

# **Remittances and the Problem of Control: A Field Experiment Among Migrants from El Salvador**

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November 2009

## **Abstract**

While remittance flows to developing countries are very large, it is unknown whether migrants desire more control over the uses to which remittances are put. This research uses a randomized field experiment to investigate the importance of migrant control over the use of remittances. In partnership with a large Salvadoran bank, we offered US-based migrants from El Salvador facilities for channeling remittances into savings accounts in their home country. We randomly varied migrant control over El Salvador-based savings, and found that migrants do seek to control savings accumulation by remittance recipients. Total remittance-recipient savings at the partner bank are 50% higher in the treatment condition where migrants were offered the highest degree of control over savings in El Salvador, compared to a comparison group not offered such control. Rather than saving autonomously in their own separate accounts, migrants exerted enhanced control over savings primarily in joint accounts that migrants and remittance recipients shared. We also document positive externalities of our savings intervention in the form of increased savings at other banks (including banks in the U.S.). This spillover effect likely reflects improvements in migrant financial literacy due to our intervention.

**Keywords:** migration, remittances, intrahousehold allocation, microfinance

**JEL codes:** F22, O16

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## **I. Introduction**

Between 1965 and 2000, individuals living outside their countries of birth grew from 2.2% to 2.9% of world population, reaching a total of 175 million people in the latter year.<sup>1</sup> The remittances that these migrants send to origin countries are an important but relatively poorly understood type of international financial flow. Migrant remittances compare in magnitude to other important financial flows destined for developing countries, such as official development assistance and direct foreign investment. In 2008, migrant remittances sent to developing countries amounted to US\$338 billion (Ratha et al 2009). Improvements in remittance data collection and continued immigration flows to developed countries have generated substantial recent interest in the remittance phenomenon, as evidenced by a proliferation of recent policy-oriented reports.<sup>3</sup>

Recent research in the economics of migration has documented several beneficial impacts of remittance flows on household well-being and investments. Households in the Philippines experiencing exogenous increases in remittances become more likely to leave poverty status, to send their children to school, and to invest in new entrepreneurial enterprises (Yang and Martinez 2005, Yang 2006, Yang 2008b). In El Salvador, households receiving more remittances have higher rates of child schooling (Cox-Edwards and Ureta 2003). In Guatemala, households receiving remittances tend to invest more in education, health and housing (Adams 2005), and international remittances are associated with lower depth and severity of poverty (Adams 2004). In Mexico, households with migrants invest more in small businesses than households without migrants (Woodruff and Zenteno 2007). In addition, remittances appear to serve as insurance, rising in the wake of negative shocks (Yang and Choi 2007, Yang 2008a).

To date, however, we know very little about how migrants make their remittance-sending decisions. In particular, it is unknown whether migrants desire greater control over how family members back home use the remittances they receive. Do migrants and remittance recipients typically agree on the uses to which remittances should be put? If not, are migrants able to control how remittances are spent by recipients? How does lack of control affect the number of

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<sup>1</sup> Estimates of the number of individuals living outside their countries of birth are from United Nations (2002), while data on world population are from U.S. Bureau of the Census (2002).

<sup>3</sup> Reports funded by the Multilateral Investment Fund of the Inter-American Development Bank include Pew Hispanic Center (2002) and Terry and Wilson (2005). The World Bank has also funded substantial publications on the topic, such as World Bank (2006) and World Bank (2007).

people remitted to, the amounts remitted, and the uses to which remittances are put? In the absence of control, do migrants simply send remittances as “gifts” with no attempt to direct their use? If migrants were to be given more control over remittance uses, how would they direct them to be used? A better understanding of these questions could have substantial impact on public policy, by suggesting policies to further stimulate remittance flows and potentially channel them towards more productive uses in migrant source countries.

This paper focuses on better understanding the extent to which remittance flows are reduced by the fact that migrants have no direct control over how remittances are used by recipients. Migrants may have greater preferences for investment and savings than recipient households, but can only imperfectly monitor how remittances are spent. Without direct control over the use of remittances, therefore, migrants may choose to keep their earnings overseas and to remit less. This research aims to shed light on the extent to which migrants’ lack of direct control over the use of remittances affects remittance flows, and on the impact of new financial products that could increase migrant control.

We focus on improving the ability of migrants to ensure that remittances are deposited and accumulated in savings accounts in the home country. In survey data collected as part of this study, U.S.-based migrants from El Salvador report that they would like recipient households to save 21.2% of remittance receipts, while recipient households prefer to save only 2.6% of receipts. Migrants often intend the savings to be for the use of the recipient household in the future, but such savings also can be intended for the migrant’s future use. In the latter case, migrants may send their own funds to be saved in El Salvador because they perceive savings held in the U.S. as relatively insecure (particularly for undocumented migrants who fear deportation and loss of their assets).

We designed a field experiment that offered new facilities for Salvadoran migrants to directly channel some fraction of their remittances into savings accounts in El Salvador. We developed these savings facilities in conjunction with Banco Agricola, El Salvador’s largest bank. To isolate the importance of migrant control over savings, we test demand for different products that offer migrants varying levels of control. For example, we investigate differential demand for savings accounts that must be solely in the name of a remittance recipient in El Salvador, versus accounts that are either jointly owned with the migrant or for which the migrant is the sole owner. In addition to raising savings rates in migrant families, these new facilities

have the potential to stimulate household-level investments (in, for example, education, health, housing, and entrepreneurship) that would be paid for via the accumulated savings.<sup>4</sup>

Impact evaluation uses a treatment-control framework. Migrants in the sample are randomly allocated to be offered one of three different savings facilities, or to a control group that is offered no new savings facilities. An innovative aspect of the project is that baseline and follow-up surveys are administered to both migrants in the U.S. and their corresponding remittance-receiving households in El Salvador.

This research makes new contributions to a large literature in economics on intra-household decision-making. Attempts to understand the extent and nature of conflict between household members are central to research on the economics of the family, in both developed and developing countries. A wide variety of empirical studies have cast serious doubt on the “unitary model” of the household, the proposition that the joint actions of a household comprised of separate optimizing individuals can be represented as the actions of a single utility-maximizing agent.<sup>5</sup>

More recent models therefore take explicit account of potential preference differences among household members. Manser and Brown (1980) and McElroy and Horney (1981) model the allocation of household resources as the solution to a Nash cooperative bargaining problem, where the extent to which an individual’s preferences hold sway depends on his or her “threat point” (utility in the event of household dissolution or divorce). Lundberg and Pollak (1993) assume instead that the threat point is determined by a non-cooperative equilibrium within the household. Browning and Chiappori (1998) make the more minimal assumption that households achieve efficiency of resource allocation; their empirical tests provide evidence in favor of the efficient household model, rejecting the unitary model. However, even the minimal assumption of efficiency has been called into question by Udry (1996), who finds productive inefficiencies in resource allocation across male- and female-controlled farm plots in Burkina Faso, and Dubois and Ligon (2003), who document intra-household allocative inefficiencies in the Philippines.

A leading candidate explanation for observed inefficiencies is asymmetry of information in the household, so that family members cannot monitor each other well enough to enforce

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<sup>4</sup> The impact of our experimental treatments on these later outcomes in recipient households in El Salvador will be the subject of a future companion paper.

<sup>5</sup> See the review in Strauss and Thomas (1995), as well as, more recently, Duflo (2003), Rangel (2006), and Martinez (2006).

mutually-beneficial cooperative agreements. This idea has motivated new research that focuses on households with migrant members, because—due to the absence of the migrant member—these are households where information asymmetries are likely to be particularly pronounced. Overseas migrants may not share the same objectives as family members remaining back home, in particular regarding the use of remittances. For example, migrants may prefer that remittances be saved or invested, while remittance recipients may prefer consumption over investment.<sup>6</sup> When overseas migrants cannot perfectly monitor how recipients use remittances, remittance amounts may be lower than under perfect information. De Laat (2005) shows that male Kenyan migrants spend considerable resources monitoring their rural wives, consistent with the existence of moral hazard in wives' implementation of husbands' remittance instructions. Chen (2006) finds evidence in China that non-cooperative behavior by wives when husbands have migrated is greater for behaviors that are more difficult to monitor.

