[0.0340]
Migrant in lowest income bracket			-21.73*			-0.0400
			[12.65]			[0.0338]
Migrant's annual total remittances to recipient hh			0.00319*			7.05e-06
			[0.00192]			[4.79e-06]
Migrant communicates with recipient hh at least weekly			-1.122			-0.0249
			[12.68]			[0.0339]
Observations	1,298	1,298	1,298	1,298	1,298	1,298
R-squared	0.004	0.133	0.159	0.003	0.123	0.149
Mean in treatment = Migrant choice **not revealed** to recipient	441.4			0.53		
Stratification group fixed effects	NO	YES	YES	NO	YES	YES

Notes: Robust standard errors in brackets. Sample is observations with non-missing values for all experiment questions and completed recipient survey. Amount sent by migrant is the amount that migrants chose to send when splitting $600 between themselves and recipients. Migrant sent everything is an indicator for whether or not the migrant chose to send everything to the recipient. Recipient is defined as close relative if migrant reports recipient to be his spouse, parent or child. Migrants in the lowest income bracket chose $400 or less as the weekly income of themselves plus their co-resident spouses. The other categories were $401-600, $601-800 and $801 and above. Annual total remittances are the combination of regular and irregular remittances. Annual regular remittances were collected by asking for the frequency of remittances sent and the average amount sent each time. Annual irregular remittances are remittances sent for special occasions or emergencies. Stratification group fixed effects are dummy variables for the groups of survey numbers within which randomization was stratified.

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

Table 1.5: Impact of monitoring treatment on amount sent by migrant: By proxies for punishment ability

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Dependent variable: Amount sent by migrant									
	Years in the United States		*Migrant has child 22 or under in El Salvador*		*Recipient is close relative of migrant*		*Migrant communicates with recipient hh weekly*		*Migrant's annual remittances to recipient hh*	
	Above sample median	Below sample median	No	Yes	No	Yes	No	Yes	Below sample median	Above sample median
Panel 1: Regressions without control variables										
Migrant choice **revealed** to recipient	8.824	39.61***	7.370	56.06***	13.22	47.36**	5.209	31.94***	1.612	45.02***
	[14.79]	[14.49]	[12.58]	[18.30]	[12.45]	[18.75]	[19.77]	[12.12]	[15.27]	[14.08]
P-value for equality of treatment effect	0.137		0.028		0.111		0.201		0.037	
Observations	639	656	853	445	896	402	379	919	611	670
R-squared	0.001	0.011	0.000	0.021	0.001	0.016	0.000	0.007	0.000	0.015
Panel 2: Regressions with stratification group fixed effects										
Migrant choice **revealed** to recipient	19.83	32.75**	10.45	59.12***	12.21	36.29*	9.414	24.97**	-1.532	36.98**
	[15.33]	[15.32]	[12.79]	[20.73]	[12.78]	[20.11]	[22.03]	[12.47]	[15.91]	[14.84]
P-value for equality of treatment effect	0.551		0.040		0.318		0.573		0.077	
Observations	656	639	853	445	889	402	376	919	611	670
R-squared	0.201	0.221	0.154	0.299	0.159	0.314	0.306	0.152	0.236	0.190
Mean in treatment = Migrant choice **not revealed** to recipient	457.8	426.1	449.3	424.3	444.4	433.9	449.4	438.4	447.0	437.5

Notes: Robust standard errors in brackets. Sample is observations with non-missing values for all experiment questions, completed recipient survey and non-missing values for variables used for division into sub-samples. Amount sent by migrant is the amount that migrants chose to send when splitting $600 between themselves and recipients. Recipient is defined as close relative if migrant reports recipient to be his spouse, parent or child. Annual total remittances are the combination of regular and irregular remittances. Annual regular remittances were collected by asking for the frequency of remittances sent and the average amount sent each time. Annual irregular remittances are remittances sent for special occasions or emergencies. The median years in the US is 10 and the median remittances sent to the recipient household are $1,800. Stratification group fixed effects are dummy variables for the groups of survey numbers within which randomization was stratified.

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

Table 1.6: Mean amounts allocated to spending groups by recipients and migrants: Recipient experiment

	Means of recipient choices by treatment group:				
	Monitoring treatment		*Communication treatment*		
	Recipient choice **not revealed** to migrant	Recipient choice **revealed** to migrant	Migrant preferences **not revealed** to recipient	Migrant preferences **revealed** to recipient	Means of migrant preferences:
Amount allocated to:					
Restaurant meals	6.11	5.46	5.38	6.17	11.74
Education	175.54	166.22	170.97	170.64	141.41
Daily expenses	66.05	75.59	72.85	68.99	76.56
Health expenses	52.30	52.73	50.80	54.20	70.28
Observations	638	660	641	657	1298

Notes: Sample is observations with non-missing values for all experiment questions and completed recipient survey. Means in columns 1 through 4 are from responses by recipients when asked to allocate $300 across four spending categories. Means in column 5 are responses from migrants when asked how they would like the recipient to allocate the funds.

Table 1.7: Differences between recipient and migrant choices by treatment group: Recipient experiment

	Monitoring treatment			*Communication treatment*		
	Recipient choice **not revealed** to migrant	Recipient choice **revealed** to migrant	P-value for difference of means: Choice not revealed and choice revealed	Migrant preferences **not revealed** to recipient	Migrant preferences **revealed** to recipient	P-value for difference of means: Pref, not revealed and pref. revealed
Difference in:						
Restaurant meals	15.89	14.80	0.604	16.66	14.05	0.215
Education	107.29	110.92	0.463	116.28	102.17	0.004
Daily expenses	78.02	81.38	0.421	83.01	76.52	0.120
Health expenses	75.02	73.47	0.709	76.55	71.97	0.271
Total difference	138.11	140.28	0.649	146.25	132.36	0.004
Observations	638	660		641	657	

Notes: Sample is observations with non-missing values for all experiment questions and completed recipient survey. Means are of the absolute difference between the recipient's choice and the migrant's preferences in each category. The total difference is the sum across the four difference variables for each observation, divided by two. P-values for differences in means were calculated by regressing the dependent variables on treatment, with standard errors adjusted for heteroskedasticity.

Table 1.8: Impact of monitoring and communication treatments on recipient allocation decision

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable: Migrant-recipient difference in…				*Dependent variable:*		
	Restaurant spending	Education spending	Daily expenses spending	Health spending	Total migrant-recipient difference		
Panel 1:							
Recipient choice **revealed** to migrant	-1.097	3.582	3.336	-1.567	2.126	3.019	3.330
	[2.100]	[4.923]	[4.172]	[4.159]	[4.753]	[4.775]	[4.827]
Migrant preference **revealed** to recipient	-2.612	-14.09***	-6.476	-4.588	-13.88***	-13.61***	-13.69***
	[2.103]	[4.926]	[4.168]	[4.159]	[4.751]	[4.753]	[4.818]
Observations	1,298	1,298	1,298	1,298	1,298	1,298	1,298
R-squared	0.001	0.007	0.002	0.001	0.007	0.105	0.122
Mean in recipient choice **not revealed**	15.9	107.3	78.0	75.0	138.1		
Mean in migrant preference **not revealed**	16.7	116.3	83.0	76.6	146.2		
Panel 2:							
Recipient choice **not revealed** to migrant,	-4.132	-17.56**	-0.621	-5.745	-14.03**	-14.51**	-13.86**
migrant preference **revealed to recipient**	[3.080]	[6.819]	[5.949]	[5.892]	[6.673]	[6.752]	[6.918]
Recipient choice **revealed** to migrant,	-2.611	0.129	9.164	-2.719	1.981	2.119	3.158
migrant preference **not revealed to recipient**	[3.253]	[6.942]	[5.738]	[5.887]	[6.489]	[6.619]	[6.714]
Recipient choice **revealed** to migrant,	-3.753	-10.61	-2.971	-6.189	-11.76*	-10.62	-10.36
migrant preference **revealed to recipient**	[3.128]	[6.940]	[5.969]	[5.735]	[6.752]	[6.844]	[6.922]
Observations	1,298	1,298	1,298	1,298	1,298	1,298	1,298
R-squared	0.002	0.007	0.004	0.001	0.007	0.105	0.122
Mean in recipient choice **not revealed**, migrant preference **not revealed**	18.0	116.2	78.3	77.9	145.2		
Stratification group fixed effects	NO	NO	NO	NO	NO	YES	YES
Control variables	NO	NO	NO	NO	NO	NO	YES

Notes: Robust standard errors in brackets. Sample is observations with non-missing values for all experiment questions and completed recipient survey. Dependent variables are the absolute difference between the recipient's choice and the migrant's preferences in each category. The total difference is the sum across the four difference variables for each observation, divided by two. Omitted category in panel 2 regressions is "Recipient choice not revealed, migrant preference not revealed." Stratification group fixed effects are dummy variables for the groups of survey numbers within which randomization was stratified and treatment status in the migrant experiment. Control variables are migrant and recipient gender, age, years of education, and household size. Controls also include migrant years in the United States, whether migrant lives with spouse, whether migrant has a child 22 or under in El Salvador, whether the migrant and recipient are close relatives, if the migrant is in the lowest income bracket, annual total remittances to recipient household, whether the migrant and recipient communicate at least weekly, and the number of days in between migrant and recipient survey.

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

Table 1.9: Impact of monitoring and communication treatments on recipient allocation decision: By proxies for punishment ability

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Dependent variable: Total migrant-recipient difference									
	Years in the United States		*Migrant has child 22 or under in El Salvador*		*Recipient is close relative of migrant*		*Migrant communicates with recipient hh weekly*		*Migrant's annual remittances to recipient hh*	
	Above sample median	Below sample median	No	Yes	No	Yes	No	Yes	Below sample median	Above sample median
Panel 1: Regressions without control variables										
Recipient choice **revealed** to migrant	11.63*	-7.420	3.895	-1.768	-1.534	9.902	21.53**	-6.626	1.450	1.675
	[6.805]	[6.647]	[5.890]	[8.059]	[5.813]	[8.352]	[8.680]	[5.674]	[6.957]	[6.619]
Migrant preference **revealed** to recipient	-11.71*	-16.83**	-9.160	-23.17***	-11.63**	-16.89**	-7.156	-17.39***	-12.92*	-15.56**
	[6.818]	[6.641]	[5.881]	[8.080]	[5.802]	[8.462]	[8.705]	[5.665]	[6.973]	[6.607]
P-values for equality of treatment effects:										
Monitoring treatment	0.045		0.570		0.261		0.007		0.981	
Communication treatment	0.591		0.161		0.608		0.324		0.784	
Observations	639	656	853	445	889	402	376	919	611	670
R-squared	0.009	0.011	0.003	0.018	0.005	0.014	0.017	0.011	0.006	0.008
Panel 2: Regressions with stratification group fixed effects										
Recipient choice **revealed** to migrant	16.45**	-6.221	3.308	2.945	-1.702	23.58**	21.30*	-4.024	1.799	2.731
	[7.228]	[6.853]	[6.013]	[9.344]	[5.901]	[9.986]	[10.84]	[5.963]	[7.462]	[7.289]
Migrant preference **revealed** to recipient	-16.28**	-15.70**	-7.565	-22.84**	-10.22*	-17.37*	-4.316	-17.29***	-10.05	-17.86**
	[7.306]	[6.790]	[6.041]	[9.235]	[5.876]	[9.414]	[10.52]	[5.815]	[7.740]	[7.064]
P-values for equality of treatment effects:										
Monitoring treatment	0.023		0.973		0.022		0.030		0.929	
Communication treatment	0.954		0.155		0.527		0.251		0.456	
Observations	639	656	853	445	889	402	376	919	611	670
R-squared	0.191	0.191	0.156	0.277	0.154	0.275	0.282	0.139	0.186	0.169
Mean in recipient choice **not revealed**	134.5	141.3	137.6	139.1	139.0	136.5	134.8	139.4	138.6	138.1
Mean in migrant preference **not revealed**	145.8	146.5	144.1	150.3	144.3	149.9	149.0	145.2	145.8	147.0

Notes: Robust standard errors in brackets. Sample is observations with non-missing values for experiment questions, completed recipient survey and non-missing values for variables used for division into sub-samples. Recipient is defined as close relative if migrant reports recipient to be his spouse, parent or child. Annual total remittances are the combination of regular and irregular remittances. Annual regular remittances were collected by asking for the frequency of remittances sent and the average amount sent each time. Annual irregular remittances are remittances sent for special occasions or emergencies. The total difference is the sum across the four difference variables for each observation, divided by two. The median years in the US is 10 and the median remittances sent to the recipient household is $1,800. Stratification group fixed effects are dummy variables for the groups of survey numbers within which randomization was stratified and treatment status in the migrant experiment.

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

Table 1.A.1: Comparison of migrants in study with DC-area Salvadorans and Hispanics in the American Community Survey

		American Community Survey: 2008-2010 3-year sample	
	Baseline survey	Salvadoran-born, not US citizen	Hispanic, not US citizen
Migrant is female	0.51	0.46	0.46
Age of migrant	36.92	36.05	36.39
	[9.30]	[10.39]	[10.85]
Migrant's years in the US	11.13	12.93	11.74
	[8.09]	[7.89]	[8.09]
Migrant's hh size in the US	4.36	4.95	4.64
	[1.96]	[2.12]	[2.14]
Migrant has less than high school education	0.62	0.61	0.47
Migrant has high school education or more	0.38	0.39	0.53
Observations	1,298	2,208	5,420

Notes: Baseline survey sample is observations with non-missing values for all experiment questions and completed recipient survey. Sample size varies slightly with variable: 1,264 for age; 1,295 for years in US; 1,290 for education variables. ACS sample is the IPUMS three year 2008-2010 ACS sample restricted to individuals 18-65 in the Washington, DC metro area (as defined by the ACS, includes MD and VA suburbs). Standard deviation in brackets for continuous variables.

Table 1.A.2: Impact of monitoring treatment on migrant remittance decision: Interaction with punishment index

	(1)	(2)	(3)
	Dependent variable: *Amount sent by migrant*		
Migrant choice **revealed** to recipient	-15.25	-8.737	-5.820
	[19.32]	[19.32]	[19.37]
Punishment index	-6.947	-3.488	-3.390
	[5.608]	[5.674]	[5.706]
Interaction: Choice revealed and punishment index	18.11**	13.31*	11.75
	[7.648]	[7.687]	[7.707]
Observations	1,268	1,268	1,268
R-squared	0.009	0.139	0.152
Mean in treatment = Migrant choice **not revealed** to recipient	441.4		
Stratification group fixed effects	NO	YES	YES
Control variables	NO	NO	YES

Notes: Robust standard errors in brackets. Sample is observations with non-missing values for all experiment questions and completed recipient survey. Amount sent by migrant is the amount that migrants chose to send when splitting $600 between themselves and recipients. The punishment index is calculated using the first principal component of the following five variables: migrant has been in the United States below the sample median number of years, migrant has a child 22 or under in El Salvador, migrant and recipient are closely related, migrant and recipient communicate weekly, and annual migrant remittances to recipient household are above the sample median. Stratification group fixed effects are dummy variables for the groups of survey numbers within which randomization was stratified. Control variables are migrant gender, age, years of education, and household size. Controls also include whether migrant lives with spouse and if the migrant is in the lowest income bracket.

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

Table 1.A.3: Impact of monitoring and communication treatments on recipient allocation decision: Raw amounts

	(1)	(2)	(3)	(4)
	Dependent variable:Amount allocated by recipient to…			
	Restaurant spending	Education spending	Daily expenses spending	Health spending
Panel 1:				
Recipient choice **revealed** to migrant	-0.650	-9.328*	9.528**	0.450
	[1.060]	[5.326]	[3.881]	[3.602]
Migrant preference **revealed** to recipient	0.789	-0.365	-3.825	3.401
	[1.058]	[5.329]	[3.887]	[3.602]
Observations	1,298	1,298	1,298	1,298
R-squared	0.001	0.002	0.005	0.001
Mean in recipient choice **not revealed**	6.1	175.5	66.0	52.3
Mean in migrant preference **not revealed**	5.4	171.0	77.9	50.8
Panel 2:				
Recipient choice **not revealed** to migrant, migrant preference **revealed to recipient**	0.407	-4.912	4.799	-0.295
	[1.474]	[7.449]	[5.393]	[5.122]
Recipient choice **revealed** to migrant, migrant preference **not revealed to recipient**	-1.030	-13.85*	18.11***	-3.229
	[1.355]	[7.582]	[5.648]	[5.119]
Recipient choice **revealed** to migrant, migrant preference **revealed to recipient**	0.128	-9.825	5.953	3.743
	[1.633]	[7.432]	[5.236]	[5.240]
Observations	1,298	1,298	1,298	1,298
R-squared	0.001	0.003	0.009	0.001
Mean in recipient choice **not revealed**, migrant preference **not revealed**	5.9	178.0	63.6	52.5
Control variables	NO	NO	NO	NO
Stratification group fixed effects	NO	NO	NO	NO

Notes: Robust standard errors in brackets. Sample is observations with non-missing values for all experiment questions and completed recipient survey. Dependent variables are the raw amounts allocated by recipient to different spending categories. Omitted category in panel 2 regressions is "Recipient choice not revealed, migrant preference not revealed."

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

Table 1.A.4: Impact of monitoring and communication treatments on recipient allocation decision: By information quality measures

	(1)	(2)	(3)	(4)
	Dependent variable: Total migrant-recipient difference			
	Migrant correctly reports student GPA		*Migrant correctly reports student mode of transport to school*	
	No	Yes	No	Yes
Panel 1: Regressions without control variables				
Recipient choice **revealed** to migrant	3.760	1.965	-3.549	12.02
	[6.001]	[10.98]	[6.960]	[7.660]
Migrant preference **revealed** to recipient	-18.99***	-5.573	-20.67***	-2.028
	[5.996]	[10.98]	[6.940]	[7.683]
P-values for equality of treatment effects:				
Monitoring treatment	0.886		0.133	
Communication treatment	0.283		0.072	
Observations	787	254	633	474
R-squared	0.013	0.001	0.014	0.005
Panel 2: Regressions with control variables				
Recipient choice **revealed** to migrant	2.248	3.501	-1.354	11.14
	[6.215]	[14.18]	[7.147]	[8.461]
Migrant preference **revealed** to recipient	-16.77***	4.624	-23.99***	-4.349
	[6.147]	[13.94]	[7.351]	[8.413]
P-values for equality of treatment effects:				
Monitoring treatment	0.928		0.257	
Communication treatment	0.118		0.078	
Observations	787	254	633	474
R-squared	0.157	0.397	0.228	0.243
Mean in recipient choice **not revealed**	136.9	128.7	139.7	132.9
Mean in migrant preference **not revealed**	148.5	132.2	148.5	140.7

Notes: Robust standard errors in brackets. Sample is observations with non-missing values for experiment questions, completed recipient survey and non-missing values for variables used for division into sub-samples. Responses to GPA and transport questions were only recorded if student was reported to be in school. Migrant's were asked to report the student's GPA within a 2 point (out of 10) range, while recipients reported an exact number. The migrant is said of have correctly reported the GPA if the recipient's response was within the range the migrant indicated. Stratification group fixed effects are dummy variables for the groups of survey numbers within which randomization was stratified and treatment status in the migrant experiment.

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

CHAPTER II

Bargaining with Grandma: The Impact of the South African Pension on Household Decision Making

2.1 Introduction and motivation

The growing importance of government cash transfers as an anti-poverty tool in the developing world has highlighted the importance of understanding how households make decisions and allocate resources. Over the past three decades the theory of household resource allocation has evolved from unitary models that treat the household as a single entity (Samuelson, 1956; Becker, 1974, 1981) to bargaining models that recognize that control of resources is important for allocation outcomes (Chiappori, 1988, 1992; Lundberg and Pollack, 1993, 1994; Manser and Brown, 1980; McElroy and Horney, 1981). This shift has been supported by a growing amount of empirical evidence rejecting the predictions of the unitary model. However, these papers are largely based on the reduced form effect of income transfers on household outcomes, negating the unitary model but providing little evidence in direct support of alternative explanations. This paper addresses this gap in the literature by examining the effects of changes in income dynamics on direct measures of bargaining power, utilizing the age discontinuity in eligibility for the South African old age pension to study how household decision making is affected by changes in individual income. The pension is distributed on an individual, not household, level and is given to both men and women, thereby providing an ideal setting for the empirical examination of economic models of household decision making and resource allocation.