This research also contributes to our understanding of the use of financial services by poor and particularly immigrant populations in the United States. Existing survey work has documented that Hispanics in the U.S. are less likely to have bank accounts than the native white population.<sup>7</sup> Jankowski, Porter, and Rice (2007) document that demand for US\$100 bills is higher in Chicago zip codes that have higher immigrant (and specifically Hispanic) population shares. They interpret this as evidence that Hispanic immigrants have poor access to formal savings facilities, leading them to resort to cash savings (\$100 bills are preferred for cash savings due to reduced bulkiness). To our knowledge, however, ours is the first study to examine demand for savings facilities by immigrants in the U.S., and in particular how the level of migrant control over such facilities influences demand.

This study assesses whether conflict and information asymmetries in the household lead to lower remittances, and whether innovative financial products—that give migrants direct control over savings—encourage migrants to raise their remittance amounts and lead to higher savings in remittance-receiving households. This is the first randomized field experimental examination of remittance-related financial services. Comparisons across the various treatment conditions will reveal the impact of offering migrant control on account take-up, remittances,

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<sup>6</sup> Ashraf (2009) shows that husbands and wives change whether they choose to consume or save their money when they are being observed by their spouse.

<sup>7</sup> The 2001 National Survey of Latinos finds that, like Latinos in general, Salvadorans in the US tend to be less likely to use financial products than whites or African-Americans. Only 67% of Salvadorans have a bank account, compared with 76% of African-Americans and 95% of whites.

and savings. This research represents a substantial improvement over existing (non-experimental) studies which must infer the existence of control problems indirectly, and where the direction of causation is not known with certainty. The intervention studied is also unusual among development economics field experiments in that it is conducted among migrants who are located in a developed country, while the main outcomes of interest are among individuals who remain behind in a developing country.

Our results provide evidence that a desire for control over remittance uses—in particular, control over the extent to which remittances are saved in formal savings accounts—is quantitatively large and has an important influence on financial decision making by migrants. Across the experimental conditions in our sample, migrants were much more likely to open the savings accounts we offered when given the option of greater control over the accounts.

What's more, when migrants were offered the option of greater control over El Salvador-based savings accounts, they accumulated more savings in them. When migrants were allocated to the treatment condition that offered the greatest degree of migrant control over El Salvador savings, total savings of remittance recipients at our partner bank 6 months after treatment were higher by roughly 50%, compared to a base of roughly \$400 in savings for a comparison group that was offered no new savings facilities.

Follow-up survey data confirms that this effect on savings at our partner bank was a true increase in savings, not simply a shift of pre-existing savings from other banks to our partner bank. In fact, we actually find that our savings intervention had a large positive externality in the form of increased savings at other, non-partner banks. We interpret this as due to the financial advice we offered as part of the treatments: migrants implemented savings strategies suggested by us but using savings facilities at other banks. Savings in the combined trans-national household (migrant plus remittance recipient) roughly double when migrants are offered joint or exclusive control of bank accounts (compared to a base of roughly \$800 in trans-national savings in the comparison group).

The remainder of this paper is organized as follows. Section 2 briefly reviews the potential impacts of increasing migrant control over remitted resources from a theoretical standpoint. Section 3 provides an overview of the study design. Section 4 describes the characteristics of the sample and in particular investigates migrant and recipient expressed

preferences over remittance uses. Sections 5 present the main empirical results. Section 6 provides discussion and interpretation of the results. Section 7 concludes.

## **II. Potential Impacts of Migrant Control**

When thinking about the potential impact of facilities that give migrants more control over savings back home, it makes sense to consider two types of effects: 1) effects on the savings rate in the household (considering the migrant overseas plus the family back home as a single household unit), holding remittance amounts constant, and 2) effects on the amount of remittances sent.

If households and migrants have identical preferences, greater migrant control should not affect the household savings rate, nor should it affect the amount of remittances sent. In this situation, even without the migrant-controlled or -monitored savings facilities, migrants could send resources to households with confidence that households would save the same fraction the migrant would if he or she were at home making the saving decisions. However, if migrants and households have different preferences, then the migrant could respond to this difference by sending fewer remittances.

Consider a simple two-period example where migrants derive utility from their own consumption as well as the utility of family members back home in the current and a future period. Households back home have analogous utility functions, into which enters both the household's consumption and the migrant's utility in current and future periods. Imagine also that savings in the U.S. and savings in the home country are not perfect substitutes, so that there is a reason for migrants to do some saving in the home country. This imperfect substitutability could be arise, for example, if U.S.-based savings are not as easily accessible to family members in the home country, which may be important in situations where emergency funds are needed on short notice due to health or other shocks.

If migrants and family members have similar preferences, then it does not matter whether migrants have the ability to directly control or monitor savings in El Salvador. The migrant can send remittances home and be confident that remittance recipients would save exactly as much as the migrant would have him- or herself.

The interesting case arises when migrants have different preferences than family members back home. Assume, in particular, that migrants have a higher preference for savings than do remittance recipients, perhaps because they discount the future at a lower rate than (are more patient than) family members back home,<sup>9</sup> or because they are more risk-averse and therefore prefer to keep higher precautionary savings. For the moment, take the amount of remittances sent home as constant, so that the only choice variable is the fraction of remittances saved. Then, if migrants have no ability to monitor or control savings in El Salvador, savings in El Salvador will be lower than the migrant would have preferred. By contrast, if migrants are given the ability to directly control the fraction of remittances that are saved, then savings will be higher.

In addition, it is reasonable to expect that the amount of remittances would also change if migrants were given more control over savings back home. Migrants might work more hours, and consume less overseas, so that they could remit more home and directly control the fraction of remittances saved.

All told, then, providing a migrant the direct ability to control savings in the home country can lead both to higher remittances and a greater savings rate in the household. In essence, the migrant enjoys greater utility from remittances when she has control over the fraction saved, and therefore chooses to send more remittances home.<sup>10</sup> Migrants may also raise their earnings and reduce their current consumption overseas if they have access to and control over savings facilities in the home country.

### **III. Study Design**

#### **Focus on El Salvador**

For several reasons, El Salvador is well-suited for this study. It is highly unusual among developing countries in its number of overseas migrants relative to the national population: at least one in seven Salvadorans lives outside of the country, primarily in the United States. Estimated total personal income of Salvadorans living in the United States was \$13.3 billion in

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<sup>9</sup> This assumption is supported by survey evidence (described below) that migrants would prefer a much higher proportion of remittances be saved than do remittance-recipient households.

<sup>10</sup> For this result to hold, it must be true that the migrant applies his or her own discount rate when discounting the household's future utility. An alternative model where the migrant applied the household's (higher) discount rate would not necessarily make such a prediction.

2001, roughly equal to El Salvador's GDP in that year.<sup>11</sup> In 2001, the number of Salvadorans living in the United States was between 0.8 and 1.1 million.<sup>12</sup> Large flows of Salvadorans into the US started with the civil war in 1980, and have continued at a remarkably steady pace since the war ended in 1992. The number of Salvadorans in the US rose substantially from 1990 to 2000 (by anywhere from 68% to 81%, depending on definitions). Concurrent with the expansion of Salvadorans overseas, the dollar value of remittances sent to El Salvador has also grown dramatically, from \$790 million to \$3.8 billion between 1991 and 2008.

### **Treatment Interventions**

We partnered with a financial institution in El Salvador, Banco Agricola, to design the savings facilities used in this project. These savings facilities either did not exist previously (in the case of treatments 2 and 3 below), or migrants in the U.S. did not have the ability to open them from outside El Salvador (in the case of treatment 1). In order to ascertain causal impacts, we randomly assigned clients to one of three treatment groups or a control group.

Migrants in the control group (labeled treatment 0) were not offered any new products. There were three treatment groups, labeled 1, 2, and 3. The presence of the control group allows us to observe remittances or savings for a comparable sample where none of the products were offered. To help track migrants' remittance behavior after the visit, all visited migrants were given a special card (called a "VIP card") that provided a discount for sending remittances via the partner institution's remittance locations in the DC area.<sup>13</sup>

A subset of participating migrants and their household members back home were surveyed at baseline (prior to the product offer). A follow-up round of the survey occurred 12 months after the product offer to test the impact of the product on total remittance receipts, savings rates, the composition of household expenditures, and other outcomes such as household asset ownership, savings, and entrepreneurship. Coinciding with the administration of the follow-up survey, data on savings in the accounts and on remittances were obtained from internal databases of the partner bank, Banco Agricola.

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<sup>11</sup> Estimated total personal income of Salvadoran-born and self-identified Salvadorans in the US from the year 2001 round of U.S. government's Census 2000 Supplementary Survey (C2SS). The data on Salvadorans in the US are from the 2001 round of the U.S. Census 2000 Supplementary Survey, a nationally-representative US household survey covering some 700,000 households.

<sup>12</sup> Depending on the definition of "Salvadoran".

<sup>13</sup> See "Survey and Treatment Protocols" Appendix for further details on the VIP card, its implementation for this study.

*Treatment 0 (control group): Generic encouragement to save*

This is the control condition. Migrants in this condition were visited by a marketer who encouraged them to remit into El Salvador savings accounts. Marketers described the benefits of remitting directly into savings accounts, such as convenience for remittance recipients (recipients could access their funds from ATMs without lining up at a teller window). Migrants were only offered the VIP card, and were not offered any new savings facilities.

A generic encouragement to remit into savings accounts was included in the control condition to ensure that any increases in savings seen in treatments 1, 2, or 3 (vs. corresponding changes in treatment 0) was not due simply to the “pitch” provided by the marketers. Because the “pitch” was provided to migrants in the control condition as well, any increases in savings in treatments 1, 2, or 3 must be due to the offer of the new savings facilities.

*Treatment 1: Remitting into a household member’s account*

In the remaining treatment conditions (1, 2, and 3), marketers also made a generic speech encouraging migrants to save and provided the VIP card. But unlike in treatment 0, in treatments 1, 2, and 3 the marketer then offered to help set up a specific type of savings account into which migrants could remit.

In treatment 1, marketers emphasized that migrants could save by remitting into a bank account in the name of an individual in El Salvador. They then offered to help the individual in El Salvador open an account into which migrants could remit. If migrants were interested, they filled out forms to provide the name, address, and phone number of the individual in El Salvador for whom the account was intended. The marketer offered to let the migrant use a project cell phone to call the person in El Salvador during the visit to inform them of the new account.<sup>14</sup> Within the next few days, project staff arranged by phone for the El Salvador individual to meet with the branch manager of the nearest Banco Agricola branch in El Salvador to complete the final account-opening procedures in person.

Because the account offered in treatment 1 is in the name of someone in El Salvador, takeup and use of this account will reflect the extent to which migrants trust or are satisfied with

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<sup>14</sup> To mitigate any possibility that talking to the primary recipient might have an effect on their savings/remittance sending behavior, migrants assigned to Treatment 0 were also offered a complimentary phone call to the primary recipient from the project cell phone.

the savings preferences of remittance recipients. Migrants may also use a recipient's savings account as a safe and convenient destination for remittances to that recipient. While remittances sent as cash need to be redeemed in cash with a bank teller in a branch, remittances sent into a bank account can be taken out in cash (if at all) at the recipient's convenience and via a wide network of ATMs across the country.

*Treatment 2: Joint account for migrant and household*

In treatment 2, after the initial pitch, marketers offered migrants a new savings facility that was designed for this project, called "Cuenta Unidos". This savings facility is a joint account in the name of the migrant as well as a designated individual in El Salvador. Joint account owners in both the US and El Salvador had ATM cards and full access to account information. Migrants could deposit funds into the account via remittances, and could withdraw using their ATM card via US ATMs. Joint account owners in El Salvador could deposit and withdraw using their ATM cards or via bank tellers.

If migrants were interested in this savings facility, they filled out account-opening forms. As in treatment 1, migrants provided contact information for the joint account holder in El Salvador, and marketers and other project staff facilitated the account opening process on the El Salvador side (by offering the migrant a free call on their project cell phone and arranging the account opening appointment in El Salvador).

Compared to treatment 1, treatment 2 provided the migrant the ability to monitor the savings account of family members, but it did not provide full control over the funds. Indeed, the joint account holder in El Salvador had complete freedom to withdraw the entire savings balance from the account should they choose to do so. Therefore, the difference in takeup and savings growth in treatment 2 vs. treatment 1 reveals the incremental impact of giving migrants better ability to monitor savings balances. In addition, it could also reflect an increase in the migrant's bargaining power over the funds due to the fact that the migrant is a joint owner of the account (unlike in treatment 1).

Treatment 2 was also designed to *nest* treatment 1, in that migrants had the option to *not* have joint ownership of the new account if they so wished. However, in none of the Treatment 2 marketing visits did migrants opt to forego joint ownership and to open an account solely in the name of the remittance recipient in El Salvador.

### *Treatment 3: Exclusive account for migrant*

Treatment 3 nests treatments 2 and 1, while adding an additional savings facility: an account exclusively in the migrant's name, known as "Ahorro Directo". This facility was also newly designed by the project. Ahorro Directo was an account only in the name of the migrant. The migrant could deposit into the account by remitting into it, and received an ATM card for withdrawals at US ATMs.

In the marketing visit for treatment 3, Ahorro Directo and Cuenta Unidos were presented to the migrant simultaneously, and described as complementary products that could be used for different purposes. Ahorro Directo was presented as being useful for funds that the migrant did not want anyone else to access. Cuenta Unidos was described as a facility for funds the migrant wanted to share or co-manage with someone in El Salvador. In addition, migrants were allowed to open an account only in the name of a beneficiary in El Salvador (as in treatment 1).<sup>15</sup>

In Treatment 3, if migrants wanted to open an Ahorro Directo account, we required them to *also* open a Cuenta Unidos account. We instituted this requirement because it was important to keep the account opening or transaction costs as similar as possible across treatments 2 and 3. For the purpose of the study, it is important to be able to interpret any differences in take-up across treatments 2 and 3 as due to increased control over savings on the part of migrants in treatment 3, and not due to a difference in transaction costs. By requiring that migrants wanting an Ahorro Directo also open a Cuenta Unidos, the migrant had to get an individual in El Salvador to physically visit a Banco Agricola branch and fill out account-opening documents. If we had not instituted this requirement, then the transaction cost for opening an Ahorro Directo would have been much lower than for opening a Cuenta Unidos, because the former would not have required a trip by someone in El Salvador to a Banco Agricola branch. The upshot of this design is that take-up of Ahorro Directo in treatment 3 will be a lower bound of what take-up would have been had we not instituted this requirement. We felt that achieving clarity of interpretation was worth the sacrifice of potentially lower take-up in treatment 3.<sup>16</sup>

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<sup>15</sup> Again, though, as in treatment 2, no migrant assigned to treatment 3 who chose to open an account for a remittance recipient opted to forego joint ownership over that account.

<sup>16</sup> Note that in treatment 1, the individual in whose name the account is opened must also go to a branch in El Salvador, so transaction costs are also equalized with treatment 1.