Empirical work on this topic has generally focused on rebutting the predictions of the unitary model by examining outcomes after exogenous changes in income. The unitary model has been rejected when it can be shown empirically that these outcomes differ by the gender of the recipient. If the model held, the identity of the recipient should not matter for how money is spent. For example, using the South African pension, Duflo (2003) finds that living with a pension eligible female leads to significant improvement in nutrition indicators of young girls (but not boys) while male pension eligibility has no effect on child nutrition.[1] These papers provide convincing evidence that control of resources matters and have resonated in the policy community. In fact, due to studies that suggest that money given to or controlled by women is spent in more productive ways than money given to men, particularly for children's outcomes (Thomas, 1990, 1994; Duflo, 2003), many cash transfer programs specifically target their benefits towards women. Yet these papers have only been able to examine the reduced form impact of income variation on outcomes; they do not shed light on the mechanisms that women use to translate control of income into household expenditures that better reflect their preferences.

This paper will use the South African pension to study how a permanent, large, and plausibly exogenous change in individual income affects the identity of the primary decision maker within the household. This focus on decision making allows for an understanding of the mechanism through which income changes are affecting household outcomes and direct observation of how changes in income affect a variable that is a direct representation of bargaining power in the household. Given that Duflo's 2003 paper on the pension and child nutrition is widely cited as one of the most important pieces of evidence against the unitary household model in the developing world, this is a particularly appropriate setting in which to investigate this question. The pension is also one of the most substantial cash transfers in the developing world so understanding how pension receipt affects recipient bargaining power is an important question for all policy makers considering the implementation of a cash transfer program. The structure of the pension is such that a vast majority of age-eligible members of the black population qualify simply by meeting an age requirement, allowing for a clean analysis of the causal impacts of the pension using the age requirement for pension eligibility to employ a regression discontinuity design.

[1] Other examples include Lundberg, Pollack and Wales (1996) and Qian (2008).

Using the South African National Income Dynamics Survey (NIDS) collected in 2008 and exploiting the fact that pension receipt for black South Africans is almost universal contingent on meeting the age requirement, I find that age eligibility for the pension results in age-eligible women being 13 to 16 percentage points more likely to be the primary decision maker in their household for both day-to-day and large, unusual purchases. There is no corresponding effect at the age of eligibility for men. The evidence suggests that this increase in decision making power for women comes through a reduction in the decision-making power of older men (in households with older men) and the extent to which the households disagree about who the decision maker is (in households without older men). The changes in female decision making power are direct evidence that bargaining power in the household shifts when control of resources changes.

The argument that the changes in decision-making power are evidence of increased bargaining power for pension-eligible women is strengthened by an analysis of personal income share, defined as the percentage of household income that can be attributed to a certain household member. Personal income share is highly correlated with reported decision making power for both men and women. Mirroring the results for decision making, income share increases significantly with pension eligibility for women but not for men. The lack of changes in income share and decision making for men is somewhat puzzling, given that men also receive the pension at the age of eligibility. However, that new income is offset by a corresponding reduction in male labor income. Consequently, there appears to be no change in male status in the household upon receipt of the pension.

The increase in female decision-making power and income share translates into positive household level impacts; female (but not male) pension eligibility results in an increase of 0.6 standard deviations in young girls' weight for height z-scores (a confirmation of Duflo's (2003) original result using data collected 15 years earlier) and a 23 percent increase in the number of household consumer durables. On their own, these results, like those in Duflo (2003), seem to support the argument that transfers, especially those intended for the support of children, should be directed towards women and not men. However, the income share results suggest that there are no changes in household outcomes when men become pension eligible, not because their preferences for expenditures necessarily differ, but because the pension does not result in a shift in their bargaining power. Therefore, there is no reason to expect changes in household outcomes with male eligibility. Indeed, an analysis of the 1993 data used in Duflo (2003) shows a similar pattern: increases in income share at pension eligibility for women but not for men. These results suggest that large transfers can have complex impacts on household income patterns and that results such as those in Duflo (2003) should be more carefully interpreted before drawing the policy conclusion that money is better directed to women than men.

This paper is related to a small number of other studies that have also tried to examine changes in bargaining power directly. De la Brière and Quisumbing (2000) find that transfers from the Progresa conditional cash transfer program in Mexico had a statistically significant negative effect on the husband being the sole decision maker (as opposed to wife alone or joint) in five of eight decision-making categories. Estimates also indicate that women are more likely to be the sole decision makers on how they spend their extra income. However, all results are very small in magnitude, and because Progresa was not known to be permanent at the time of the evaluation, these results may not be generalizable to permanent increases in income for women. Additionally, the fact that Progresa transfers were only made to women does not allow for a comparison of the effects between genders.

Providing poor women with access to financial services and employment opportunities in developing countries has long been billed as a tool for women's empowerment by giving women the means to increase their income. However, while a recent randomized evaluation of microfinance in India finds impacts of credit access on expenditures in treatment villages, it finds no effects on female decision making or child health (Banerjee, Duflo, Glennerster and Kinnan, 2013). Yet, another randomized experiment that provided commitment savings accounts in the Philippines found that women with below median decision-making power at baseline who were offered the savings account show improvements in an index of decision-making variables. There is a corresponding increase in female-oriented consumer durables in the household (Ashraf, Karlan and Yin, 2010). Because the authors argue that the savings accounts do not increase income overall, just savings, this effect is attributed to the control over resources that women gain through their individual savings accounts. Additionally, Majlesi (2013) finds that decreases in employment opportunities for women in Mexico decrease the share of household decisions made by wives, and has negative impacts on the health of girls.

The paper proceeds as follows. Section II provides background on the South African pension and the data used in this paper. Section III discusses the identification strategy. Section

IV provides the main analysis of the impacts of the pension on decision making. Section V discusses the analysis of income share. Section VI examines the impact of the pension on child nutrition and durable goods ownership. Section VII provides additional discussion of the results. Section VIII does a series of robustness checks, and Section IX concludes.

2.2 Background

2.2.1 The South African pension

The old age pension in South Africa originally existed primarily for white workers who did not have access to an occupational pension. Although it was gradually introduced for other racial groups, the pension system remained discriminatory throughout the apartheid period. (Lund, 1993). As the country began to transition from apartheid, the government made the commitment to equalize the pension across races. Benefits for blacks were increased throughout the 1980s while those for whites fell quickly. In 1992 the means tests was equalized for people of all races. (Duflo, 2003).

Eligibility for payments is determined by age and a means test. Women ages 60 and older are eligible for the pension. At the time the data used in this paper was collected, men did not become eligible until age 65.[2] The means test considers only the income of the pension eligible individual and his or her spouse and therefore should not incentivize potential recipients to alter their household structure in order to become eligible. The test is such that the vast majority of the black population easily qualifies (even if labor income is taken into account), but the majority of whites or anyone with a separate pension do not (Lam, Leibbrandt and Ranchhod, 2006). Because of this, as is standard in this literature, my analysis will be restricted to the black population.

According to the survey data used in this paper, in 2008, 86 percent of age eligible men and 92 percent of age eligible women report receiving the pension (NIDS, 2008). The maximum pension benefit was R870 during most of the survey period, and was raised to R940 as survey work was concluding. Although the structure of the means test is such that there is some phase

[2] In 2008 a law was passed to equalize the age of eligibility between men and women by 2010. This was done in stages and the male age of eligibility immediately dropped to 63 (SouthAfrica.info, 2008). Because the law was enacted in mid 2008 and the data used in this paper were largely collected in the first half of 2008, the age of eligibility for men will be considered to be 65 for the purposes of this analysis. Fewer than 5% of the elderly men in the sample used in this study were interviewed in the second half of 2008.

out of benefits above certain income levels, in practice fewer than 15 percent of pension recipients in 2008 report receiving less than the maximum amount.[3] The maximum benefit of R870 was 2 times the median per capita household income of non-eligible older women and 1.6 times the median per-capita income of non-eligible older men in the survey data; a substantial sum for most recipients.

Because pension receipt is widespread and the size of the grants is large, the pension system has prompted extensive research on its impact. Studies include analyses of pension take-up patterns and behavioral changes (Case and Deaton, 1998), impacts on labor markets (Bertrand, Mullainathan, and Miller, 2003; Posel, Fairburn, and Lund, 2006; Lam, et al., 2006; Ranchod, 2006; Ardington, Case and Hosegood, 2009), child outcomes (Duflo, 2000, 2003; Edmonds, 2006), private transfers to the household (Jensen, 2004) and household composition (Edmonds, Mammen, and Miller 2005; Hamoudi and Thomas, 2005). Despite this literature, this will be the first study to directly examine how this large change in income affects decision-making dynamics within the household. Many of the cited studies come from the early years after the expansion of the pension to the black population, so this paper will additionally provide some of the first longer-term evidence on the effects of the pension fifteen years after its expansion.

2.2.2 Data

This paper utilizes data from the first wave of the National Income Dynamics Survey (NIDS) conducted by the Southern African Labour and Development Research Unit (SALDRU) in 2008. NIDS is a nationally representative survey of approximately 7,300 households and 28,250 individuals. Detailed information was collected both at the household and individual level through a household survey and individual adult surveys for all people age 15 and over, as well as child surveys for children under 15. This dataset is the first wave of a planned long term panel study in South Africa.[4] Households were selected through a two-stage cluster sampling design based on a master sample of Primary Sampling Units (PSUs) developed by Statistics South Africa and dwelling clusters within those PSUs (Leibbrant, Woolard and de Villiers, 2009). Survey post stratification weights are used in regression analyses when noted.

[3] The exchange rate over the survey period ranged from 7 to 8 South African Rand to the United States dollar.
[4] The second wave of the NIDS data is also publicly available, but because the identification strategy employed in this analysis is cross-sectional only one wave is needed. Wave 1 was chosen for two reasons. First, due to attrition it is larger than Wave 2. Second, due to a two phase interview process, the main variable of interest (the decision making question) is not available for all respondents in Wave 2.

The main variables of interest for this study are derived from the decision-making section of the individual adult questionnaire. Respondents are asked who in their household makes decisions in five different categories: day-to-day household expenditures; large unusual purchases such as appliances, vehicles, or furniture; where children should go to school; who is allowed to live in the household; and decisions about where the household should live. Interviewers note the person code of the main decision maker, and if the decision making is joint, they also note the person code of the second decision maker.

I define indicator variables for each of the five decision-making categories that are equal to one if the person is the primary decision maker in the relevant category and zero otherwise. I consider someone to be the primary decision maker if everyone in the household who answered the survey question listed this person as the main decision maker and zero otherwise. The results in this paper are robust to a definition of the decision-making variables that uses only the self reports of the elderly person in questions.

In this paper I will focus primarily on the first two measures of decision making (day-to-day purchases and large, unusual purchases) because, given that they relate to expenditures, they are the measures most likely to be affected by a change in income. They are also the most closely related to the child nutrition and consumer durable outcomes that I will examine later in the paper. Additionally, the measures of decision making are highly correlated and the results are similar across the different decision-making categories.

Table 2.1 presents summary statistics for the sample of older adults that will form the main analysis sample broken down by gender and pension eligibility. Differences in several key variables including years of schooling, residence in a rural area, percent married and employment status are evident. For example, 63 percent and 72 percent respectively of non-eligible and eligible women live in rural areas and 42 percent and 16 percent respectively of non-eligible and eligible women are employed. However, these differences alone do not invalidate the empirical strategy; pension eligible adults are by definition older than almost eligible adults, and these statistics reflect age trends in these variables. For example, the differences in living in a rural area and employment may be due to the fact that elderly people tend to stop working and return to rural areas they may have grown up in as they age. Consequently, the analysis will control for age, estimating a break in an otherwise smooth trend at the age of eligibility.

Elderly adults tend to live in extended family households; more than half live with a younger woman and a lower but still significant fraction live with a younger man. Many also live with an elderly adult of the other gender, although that number is higher for men than women, reflecting the longer life span of females and the fact that husbands are, on average, older than their wives. The presence of other adults in these households in South Africa makes the analysis of household decision making particularly interesting because the options for who is the decision maker are greater than just the elderly adult and her or his spouse. Studies of household decision making are usually concentrated only on spouses, but the presence of many multi-generational households in this sample and the format of the survey question allows for the examination of changes in decision making across a variety of household members.

2.3 Identification strategy

Although a simple comparison of those who receive the pension with those that do not would confound the impacts of the pension with systematic differences between the two groups, the age requirement for eligibility provides a discontinuity in receipt of the pension that allows for estimation of the causal impact of the pension at the age of eligibility using a regression discontinuity design.[5] Because the means test for eligibility is not binding for the vast majority of black South Africans, this paper (and other studies on the pension) considers only the age eligibility rule when determining pension impacts. I employ an identification strategy that essentially compares people who are age eligible for the pension to those who are almost eligible. The identification assumption underpinning the results in this paper is that individuals or households just below pension eligibility differ from those just above eligibility only through the effect of the pension itself. Even though age trends independent of the effects of the pension are expected in many outcome variables (including household decision making), these trends should not result in large changes right at the age of eligibility. Therefore, discontinuous changes in outcomes that occur at age 60 for women and age 65 for men can be causally attributed to the pension.

The plausibility of the identification assumption is greater the more similar are the individuals included in the analysis. Consequently, as in Edmonds (2006) I limit my main

[5] The empirical strategy utilized in this paper utilizes only cross-sectional data. Although a second wave of NIDS data is available panel methods are not employed for two reasons. First, because there are only two years between waves, only a small number of respondents become pension eligible between waves. Second, the decision making data does not appear to have been consistently collected between the two waves. For example, older women are much more likely to be classified as decision makers in Wave 2, independent of pension eligibility.

estimation sample to black older adults who are 50 to 75 years old, resulting in a sample of 1,750 women and 1,092 men.[6] However, despite limiting the sample to older individuals, identification could still be threatened if there are discontinuities in individual and household characteristics other than pension receipt that might be driving the results. An example of this would be if another large social program was implemented in the same population with the same eligibility rules. Although there are several other government grant programs in South Africa, none of them are similar to the pension in ways that might invalidate the identification strategy (Duflo, 2003).

A greater concern is that receipt of the pension may induce households to reorganize, and that consequently effects that are attributed to the pension are actually characteristics of the types of households that form around the pension, rather than the pension income itself. Several papers have addressed this problem directly. Edmonds, Mammen and Miller (2005) find evidence of an increase in the number of young children and young women in pension eligible households and a decrease in the number of women in prime working ages. Hamoudi and Thomas (2005) find that adults who live with pension eligible adults are shorter and have lower levels of education, characteristics that are presumably not impacted by the pension itself. This work makes it clear that it is difficult to argue that the pension has no effect on household composition; rather it is important to understand whether or not the reorganization that is occurring is the cause of any results that are found. If, for example, an elderly woman were to use the pension income to move out of the extended family home and live independently, any increase in decision-making power would be due to her new living situation and not her improved position within her original household. These issues will be given careful attention in this paper in robustness checks presented in Section VIII. The evidence from these tests is convincing that changes in household composition are not driving the results.

In order to validate the use of the age discontinuity, I must first establish that the pension system works as it is described: that is, that there is actually a discontinuity in receipt of the pension at age 60 for women and age 65 for men. The NIDS survey asked each adult individually whether or not they had received the government old age pension in the past month. The averages of these responses in Table 2.1 clearly show that the likelihood of receiving the pension increases dramatically among the age eligible, from 9 percent to 91 percent for women,

[6] This excludes 6 women and 5 men for whom the primary decision making variable for day-to-day purchases is missing.

and from 8 percent to 84 percent for men. Figure 2.1 shows the age discontinuity in pension receipt graphically, plotting the average receipt by age as well the regression line of pension receipt on age calculated on both sides of eligibility. Although there is some slippage in pension receipt prior to eligibility, the discontinuity is unmistakable. Some of the slippage may be due to age misreporting, but there is also evidence that age ineligible people are able to receive the pension in some cases. This is especially true for men, given that the higher age of male eligibility was largely considered to be unfair, and the survey was conducted just months before the threshold was lowered. Indeed the discontinuity in pension receipt, while strong, is smaller for men than for women.

The regression analysis will allow for a more precise estimation of effect sizes and an exploration of heterogeneity in impacts. I estimate the following linear probability model on the sub-sample of females:

$$(1) \qquad DecisionMaker_{ij} = \alpha_f Eligible_{ij} + \theta_1 EligibleMale_j + \theta_2 OlderMale_j + \gamma(Age_{ij}) + \delta Controls_{ij} + \varepsilon_{ij}$$

where $Eligible$ is an indicator for whether or not woman i in family j is pension eligible. $EligibleMale_j$ and $OlderMale_j$ are indicators for whether or not a pension eligible male or any male aged 50 or older also lives in the household. (Age_{ij}) is a third order polynomial in the age of the woman and flexibly controls for smooth age trends in the outcome variable. The inclusion of a polynomial to control for trends in the variable that determines treatment is one standard method in the regression discontinuity literature (Lee and Lemieux, 2010). A cubic in age was chosen because of its high level of flexibility, but the results are robust to the inclusion of linear or quadratic trends instead.

Included controls are a set of indicators for educational attainment, the number of household members aged 0-5, 6-14, 15-24, and 25-49, and rural status. Robust standard errors are clustered at the household level, and all regressions make use of survey post-stratification weights.[7] I estimate the analogous model for the male sample controlling instead for the presence of an eligible female and older female. The coefficient α on the eligibility indicator is the coefficient of interest. It is an estimate of the impact of pension eligibility on the decision-making outcome variable at the age of eligibility. Although some papers in this literature have used pension eligibility as an instrumental variable for actual pension receipt (Duflo, 2003;

[7] The results are robust to the exclusion of the weights.

Hamoudi and Thomas, 2005), I choose to focus only on the reduced form impact of pension eligibility given the very high rates of pension receipt among the eligible population. The reduced form impact of pension eligibility is a lower bound for the impact of actual pension receipt.

2.4 Impacts of pension eligibility on decision making

The percentages of elderly women and men who are the primary decision maker across four categories are listed in the bottom panel of Table 2.1. Both older men and older women are highly likely to be the primary decision maker in their households. In all cases those who are pension eligible are more likely to be the decision maker than those who are not, but these differences are much smaller for men than for women. However, as noted in Section III, this simple comparison does not provide a causal estimate of the impact of the pension due the probable existence of age trends in decision making that are independent of pension receipt. I now turn to the regression discontinuity analysis that will allow for an estimation of the causal impact of the pension at the age of eligibility.

2.4.1 Individual level decision-making analysis

As a first step in my analysis, in Figure 2.2, I graph the means of the day-to-day decision-making variable by age separately for women and men, and as in Figure 2.1, I also plot the regression line of decision making on age estimated separately on either side of the discontinuity with 95 percent confidence intervals. These graphs are illustrative of the main result of this paper. Despite a pronounced negative age trend in decision making for elderly women there is a large jump upwards at age 60, a difference that is statistically significant even given wide confidence intervals. The same is not true for men. Although the estimated discontinuity is positive, it is small and does not approach statistical significance. Despite this large jump in the regression lines for women, there is quite a bit of noise evident in the raw means. Figure 2.3 shows the same data but with the means smoothed over two year (instead of one year) age bins. Although this smoothing reduces the number of bins, it also reduces the noise significantly, lending further credence to the estimated discontinuity for women.

Next, I address the question of how the pension impacts the identity of primary decision maker in a formal regression framework to provide precise estimates of the effects. Table 2.2 presents the results from the estimation of the regression model described in Section III. This is an individual-level analysis and includes all black men and women aged 50 to 75. The dependent variable in columns 1 and 2 is a binary variable equal to one if everyone in the household agrees that the person is the primary decision maker for day-to-day purchases, and in columns 3 and 4, it is a binary variable equal to one if everyone in the household agrees that the person is the primary decision maker for large, unusual purchases. Columns 2 and 4 include household controls. The results for women are in panel 1 and the results for men in panel 2.