In sum, treatment 3 provides the migrant full ability to control funds in a savings account in El Salvador, unlike treatment 2 where ownership is joint with someone else. Any difference in takeup and savings growth in treatment 3 vs. treatment 2 will reveal the incremental impact of giving migrants the ability to exclusively control their savings balances in El Salvador.

### **Estimation Strategy**

Two types of outcomes are of interest: (1) take-up rates of the new products, and (2) impacts of the treatments on outcomes such as savings, remittances, educational expenditures, housing investments, and the like. This paper focuses on take-up rates, savings, and remittances as dependent variables. Subsequent companion papers will examine other outcomes as well.

To examine take-up of the new products for treated migrants, let  $Y_i$  be the dependent variable of interest (savings, or an indicator variable for account opening). Let  $Z1_i$  be an indicator variable for assignment to Treatment 1,  $Z2_i$  be an indicator variable for assignment to Treatment 2, and  $Z3_i$  be an indicator variable for assignment to Treatment 3.

Estimating the impact of the treatments involves estimating the following regression:

$$Y_i = \delta + \alpha_1 Z1_i + \alpha_2 Z2_i + \alpha_3 Z3_i + \mathbf{X}_i' \phi + \mu_i \quad (1)$$

Coefficients  $\alpha_1$ ,  $\alpha_2$ , and  $\alpha_3$  are the impact on the dependent variable of Treatments 1, 2, and 3 (respectively). The difference  $(\alpha_3 - \alpha_2)$  represents the difference in the impact of Treatment 3 vis-à-vis Treatment 2, and the difference  $(\alpha_2 - \alpha_1)$  represents the difference in the impact of Treatment 2 vis-à-vis Treatment 1.  $\mathbf{X}_i$  is a vector of baseline control variables (savings in August through October 2007, prior to the beginning of treatments in November 2007) and fixed effects.  $\mu_i$  is a mean-zero error term.

If  $\alpha_1$  were positive and statistically significantly different from zero, this would be evidence that the account-opening assistance was effective and that some migrants value remitting into the savings account of someone else in El Salvador. If the differences  $\alpha_3 - \alpha_2$  and  $\alpha_2 - \alpha_1$  are similarly positive and statistically significantly different from zero, this would be evidence in favor of the study's hypothesis that migrants value the ability to monitor or control their savings.

#### **IV. Summary statistics**

The sample consists of migrants from El Salvador who received a marketing visit carried out by a project team member. To screen out individuals who were likely to have relatively weak ties to the home country, enrollment into the study was limited to Salvadorans who had made their first entry into the U.S. within the last 15 years, and who had sent a remittance to someone in El Salvador within the last 12 months. Survey and treatment protocols are described in more detail in the Appendix.

For all 1,451 DC-area migrants who received a marketing visit, baseline data are available on the following variables that were collected at the beginning of the interaction (before any new financial product was offered): gender, whether the individual has a US bank account, relationship to the primary remittance recipient (parent, spouse, child, or other), a categorical variable for years in the U.S. (0-5 years, 6-10 years, 11-15 years), and the migrant's allocation of remittance raffle winnings across various expenditure categories (see below for more detail on the remittance raffle questions).

A subset of migrants who received a marketing visit were recruited beforehand and were administered a comprehensive baseline socioeconomic survey prior to the marketing visit (from 6-12 months before). For these migrants surveyed before the marketing visit, we also sent a survey team to interview the household in El Salvador that the migrant identified as his or her primary remittance recipient. We have comprehensive baseline survey data for 900 of the migrants, and for 741 of the households in El Salvador receiving remittances from these migrants.

We also implemented a follow-up household survey from March through June 2009. Households in El Salvador were interviewed in person by a survey team in El Salvador. Interviews of DC-based migrants conducted via telephone by the same survey team calling from El Salvador. The follow-up survey collected data on savings outside of the partner bank as well as other migrant and household outcomes. We were successful in fielding the follow-up survey to both members of the migrant/remittance-recipient pairs in 652 out of the 1,451 cases. It is for these observations that we are able to examine the impact of treatments on total savings in the

integrated trans-national household consisting of the DC-based migrant and the primary recipient household in El Salvador.<sup>17</sup>

### **Characteristics of migrants and remittance-receiving households**

Summary statistics are presented in Table 1. Migrants in the DC sample are 29% female. Their mean age is 30.9, and mean years in the US is 5.6. 51% of migrants have been in the U.S. for 5 years or less. Only 1% of migrants surveyed are U.S. citizens.

Migrants live in households with a mean of 4.8 individuals. 59% of migrants are married or have an unmarried partner. Of those who are married or partnered, 73% have their partner with them in the US.

Median annual income is \$18,430 for the surveyed migrant, and \$26,840 for the household in which the migrant lives (means are somewhat higher due to large positive outliers). Median annual remittances sent to El Salvador by DC-area migrants is \$3,900. Median remittances as a percentage of migrant household annual income is 16%. Recipient households in El Salvador have median annual income of \$3,540. The most common type of recipient (in terms of familial relationship to migrant) is a parent, accounting for 49% of recipients. Spouses, children, and other relatives account respectively for 14%, 5%, and 32% of recipients.

A relatively high fraction of DC-area migrants have bank accounts, but their savings levels are quite low. 53% of migrants have an account in the US only, 7% have an account in El Salvador only, and 10% have an account in both countries (so overall, 70% of migrants already have an account in either the US or El Salvador, or both). Median savings, however, is just \$750, and median savings as a percentage of annual household income in migrant households is 3.0%.

### **Balance of baseline characteristics across treatment groups**

To confirm that the randomization across treatments achieved the goal of balance in terms of pre-treatment migrant and recipient household characteristics, Table 2 presents the means of a number of baseline variables for each treatment group as reported prior to treatment.

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<sup>17</sup> Regression analysis of attrition patterns indicates that bias due to differential attrition related to treatment status is not a significant worry in this context. Appendix Table 1 examines the impact of treatment status on attrition from the follow-up survey (selection into the 652-observation sample with complete follow-up survey data on the migrant/recipient pair). Treatments 3 and 1 are uncorrelated with attrition in specifications without and with control variables. In the specification without control variables, Treatment 2 is associated with a 6-percentage-point decline in the likelihood of attrition from the sample (which is significant at the 10% level), but significance drops below conventional levels once control variables are included in the regression.

The reported P-values are for F-tests of equality of means across the treatment groups, for each variable separately. The first 9 variables listed in table are stratification variables: migrants were first sorted into 48 cells (based on gender, US bank account, relationship to remittance recipient, and years on US category) before randomization into treatments 0, 1, 2, or 3. By design, the likelihood of statistically significant differences in baseline characteristics arising by chance is minimized for these variables, and this is reflected in the P-values all being very far from conventional significance levels.

The remaining variables in the table are other variables but for which the data were not stratified prior to treatment assignment. With just two exceptions, for all these remaining variables, the P-values are also large and we cannot reject the hypothesis that the means are identical across treatment groups. The variables that are exceptions are migrant's remittances as a share of household income (for which the P-value is 0.057) and recipient household size (P-value 0.032). We do not view these two rejections of the hypothesis of equal means as problematic, since with such a large list of variables it is unsurprising that chance variation would lead a small number to exhibit statistically significant differences across treatment groups.

### **Expressed preferences regarding use of remittances**

The data also provide evidence in support of the hypothesis that migrants have stronger preferences that their remittances be used for savings than do the remittance-receiving households.

One of the main goals of the study is to examine preferences over how remittances should be used, and compare the preferences of remittance senders (the DC-area migrants) with those of the remittance-receiving households in El Salvador. The comprehensive surveys fielded in DC and El Salvador contained a unique module intended to test for such preference differences. A concern we had was that simply asking migrants and households about their preferences over how remittances should be used might not yield useful answers. Their answers might have been automatic, conditioned by what respondents thought was the "right" answer. Or respondents might not have thought carefully (as opposed to as situation where actual money were at stake).

Mindful of these issues, our approach was to tell survey respondents that their household in El Salvador was being entered into a raffle as part of the study. Respondents were told that 10

households in the study would win a prize of \$100. Each migrant was told that if their household in El Salvador won, the migrant would be able to specify exactly how the \$100 prize would be spent. The migrant was given a list of expenditure items, and was asked to divide the \$100 across one or more of these items. The list did not contain a “cash” option (the migrant could not say that some or all of the winnings would be given over in cash). A project representative would personally visit the household to ensure that the \$100 was spent exactly how the migrant specified.

Households in El Salvador were told of this raffle as well, and the household respondent was asked how he or she would like the \$100 to be allocated across the same expenditure categories. They were similarly told that a project representative would enforce that expenditure allocation should the household win the raffle. Households were not told how the DC-area migrant had previously responded to the same question (and survey staff in El Salvador did not have that information).<sup>18</sup>

This set-up gave both migrants and households incentives to answer thoughtfully and truthfully as to how they would prefer the funds to be used, because real money would be at stake if the household won the raffle. We would then expect that differences in preferences between migrants and households over how funds should be used would be reflected in their allocations of the possible \$100 raffle winnings.

Stark differences have indeed emerged between migrants’ and households’ allocations. Figure 1 presents the average breakdown of allocations across 13 expenditure categories for migrants (left-side pie chart) and households (right-side pie chart) for 740 pairs of migrants and households for which corresponding data on these raffle allocations are available, while Table 3 presents the mean allocations and the P-value of the F-test of the equality of the migrant vs. household means. The most obvious difference is that migrants allocate a much smaller amount (\$42.38) to “daily consumption” expenditures than do recipient households, who allocate \$64.82 to daily consumption on average. A large fraction of that difference is accounted for by the fact that migrants allocate \$21.16 to savings, while households allocate just \$2.55 to savings on average. Both these differences are statistically significantly different from zero at the 1% level.

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<sup>18</sup> The raffle was held at the beginning of 2009. 10 El Salvador households were chosen at random to win the \$100. In half of these the migrant’s expenditure allocation was implemented, and in the other half the household’s expenditure allocation was implemented by one of our research assistants at the time the prizes were given out.

The pattern is suggestive that migrants have substantially stronger preferences for savings than do recipient households.<sup>19</sup>

Some of the less-important expenditure categories also reveal differences between migrants and households. The categories where the differences are statistically significantly different from zero (at the 5% level) are as follows: phone bills (migrants \$1.46, households \$0.47), durable goods (migrants \$4.68, households \$0.66), and “other” (migrants \$1.20, households \$5.05). The higher allocation by migrants to phone expenditures may reflect a greater desire on the part of migrants to maintain connections with their families back home.

## **V. Impact of Treatments on Savings Account Opening, Savings Balances, and Remittances**

In this section we examine the impact of the treatments on account opening and on savings at the partner bank as well as savings held at other institutions or in cash. Data on savings at the partner bank are available for the full sample, while data on savings held elsewhere are only available for a restricted sample of migrant/remittance-recipient pairs that both completed the follow-up survey. However, examining savings held outside of the partner bank is important to shed light on whether increases in savings held at the partner bank are simply shifted over from other types of savings (and therefore might not represent true increases in savings). What’s more, examining savings held outside of the partner bank can possibly reveal any positive spillovers into other types of savings from our treatments.

We also examine impacts of the treatments on remittances to see whether any increases in savings are due to increases in remittance flows from migrants to recipient households or just increases in savings out of existing flows.

### **Impact on account opening**

We first estimate equation (1) examining the impact of the various treatment conditions on take-up of savings accounts. The basic equation regresses an indicator for the existence of a certain type of account 6 months after treatment on indicators for being assigned to each of

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<sup>19</sup> Another possibility is that migrants did not know exactly what remittance recipients in El Salvador would want to spend the raffle winnings, and so chose “savings” to give remittance recipients flexibility in how they ultimately spent the funds. El Salvador remittance recipients, on the other hand, had better information on their own spending needs and therefore found it less necessary to allocate raffle winnings to an open-ended, flexible category such as savings.

treatment conditions 1 through 3. The data on existence of these accounts come from our partner bank's internal databases. We focus on accounts of the type that were established by this research project (which we call "project accounts"). These accounts did not exist before and were allocated particular internal tracking codes by our partner bank. We examine three types of project accounts separately: 1) accounts in the name of recipients, which includes Cuenta Unidos accounts opened jointly with migrants in Treatment 2 as well as accounts opened in Treatment 1 solely in the name of recipients, 2) accounts in the name of migrants only (Ahorro Directo), and 3) accounts opened in the name of individuals in El Salvador *other than* the primary remittance recipient (which was allowed by our marketing staff to offer migrants maximum flexibility in how they used the accounts).

Coefficient estimates for this regression equation are reported in column 1 of Table 4. The coefficient of 0.035 on the constant term indicates that 3.5% of primary remittance recipients in El Salvador whose DC-based migrant was assigned to treatment 0 (the control group) had a project account at Banco Agricola 6 months after treatment. Individuals in the control group could have obtained one of the project accounts if they learned about their existence independently of our marketing team, or if they were offered this account independently by Banco Agricola personnel in El Salvador.

The coefficients in column 1 on Treatments 1, 2, and 3 are all positive in sign, and are all statistically significantly different from zero at the 1% level. The coefficients indicate that recipients in Treatments 1, 2, and 3 were respectively 10.3, 13.6, and 17.5 percentage points more likely to have project accounts at Banco Agricola. These coefficients are very similar in column 2 of the table when the controls for controls for pre-treatment savings as well as fixed effects for marketer, treatment month, stratification cell are added to the regression.

Regressions in columns 3 and 4 of the table are similar, except that the dependent variable is replaced with an indicator for the DC-based migrant opening a project account solely for him- or herself (Ahorro Directo). The statistically constant term in column 3 indicates that 5.7% of migrants in the control group were able to open such accounts independently of the assistance of our marketing staff. The proportion is similar among migrants in Treatments 1 and 2: the coefficients on the indicators for those treatments are small and not statistically different from zero. The coefficient on Treatment 3, on the other hand, is large and statistically significant

at the 1% level, indicating that migrants in that treatment condition were 22.7 percentage points more likely to open an Ahorro Directo account than those in the control group.

Finally, columns 5 and 6 replace the dependent variable with an indicator for the migrant opening a project account for a person in El Salvador other than the primary remittance recipient. This did not happen at all in the control group (the coefficient on the constant term in column 5 is zero), but did occur to some extent in other treatment conditions: all treatment coefficients are positive, and are consistently statistically significant at the 1% level (and similar in magnitude) for Treatments 2 and 3. Coefficients on Treatments 2 and 3 in column 6 indicate that those treatments led roughly 4.5 percent of migrants to open accounts for individuals other than the primary remittance recipient.

These account opening patterns provide suggestive evidence in support of the hypothesis that migrants value the additional control over savings accounts afforded by the Treatment 3, compared to the degree of control offered in Treatments 2 and 1. The bottom rows of the table present p-values of F-tests of the difference between pairs of treatment coefficients. For opening of primary remittance recipient accounts (columns 1 and 2), the impact of Treatment 3 is marginally statistically significantly different from the impact of Treatment 2 in either specification (p-values are 0.1214 and 0.1351, respectively). The impact of Treatment 3 is statistically significantly different from the impact of Treatment 1 at roughly the 1% significance level in both specifications. Meanwhile, the impacts of Treatments 2 and 1 are statistically indistinguishable from one another.