The results in column 1 show that eligible women are 15.5 percentage points more likely to be the primary decision maker for day-to-day purchases, a result that is highly statistically significant. The estimated effect is an economically significant 24 percent of the sample mean. The coefficient is not sensitive to the addition of control variables, dropping only slightly, to 14.2 percentage points, in column 2. The stability of this coefficient further strengthens the identification assumption in this analysis: if the effects were being driven by other differences around the age of eligibility, the additional of control variables should have significantly attenuated the coefficient on eligibility. The South African pension has a large and significant impact on the decision-making power of eligible women. The impact of pension eligibility on decision making for large, unusual purchases is quite similar, 13 to 14 percentage points compared to a slightly lower sample mean. The presence of an elderly male in the household has a large, negative effect on both outcome variables, but interestingly, it does not matter if the male is himself pension eligible. Regressions where the dependent variable is the primary decision maker for where the household lives and who can live in the household show quantitatively similar, significant coefficients for pension eligibility (results not shown). The consistency of this result across decision making categories unrelated to expenditures reinforces the claim that these results are evidence of a real shift in women's bargaining power, not just control over their increased personal income.

No significant effects of pension eligibility are present for the male subsample, and given that the estimated coefficients are negative, there is not even suggestive evidence of a positive effect. The coefficients on male eligibility are also more sensitive to the addition of control variables and other changes in specification (such as using a linear or quadratic age trend instead of a cubic) than the coefficient on female eligibility. These results, combined with the fact that Figure 2.2b shows no true discontinuity, make it unlikely that there is any true effect, positive or negative, for men.[8] Possible explanations for this asymmetry between men and women will be discussed in Section V.

2.4.2 Impacts on decision making power of other household members

The robust result that women's decision-making power increases with pension eligibility leads to the question of from whom in the household that power is coming. The fact that bargaining power appears to be following income controlled by women is an important economic finding, but it also has substantial policy implications. For example, advocates for female empowerment may argue that this result is evidence in favor of expanding transfer programs, but it is possible that this increase comes at the expense of other women in the household, leading to no overall increase in female bargaining power.

To explore this issue I examine the identity of the primary decision maker for day-to-day purchases in different types of households. I create five categories of potential decision makers: women 50 and over, men 50 and over, women 18 to 49, men 18 to 49, and cases where the household does not agree on the identity of the decision maker. Because these are household level designations, analyses are done at the household level, separately for households with a woman 50 to 75 and households with a man 50 to 75. The sample differs from that used in the individual level analysis only in that households where there is more than one elderly woman or elderly man of the same gender are collapsed into one observation. Fewer than 3 percent of households have more than one elderly person of the same gender.

Table 2.3 presents the percentages of households that fall into the different decision-making categories separately by whether a household contains a woman or man aged 50 to 75, and within those categories, whether an elderly person of the opposite gender is present.[9] For both older women and men, when no older member of the opposite sex is present, they are the primary decision makers in the large majority of households (85 percent for women and 70 percent for men). When both an older man and woman are present, men dominate: older males

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[8] I also find no impact on the likelihood of being the secondary decision maker for either men or women. Additionally, to draw a comparison with other papers that utilize survey questions with a more common format that focuses on sole and joint decision making, in results not shown I also create and analyze variables that indicate whether or not the person in question is the sole decision maker, a joint decision maker, or a joint-primary decision maker. This analysis follows the same pattern for men as in the main analyses. The positive effect for women is fully concentrated in the sole decision maker variable (there are no increases in women as joint-primary decision makers with pension eligibility).

[9] Although columns 2 and 4 represent roughly the same households, the numbers differ slightly because I condition on the member of the opposite sex being 50 or over, not age 50 to 75. This restriction best mirrors the regression framework employed throughout the paper.

are the decision maker in approximately 50 percent of households and older women in approximately 30 percent. Interestingly, younger adults play only a small part in decision making except in the case of older men living without older women where younger women are the decision makers in 14 percent of households. Cases of disagreement over who is the decision maker are also important (9 percent of households with an older woman but no older man and 14 percent of households with an older man but no older woman), and especially so in households where there is both an elderly woman and elderly man (22 percent). This is logical as the presence of a competitor for decision making power makes it more likely that all household members may not agree on the identity of the primary decision maker.

I now examine ways in which the distribution of household decision making power may have changed when a household member becomes pension eligible. The regression specification that I employ is similar to that used to estimate changes in decision making on the individual level, but because the outcome variables are at the household level, the regression sample is households with an elderly woman (or man) aged 50 to 75. I include a third order polynomial in the age of the oldest man or woman in the 50 to 75 age range and indicators for the presence of an elderly person and pension eligible person of the other gender. Control variables are the number of household members in different age ranges and rural status. Standard errors are clustered at the survey cluster level. I focus on the two main competitor categories for decision making power: a person 50 and over of the opposite sex and household disagreement over who is the principal decision maker.

Table 2.4 presents the results. Panel 1 displays the results for households with a woman aged 50 to 75 and panel 2 the results for households with a man aged 50 to 75. All regressions include control variables. Column 1 displays results for whether or not an older person of the opposite sex is the primary decision maker for day-to-day purchases in households where an elderly member of the opposite sex is present. The coefficient on female pension eligibility estimates that men 50 or over are 15 percentage points less likely to be the primary decision maker when a woman becomes pension eligible. The coefficient falls short of statistical significance but is suggestive of a negative impact on the decision-making power of elderly men. The coefficient on male pension eligibility in column 1 of panel 2 is positive and large (0.16), though it is not statistically significant. This coefficient is somewhat puzzling, but it is possible that this regression indicates that some older women gain decision making power when their husbands become pension eligible. Both these results are only suggestive, and, given the small sample size, it is difficult to draw any definitive conclusions.

Columns 2 and 3 examine the impact of pension eligibility on disagreement over decision making in the household separately by whether or not an older member of the opposite sex lives in the household. In households with an elderly woman but no elderly man, female pension eligibility results in a 15 percentage point decrease in household disagreement, a result that is both large relative to the sample mean of 0.09 and highly statistically significant. The result is also evident in graphical regression discontinuity analysis. There is no corresponding reduction in disagreement in households with elderly men in column 3 and no impact of male eligibility in columns 2 and 3 of panel 2. This analysis suggests the following story about where women are finding their gains in decision-making power. When they do not live with an older male they are benefitting from increased certainty among household members, an indication of increased bargaining power. The receipt of the pension has the impact of solidifying the woman's position as the decision maker among the members of the household. In cases where the pension eligible woman lives with an older man, there is suggestive evidence that the increase in her power is coming, at least in part, from a reduction in the decision making power of older men.

2.5 Pension eligibility and personal income share

The household models that have informed the research question that this paper addresses are based on the assumption that the control of resources affects decision-making power within the household. If this is not true, then the pension should not necessarily have an effect on how decisions are made (and how resources are allocated) within the household. However, if the interpretation of the result that women's decision-making power increases when they are eligible for the pension is to be guided by these theories, then the non-result in the case of pension eligible men becomes puzzling. Given that men also experience a discontinuous increase in pension receipt at their age of eligibility (Figure 2.1b), this increase in income should also increase their position in the household in regards to decision making.

The lack of increase in decision making power for men could be attributed to several things. One possibility is that men were already the decision maker and therefore there is no room for improvement. However, the breakdown of decision making in the household by age and gender in Table 2.3 makes it clear that older men, although likely to be the decision makers, are by no means always the decision maker. Being the decision maker prior to the pension may have contributed to a smaller increase in decision-making power for men than for women, but it should not have erased it altogether.

It is instructive therefore to understand whether or not control of resources is actually the channel through which the impact on female decision making is operating. First, I examine whether or not control of income is correlated with decision making in the household. Using the reports of individually earned income from the NIDS adult survey, for each elderly adult I calculate his or her income share (the percentage of total household income that he or she reports individually[10]) as a proxy for income control. Figure 2.4 graphs the mean value of the primary decision maker for day-to-day purchases indicator variable against this personal income share variable in five percentage point bins by gender.[11] There is a strong, clear relationship between income share and decision making for both men and women, and the relationship holds regardless of pension eligibility (not shown).[12,13]

This strong relationship between income share and decision-making power draws a clear line to why there are strong impacts of the pension on decision making, at least for women. Consequently, we should also see a discontinuity in income share at the age of pension eligibility. In Figure 2.5 I plot the mean of the elderly individual's income share by age and the regression line, again estimated on either side of the age discontinuity. The discontinuity is clear and striking in the female sample and provides a convincing channel through which the increase in decision making occurs. However, the corresponding increase at age 65 in the male sample is much smaller and noisy. If household decision making is determined through income control then this lack of significant increase in men's income share provides an explanation for why there is no increase in decision making in the male sample. This same pattern is evident, although noisier, when examining raw individual income (results not shown). The idea that increased income share is driving the increase in decision-making power for women is bolstered

[10] The NIDS survey collects income data individually, except in the case of agricultural income which is collected on the household level.
[11] In Figures 2.4 – 2.7 I drop the top half percent of elderly male and female household income earners to eliminate several extreme outliers. Dropping the outliers is done only to allow for cleaner presentation of results and does not affect the qualitative implication of the figures. All results in the paper are robust to the exclusion of these observations.
[12] The relationship also holds in the entire sample of NIDS individuals, not just the elderly population.
[13] To avoid losing observations to missing income data, I utilize income data with imputations done by NIDS. However, dropping observations with imputations does not affect the results.

by regression results. Adding income share to the main decision-making regressions presented in Table 2.2 reduces the magnitude of the coefficient on female eligibility by 40 percent and the effect loses statistical significance. There is no change in the coefficient on male eligibility (Table 2.A.1).

In light of these results for individual income, it is interesting to consider what happens to overall household income with pension eligibility. In particular, the increase in individual income for pension eligible females but not for pension eligible males suggests that any impacts on household outcomes (to be addressed in Section VI) assumed to be a result of increased bargaining power for women could simply be a result of the increased income flowing into households with eligible females. Figure 2.6 plots the results for household income. Interestingly, there is no increase for men or women, suggesting that the pension income is being offset by reductions in other income. With no increase in income, any changes in household outcomes associated with female eligibility must be due to shifts in bargaining power in the household.

Although the lack of increase in male income share provides a convincing explanation for why there is not an increase in male decision making, it is perplexing given the fact that there is a discontinuity in male pension receipt at 65. If male income is not increasing, where is the pension money going? Given previous evidence that both men and women exit the labor force in large numbers at the age of eligibility (Lam et al., 2006; Ranchod, 2006) one explanation is that the pension income received by men is cancelled out by the reduction in labor income caused by withdrawal from the labor force. However, given that the drops in employment were found for both men and women it is not immediately clear why this cancelling out would occur for one gender and not the other.[14]

An examination of labor income is more instructive than a simple examination of employment. Figure 2.7 graphs the age trend, separately by gender, for individual labor income as a percentage of household, non-pension, income.[15] Strong discontinuities are present for both genders, but the means prior to pension eligibility for women are much lower than they are for

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[14] 42% and 16% of non-eligible and eligible women respectively are working. 54% and 22% of non-eligible and eligible men respectively are working. It is interesting to note that labor force withdrawal is not a requirement for pension receipt for most people as employment income in this population is generally below the means test for eligibility.

[15] I perform the calculation in this way to mitigate the mechanical decrease in labor income as a percentage of household income if household income were to increase with the pension.

men. Men earned more than women in the labor market prior to receiving the pension and their labor income was a more important part of the total household budget. Therefore, while the pension represents an increase over what women were earning in the labor market, it is more of a replacement for what men previously earned. This, combined with the fact that the age discontinuity in pension receipt is not as strong for men as for women, can explain why men do not see an increase in personal income share when they become pension eligible. Consequently, male status in the household remains roughly constant.

2.6 Household outcomes

This analysis of how pension eligibility affects decision making and income share in the household is interesting largely because we expect these changes to translate into changes in measures of well being in the household. Although impacts of the pension have been extensively documented in the literature, this is the first study to look for them in the NIDS dataset, and it is important to document that they still exist in 2008, 15 years after the expansion of benefits. Here I examine impacts on child nutrition and ownership of consumer durables, two measures that are likely to be associated with the two main decision-making categories that I have addressed in this paper, decisions about day-to-day purchases such as groceries and decisions about large, unusual purchases such as many consumer durables.

2.6.1 Child nutrition

One of the most well-known results in the pension literature is Duflo's finding that female pension eligibility results in higher values of anthropometric indicators for young girls but not young boys (Duflo, 2003). Utilizing anthropometric data collected from young children, she examines the impact of the pension on standardized measures of child nutritional status, including weight for height.[16] Weight for height is a flow measure of nutrition, a marker that responds quickly when a child's conditions changes. In her main results, Duflo finds a 0.61 standard deviation increase in the weight for height measure for young girls with the presence of a pension eligible woman but a small and insignificant effect with the presence of a pension eligible man. There are no statistically significant impacts for boys.

Duflo uses a nationally representative household survey from 1993, similar in structure to

[16] Weight for height Z-scores are calculated by subtracting the median and dividing by the standard error for the child's height and sex in a standard reference population. Duflo uses the reference group of well-nourished US children provided by the U.S. National Center for Health Statistics, standard prior to 2006.

NIDS, to conduct her analysis, making this a feasible result to examine with the current data.[17] Additionally, because this result is widely used to make inferences about household models and support arguments that giving income to women over men leads to improved outcomes for children, it is particularly appropriate in the context of the current paper.

The NIDS survey collects anthropometric data from both children and adults, allowing for the construction of standardized weight for height z-scores. As discussed earlier, weight for height is typically seen as an indicator of acute malnutrition as it responds quickly to improvements in nutrition. Children who are severely malnourished are two or more standard deviations below the median weight for height. To construct the z-scores I use the WHO international child growth standards for children up to age five as the reference population (WHO, 2006). In all analyses, I drop observations with z-scores deemed biologically impossible (absolute z-scores greater than 5 for weight for height).

Standardized weight for height measures are defined only for young children. Consequently, I limit my sample to children aged 6 to 60 months.[18] Following the empirical strategy employed in the rest of the paper, I also limit the sample to black children who live with a person aged 50 to 75. There are 593 boys and 572 girls aged 6 to 60 months who live with a person aged 50 to 75 in the NIDS database. Unfortunately, a significant amount of the sample is lost to missing or unfeasible anthropometric data, leaving 413 boys and 389 girls for analysis purposes.[19]

In this sample I estimate the following equation:

$$(2) \qquad WeightforHeight_{ij} = \alpha_f EligibleFemale_j + \alpha_m EligibleMale_j + \theta_f OlderFemale + \theta_m OlderMale_j + \gamma(AgeMale_j, AgeFemale_j) + \beta AgeChild_{ij} + \delta Controls_{ij} + \varepsilon_{ij}$$

where $EligibleFemale$ and $EligibleMale$ are indicators for the presence of an age-eligible man or woman in the household. $OlderFemale$ and $OlderMale$ are indicators for whether or not

[17] The 1993 survey is composed only of a household survey; it did not incorporate individual interviews with household members and does not contain questions on decision making.
[18] Table 2.A.2 shows that the same patterns in decision making are present among the elderly who live with children in this age group, although the effects are somewhat smaller and less precise.
[19] A comparison of children with valid anthropometric data and those without shows few differences across a variety of relevant household characteristics. The exception is that children with missing data are more likely to live in an urban area.

there is a woman or man aged 50 to 75 in the household. Following Edmonds (2006) $(AgeFemale_j, AgeMale_j)$ is a third order polynomial in the age of the oldest woman and the oldest man in the household. In all specifications I include a set of indicators for child's age and mother's educational attainment and further include controls for the number of household members who are 0-5, 6-14, 15-24, and 25-49, and presence of mother and father in the household. [20] α_f and α_m can then be interpreted as the difference in weight for height between a child living with a pension eligible woman (man) and a child living with a woman (man) who is almost eligible. This specification is similar to those used to estimate the impacts on decision making, but because the level of observation is the child, not the older adult, it controls for age trends in the age of the oldest man and woman in the household.

Table 2.5 shows the results of estimating this equation. Columns 1 and 4 present results when a single eligibility indicator is used. The coefficient on pension eligibility is large, positive and statistically significant for girls, but small and negative for boys. Columns 2 and 5 include separate indicators for an eligible woman and an eligible man and control for the presence of a woman or man aged 50 to 75 and columns 3 and 6 add the set of control variables. The coefficient on woman eligible is large and stable to the addition of control variables for girls. The presence of a pension eligible woman increases weight for height of girls by about 0.6 standard deviations. The coefficients for eligible man are close to zero and have large standard errors. In the boys sample the coefficients on male and female eligibility are small and imprecise. A clear pattern emerges from these results, namely that the presence of a pension eligible woman (but not a pension eligible man) increases the weight for height of girls. There is no effect of pension eligibility of either gender for boys. This pattern of results is the same as those reported by Duflo (2003).

2.6.2 Ownership of consumer durables

The significant increase in income provided by the pension provides the opportunity not only to improve the quality of day-to-day purchases on food, but also to invest in larger household items that have the ability to improve quality of life. Certain consumer durables like modern stoves and refrigerators can also contribute to improved health in the household, particularly for children. The NIDS survey collects information on 27 separate durable goods

[20] I do not control include controls for father's educational attainment because of the large number of missing values.

that may be owned by households. Here I consider the total number of what I term “household” durable goods, which are the 16 goods listed on the survey excluding ownership of vehicles, bikes, and large agricultural tools. The household durable goods include radios, televisions, cell phones, stoves, refrigerators, washing machines, and living room furniture.[21] I observe only whether or not a household possesses each type of good and do not know if they have more than one of each type. Consequently, I can detect if pension eligible households buy types of goods that they did not previously own, but not if they buy more of or replace goods that they already had.

I estimate the same household level model that I use in Section IV to examine changes in decision making by other members of the household; the dependent variable is the number of household durable goods. Table 2.6 presents the results. Results are shown for all households with an elderly man or woman. Columns 1 and 2 show results for households with an elderly woman aged 50 to 75. The results for households with an elderly man are in Columns 3 and 4. Columns 2 and 4 include control variables.

Among households with an elderly woman, female eligibility results, on average, in 1.1 more household durable goods, a 23 percent increase in the sample mean of 4.9. This is robust to the addition of controls. Women do appear to be channeling some of their pension income into the purchase of consumer durables, a complement to the fact that they were found to be significantly more likely to be the primary decision maker for large, unusual purchases in the household. The increase in consumer durables also provides another channel beyond healthier food through with the improvement in girls’ nutrition could be occurring. The coefficient on male eligibility is small and imprecise; there is no evidence that male pension income leads to the purchase of more consumer durables.

2.7 Discussion

Empirical analyses that find positive impacts on household outcomes associated with increases in female resources but no such benefits associated with increases in male resources are often used to argue that cash transfer funds may be more productively used when given to women instead of men. On their face, the results in the previous section (positive impacts on

[21] The full list of included durable goods is: radio; Hi-Fi stereo; CD player; MP3 player; television; satellite dish; VCR or DVD player; computer; camera; cell phone; electric stove; gas stove; paraffin stove; microwave; fridge/freezer; washing machine; sewing/knitting machine; lounge suite.

household outcomes with female eligibility but not with male eligibility) would also seem to support this contention. However, when taken together with the income share results described in Section V, the interpretation is no longer so clear cut. Given that there is no increase in income with male pension eligibility there is no *a priori* reason to expect to see evidence of positive impacts on household well-being as in the case of female eligibility. There may have been positive impacts if male income had increased. The results in this study highlight the need for caution when interpreting results (such as those on girls' nutrition and ownership of consumer durables in this paper) that seem to indicate that money given to women is better spent than money given to men. Even if the narrow goal of a transfer is an improvement in child nutrition, it is not possible to predict what would have happened had the transfers been given to, for example, younger men and women who would have been less likely to leave the labor force. Indeed, in a study in Burkina Faso, Akresh, de Walque, and Kazianga (2012) find that conditional cash transfers have the same positive effects on the utilization of preventative health services when given to the father as when given to the mother.

Given that the Duflo (2003) study on pension eligibility and child health is one of the papers that is most widely cited in support of the contention that transfer programs should be directed towards women, it is interesting to examine the income dynamics in the data used in Duflo's study. The analysis in that paper was done using the Project for Statistics on Living Standards and Development (PSLSD), a 1993 survey similar to NIDS in sample size, goals, and structure, although it consisted of a single household level survey. Using the PSLSD data I construct figures 8 through 10. Figure 2.8 shows that the discontinuity in pension receipt in 1993 does exist at age 60 and 65 for women and men respectively, although it is smaller than in 2008, particularly for men. Figures 2.9 and 2.10 are analogous to Figures 2.5 and 2.6 from the NIDS data examining the changes in personal income share and total household income as a result of male and female eligibility. I do this only for elderly people who live with a child 6 to 60 months old, as that is the sample of interest in Duflo's paper.[22] Decision making data was not collected in the PSLSD, so my analysis is limited to the income data.

The results of this analysis roughly replicate the results in the NIDS data. A strong increase in personal income share (Figure 2.9) is present for pension eligible females, but not for pension eligible males, suggesting that women experience an increase in bargaining power when

[22] As in Section V, I drop observations in the top half percent of household income, by female and male.

they become pension eligible while male status in the household remains constant. Additionally, there is no increase in household income with female or male eligibility (Figure 2.10), indicating that the observed changes in nutrition must be due to shifts in female bargaining power, not increased household income. These parallels to the results in the NIDS data suggest that the use of the Duflo paper to strongly argue for the targeting of programs towards women should be reconsidered. The pension, in both 1993 and 2008, appears to leave men's status in the household unchanged.

2.8 Robustness checks

Although the main decision-making results in this paper are robust to the addition of control variables and changes in sample, given the large literature that exists documenting a variety of impacts of the pension, it is important to explore the possibility that the increases in decision making shown here are an effect of one of these other changes rather than a direct impact of shifts in who controls household income.

2.8.1 Changes in employment

One important impact of the pension is its effect on the labor supply of those who are eligible. A potential explanation, then, for the increases in female decision-making power is that these effects are being driven, at least in part, by women who are leaving the work force when they begin to receive the pension. Now retired, they spend more time at home and therefore assume more household duties, such as making decisions about purchases. If true, then the increase in decision-making power is not due to the income that elderly women are now earning, but to their new role in the household. Two analyses already done in this paper rebut this explanation. First, the increases in decision-making power are seen not only for day-to-day purchases but also for large, unusual purchases, a category for which the labor supply explanation seems less well suited. While the daily shopping may be seen as a chore, it is less likely that infrequent, large purchases are. Additionally, if the extra time at home that resulted from leaving the labor force was the driving force behind the increase in decision-making power, then there should an upward trend in decision-making in the years prior to eligibility as women steadily stop working. However, in Figure 2.2a, the age trend in decision making prior to pension eligibility is negative, suggesting that this is not the case.

The possibility that changes in employment are causing the observed increases in female decision making can also be tested directly. I re-estimate regression equation 1, now including an interaction term between pension eligibility and employment.[23] If the decision-making impacts are being driven by those who have left the labor force, then the coefficient on the interaction term should be negative. The results are presented in Table 2.7. Columns 1 and 2 present the results for women and columns 3 and 4 present the results for men. Columns 1 and 3 include the full set of controls as well as an indicator variable for employment status. Columns 2 and 4 add the interaction between eligibility and employment. The results for women are not suggestive of heterogeneous effects by employment status. The coefficient on the interaction term, while negative, is small compared to the main effect of eligibility and insignificant. Additionally, the coefficient on employment status in Column 1 is a significant 6 percentage points, suggesting that being employed tends to have a positive, not negative, impact on decision making. Again no significant effects related to eligibility are present for men, although employment does have a similar positive effect on decision making in the male sample.

2.8.2 Changes in household composition

The most important threat to the validity of the results in this paper is the possibility that receipt of the pension causes households to reorganize and that the results are an artifact of this change in household structure rather than a direct impact of the pension itself. Because previous work has shown that some changes in household composition do seem to occur (Edmonds et al., 2005; Hamoudi and Thomas, 2005) the goal in this paper is to argue that any changes that may be occurring are not likely to affect the validity of the results. For example, the existing literature finds increases in the numbers of young children and young woman and decreases in prime working age women with female pension eligibility. Even if this pattern were to be found in the NIDS data it is unlikely to be the cause of the results that I find. As shown in Table 2.3, only a very low percentage of non-elderly women are the decision makers in households with an elderly women, therefore it is difficult to describe a scenario in which their movements in and out of households could impact the decision-making power of elderly women to such a great extent.

Figure 2.11 explores whether there is a change in household size at pension eligibility. I plot the mean of household size against age and show the regression line and 95 percent confidence intervals estimated on either side of the discontinuity. Figure 2.11a examines

[23] This is an individual level analysis, as in Table 2.2. Sample size differs slightly because some observations have missing employment data.

household size for elderly women. Although the slope of the regression line changes at the discontinuity, there is no discernible jump. However, the analysis for men, in Figure 2.11b, is highly suggestive of an increase in household size, a result that is supported by regression analysis (results not shown). In order to understand where this increase is coming from, I examine changes in the number of children aged 14 and below in the household in Figure 2.12. Figure 2.12 shows that for elderly women there is essentially no change in the number of children at the age of eligibility, but there is a large discontinuity at age 65 in the male sample. The jump in the regression line is roughly equal in size to the increase in household size seen in Figure 2.11b. There is no change in the mean number of adults in the household or in the number of elderly men or women in the household for men or women (results not shown).

In the NIDS data there is not strong evidence of changes in household composition that could be driving the decision making results found for elderly women. However, pension eligibility is associated with an increase in the number of children living with elderly men. One possible explanation for this is that men stop working when they receive the pension and move in with extended family where there are more children. To address this issue, in Table 2.8, I re-estimate the main results presented in Table 2.2 and add flexible controls for number of children in the household, in the form of dummy variables for the presence of one, two, three and four or more children. No children is the omitted category. Columns 1 (women) and 3 (men) repeat the results presented in column 1 of Table 2.2. Columns 2 and 4 add the controls for number of children.

Controlling for number of children has very little effect on the eligibility coefficient in the female sample, and with the exception of the four or more category, the presence of children has no significant relationship with decision-making power. This is not true for men; adding the controls reduces the magnitude of the eligibility coefficient from -0.08 to essentially zero. Additionally, the presence of children in the household has a strong, negative relationship with male decision making. In short, considering changes in household composition does not affect the estimates for female decision making. Household composition matters more for men, but controlling for these changes only strengthens the finding of a zero result for men: the negative (insignificant) coefficient is reduced to zero, and there is a no suggestion of a positive effect. These results are consistent with the results in Table 2.2 that show that the estimated effect of pension eligibility is more sensitive to the addition of covariates in the male sample than in the female sample.

2.9 Conclusion

The results in this study show that women experience an increase in personal income share when they become eligible for the pension. Bargaining models of the household predict that this increase should result in an increase in bargaining power, and I find that pension eligible women are more likely to be the primary decision maker in their households across a variety of categories. This shift in decision-making power is accompanied by improved nutritional status for young girls and an increase in the ownership of consumer durables. This is one of the first studies to show how specific mechanisms within the household (namely decision making) are reacting to gender specific changes in income instead of relying solely on the reduced form impact on household outcomes to make arguments about bargaining power.

The improvements in household outcomes with female, but not male, eligibility, echo previous results (Duflo, 2003) that have been used both to support bargaining models of the household and to argue that social programs should channel resources towards women as they will direct the money towards more productive uses. The findings in this paper that the lack of impacts for men may be due to the lack of increases in personal income share and bargaining power suggests caution in advocating for such targeting based on a reduced form analysis alone. Any evaluation of a cash transfer program such as the South African pension must acknowledge that households will react in varied and complex ways and changes in outcomes cannot be interpreted in the absence of an understanding of changes in intra-household income dynamics.

Figure 2.1: Pension receipt by age

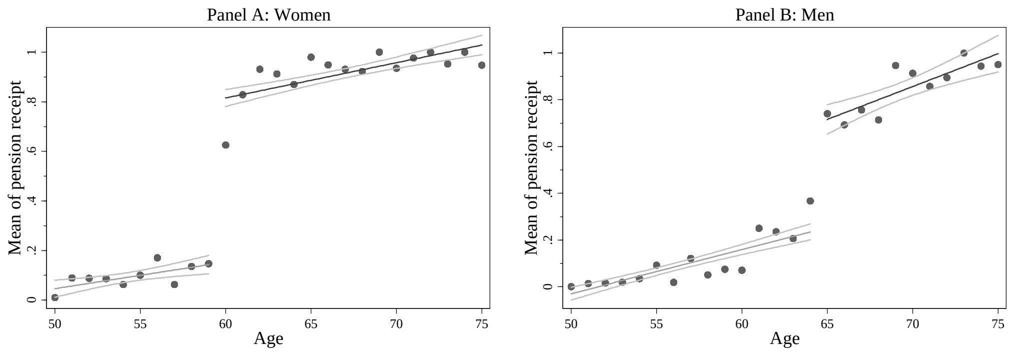


Figure 2.2: Primary decision making for day-to-day purchases by age

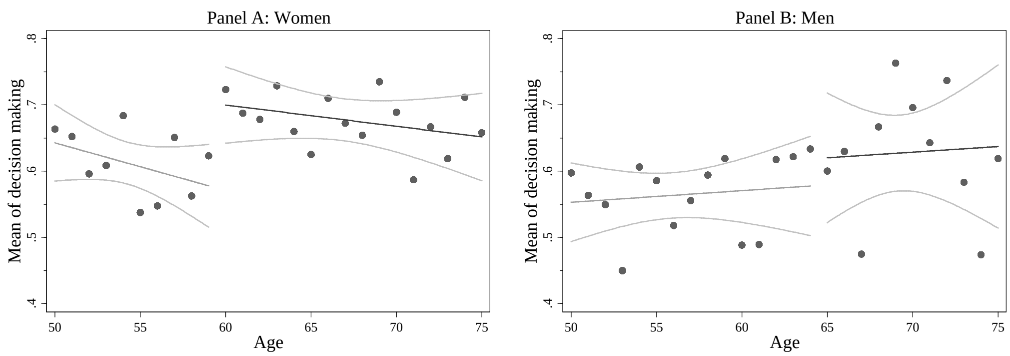
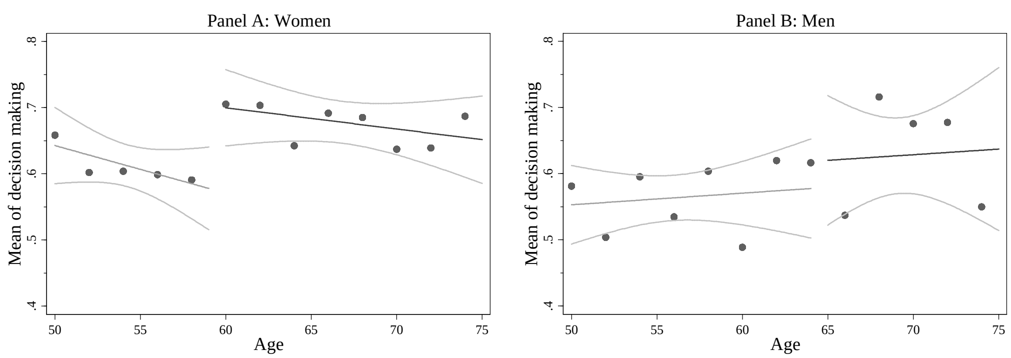


Figure 2.3: Primary decision making for day-to-day purchases by age, two year age bins



Notes: Sample is individuals aged 50 to 75, women in panel A and men in panel B. Scatterplots are unweighted means of y-axis variable by age in years. Unweighted OLS regression lines of y-axis variable on age are estimated on either side of the discontinuity (age 60 for women and age 65 for men). 95% confidence intervals are shown around the regression lines. Y-axis variable in Figure 2.1 is a dummy variable for pension receipt. Y-axis variable in Figures 2.2 and 2.3 is a dummy variable for whether or not everyone in household agrees that individual is the primary decision maker for day-to-day purchases.

Figure 2.4: Decision making by percent of personal income share

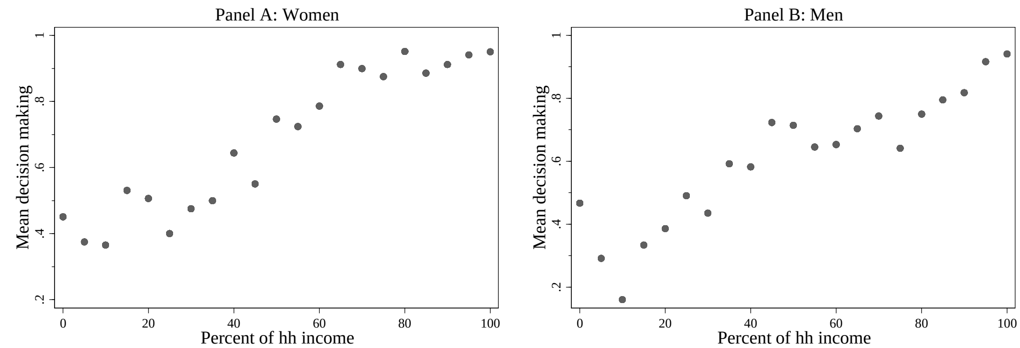


Figure 2.5: Personal income share by age

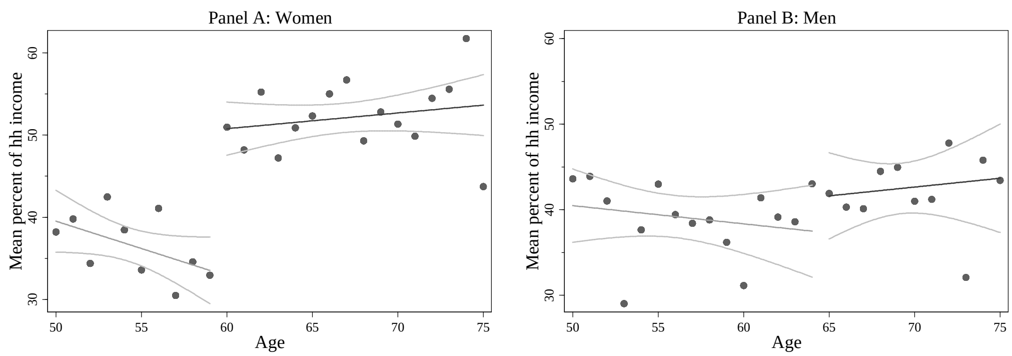
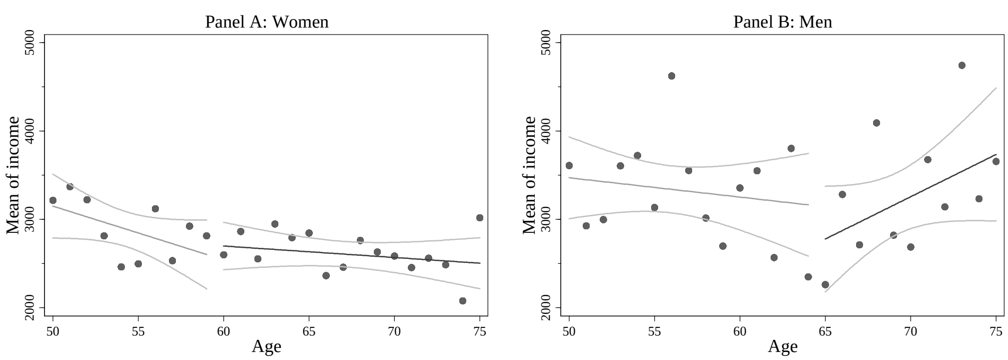


Figure 2.6: Total household income by age



Notes: Sample is individuals aged 50 to 75, women in panel A and men in panel B. The top half percent of male and female household income earners are trimmed. Figure 2.4: Scatterplot is the mean of whether or not everyone in the household agrees the individual is the primary decision maker for day-to-day purchases by 5 percentage point bins of personal income share. Figures 2.5 & 2.6: Scatterplots are unweighted means of y-axis variable by age. Unweighted OLS regression lines of y-axis variable on age are estimated on either side of the discontinuity (age 60 for women and age 65 for men). 95% confidence intervals are shown around the regression lines. Y-axis variable in Figure 2.5 is the percent of total household income reported to be earned or received by the individual. Y-axis variable in Figure 2.6 is total household income.

Figure 2.7: Individual labor income as percent of household non-pension income by age

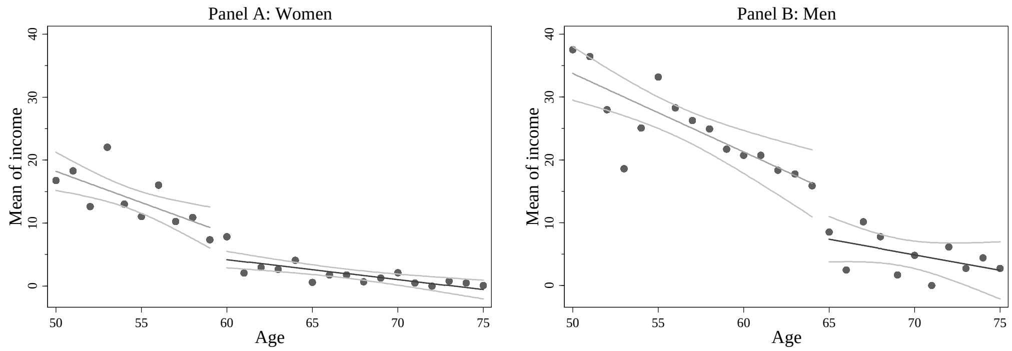


Figure 2.8: Pension receipt by age in 1993: HHs with young children

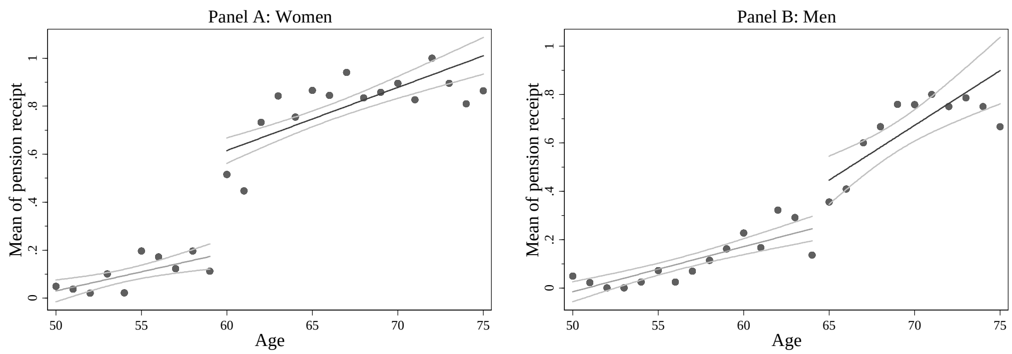
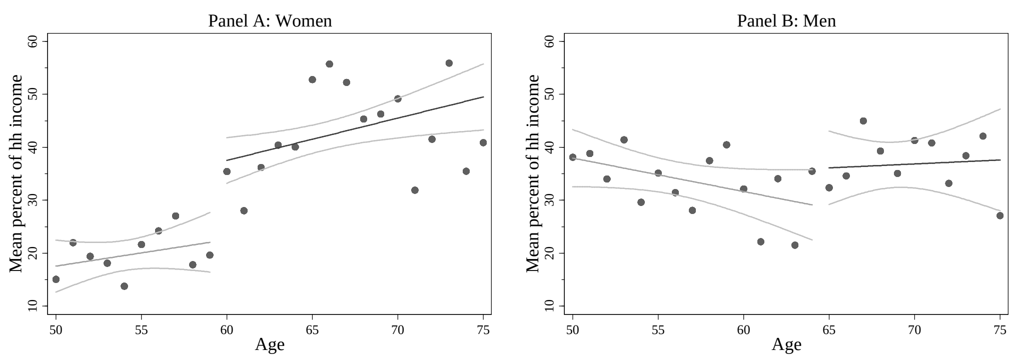


Figure 2.9: Personal income share by age in 1993: HHs with young children



Notes: Scatterplots are unweighted means of y-axis variable by age in years. Unweighted OLS regression lines of y-axis variable on age are estimated on either side of the discontinuity (age 60 for women and age 65 for men). 95% confidence intervals are shown around the regression lines. Figure 2.7: Sample is individuals aged 50 to 75, women in panel A and men in panel B. Y-axis variable is individual labor income as a percent of household non-pension income. Figures 2.8 and 2.9: Source is 1993 PSLSD dataset. Sample is individuals aged 50 to 75 living with a child 6 to 60 months. Y-axis variable in Figure 2.8 is pension receipt. Y-axis variable in Figure 2.9 is the percent of total household income reported to be earned or received by the individual.