### **Impact on savings at partner bank**

We estimate equation (1) in examining the impact of the different treatment conditions on savings balances in Banco Agricola bank accounts. In addition to looking at savings in project accounts, we also examine savings in non-project accounts (those other than the special accounts established for the project) to determine whether any savings accumulated in project accounts appear to have been shifted from non-project accounts at the partner bank. When looking at non-project accounts, we will also separately examine accounts owned by primary remittance recipients (whether or not shared with the migrant) from accounts owned by the migrant independently of the primary remittance recipient (but which may be jointly owned with a third individual).

In Table 5, the dependent variables are savings balances 6 months after treatment in various subcategories of accounts at the partner bank. In the first two columns, the dependent variable is savings in project accounts of the primary remittance recipient. The first column reports coefficient estimates for a regression without control variables, while the second column provides corresponding estimates but where control variables are included in the regression (this format is repeated for four additional dependent variables in subsequent columns).

The results indicate that Treatment 3 has a large impact on savings balances in recipient project accounts, and this impact is larger than the corresponding impacts of Treatments 2 and 1. The coefficient on the Treatment 3 indicator is positive and statistically significant at the 1% level in columns 1 and 2, and does not change much in magnitude with the addition of controls to the regression. The coefficient in column 2 indicates that savings in recipient project accounts are higher by \$192 in Treatment 3 than in the control group (where savings is just \$8 in this type of account). In contrast, coefficients on the Treatment 1 and 2 indicators, while positive, are substantially smaller in magnitude and are not statistically significantly different from zero. The F-tests reported at the bottom rows of the table indicate that the Treatment 3 coefficient is statistically significantly larger than the corresponding coefficients on Treatments 2 and 1 at the 5% and 1% levels, respectively.

When it comes to savings held in migrant project accounts (Ahorro Directo), results are perhaps surprisingly quite different. Even though Table 4's results indicated that a large fraction of migrants in Treatment 3 took the opportunity to open project accounts solely in their own names, the results indicate that savings balances in such accounts are not statistically significantly different between Treatment 3 migrants and the control group: the coefficient on the Treatment 3 indicator, while positive, is only a quarter the magnitude of the corresponding coefficient in the previous regressions, and is not statistically significantly different from zero.

Treatment 3 does appear to have a positive effect on savings in joint accounts shared by migrants and individuals in El Salvador other than the primary remittance recipient: the coefficient on Treatment 3 in the 5<sup>th</sup> and 6<sup>th</sup> columns of the table is positive and statistically significantly different from zero at the 5% level. The coefficient on Treatment 3 in the regression with control variables indicates that savings in this type of project account are larger by roughly \$46 in Treatment 3 compared to the control group (where savings is \$0).

In the next four columns we present results from analogous regressions but for savings in non-project accounts at our partner bank. Examining these types of savings is important to determine whether increases in savings in project accounts were simply shifted from other partner bank accounts (and therefore might not represent true increases in savings).

As it turns out, there is no evidence that increases in savings in project accounts came from reductions in savings in non-project accounts: none of the coefficients on indicators for Treatment 3 (as well as the other treatments) are statistically significantly different from zero. The treatments do not appear to have any identifiable effect on savings in other types of accounts at the partner bank.

Finally, the last three columns of the table present total savings summed across sub-types of accounts, to examine the impact on aggregate savings at the partner bank. Consistent with previous results, the regression for summed savings in all migrant accounts (project accounts solely in the migrants name or shared with someone other than the primary remittance recipient, plus other non-project accounts of the migrant) shows no statistically significant effect of Treatment 3.<sup>20</sup>

By contrast, the regression for summed savings in all recipient accounts (project accounts in the recipient's name plus other non-project accounts of the recipient) shows a positive and statistically significant effect of Treatment 3. The magnitude of this effect, \$207.83, is large representing a 52% increase in savings vis-à-vis the mean savings of primary recipients in the control group (which is \$396.43).

The dependent variable in the last column of the table is the sum of all savings in migrant and recipient accounts at the partner bank (whether project or non-project). This outcome is worth examining to the extent that one considers DC-based migrants and primary recipient households to be part of the same household. The positive and significant coefficient on Treatment 3 indicates that total savings in the combined trans-national household are larger by \$283.63, a 52% increase vis-à-vis the control group mean of \$544.20.

A question that may arise at this point is whether and how the effect of the treatments on savings varies with the number of months that have passed since treatment. Figure 2 graphically

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<sup>20</sup> Curiously, there is a positive and statistically impact of Treatment 1 on total migrant savings at the partner bank. This is surprising because Treatment 1 did not emphasize savings for the migrant and did not offer any savings facilities for migrants alone. In subsequent results tables, we do not find analogous positive effects of Treatment 1 on migrant savings, and so we do not emphasize this result here. It is possible that this result may simply be due to random variation.

presents coefficient estimates (solid line) and 95% confidence intervals (dashed lines) for Treatments 3, 2, and 1. These estimates derive from regressions with all control variables analogous to column (a) of Table 5, but where the dependent variable refers to savings in months ranging from 12 months prior to treatment (month -12) to 11 months after treatment (month 11). Month 0 is the treatment month. (For months -12 to -1, controls for pre-project savings are average end-of-month balances in partner bank accounts from Aug-Oct 2006, while for months 0 to 11 the corresponding controls are from Aug-Oct 2007.)

Figure 2 reveals, first of all, that there are no “effects” of the treatments in the months prior to treatment, and so serves as a useful specification check. It also reveals that the effect of Treatment 3 is relatively stable from months 4 through 11, so that month 6 is not an unusual month by any means. Effects of Treatments 2 and 1 are also relatively stable in the post-treatment period. A slight exception to that is the (marginally significant) uptick in savings in months 0-3 associated with Treatment 2, which drops again and stays low from months 4 onwards. This may reveal that savings may have been deposited into project accounts of Treatment 2 participants initially, but were eventually withdrawn (we return to this in the interpretation section below).

### **Impacts on savings outside of partner bank, and on overall savings**

Regressions presented in Table 5 provide the first indication that increases in savings seen in project accounts of study participants assigned to Treatment 3 were true increases in savings, since they were not simply shifted over from non-project accounts of the partner bank. We now bolster this conclusion by examining savings held outside of the partner bank. This analysis will also allow any possible positive spillovers to other types of savings to reveal themselves.

Data on savings outside of the partner bank are available for the 652 migrant/recipient-household pairs that we successfully interviewed in the follow-up survey that (fielded from March through June 2009). The first question that arises is whether this smaller subsample differs in substantial ways from the full sample of 1,451 individuals whose partner bank balances we have been examining so far.

Appendix Table 2 presents means of the main stratification variables as well as pre-treatment savings levels at the partner bank for the 652-observation subsample (first data

column) as well as the 799 observations (2<sup>nd</sup> column) who were in the full sample but not in the smaller follow-up survey sample. The p-value of the F-test of the equality of the means across the subsamples is presented in the 3<sup>rd</sup> column. As it turns out, the two subsamples appear quite similar in terms of the stratification variables: none of the means of the variables in the top panel of the table are statistically significantly different across the subsamples.

The main difference between the subsamples is that the follow-up survey sample has higher savings in remittance-receiving households, lower savings in DC-based migrant households, and higher remittances (reported as sent by migrants as well as reported as received by recipients). Differences between the subsamples in several of these variables are significant at conventional levels. For example, pre-treatment savings observed in recipient accounts at the partner bank are \$754 in the follow-up subsample and just \$348 in the remaining observations, and this difference is significant at the 10% level. These differences may reflect that the follow-up survey subsample have higher levels of trust between migrants and recipient households, resulting in migrants sending more remittances to El Salvador households and accumulating relatively more savings in El Salvador compared to DC. These differences should be kept in mind in interpreting results to follow.