Figure 2.10: Total HH income by age in 1993: HHs with young children

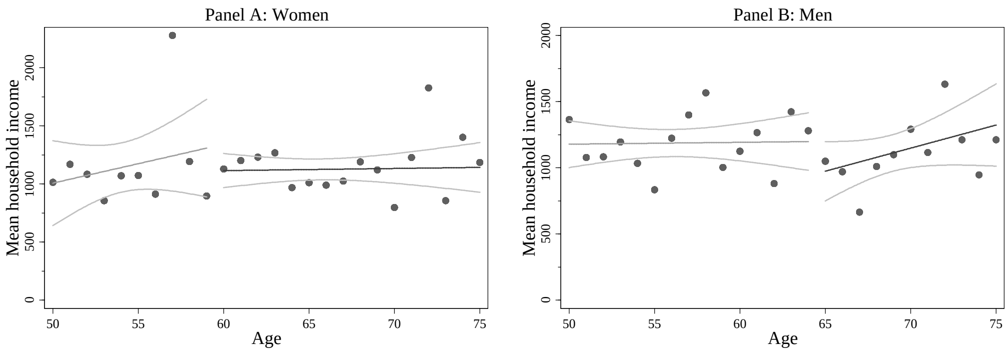


Figure 2.11: Household size by age

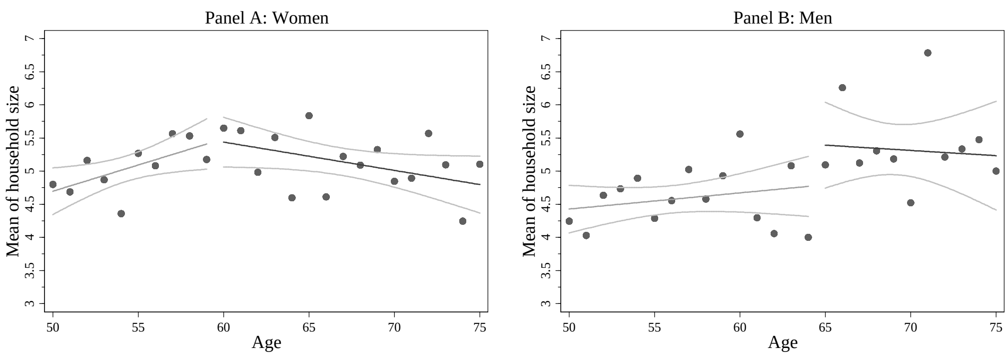
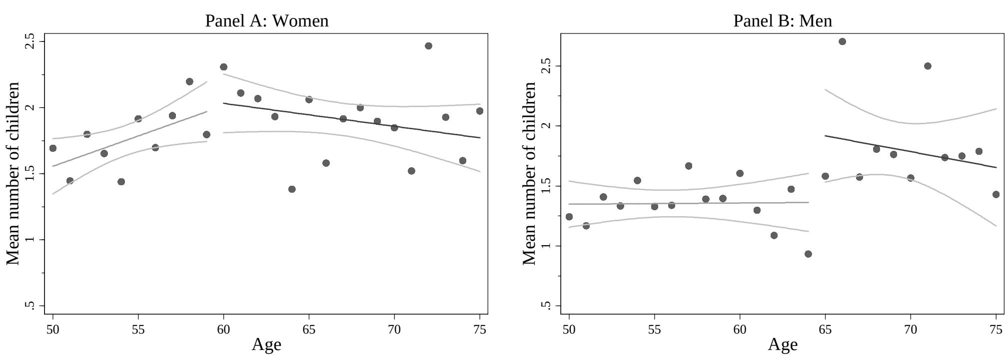


Figure 2.12: Number of children aged 0-14 by age



Notes: Scatterplots are unweighted means of y-axis variable by age in years. Unweighted OLS regression lines of y-axis variable on age are estimated on either side of the discontinuity (age 60 for women and age 65 for men). 95% confidence intervals are shown around the regression lines. Figure 2.10: Source is 1993 PSLSD dataset. Sample is individuals aged 50 to 75 living with a child 6 to 60 months. Y-axis variable in Figure 2.10 is total household income. Figures 2.11 and 2.12: Sample is individuals aged 50 to 75, women in panel A and men in panel B. Y-axis variable in Figure 2.11 is household size. Y-axis variable in Figure 2.12 is number of children aged 0-14 in the household.

Table 2.1: Summary statistics for adults aged 50 to 75

	Women		*Men*	
	Not eligible	Eligible	Not eligible	Eligible
Demographics				
Age (mean)	54.3 (2.8)	66.8 (4.5)	55.9 (4.2)	69.2 (3.0)
Household size (mean)	5.0 (3.0)	5.1 (3.0)	4.6 (3.0)	5.3 (3.2)
Years of schooling (mean)	4.3 (4.0)	2.8 (3.5)	4.76 (4.1)	2.73 (3.5)
Rural (%)	63.5	71.7	55.7	75.3
Married (%)	47.1	32.5	73.9	78.8
Presence of child under fifteen (%)	71.8	74.6	58.9	68.0
Presence of child under five (%)	42.2	41.8	31.6	42.6
Presence of man (woman) 50+ (%)	35.8	28.0	42.1	70.2
Presence of woman 18 - 49 (%)	51.4	54.4	56.1	56.4
Presence of man 18 - 49 (%)	44.1	43.9	34.2	39.9
Income and employment				
Employed (%)	42.3	16.3	54.0	22.1
Per-capita hh income (median)	425	480	530	570
Personal income (median)	650	940	870	870
Personal income as percent of total hh income (median)	32.0	48.1	36.8	39.8
Pension receipt				
Received pension (%)	9.2	91.4	8.3	83.9
Amount received (median, conditional on receipt)	870	870	920	870
Is primary decision maker for				
Day-to-day purchases (%)	61.2	67.8	56.3	62.7
Large, unusual purchases (%)	57.8	65.0	64.5	66.9
Who can live in household (%)	55.8	64.9	68.6	72.0
Where household lives (%)	55.4	64.6	69.6	71.7
Observations	915	835	813	279

Notes: Author's calculations from 2008 NIDS. Standard deviations for means are in parentheses. Number of observations is based on black individuals aged 50-75 with non-missing values for decision making on day-to-day purchases which is the main regression sample. All money amounts are in South African rand, the exchange rate varied from 7-8 rand to the US dollar over the survey period. Employment is defined as working in any capacity including casual labor, self employment, and own farm labor. Personal income is any income that can be attributed directly to the individual, all income is collected individually except for agircultural income. Decision making variables are dummy variables for whether or not everyone in the household agrees that the individual is the primary decision maker in that category.

Table 2.2: Effect of pension eligibility on household decision making

	(1)	(2)	(3)	(4)
	Dependent variable: Primary decision maker for day-to-day purchases		Dependent variable: Primary decision maker for large, unusual purchases	
Panel 1: Women				
Pension eligible	0.155***	0.142**	0.139**	0.127**
	[0.0573]	[0.0565]	[0.0619]	[0.0610]
Presence of man 50+	-0.543***	-0.551***	-0.603***	-0.603***
	[0.0371]	[0.0362]	[0.0343]	[0.0350]
Presence of pension eligible man	-0.0447	-0.0225	-0.0246	-0.0156
	[0.0524]	[0.0521]	[0.0482]	[0.0490]
Observations	1,750	1,750	1,726	1,726
R-squared	0.310	0.334	0.355	0.364
Sample mean	0.64		0.61	
Panel 2: Men				
Pension eligible	-0.0788	-0.0477	-0.138	-0.0984
	[0.104]	[0.0905]	[0.106]	[0.0913]
Presence of woman 50 to 75	-0.274***	-0.253***	-0.185***	-0.166***
	[0.0461]	[0.0446]	[0.0468]	[0.0454]
Presence of pension eligible woman	-0.0593	-0.0212	-0.0614	-0.0246
	[0.0582]	[0.0565]	[0.0608]	[0.0577]
Observations	1,092	1,092	1,078	1,078
R-squared	0.080	0.182	0.046	0.146
Sample mean	0.58		0.65	
Control variables	NO	YES	NO	YES
Cubic in age of person	YES	YES	YES	YES

Notes: Robust standard errors in brackets are clustered at the household level. Regressions are weighted with survey post-stratification weights. Sample is restricted to black men and women aged 50-75. Control variables are number of household members who are 0-5, 6-14, 15-24, and 25-49, educational attainment category, and rural/urban status.

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

Table 2.3: Identity of household decision maker: Primary decision maker for day-to-day purchases

	Households with a:			
	Woman 50 - 75		Man 50 - 75	
	No man 50+ in hh	*Man 50+ in hh*	*No woman 50+ in hh*	*Woman 50+ in hh*
Decision maker is woman 50+ (%)	85.4	27.3		28.2
Decision maker is man 50+ (%)		48.7	70.2	48.5
Decision maker is woman 18 - 49 (%)	2.8	1.5	14.4	1.7
Decision maker is man 18 - 49 (%)	2.7	0	1.5	0
Household disagrees on decision maker (%)	9.2	22.2	13.9	21.5
Observations	1163	546	541	522

Notes: Author's calculations from 2008 NIDS.

Table 2.4: Effect of pension eligibility on decision making of others in the household

	(1)	(2)	(3)
	Dependent variable: Person of opposite sex aged 50+ is primary decision maker for day-to-day purchases	Dependent variable: Household disagreement on identity of primary decision maker for day-to-day purchases	
Panel 1: Households with a woman 50 - 75			
	Man 50+ in hh	*No man 50+ in hh*	*Man 50+ in hh*
Pension eligible woman	-0.149	-0.153***	0.0576
	[0.118]	[0.0445]	[0.127]
Presence of pension eligible man	0.0952		-0.0230
	[0.0707]		[0.0610]
Observations	546	1,163	546
R-squared	0.064	0.064	0.084
Sample mean	0.49	0.09	0.23
Panel 2: Households with a man 50 - 75			
	Woman 50+ in hh	*No woman 50+ in hh*	*Woman 50+ in hh*
Pension eligible man	0.161	-0.0416	-0.0687
	[0.118]	[0.0848]	[0.104]
Presence of pension eligible woman	0.0408		-0.0456
	[0.0561]		[0.0523]
Observations	522	541	522
R-squared	0.043	0.173	0.101
Sample mean	0.28	0.14	0.21
Control variables	YES	YES	YES
Cubic in age of oldest woman (man)	YES	YES	YES

Notes: Robust standard errors in brackets are clustered at the survey cluster level. Regressions are weighted with survey post-stratification weights. Sample is restricted to households with a black woman (man) aged 50-75. Control variables are number of household members who are 0-5, 6-14, 15-24, and 25-49.

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

Table 2.5: Effect of pension eligibility on weight for height z-scores

	(1)	(2)	(3)	(4)	(5)	(6)
		Girls			*Boys*	
Eligible person	0.573**			-0.121		
	[0.261]			[0.256]		
Eligible woman		0.598*	0.550*		0.0293	-0.0341
		[0.349]	[0.324]		[0.336]	[0.337]
Eligible man		0.221	0.0508		-0.144	-0.173
		[0.417]	[0.412]		[0.393]	[0.414]
Presence of woman 50-75		-0.128	-0.0635		0.132	0.273
		[0.305]	[0.288]		[0.390]	[0.417]
Presence of man 50-75		-0.251	-0.598		0.355	0.293
		[0.428]	[0.457]		[0.605]	[0.605]
Observations	389	389	389	413	413	413
R-squared	0.108	0.107	0.146	0.060	0.063	0.102
Cubic in age of oldest man, woman	YES	YES	YES	YES	YES	YES
Control variables	NO	NO	YES	NO	NO	YES

Notes: Robust standard errors in brackets are clustered at the household level. Regressions are weighted with survey post-stratification weights. Sample is restricted to black boys and girls aged 6 to 60 months who live with a person aged 50-75 and have non-missing, valid anthropometric data. All regressions control for age of child. Control variables are number of household members who are 0-5, 6-14, 15-24, and 25-49, mother's educational attainment category, and presence of mother and father in the household.

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

Table 2.6: Effect of pension eligibility on number of household consumer durables

	(1)	(2)	(3)	(4)
	Households with a woman 50-75		*Households with a man 50-75*	
Eligible woman (man)	1.120***	1.057**	0.00753	0.345
	[0.431]	[0.433]	[0.745]	[0.655]
Presence of man (woman) 50+	0.675**	0.629**	1.326***	1.472***
	[0.328]	[0.286]	[0.365]	[0.318]
Eligible man (woman)	-0.186	0.0966	0.172	-0.0312
	[0.454]	[0.384]	[0.447]	[0.384]
Observations	1,709	1,709	1,063	1,063
R-squared	0.025	0.179	0.051	0.234
Sample mean	4.95		4.96	
Cubic in age of oldest woman (man) 50-75	YES	YES	YES	YES
Control variables	NO	YES	NO	YES

Notes: Robust standard errors in brackets are clustered at the survey cluster level. Regressions are weighted with survey post-stratification weights. Sample is restricted to households with a black woman (man) aged 50 -75. Control variables are number of household members who are 0-5, 6-14, 15-24, and 25-49. Household durable goods include radio; Hi-Fi stereo, CD player, MP3 player; television; satellite dish; VCR or DVD player; computer; camera; cell phone; electric stove; gas stove; paraffin stove; microwave; fridge/freezer; washing machine; sewing/knitting machine; lounge suite.

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

Table 2.7: Effect of pension eligibility on household decision making: Interactions with employment

	(1)	(2)	(3)	(4)
	Dependent variable: Primary decision maker for day-to-day purchases			
	Women		*Men*	
Pension eligible	0.150***	0.166***	-0.0520	-0.0571
	[0.0544]	[0.0559]	[0.0904]	[0.0920]
Presence of man (woman) 50+	-0.549***	-0.548***	-0.233***	-0.233***
	[0.0365]	[0.0366]	[0.0478]	[0.0479]
Eligible man (woman)	-0.0135	-0.0141	0.00619	0.00764
	[0.0532]	[0.0530]	[0.0595]	[0.0604]
Employed	0.0627**	0.0825**	0.0669*	0.0627
	[0.0278]	[0.0348]	[0.0406]	[0.0447]
Employed*Pension eligible		-0.0595		0.0208
		[0.0572]		[0.109]
Observations	1,709	1,709	1,017	1,017
R-squared	0.339	0.340	0.184	0.184
Sample mean	0.65		0.60	
Control variables	YES	YES	YES	YES
Cubic in age of person	YES	YES	YES	YES

Notes: Robust standard errors in brackets are clustered at the household level. Regressions are weighted with survey post-stratification weights. Sample is restricted to black men and women aged 50 - 75 with non-missing employment data. Control variables are number of household members who are 0-5, 6-14, 15-24, and 25-49, educational attainment category, and rural/urban status.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 2.8: Effect of pension eligibility on household decision making: Controls for number of children

	(1)	(2)	(3)	(4)
	Dependent variable: Primary decision maker for day to day purchases			
	Women		*Men*	
Pension eligible	0.155***	0.164***	-0.0788	-0.0169
	[0.0573]	[0.0578]	[0.104]	[0.101]
Presence of man (woman) 50+	-0.543***	-0.544***	-0.274***	-0.189***
	[0.0371]	[0.0373]	[0.0461]	[0.0481]
Eligible man (woman)	-0.0447	-0.0361	-0.0593	-0.0668
	[0.0524]	[0.0527]	[0.0582]	[0.0576]
Number of children = 1		-0.00281		-0.220***
		[0.0334]		[0.0552]
Number of children = 2		-0.0495		-0.226***
		[0.0358]		[0.0573]
Number of children = 3		-0.0431		-0.222***
		[0.0408]		[0.0738]
Number of children = 4+		-0.100**		-0.284***
		[0.0456]		[0.0637]
Observations	1,750	1,750	1,092	1,092
R-squared	0.310	0.316	0.080	0.127
Sample mean	0.64		0.58	
Control variables	NO	NO	NO	NO
Cubic in age of person	YES	YES	YES	YES

Notes: Robust standard errors in brackets are clustered at the household level. Regressions are weighted with survey post-stratification weights. Sample is restricted to black men and women aged 50 - 75. Columns 2 and 4 include dummy variables for the number of children 14 and under in the household. The omitted category is 0.

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

Table 2.A.1: Effect of pension eligibility on household decision making: Personal income share

	(1)	(2)	(3)	(4)
	Dependent variable: Primary decision maker for day to day purchases			
	Women		*Men*	
Pension eligible	0.142**	0.0884	-0.0477	-0.0499
	[0.0565]	[0.0543]	[0.0905]	[0.0891]
Presence of man (woman) 50+	-0.551***	-0.488***	-0.253***	-0.215***
	[0.0362]	[0.0372]	[0.0446]	[0.0469]
Eligible man (woman)	-0.0225	-0.00521	-0.0212	-0.00519
	[0.0521]	[0.0502]	[0.0540]	[0.0534]
Personal income share		0.00337***		0.00193***
		[0.000472]		[0.000612]
Observations	1,750	1,749	1,092	1,092
R-squared	0.334	0.365	0.182	0.196
Sample mean	0.64		0.58	
Control variables	YES	YES	YES	YES
Cubic in age of person	YES	YES	YES	YES

Notes: Robust standard errors in brackets are clustered at the household level. Regressions are weighted with survey post-stratification weights. Sample is restricted to men and women aged 50 to 75 with non missing employment data. Control variables are number of household members who are 0 -5, 6 -14, 15 - 24, and 25 - 49, educational attainment category, and rural/urban status. Personal income share is the percent of household income attributed to the individual.
*** p<0.01, ** p<0.05, * p<0.1

Table 2.A.2: Effect of pension eligibility on household decision making: Households with young children

	(1)	(2)	(4)	(5)
	Dependent variable: Primary decision-maker for day-to-day purchases			
	Women		*Men*	
Pension eligible	0.137	0.120	0.169	0.0171
	[0.0858]	[0.0867]	[0.170]	[0.152]
Presence of man (woman) 50+	-0.554***	-0.548***	-0.0683	-0.152**
	[0.0557]	[0.0565]	[0.0822]	[0.0711]
Eligible man (woman)	0.0195	0.0200	-0.00884	0.00360
	[0.0726]	[0.0744]	[0.0839]	[0.0770]
Observations	735	735	376	376
R-squared	0.287	0.307	0.032	0.169
Sample mean	0.60		0.48	
Control variables	NO	YES	NO	YES
Cubic in age of person	YES	YES	YES	YES

Notes: Robust standard errors in brackets are clustered at the household level. Regressions are weighted with survey post-stratification weights. Sample is restricted to black men and women aged 50-75 who live with a child 60 months old or younger. Control variables are number of household members who are 0-5, 6-14, 15-24, and 25-49, educational attainment category, and rural/urban status.

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

CHAPTER III

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

3.1 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, 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.[1] Subsidies for private education are another approach, of which the Colombian voucher program is an example (Angrist et al. 2002). In this paper we also study an educational subsidy program in a developing country, but one with novel features compared to programs that have been subjects of previous research.

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

[1] 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.

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).[2] 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.[3]

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 NGO 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.[4] 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

[2] 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).

[3] 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.

[4] "Remesa" is the Spanish word for "remittance." El Salvador uses the US dollar as its national currency.

funds.[5] 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.[6,7]

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.[8] 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.[9]

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,

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[5] 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.

[6] 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.

[7] 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 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).

[8] This was done prior to treatment so that choice of target students was not influenced by treatment status.

[9] 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.

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, these funds appear to be stimulating 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 3.7 (each dollar of EduRemesa funds leads to $3.7 in additional spending). Crowd in is driven entirely by females: the crowd-in ratio for female target students is 5.0 vs. only 1.7 for males.[10]

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 find 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. 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

[10] 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.

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 household expenditure categories, borrowing, other transfer receipts, and increases in earnings on the part of others in the household.[11]

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 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.[12] In contrast to these studies, ours is the first to find evidence of a crowd in of household resources in response to a transfer.

While existing analyses of conditional cash transfer programs have not examined impacts on household education expenditures,[13] 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 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 Progresa program 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 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.[14]

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

[11] 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.
[12] 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.
[13] 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).
[14] 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).

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.

3.2 Theory

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).[15]

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

[15] In a related paper, Peltzman (1973) shows 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 in-kind and not valid at private institutions.

choosing a "quality level" of schooling *E*.[16] 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 3.1 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, ΔY. 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 3.2 illustrates the case of crowd in. All elements of the figure are identical to those in Figure 3.1, 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 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 3.3. The budget constraint is partitioned into two parts, with a void in

[16] 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.

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 3.4 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, ΔY (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.

3.3 Project description

3.3.1 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. Table 3.A.1 shows figures from the 2010 *Encuesta de Hogares de Propositos Multiples* (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. Table 3.A.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.

3.3.2 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.[17] While waiting for consular services, migrants

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

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.[18] 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.[19] 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 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

[18] Relatives were defined as “close family members” or children, siblings, nieces and nephews, grandchildren, and cousins.
[19] Target students were not required to be currently enrolled in school.

treatment groups were offered the EduRemesa with a subsidy level corresponding to their treatment group.[20]

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.

3.3.3 Details of EduRemesa treatments

We partnered with the Fundación Empresarial para el Desarrollo Educativo (FEPADE),[21] 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 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

[20] 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 we do not consider these phone El Salvador surveys to be "baseline" data.
[21] In English, "Business Foundation for Educational Development."

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 3.5. The following is a brief description of the information provided to the different groups.[22]

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 with the informational pamphlet that informed them of this.[23] 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

[22] 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.
[23] FEPADE charges administrative fees of 15% of the total EduRemesa amount.

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.[24] 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 3.1.

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

[24] 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.

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. 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.[25]

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 due solely to the funds provided, or whether improved monitoring of student grades may be an additional mechanism.[26]

3.4 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 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

[25] In four cases, FEPADE suspended monthly transfers to EduRemesa recipients who had stopped attending school.
[26] 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.

(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.[27] Outcomes related to educational expenditures and remittances 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 for all variables and imputation procedures can be found in Appendix C.

Table 3.2 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 do remit to the target households.[28] 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 Table 3.A.3A. Tables 3.A.3B and 3.A.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 3.3 examines balance across the treatment groups in the El Salvador follow-up sample using the same variables reported in Table 3.2. Table 3.A.4A examines balance by gender of the target student and Tables 3.A.4B and 3.A.4C examine balance in the full sample

[27] 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.

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

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 .1 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. Table 3.A.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.[29]

3.5 Empirical results

3.5.1 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:

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

where *i* indexes each migrant-student pair, j indexes the location of the initial interaction, 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. 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

[29] 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.

on the outcomes of interest. X_{jt} is the set of 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.

3.5.2 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 3.4 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. Not a single migrant 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).[30]

Table 3.5 estimates the impact of the treatments on take-up using equation (1). The results shown in Table 3.5 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 Tables 3.A.6A and 3.A.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 in the 1:1 match group 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).

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

[30] 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.

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 student when that student is female.[31]

3.5.3 Impact on educational expenditures

We now turn to the principal question of the paper: how did the EduRemesa affect the education spending of those that received it? 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. Because 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 3.6 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.[32] The main result in Table 3.6 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, due to lower take-up there is a smaller increase of $75 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

[31] In column 7 of Table 3.5 (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.

[32] These amounts are reported by the target student. When the target student's report is missing, the responsible adult's report is used. In order to keep the sample consistent across columns, in the few cases where both are missing the expenditures are imputed. Further details on variable construction can be found in Appendix C and all results are robust to the exclusion of the observations with imputed values.

there was no 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.[33] 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 lower in magnitude. 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 3.6, 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 below that of 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.

These results on target student education expenditures suggest that, at least for female target students, the EduRemesa is being successfully targeted towards their education. However, 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 3.6 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

[33] 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.

the household.[34] The impact of the match treatment on total household educational expenditures is presented in Table 3.7. The set-up of the table is parallel to Table 3.6, 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 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 3.6 and 3.7 reveal that the 3:1 match treatment increases target student education expenditures. Following the model presented in Section 2, we now turn to asking whether the entire EduRemesa "sticks" to education, or whether some of it is shifted to other purposes. 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 receipt of EduRemesa funds of $85 overall and $108 among females resulting from that treatment (Table 3.5, 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 3.8 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. Panels 3 and 4 contain results to be discussed later in this section. Column 1

[34] 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. Variable construction is described in Appendix C and all results are robust to the exclusion of imputed values.

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 the sample of female target students.

The estimated coefficient in panel 2 reveals the impact of each dollar of EduRemesa funds on target student educational expenditures. As such, 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.”

3.5.4 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 3.4). We now turn to the impacts of this spending on other education-related outcomes. First, in Table 3.9 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 22 percentage points more likely to be in private school, and those in the 1:1 match group are 13 percentage points more likely. These are large increases relative to the control group private school attendance rate of 28 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 3.10 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 3.7 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 above 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.[35] 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 3.8. 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 a crude 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 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 get an understanding of the impact of the EduRemesa on total resources devoted to target student education.[36] This is shown in panel 4 of Table 3.8 where the dependent variable is total target student education expenditures minus 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.

[35] For the 1:1 match treatment there are impacts only for female target students, however because takeup 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.

[36] Of course there may be other resources that we do not measure that are also being affected.

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 contribution to the target student's education.[37]

3.5.5 Impact on remittances

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 3.11 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).[38] 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).[39, 40]

There is no evidence in Table 3.11 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

[37] It should be noted that these estimates are conservative in that they place no value on unpaid work.

[38] 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 C.

[39] 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).

[40] All the previously-reported results in Tables 3.6 and 3.7 relating to education expenditures are robust to trimming of the top 1% and the hyperbolic sine transformation.

positive crowd-in ratios therefore do not appear to be funded via additional inflows of funds from migrants.

3.6 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

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.[41]

[41] 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.

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 not zero 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 3.5, 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 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 3.6 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.

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.

Table 3.A.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 (Table 3.A.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.

3.7 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 $800 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 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*).

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.

Figure 3.1: Standard budget constraint, crowd-out

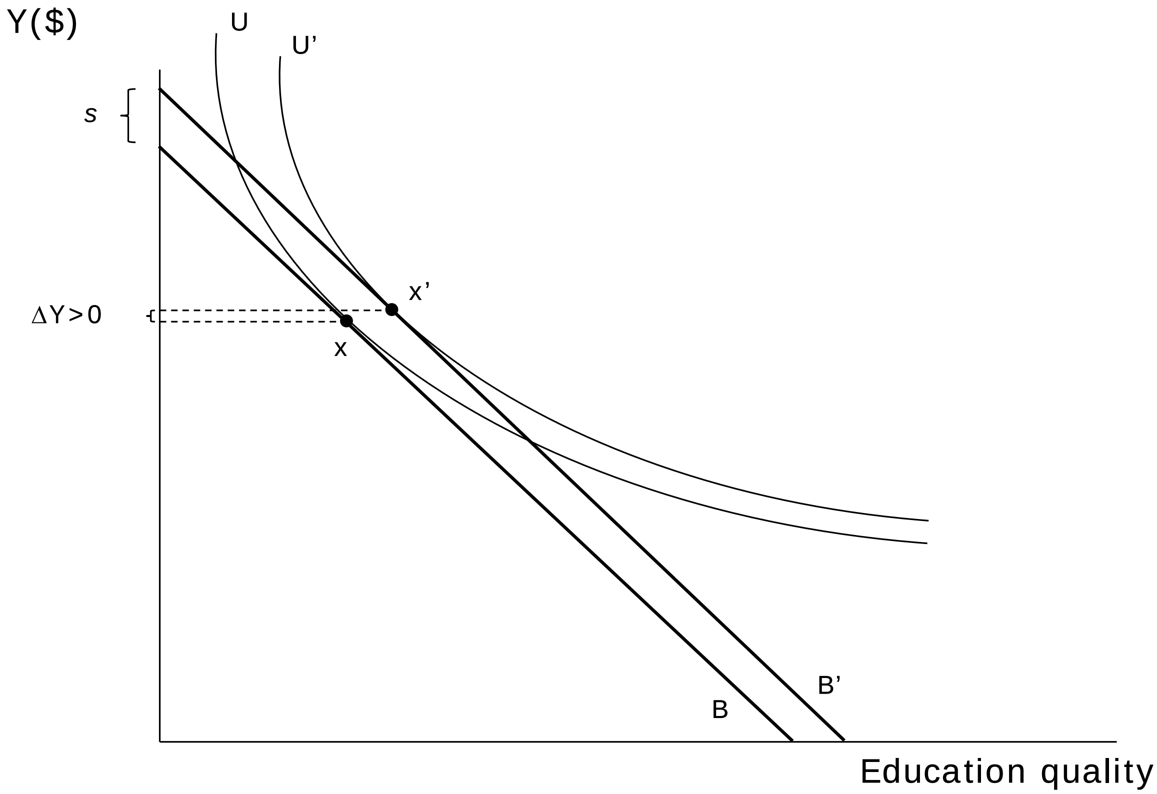

Figure 3.2: Standard budget constraint, crowd-in

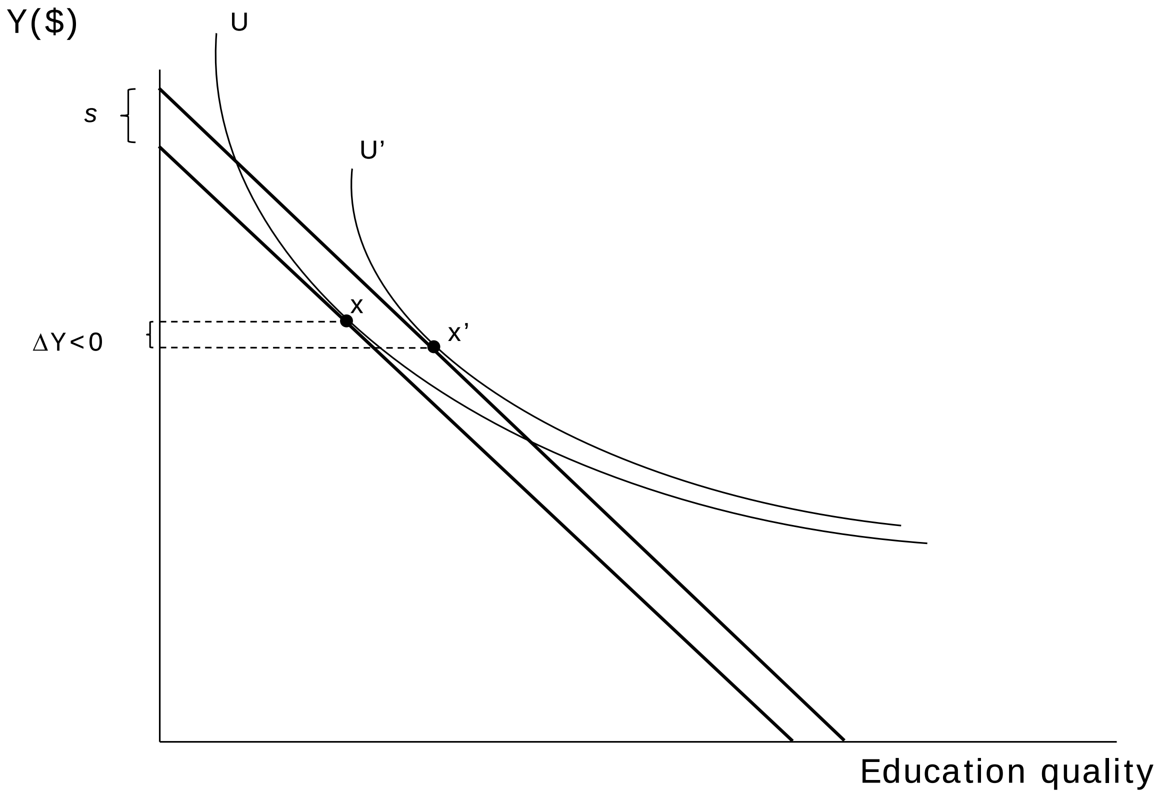

Figure 3.3: Budget constraint when intermediate quality levels are unavailable

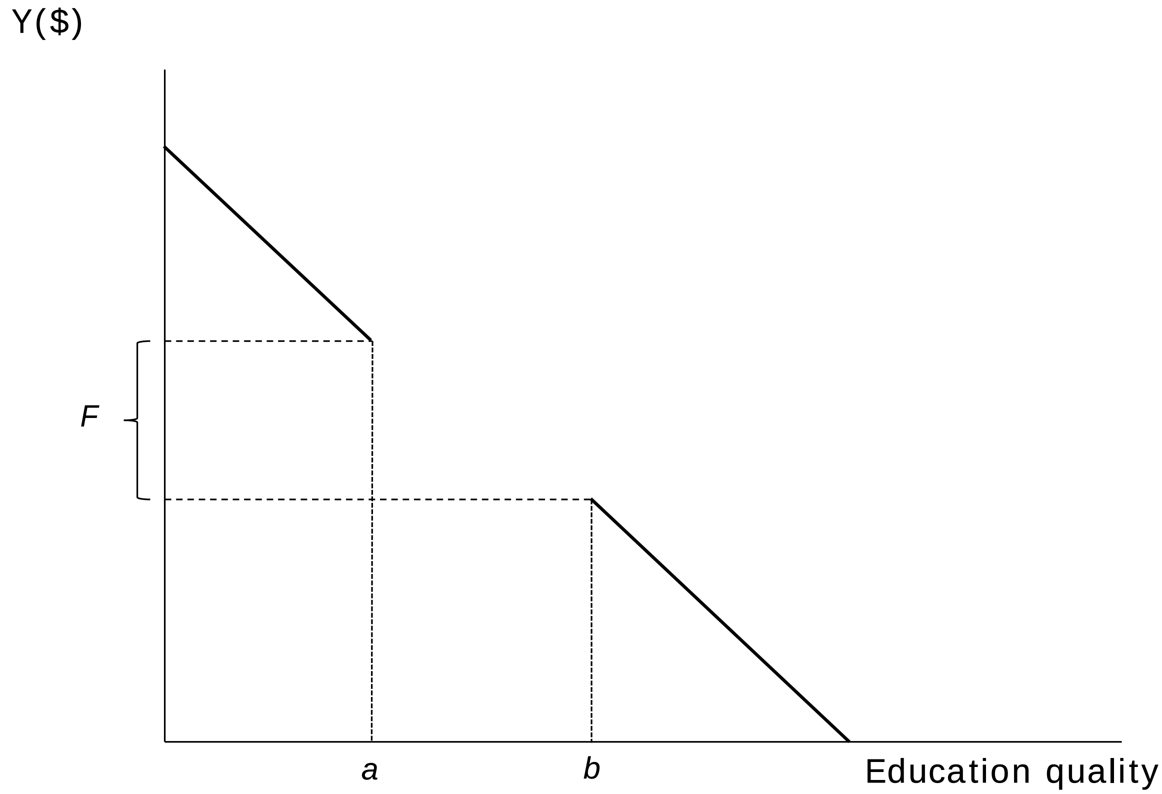

Figure 3.4: Impact of increased funds when intermediate quality levels are unavailable

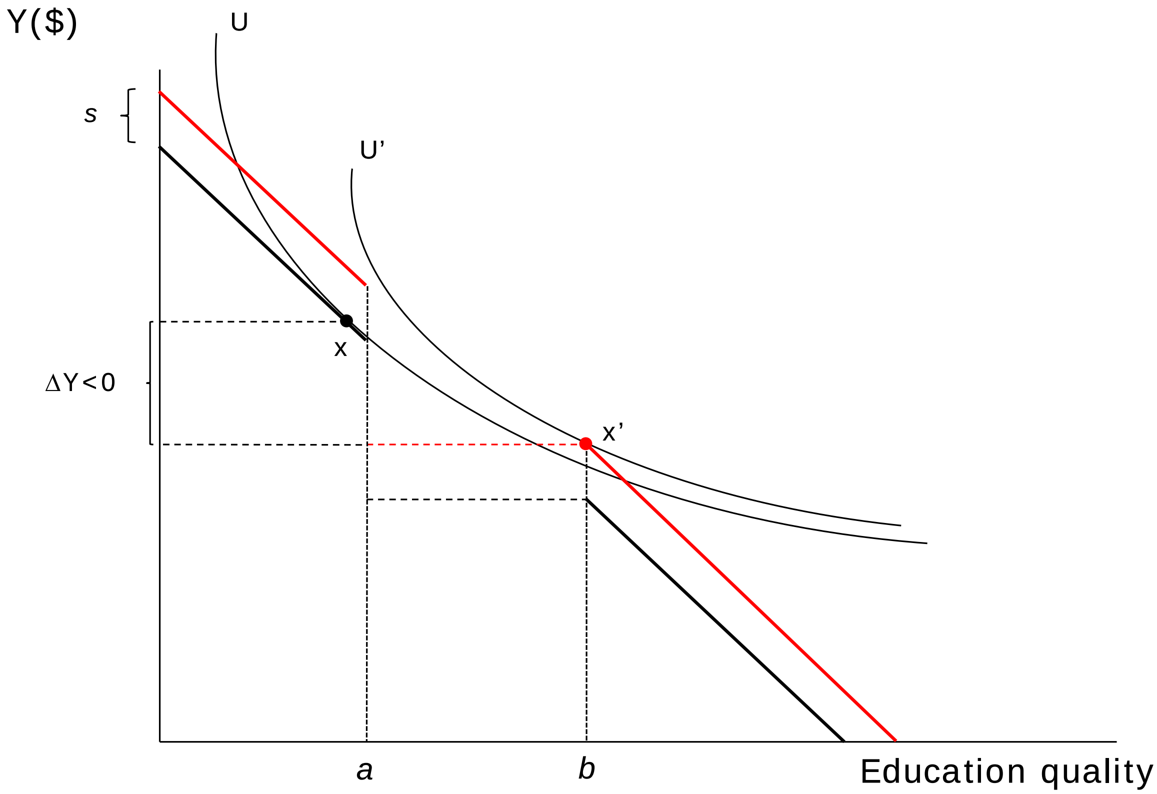

Figure 3.5: Treatment groups

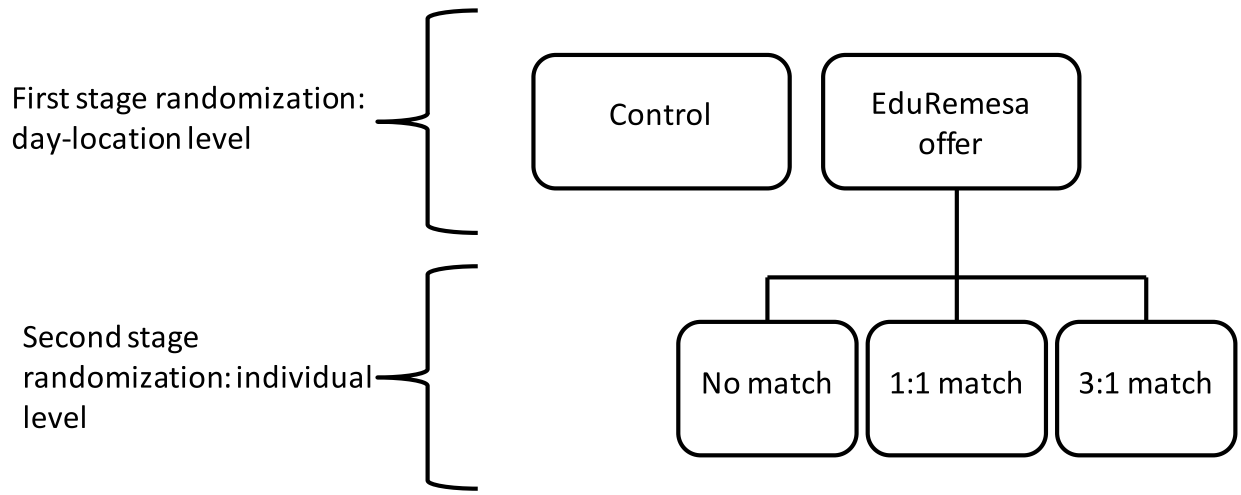

Figure 3.6: Cumulative distribution functions of total target student education expenditure