Before examining the impact of the treatments on savings outside the partner bank for observations in the follow-up survey, we confirm that the results of Table 5 (that were from regressions in the full 1,451-observation sample) carry through in the smaller follow-up survey sample. Table 6 therefore repeats the key regressions of Table 5 but for the smaller follow-up survey sample.<sup>21</sup> The impact of Treatment 3 on savings in project accounts of primary remittance recipients in column (a) is very similar (\$221.29) to that estimated in the full sample, and is statistically significant at the 5% level. The impact of Treatment 3 on savings in joint accounts of migrants and individuals other than the primary recipient in column (c) is nearly identical to the full sample estimate, but due to the smaller sample size is not statistically significant at conventional levels. In Table 6, as in the full sample results, the treatments have no statistically significant effect on savings in non-project accounts (columns (d) and (e)).

Some of the other point estimates in Table 6 are larger than those for the full-sample regressions. In column (b), the impact of Treatment 3 on savings in project accounts for migrants

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<sup>21</sup> For brevity here, and in all remaining results tables of the paper, specifications without the control variables are omitted. In no case are coefficient estimates substantially affected by inclusion of the control variables; results are available from the authors on request.

alone (Ahorro Directo accounts) is about 75% larger in magnitude than the corresponding coefficient in Table 5, and is statistically significant at the 10% level. In addition, in the last two columns of Table 6 (where the dependent variables are savings summed across all recipient accounts, or in all recipient plus migrant accounts) the estimates of the Treatment 3 effect are double the magnitude of the corresponding Table 5 coefficients. For example, in the follow-up survey sample, the impact of Treatment 3 on savings summed across all partner bank accounts of migrants and recipients (last column of Table 6) is \$541.23, a 92% increase vis-à-vis the control group mean of \$589.21.

In Table 7, we now turn to examining the impact of the treatments on savings within and outside of the partner bank, for the household of the primary remittance recipient, for the DC-based migrant, and for the combined trans-national household, at the time of the follow-up survey (Mar – Jun 2009).

The first four columns present impacts on savings reported by the primary remittance recipient household, (a) at the partner bank, (b) in non-partner banks, (c) in cash, and (d) in total across the previous three categories. The estimated Treatment 3 impact on savings at the partner bank is positive and marginally significant (the t-statistic is 1.5) and comparable in magnitude (\$128.37) to estimates in previous tables based on actual partner bank data. Point estimates for savings in non-partner banks are positive and larger in magnitude, and interestingly for Treatment 2 the coefficient is significant at the 10% level. There is no large or significant effect on savings in cash. Impacts on total savings across these three categories is positive for all treatments, significant at the 10% level for Treatment 3, and marginally significant (t-statistic of 1.6) for Treatment 2.

The next five columns present impacts on savings reported by the DC-based migrant, (e) at the partner bank in El Salvador, (f) in non-partner banks in El Salvador, (g) in United States banks, (h) in cash, and (i) in total across the previous four categories. There is no indication that any of the treatments substantially affected migrants' savings in the partner bank: coefficients in column (e) are relatively small in magnitude and are not statistically significantly different from zero. Coefficients on the treatments (particularly Treatments 2 and 3) are larger in magnitude in column (f) for savings at non-partner banks in El Salvador, but are also not statistically significantly different from zero. However, for savings in U.S. banks, there is a large positive impact of Treatment 3 (\$425.64, compared to a base of \$351.00 in the control group) that is

statistically significant at the 10% level. Point estimates of the impact of the treatments on cash held outside of banks in column (h) are negative, but are close to zero and not statistically significantly different from zero. Impacts on total savings across these four categories are large and positive for all treatments, and statistically significant at the 10% level for Treatments 3 and 2.

Column (j) presents impacts on total savings in the combined trans-national migrant/remittance-recipient household. The dependent variable here is the sum of total savings reported in the migrant and recipient-household surveys, and makes sure to avoid double-counting of savings in jointly-owned migrant/remittance-recipient accounts.<sup>22</sup> The effect of Treatment 3 is statistically significant at the 5% level and large: the estimated impact of \$1,072.51 represents a 136% increase over the control group mean (which is \$786.58). The effect of Treatment 2 is statistically significant at the 10% level and also large in magnitude: the estimated impact of \$754.86 represents a 96% increase over the control group mean. The effect of Treatment 1, while positive, is not statistically significantly different from zero.

It is worthwhile comparing these results on reported savings in the follow-up survey with actual data from the partner bank.<sup>23</sup> Because the follow-up survey occurred in Mar-Jun 2009, the last three columns of Table 7 present savings at the partner bank (average end-of-month balances) over the same time period. Estimated impacts of Treatment 3 are positive for all three categories of savings (savings in migrant accounts, recipient accounts, and migrant plus recipient accounts together), but none of the coefficients are statistically significant at conventional levels. That said, the magnitudes of the results are roughly comparable to the effects estimated in Table 6, if slightly smaller in magnitude. Given the imprecision of these estimates, we cannot reject that Table 7's estimates of impacts on partner bank savings from Mar - Jun 2009 are identical to the corresponding estimates in Table 6 (that refer to a somewhat earlier range of months).

In sum, two key conclusions emerge from Table 7's results. First, it appears that Treatment 3's positive impact on savings held at the partner bank represents a true increase in savings, and not just a shift in savings from other institutions or from cash: for all treatments, in no category of savings outside the partner bank is there a decline in savings.

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<sup>22</sup> To be specific, in creating this dependent variable, we add up all savings reported by migrants and primary remittance-recipient households and then subtract from the total all savings in jointly-owned migrant/remittance-recipient-household accounts reported by the migrant (but not by the recipient).

<sup>23</sup> The dependent variables in previous results tables are savings at 6 months post-treatment, which for all observations precedes the Mar-Jun 2009 follow-up survey (treatments occurred in Nov 2007 through July 2008).

Second, Treatments 2 and 3 actually appear to have had positive effects on savings outside the partner bank, so much so that total savings in the combined transnational household experience very large gains. For migrants, the most prominent effect is Treatment 3's positive impact on savings held at banks in the United States. For recipient households, Treatment 2 raises savings held in El Salvador at banks other than the partner bank.

### **Impacts on remittances**

To shed light on whether impacts of the treatments on savings are due to increases in remittance flows from migrants to recipient households or just increases in savings out of existing flows, Table 8 presents estimates of equation (1) where the dependent variable is remittances sent by the DC-based migrant through the partner bank to the primary remittance recipient in El Salvador. The first six columns of the table present impacts on remittances sent in each of months 1 through 6 post-treatment, and the seventh column presents impacts on the sum of all flows in those months.

Each of the three treatments has positive effects on migrant remittances sent, and for Treatments 2 and 3 the time path is generally upward (larger coefficient estimates in later months compared to earlier ones). Many treatment coefficient estimates of impacts in individual months are statistically significant, but mainly for Treatments 2 and 3.

Impacts of all three treatments on remittances across the six months in column (g) are positive and statistically significant, at the 5% level for Treatment 3, the 1% level for Treatment 2, and the 10% level for Treatment 1. While in column (g) the largest coefficient estimate is the one for Treatment 2 (followed by Treatment 3 and then Treatment 1), the estimates are imprecise enough that we cannot reject the hypothesis that the Treatment 2 and 3 coefficients are identical to one another. Treatment 2 and 1 coefficients, on the other hand, are marginally statistically significantly different from one another (the p-value on the F-test is 0.167).

The magnitudes of these effects are large, as high as \$680.96 for Treatment 2 (which is a 60% increase over the control group mean, \$1,135.28). All of these treatment effects on remittances exceed the increase in savings observed in partner bank accounts. It appears therefore that all of the increase in savings could very well have been funded by the observed

increases in remittances.<sup>24</sup> Results for the 652-observation subsample for which the follow-up survey was administered are consistent with results for the full sample, being similar in magnitude but at somewhat lower significance levels due to the smaller sample size.