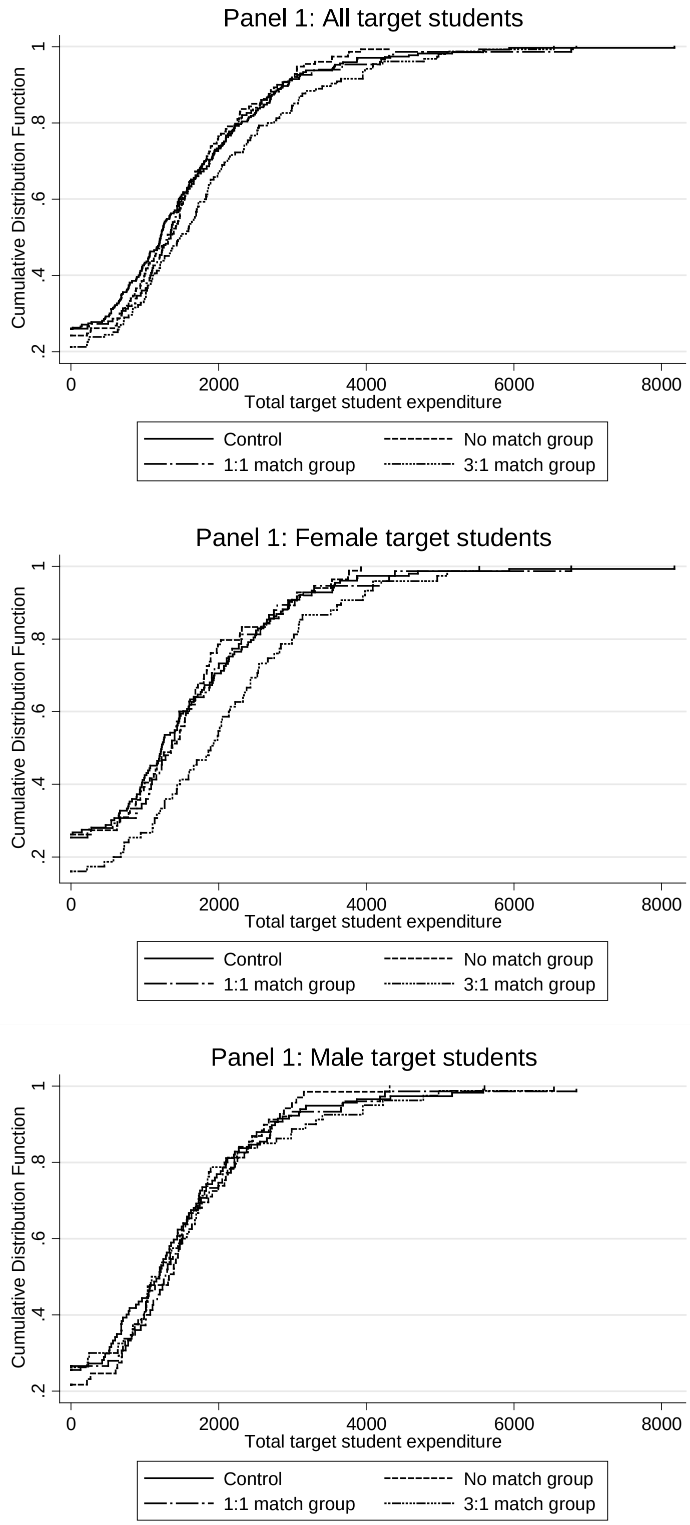

Figure 3.7: Cumulative distribution functions of total target student hours worked

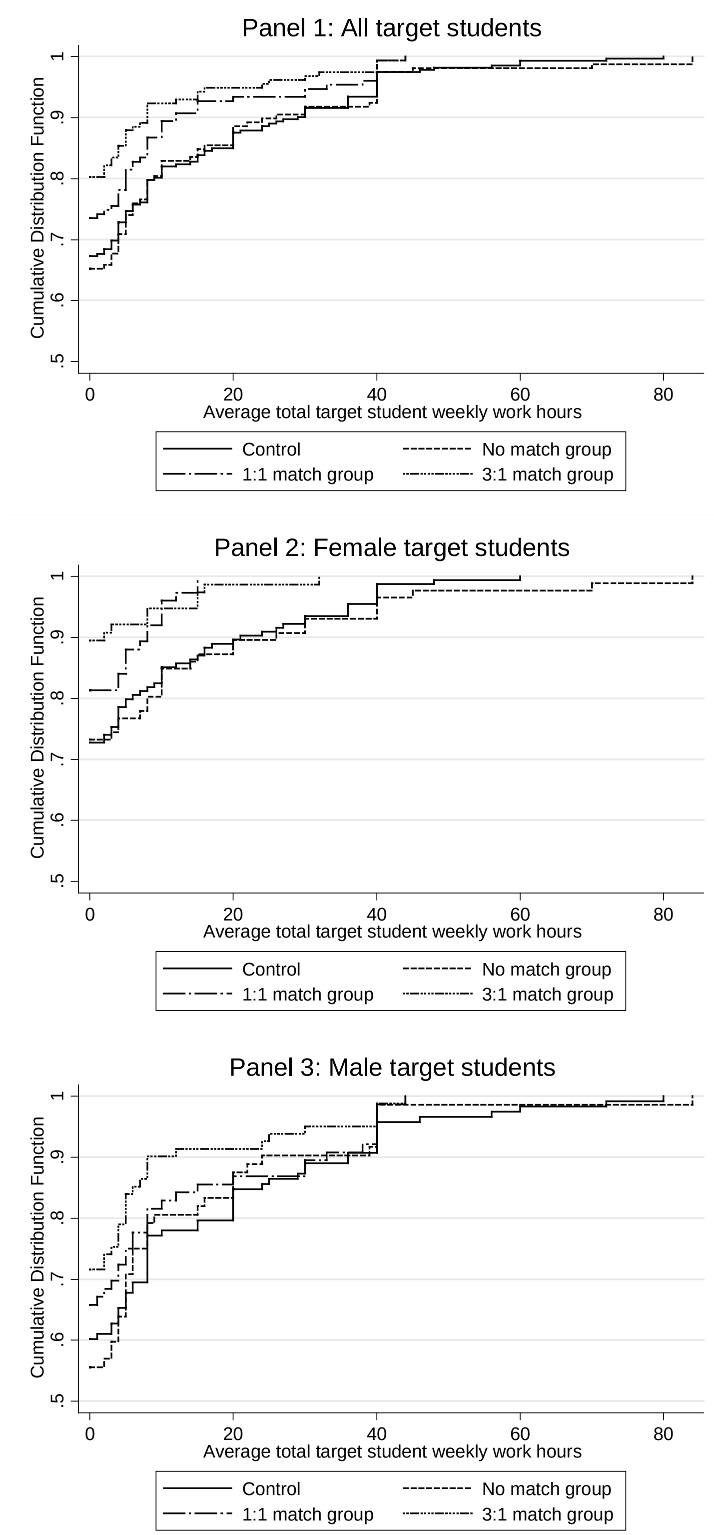

Table 3.1: EduRemesa amounts and migrant contributions by treatment group

			Treatment groups		
			No match	**1:1 match**	**3:1 match**
EduRemesa amounts	**Secondary**	**$300**	$300	$150	$75
		$500	$500	$250	$125
	Tertiary	**$600**	$600	$300	$150
		$800	$800	$400	$200

Table 3.2: Baseline summary statistics

Variable	Mean	Std. Dev.	Min	10th pct.	Median	90th pct.	Max	N
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
Target student is migrant's…								
…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 slighly with missing values for each variable.

Table 3.3: Baseline balance

	Means				*P-values*				
	Control	No match	1:1 match	3:1 match	C = NM = 1:1 = 3:1	C = NM	C = 1:1	C = 3:1	*N*
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
Target student is migrant's…									
…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 3.4: 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 3.5: 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
Panel 1: All target students							
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 coeffcients							
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
Panel 2: Female target students							
3:1 match	0.178***	0.233***	35.82***	141.2***	0.178***	27.60***	108.4***
	[0.0464]	[0.0629]	[9.666]	[38.26]	[0.0464]	[6.985]	[27.21]
1:1 match	0.101***	0.111***	31.13***	67.51***	0.101***	29.23***	59.89***
	[0.0346]	[0.0372]	[10.79]	[23.67]	[0.0346]	[10.56]	[21.93]
No match	0.00990	0.0176	4.590	13.23	0.00990	3.150	7.475
	[0.0136]	[0.0185]	[3.966]	[12.25]	[0.0136]	[3.239]	[8.565]
P-values for tests of equality of coeffcients							
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
Panel 3: Male target students							
3:1 match	0.180***	0.246***	30.91***	127.0***	0.115***	13.57***	55.96***
	[0.0435]	[0.0724]	[9.659]	[37.66]	[0.0366]	[4.848]	[18.83]
1:1 match	0.0281	0.0508	13.59	29.36	0.00842	5.466	7.756
	[0.0230]	[0.0364]	[8.474]	[20.01]	[0.0184]	[4.737]	[10.88]
No match	-0.0123	-0.0106	-2.886	-6.829	-0.0129	-2.389	-7.167
	[0.0162]	[0.0225]	[4.292]	[13.15]	[0.0143]	[2.518]	[7.916]
P-values for tests of equality of coeffcients							
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 3.6: Target student education expenditures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Dependent variable: Annualized target student expenditure on								
	Total	Tuition	School supplies	Uniforms	Books	Transport	Food	Computer use	Other
Panel 1: All target students									
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]
P-values for tests of equality of coeffcients									
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
Panel 2: Female target students									
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]
P-values for tests of equality of coeffcients									
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
Panel 3: Male target students									
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]
P-values for tests of equality of coeffcients									
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 3.7: Total household education expenditures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Dependent variable: Annualized target student expenditure on								
	Total	Tuition	School supplies	Uniforms	Books	Transport	Food	Computer use	Other
Panel 1: All target students									
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 coeffcients									
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
Panel 2: Female target students									
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]
P-values for tests of equality of coeffcients									
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
Panel 3: Male target students									
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]
P-values for tests of equality of coeffcients									
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 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 3.8: Instrumental variables regressions

	(1)	(2)	(3)
	Full El Salvador follow-up sample	Female target students	Male target students
Panel 1: First stage: Dependent variable is total target student EduRemesa funds			
3:1 match	85.34***	108.5***	51.70***
	[16.08]	[27.90]	[18.37]
F-statistic on first stage	28.17	15.12	7.92
Observations	425	228	197
Panel 2: IV: Dependent variable is total target student annualized education expenditures			
Total target student EduRemesa funds	3.720**	4.989**	1.730
	[1.647]	[2.035]	[3.424]
P-value for equality of coefficient to 1	0.099	0.050	0.831
Observations	425	228	197
Panel 3: IV: Dependent variable is estimated target student annualized earnings			
Total target student EduRemesa funds	-1.661***	-0.861**	-3.080
	[0.582]	[0.429]	[2.004]
Observations	425	228	197
Panel 4: IV: Dependent variable is target student expenditures minus estimated earnings			
Total target student EduRemesa funds	5.381***	5.850***	4.811
	[1.946]	[2.257]	[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 3.9: 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
Panel 1: All target students				
3:1 match	0.0309	0.145***	0.0504	0.0942**
	[0.0398]	[0.0419]	[0.0396]	[0.0370]
1:1 match	-0.0210	0.0599	-0.00488	0.0647*
	[0.0381]	[0.0426]	[0.0370]	[0.0357]
No match	0.0182	0.0922*	0.0361	0.0561
	[0.0440]	[0.0480]	[0.0379]	[0.0396]
P-values for tests of equality of coeffcients				
3:1 = 1:1	0.244	0.107	0.260	0.554
3:1 = No match	0.819	0.400	0.778	0.478
1:1 = No match	0.426	0.550	0.373	0.857
3:1 = 1:1 = No match	0.453	0.271	0.477	0.758
Observations	728	728	728	728
R-squared	0.048	0.041	0.036	0.041
Control group mean	0.741	0.300	0.185	0.115
Panel 2: Female target students				
3:1 match	0.0836	0.223***	0.0485	0.174***
	[0.0599]	[0.0665]	[0.0570]	[0.0611]
1:1 match	-0.0166	0.128**	0.0370	0.0911*
	[0.0691]	[0.0636]	[0.0556]	[0.0514]
No match	-0.00889	0.0698	-0.0112	0.0810
	[0.0628]	[0.0662]	[0.0507]	[0.0563]
P-values for tests of equality of coeffcients				
3:1 = 1:1	0.189	0.277	0.877	0.280
3:1 = No match	0.220	0.0821	0.375	0.237
1:1 = No match	0.920	0.457	0.433	0.883
3:1 = 1:1 = No match	0.335	0.219	0.588	0.448
Observations	387	387	387	387
R-squared	0.082	0.076	0.060	0.092
Control group mean	0.739	0.281	0.176	0.105
Panel 3: Male target students				
3:1 match	-0.0595	0.0485	0.0516	-0.00308
	[0.0681]	[0.0656]	[0.0633]	[0.0513]
1:1 match	-0.0536	-0.0204	-0.0498	0.0293
	[0.0587]	[0.0688]	[0.0553]	[0.0592]
No match	0.0115	0.0955	0.0886	0.00692
	[0.0709]	[0.0727]	[0.0656]	[0.0550]
P-values for tests of equality of coeffcients				
3:1 = 1:1	0.934	0.358	0.156	0.627
3:1 = No match	0.385	0.542	0.642	0.871
1:1 = No match	0.397	0.167	0.0426	0.739
3:1 = 1:1 = No match	0.628	0.375	0.0955	0.886
Observations	341	341	341	341
R-squared	0.109	0.061	0.055	0.076
Control group mean	0.744	0.325	0.197	0.128

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 3.10: Target student labor force outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variables refer to work currently being done by the target student					
	Any work	Average hours per week any work	Paid work	Average hours per week paid work	Unpaid work	Average hours per week unpaid work
Panel 1: All target students						
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 coeffcients						
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
Panel 2: Female target students						
3:1 match	-0.157***	-3.260***	-0.110***	-2.277**	-0.0706*	-0.983*
	[0.0481]	[1.155]	[0.0406]	[0.899]	[0.0370]	[0.521]
1:1 match	-0.0817	-3.275***	-0.0902**	-2.458***	-0.0305	-0.817
	[0.0528]	[1.045]	[0.0397]	[0.871]	[0.0381]	[0.550]
No match	0.00582	1.371	-0.0705	0.652	0.0535	0.718
	[0.0554]	[1.683]	[0.0430]	[1.553]	[0.0411]	[0.678]
P-values for tests of equality of coeffcients						
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
Panel 3: Male target students						
3:1 match	-0.116*	-5.144***	-0.0216	-3.103*	-0.111**	-2.042***
	[0.0701]	[1.866]	[0.0680]	[1.838]	[0.0467]	[0.774]
1:1 match	-0.0441	-2.555	-0.00319	-0.840	-0.0434	-1.714**
	[0.0666]	[2.028]	[0.0625]	[1.955]	[0.0515]	[0.660]
No match	0.0310	-1.852	0.0571	-0.682	-0.0454	-1.169
	[0.0681]	[2.332]	[0.0661]	[2.245]	[0.0592]	[0.968]
P-values for tests of equality of coeffcients						
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 3.11: Remittances sent by migrant

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Dependent variable is migrant report of remittances sent since January 1, 2012								
	Full migrant follow-up sample			*Trimmed top 1% of each column*			*Inverse hyperbolic sine transformation*		
	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
Panel 1: All target students									
3:1 match	-167.9	-74.69	-242.6	-2.336	-71.33	-49.84	-0.124	-0.252	-0.296
	[192.2]	[70.59]	[208.7]	[160.1]	[48.14]	[162.4]	[0.333]	[0.292]	[0.280]
1:1 match	-365.1**	-63.63	-428.8**	-153.1	29.36	-128.5	-0.441	0.132	-0.424
	[180.7]	[66.62]	[189.9]	[152.4]	[60.67]	[160.5]	[0.410]	[0.330]	[0.333]
No match	-482.9***	-141.9**	-624.8***	-213.1	-60.65	-316.6**	-0.271	-0.171	-0.456
	[165.6]	[54.85]	[175.6]	[136.5]	[49.59]	[138.9]	[0.323]	[0.302]	[0.275]
P-values for tests of equality of coeffcients									
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
Panel 2: Female target students									
3:1 match	-59.45	-20.15	-79.60	-12.05	-134.0**	-60.68	0.0210	-0.632	-0.226
	[301.2]	[107.6]	[321.8]	[224.3]	[64.18]	[243.4]	[0.423]	[0.392]	[0.369]
1:1 match	-347.3	-1.052	-348.4	-120.3	32.48	-114.5	-0.508	0.128	-0.600
	[242.3]	[88.84]	[257.0]	[206.7]	[85.13]	[226.4]	[0.538]	[0.431]	[0.490]
No match	-446.5**	-59.51	-506.0**	-213.8	-30.57	-262.9	-0.325	0.106	-0.536
	[210.4]	[74.55]	[223.8]	[172.8]	[70.27]	[191.2]	[0.416]	[0.393]	[0.384]
P-values for tests of equality of coeffcients									
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
Panel 3: Male target students									
3:1 match	-298.4	-130.3	-428.8	3.236	9.058	-24.91	-0.346	0.0502	-0.331
	[239.5]	[103.3]	[259.6]	[193.7]	[75.67]	[195.0]	[0.453]	[0.474]	[0.381]
1:1 match	-390.1	-77.67	-467.8*	-173.0	86.16	-50.97	-0.329	0.324	-0.126
	[254.6]	[102.4]	[264.8]	[191.0]	[84.79]	[216.8]	[0.541]	[0.483]	[0.449]
No match	-528.9**	-211.9***	-740.8***	-151.1	-94.41	-309.2*	-0.246	-0.596	-0.368
	[238.9]	[79.59]	[239.2]	[180.2]	[62.21]	[174.6]	[0.470]	[0.458]	[0.410]
P-values for tests of equality of coeffcients									
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

Table 3.A.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%
Parochial	4.34%	5.31%	5.76%
Other private	6.60%	15.78%	55.32%

Notes: Source is El Salvador Encuesta de Hogares de Propositos Multiples 2010.

Table 3.A.2: Average annual education expenditures (USD), current students

	Secondary		*Tertiary*	
	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.

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

Variable	Mean	Std. Dev.	Min	10th pct.	Median	90th pct.	Max	N
Panel 1: Migrant-student pairs with female target student								
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
Target student is migrant's…								
…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
Panel 2: Migrant-student pairs with male target student								
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
Target student is migrant's…								
…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 slighly with missing values for each variable.

Table 3.A.3B: Baseline summary statistics: Full sample

Variable	Mean	Std. Dev.	Min	10th pct.	Median	90th pct.	Max	N
Panel 1: All migrant-student pairs								
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
Target student is migrant's…								
…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
Panel 2: Migrant-student pairs with female target student								
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
Target student is migrant's…								
…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
Panel 3: Migrant-student pairs with male target student								
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
Target student is migrant's…								
…child	0.26	0.44	0	0	0	1	1	468
…sibling	0.23	0.42	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 slighly with missing values for each variable.

Table 3.A.3C: Baseline summary statistics: Migrant follow-up sample

Variable	Mean	Std. Dev.	Min	10th pct.	Median	90th pct.	Max	N
Panel 1: All migrant-student pairs								
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
Target student is migrant's…								
…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
Panel 2: Migrant-student pairs with female target student								
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
Target student is migrant's…								
…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
Panel 3: Migrant-student pairs with male target student								
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
Target student is migrant's…								
…child	0.27	0.44	0	0	0	1	1	334
…sibling	0.22	0.42	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 slighly with missing values for each variable.

Table 3.A.4A: Baseline balance: El Salvador follow-up sample

	Means				*P-values*				
	Control	No match	1:1 match	3:1 match	C = NM = 1:1 = 3:1	C = NM	C = 1:1	C = 3:1	*N*
Panel 1: Migrant-student pairs with female target student									
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
Target student is migrant's…									
…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
Panel 2: Migrant-student pairs with male target student									
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
Target student is migrant's…									
…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.

Table 3.A.4B: Baseline balance: Full sample

	Means				*P-values*				
	Control	No match	1:1 match	3:1 match	C = NM = 1:1 = 3:1	C = NM	C = 1:1	C = 3:1	*N*
Panel 1: All migrant-student pairs									
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
Target student is migrant's…									
…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
Panel 2: Migrant-student pairs with female target student									
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
Target student is migrant's…									
…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
Panel 3: Migrant-student pairs with male target student									
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
Target student is migrant's…									
…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.

Table 3.A.4C: Baseline balance: Migrant follow-up sample

	Means				*P-values*				
	Control	No match	1:1 match	3:1 match	C = NM = 1:1 = 3:1	C = NM	C = 1:1	C = 3:1	*N*
Panel 1: All migrant-student pairs									
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
Target student is migrant's…									
…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
Panel 2: Migrant-student pairs with female target student									
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
Target student is migrant's…									
…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
Panel 3: Migrant-student pairs with male target student									
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
Target student is migrant's…									
…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.

Table 3.A.5: Attrition

	(1)	(2)	(3)
	El Salvador follow-up complete	Migrant follow-up complete	Both follow-ups complete
Panel 1: All target students			
3:1 match	-0.0345	0.0178	-0.000549
	[0.0355]	[0.0365]	[0.0426]
1:1 match	-0.0240	-0.0459	-0.0577
	[0.0363]	[0.0368]	[0.0422]
No match	-0.0266	-0.00370	-0.0464
	[0.0374]	[0.0390]	[0.0468]
P-values for tests of equality of coeffcients			
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
Panel 2: Female target students			
3:1 match	-0.0160	0.0221	0.00986
	[0.0512]	[0.0490]	[0.0583]
1:1 match	-0.0412	-0.0145	-0.0425
	[0.0535]	[0.0523]	[0.0630]
No match	-0.0649	-0.0297	-0.0882
	[0.0491]	[0.0490]	[0.0555]
P-values for tests of equality of coeffcients			
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
Panel 3: Male target students			
3:1 match	-0.0651	0.0155	-0.0143
	[0.0513]	[0.0445]	[0.0582]
1:1 match	-0.0133	-0.0881*	-0.0877
	[0.0462]	[0.0516]	[0.0535]
No match	0.00733	-0.00127	-0.0245
	[0.0566]	[0.0595]	[0.0688]
P-values for tests of equality of coeffcients			
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

Table 3.A.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
Panel 1: All target students							
3:1 match	0.145***	0.188***	26.24***	105.0***	0.120***	16.92***	67.25***
	[0.0245]	[0.0354]	[4.997]	[19.47]	[0.0216]	[3.131]	[11.96]
1:1 match	0.0520***	0.0633***	17.03***	36.20***	0.0443***	13.55***	26.93***
	[0.0153]	[0.0194]	[5.446]	[11.62]	[0.0144]	[4.508]	[9.207]
No match	-0.000802	0.00213	0.242	1.838	-0.00235	-0.130	-0.380
	[0.00735]	[0.00988]	[1.853]	[5.245]	[0.00704]	[1.414]	[3.701]
P-values for tests of equality of coeffcients							
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
Panel 2: Female target students							
3:1 match	0.164***	0.203***	30.61***	120.0***	0.164***	24.99***	97.47***
	[0.0364]	[0.0471]	[7.112]	[28.19]	[0.0364]	[5.461]	[21.39]
1:1 match	0.0754***	0.0792***	22.29***	46.46**	0.0754***	21.62***	43.77**
	[0.0267]	[0.0284]	[8.296]	[18.01]	[0.0267]	[8.121]	[16.87]
No match	0.00193	0.00498	1.683	5.001	0.00193	1.009	2.305
	[0.0110]	[0.0145]	[2.902]	[8.999]	[0.0110]	[2.413]	[6.613]
P-values for tests of equality of coeffcients							
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
Panel 3: Male target students							
3:1 match	0.121***	0.168***	20.77***	86.07***	0.0771***	8.906***	37.13***
	[0.0300]	[0.0528]	[7.012]	[27.23]	[0.0249]	[3.224]	[12.39]
1:1 match	0.0261	0.0470*	11.21*	25.96*	0.0106	4.604	7.761
	[0.0161]	[0.0266]	[6.493]	[14.80]	[0.0126]	[3.619]	[7.801]
No match	-0.00714	-0.00565	-2.380	-3.963	-0.0112	-2.322	-6.331
	[0.0122]	[0.0195]	[3.617]	[10.57]	[0.0116]	[2.050]	[6.109]
P-values for tests of equality of coeffcients							
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

Table 3.A.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
Panel 1: All target students							
3:1 match	0.163***	0.221***	30.41***	122.1***	0.137***	19.08***	76.12***
	[0.0302]	[0.0453]	[6.643]	[25.95]	[0.0267]	[3.912]	[14.85]
1:1 match	0.0718***	0.0923***	25.09***	54.58***	0.0611***	19.25***	37.97***
	[0.0215]	[0.0278]	[7.838]	[16.90]	[0.0202]	[6.371]	[13.12]
No match	-0.00184	0.00417	1.157	4.503	-0.00414	0.104	-0.359
	[0.00997]	[0.0137]	[2.572]	[7.475]	[0.00990]	[1.931]	[5.275]
P-values for tests of equality of coeffcients							
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
Panel 2: Female target students							
3:1 match	0.168***	0.225***	34.15***	135.1***	0.168***	25.31***	99.77***
	[0.0437]	[0.0674]	[11.15]	[44.10]	[0.0437]	[6.642]	[25.69]
1:1 match	0.0947***	0.102***	29.35***	62.64***	0.0947***	27.78***	56.38***
	[0.0332]	[0.0369]	[10.44]	[23.84]	[0.0332]	[10.01]	[20.92]
No match	0.00317	0.0122	3.336	12.14	0.00317	1.423	4.485
	[0.0142]	[0.0220]	[4.418]	[15.14]	[0.0142]	[3.101]	[8.821]
P-values for tests of equality of coeffcients							
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
Panel 3: Male target students							
3:1 match	0.149***	0.209***	25.26***	104.8***	0.102***	11.88***	49.34***
	[0.0394]	[0.0680]	[9.115]	[34.86]	[0.0332]	[4.432]	[16.71]
1:1 match	0.0342	0.0685	17.21	37.38	0.0116	6.734	9.088
	[0.0273]	[0.0442]	[10.94]	[24.61]	[0.0221]	[6.202]	[13.68]
No match	-0.0189	-0.0197	-4.525	-10.60	-0.0258	-4.349	-13.55
	[0.0205]	[0.0310]	[5.819]	[16.60]	[0.0200]	[3.459]	[10.67]
P-values for tests of equality of coeffcients							
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

Table 3.A.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
Panel 1: All target students							
3:1 match & no grades	0.180***	0.227***	31.43***	123.9***	0.138***	20.83***	81.59***
	[0.0444]	[0.0670]	[8.802]	[34.59]	[0.0395]	[5.768]	[22.38]
1:1 match & no grades	0.126***	0.157***	44.33***	95.28***	0.111***	35.10***	71.39***
	[0.0315]	[0.0414]	[12.06]	[24.82]	[0.0309]	[10.00]	[20.10]
No match & no grades	0.00794	0.0139	2.842	9.908	0.00655	1.903	5.431
	[0.0123]	[0.0171]	[3.045]	[9.146]	[0.0116]	[2.197]	[6.058]
3:1 match & grades	0.190***	0.269***	38.55***	155.1***	0.164***	22.27***	89.16***
	[0.0480]	[0.0670]	[9.677]	[39.13]	[0.0415]	[5.631]	[22.64]
1:1 match & grades	0.0100	0.0101	1.620	3.453	0.00887	1.576	2.366
	[0.0140]	[0.0163]	[2.797]	[7.709]	[0.0132]	[2.489]	[5.786]
No match & grades	-0.0109	-0.00508	-1.044	-1.771	-0.00902	-1.356	-3.986
	[0.0115]	[0.0146]	[2.659]	[8.051]	[0.0106]	[2.053]	[5.686]
P-values for tests of equality of coeffcients in no grades and grades treatment:							
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
Panel 2: Female target students							
3:1 match & no grades	0.195***	0.204***	33.94***	132.6***	0.195***	32.38***	126.4***
	[0.0605]	[0.0621]	[10.25]	[40.58]	[0.0605]	[9.991]	[39.55]
1:1 match & no grades	0.176***	0.186***	54.74***	117.8***	0.176***	52.98***	110.8***
	[0.0514]	[0.0519]	[17.21]	[34.61]	[0.0514]	[17.23]	[34.27]
No match & no grades	0.0236	0.0362*	7.986*	24.38*	0.0236	5.865	15.89
	[0.0165]	[0.0201]	[4.340]	[13.25]	[0.0165]	[3.804]	[10.42]
3:1 match & grades	0.160**	0.266**	38.04**	151.1**	0.160**	22.57**	89.21**
	[0.0693]	[0.105]	[14.93]	[60.94]	[0.0693]	[8.709]	[36.07]
1:1 match & grades	0.0228	0.0302	6.016	13.83	0.0228	4.327	7.079
	[0.0320]	[0.0358]	[6.253]	[17.17]	[0.0320]	[5.708]	[14.19]
No match & grades	-0.00589	-0.00273	0.475	0.884	-0.00589	-0.352	-2.427
	[0.0189]	[0.0247]	[4.696]	[15.13]	[0.0189]	[3.875]	[11.27]
P-values for tests of equality of coeffcients in no grades and grades treatment:							
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
Panel 3: Male target students							
3:1 match & no grades	0.155**	0.245**	28.27*	111.7*	0.0733	8.218	31.84
	[0.0621]	[0.122]	[15.18]	[59.70]	[0.0452]	[5.054]	[19.25]
1:1 match & no grades	0.0653	0.115*	31.03*	66.76*	0.0318	14.12	24.06
	[0.0407]	[0.0670]	[17.08]	[37.27]	[0.0340]	[9.705]	[20.93]
No match & no grades	-0.00163	-0.00496	-1.285	-1.062	-0.00811	-1.326	-3.291
	[0.0160]	[0.0255]	[4.636]	[13.55]	[0.0165]	[2.767]	[8.611]
3:1 match & grades	0.202***	0.248***	33.34***	140.6***	0.152***	18.33**	77.28**
	[0.0602]	[0.0784]	[11.37]	[44.97]	[0.0554]	[7.589]	[30.11]
1:1 match & grades	-0.00362	-0.00733	-2.090	-3.431	-0.00960	-1.991	-5.148
	[0.0148]	[0.0238]	[4.265]	[12.99]	[0.0131]	[2.507]	[7.165]
No match & grades	-0.0246	-0.0193	-5.062	-13.57	-0.0150	-3.367	-9.938
	[0.0256]	[0.0328]	[6.227]	[20.87]	[0.0215]	[3.809]	[12.85]
P-values for tests of equality of coeffcients in no grades and grades treatment:							
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

Table 3.A.8: Target student education expenditures: Interactions with grades treatment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
			Dependent variable: Annualized target student expenditure on						
	Total	Tuition	School supplies	Uniforms	Books	Transport	Food	Computer use	Other
Panel 1: All target students									
3:1 match & no grades	347.2**	129.4***	-7.346	14.14*	3.387	103.7*	122.9*	23.76	-42.78
	[171.1]	[44.70]	[9.334]	[7.494]	[9.713]	[53.17]	[71.78]	[38.25]	[35.56]
1:1 match & no grades	2.681	97.33**	-10.08	-5.372	0.968	23.44	0.241	-32.89	-70.96**
	[131.6]	[45.66]	[9.030]	[5.021]	[8.664]	[34.50]	[50.70]	[30.07]	[33.48]
No match & no grades	7.252	44.10	5.482	-6.593	-15.02**	26.30	31.60	-23.15	-55.47*
	[144.5]	[45.52]	[10.37]	[6.037]	[7.232]	[39.73]	[61.11]	[34.91]	[33.37]
3:1 match & grades	256.7	81.95*	0.714	-0.154	11.19	50.39	164.3**	-23.43	-28.25
	[155.5]	[42.05]	[10.62]	[8.128]	[11.17]	[46.87]	[77.21]	[31.67]	[28.12]
1:1 match & grades	147.6	68.14*	-12.26	-12.14*	9.203	48.56	97.82	-27.09	-24.64
	[162.5]	[40.78]	[9.292]	[6.711]	[11.81]	[68.43]	[81.49]	[33.22]	[45.31]
No match & grades	32.07	89.51*	-8.145	-9.059	-6.785	-27.84	43.28	-17.80	-31.09
	[135.4]	[49.21]	[9.239]	[5.762]	[7.916]	[38.37]	[57.01]	[29.97]	[30.16]
P-values for tests of equality of coeffcients in no grades and grades treatment:									
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
Panel 2: Female target students									
3:1 match & no grades	553.1**	248.7***	3.244	20.43	1.299	183.3**	173.3	16.08	-93.25**
	[253.3]	[71.75]	[13.54]	[13.13]	[13.90]	[71.18]	[120.7]	[57.37]	[42.99]
1:1 match & no grades	158.4	162.8**	10.46	-8.952	9.519	94.88*	30.99	-50.02	-91.33
	[206.2]	[68.57]	[19.62]	[6.382]	[16.12]	[52.16]	[83.88]	[48.68]	[63.52]
No match & no grades	43.94	81.13	-2.851	-2.386	-15.54	42.34	74.84	-27.91	-105.7*
	[195.3]	[66.92]	[9.985]	[8.381]	[10.06]	[50.85]	[87.17]	[60.09]	[62.18]
3:1 match & grades	465.0**	152.3*	9.378	4.201	14.34	76.53	263.2**	-30.34	-24.52
	[229.5]	[78.34]	[15.08]	[12.80]	[16.52]	[64.60]	[116.6]	[48.89]	[47.22]
1:1 match & grades	-68.88	34.09	-12.99	-18.80**	-0.538	30.90	52.07	-63.09	-90.52
	[262.3]	[61.09]	[14.92]	[7.194]	[15.58]	[107.9]	[142.7]	[50.72]	[62.19]
No match & grades	-151.5	50.48	-2.066	-11.89*	-9.320	-41.24	11.00	-74.46	-73.99
	[222.0]	[68.22]	[14.85]	[6.264]	[11.29]	[51.80]	[82.86]	[53.18]	[48.70]
P-values for tests of equality of coeffcients in no grades and grades treatment:									
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
Panel 3: Male target students									
3:1 match & no grades	10.74	-37.79	-23.55*	1.890	-2.291	23.45	28.41	16.90	3.732
	[233.3]	[63.29]	[13.94]	[9.389]	[13.73]	[61.29]	[90.38]	[50.43]	[44.30]
1:1 match & no grades	-226.0	11.46	-38.27***	-8.213	-9.525	-40.09	-62.84	-18.13	-60.42
	[193.5]	[67.39]	[9.137]	[8.468]	[9.907]	[52.22]	[75.28]	[37.04]	[39.78]
No match & no grades	-129.1	1.739	3.325	-13.03	-12.89	-10.84	-46.40	-35.52	-15.48
	[223.6]	[61.54]	[17.20]	[7.902]	[10.58]	[65.47]	[83.48]	[34.02]	[36.57]
3:1 match & grades	72.03	25.04	-11.12	-4.316	12.65	16.27	75.33	-17.83	-23.99
	[232.2]	[63.90]	[14.28]	[9.405]	[16.31]	[71.40]	[90.45]	[43.05]	[33.15]
1:1 match & grades	328.8	87.63	-20.33	-7.677	21.20	61.11	135.6	10.14	41.14
	[259.0]	[68.66]	[13.51]	[11.49]	[18.79]	[81.58]	[91.11]	[47.73]	[98.75]
No match & grades	120.0	129.2	-19.52	-5.874	-8.460	-19.57	37.64	16.65	-10.16
	[234.5]	[90.15]	[12.35]	[9.749]	[13.13]	[50.48]	[94.35]	[48.26]	[48.41]
P-values for tests of equality of coeffcients in no grades and grades treatment:									
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

APPENDICES

APPENDIX A

Text used in migrant experiment

To thank you and your family for you participation in this study now we are going to give you the opportunity to participate in two more lotteries. Let me tell you about them.

Question 1:

First, you have the chance to win \$600. You can keep this money or you can choose to send some or all of it to *name of person to be surveyed* in El Salvador. However, you must tell me now how much you want to keep and how much you want to send and if you win the choice you make now will be carried out.

Treatment 0: *Keep in mind that because of the rules of this project we cannot inform* name of person to be surveyed *about what you decide to do with the money. This means that your decision is a secret.* Name of person to be surveyed *will not be told how much you have decided to send and how much you have decided to keep.*

Treatment 1: *Keep in mind that because of the rules of this project we have to inform* name of person to be surveyed *about what you decide to do with the money. This means that your decision is not a secret.* Name of person to be surveyed *will be told how much you have decided to send and how much you have decided to keep.*

Let's make this decision now. You have the following options: *(surveyor shows options to migrant)*

☐	KEEP: \$600	and	SEND: \$0
☐	KEEP: \$500	and	SEND: \$100
☐	KEEP: \$400	and	SEND: \$200
☐	KEEP: \$300	and	SEND: \$300
☐	KEEP: \$200	and	SEND: \$400
☐	KEEP: \$100	and	SEND: \$500
☐	KEEP: \$0	and	SEND: \$600

Question 2:

Now I am going to tell you about a second lottery that is completely different and separate from the first one. Because you have participated in our survey, *name of person to be surveyed* will have the opportunity to win a remittance worth $300 and will need to choose how he/she would like to receive it if he/she wins. He/she cannot pick anything but must choose among the following categories: meals at local restaurants, education related expenses, daily expenses like groceries, and health related expenses**.** He/she can spend it all on one thing or break it up among different things.

Name of person to be surveyed will decide how he/she would like to receive the remittance. However, we would like to know how you would prefer that *name of person to be surveyed* allocate this remittance.

Spending category:	Amount:
1. Meals at local restaurants (ex: Pollo Campero, Burger King)	
2. Education related expenses (ex: supplies, uniforms, books)	
3. Daily expenses like groceries	
4. Health related expenses (ex: medicine, doctor's visits)	
Total (verify adds up to $300):	

APPENDIX B

Text used in recipient experiment

Question 1: Because *name of migrant* participated in our study, you now have the chance to receive a remittance worth $300. Some participants like you will be chosen to receive this remittance. However, this remittance can only be spent on a limited number of things. In order to participate you must tell me now how you would like to allocate the remittance among the following categories, and if you win, you will receive exactly what you have told me that you want. The categories are: meals at local restaurants, education related expenses, daily expenses like groceries, and health related expenses**.** You can spend it all on one thing or break it up among different things.

Treatment 1: *You can choose anything that you like.*

Keep in mind that because of the rules of this project we cannot inform name of migrant *about what you decide to do. This means that your decision is a secret.* Name of migrant *will not be told about what you decide to spend the money on.*

Treatment 2: *When we spoke with* name of migrant *we asked him/her what he/she prefers for you to spend this money on and he/she indicated that he/she would like you to choose ______.   However, you can choose anything that you like.*

Keep in mind that because of the rules of this project we cannot inform name of migrant *about what you decide to do. This means that your decision is a secret.* Name of migrant *will not be told about what you decide to spend the money on.*

Treatment 3: *You can choose anything that you like.*

Keep in mind that because of the rules of this project we have to inform name of migrant *about what you decide to do. This means that your decision is not a secret.* Name of migrant *will be told about exactly what you decided to spend the money on.*

Treatment 4: *When we spoke with* name of migrant *we asked him/her what he/she prefers for you to spend this money on and he/she indicated that he/she would like you to choose ______.   However, you can choose anything that you like.*

Keep in mind that because of the rules of this project we have to inform name of migrant *about what you decide to do. This means that your decision is not a secret.* Name of migrant *will be told about exactly what you decided to spend the money on.*

Let’s make this decision now. How would you like to allocate this remittance among the following categories?

Spending category:	Amount:
1. Meals at local restaurants (ex: Pollo Campero, Burger King)	
2. Education related expenses (ex: supplies, uniforms, books)	
3. Daily expenses like groceries	
4. Health related expenses (ex: medicine, doctor’s visits)	
Total (verify adds up to $300):	

APPENDIX C

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 a 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. This is added to 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. This is added to 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

<u>Target student expenditures on education:</u>

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 are 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%

<u>Total household expenditures on education:</u>

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. For target students not currently in school the question refers to the last school they attended.

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 or public school. For target students not currently in school the question refers to the last school they attended.

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 or public school. For target students not currently in school the question refers to the last school they attended.

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 report 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. This is added to 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%

Remittances 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. This is added to 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%

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

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