## **VI. Interpretation of results**

We believe that a reasonable interpretation of the results of the experiment is that two types of effects are operative: a “financial literacy effect” and a “control effect.”

The results in Table 7 reveal that savings grew substantially in response to Treatments 2 and 3. Indeed, the increases in such savings are so large that we cannot statistically distinguish the positive impacts of Treatments 2 and 3 on total recipient household savings (column d), total migrant savings (column i), and total savings in the combined trans-national household (column j). At the same time, we believe it is revealing that in both Treatments 2 and 3 substantial amounts of savings are accumulated by recipient households and migrants in non-partner banks.

A potential explanation for this finding is that the information on savings strategies conveyed by the marketing visits for Treatments 2 and 3 convinced migrants of the importance of savings. For example, the marketing visit for Treatment 2 emphasized the value in monitoring funds in accounts held jointly by the migrant and the recipient. The marketing for Treatment 3 involved the same discussion as Treatment 2, and added a discussion of the value of having control over one’s own account separate from an account shared with a remittance recipient. Such discussions were an integral part of the offer of the new products for study participants.

We describe this result as a “financial literacy effect” because it is possible that we would have seen increases in savings even if we had not offered the migrants any new savings products: large fractions of the savings were held in separate bank accounts that the experiment had no role in facilitating.

That said, while Treatments 2 and 3 did have similar effects on overall savings, they differed substantially in the composition of savings across account types and locations. We believe that the differences in the impacts of Treatments 2 and 3 are due to the “control effect,” wherein Treatment 3 led migrants to exert greater control over savings.

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<sup>24</sup> This project therefore cannot reveal whether there would have been any increase in savings if remittances had been held constant.

However, the “control effect” associated with Treatment 3 appears to manifest itself in a manner that we did not initially anticipate when we designed the experiment. One of our initial hypotheses was that Treatment 3 would lead to higher overall savings levels in the combined trans-national household because it offered migrants a new El Salvador-based savings facility in the migrant’s name alone (Ahorro Directo) that a migrant could use to save independently of the El Salvador household. As it turned out, however, migrants in the Treatment 3 condition did not save in the Ahorro Directo accounts, but instead accumulated substantial savings in bank accounts held in the United States. It appears that the DC-based migrants had sufficient access to savings facilities in the U.S., and the El Salvador-based facilities in and of themselves did not provide substantial improvements in access to formal savings. At the same time, savings held in the joint accounts shared by migrants and remittance recipients also grew substantially.

We believe that the most likely explanation for these results is that Treatment 3 encouraged migrants to do two things differently that enhanced their control over savings. First, it convinced them of the benefits of saving more on their own, independently of the primary remittance recipient household in El Salvador. They then did save more, but primarily in (more convenient and accessible) bank accounts in the U.S. rather than in bank accounts established by the project in El Salvador.

Second, it led them to attempt to more actively influence the savings levels of primary remittance recipient households in El Salvador. An open question is whether Treatment 3 led to an *increase* in migrants’ bargaining power over remittance recipients, or whether it simply convinced migrants to *exert* their existing level of bargaining power to induce recipient households to save more. It is difficult to distinguish between these two possibilities because migrants opened but did not actually use the project migrant-only Ahorro Directo accounts. The existence of these accounts in El Salvador (even if they were not actually used) may have made more credible any threats by the migrants to save on their own independently of the recipient household, particularly if savings held in El Salvador accounts are seen as having attractive features not shared with U.S.-based accounts.<sup>25</sup> But we cannot rule out that migrants simply chose to exert an existing level of bargaining power by making (and to partially carrying out) threats to save on their own in the U.S. independently of the remittance-recipient household.

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<sup>25</sup> Such features could include easier accessibility from El Salvador (say, if the migrant is home for a visit) and greater security and access should the DC-based migrant be deported.

To return to the difference in results across Treatments 2 and 3, we believe a reasonable explanation is as follows. Treatments 2 and 3 both increase remittance flows via the partner bank to primary remittance recipients (as shown in Table 8), but then remittance recipients in Treatment 2 divert such flows to non-partner bank accounts to avoid having the migrant monitor the account balances.<sup>26</sup> In Treatment 3, on the other hand, migrants exert greater control by: 1) saving in accounts (in the U.S.) not shared with the primary recipient, and 2) enforcing that a greater proportion of remittance flows remain in joint accounts that migrants can monitor.

Evidence in support of this interpretation is provided in Table 9. The table reports regression results for savings reported by the DC migrant analogous to those in Table 7, but in which the dependent variable categorizes savings according to whether the migrant reports that he or she is an owner of the account. Savings in accounts where the migrant does *not* report ownership see a large increase in response to Treatment 2 (which is statistically significant at the 10% level, column a). By contrast, savings in accounts where the migrant does report ownership (column b) see a large and statistically significant (at the 5% level) increase in response to Treatment 3 but not Treatment 2. The difference between the Treatment 3 and 2 effects in column (b) is marginally significant (the p-value of the test for equality is 0.1469). These patterns are consistent with recipients in the Treatment 2 condition shifting remitted funds towards their own accounts because migrants are unable or unwilling to enforce that funds remain in jointly-held accounts. On the other hand, in the Treatment 3 condition, migrants exert control over savings by ensuring that they are official owners of accounts.

## **VII. Conclusion**

This paper contributes to knowledge in two areas. First, it expands our currently very limited knowledge about the determinants of international remittance flows, which have emerged in recent years as one of the the largest and fastest-growing types of international financial flow to developing countries. Second, it contributes to the development economics literature on intrahousehold resource allocation and decision making, by documenting a demand on the part of

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<sup>26</sup> Figure 2's graph of the Treatment 2 effect on balances in partner bank accounts is suggestive of this: there is an uptick in savings in months 0-3 and a downtick in month 4 and afterwards.

migrants for greater control over the use of remittances sent to households in their country of origin.

We implemented an experiment that offered U.S.-based migrants from El Salvador the opportunity to open bank accounts in the home country, where the bank accounts offered varied randomly in the migrant's ability to be an official "owner" of the account. Our results provide support for the hypothesis that a desire for control over remittance uses is quantitatively large and has an important influence on financial decision making by migrants. We find that migrant demand for savings accounts and savings accumulation is higher when migrants have the option of being account owners. We also find evidence that migrants seek control over savings: savings were substantially higher 6 months after treatment in El Salvador households whose U.S.-based migrants were offered higher degrees of control over El Salvador savings accounts. While effects on savings at our partner bank were substantial, there was also a substantial increase in savings *outside of* our partner bank (including U.S.-based banks). We interpret this as due to the financial advice we offered as part of the treatments: migrants implemented savings strategies suggested by us but using savings facilities at other banks. We document large treatment effects: compared to a base of roughly \$800 in reported comparison group savings, offering joint or exclusive control of bank accounts leads total savings in the combined trans-national household (migrant plus remittance recipient) to roughly double.

The follow-up survey implemented for this study also allows us to determine the impact of the experimental treatments on a variety of other outcomes on the part of US-based migrants as well as remittance-receiving households in El Salvador. On the US side, key outcomes include employment, labor supply, entrepreneurship, and onward or return migration. Outcomes of interest on the El Salvador side include consumption, nutrition, child schooling, health status, and entrepreneurial investment. Impacts on these outcomes will be the subject of future companion papers.

### **Appendix: Survey and Treatment Protocols**

The subjects of the field experiment are immigrants in the greater Washington D.C. area. To be eligible for inclusion in the sample, immigrants had to have met the following conditions: 1) they had to be from El Salvador, 2) their first entry into the U.S. had to have been within the last 15 years, and 3) they had to have sent a remittance to someone in El Salvador within the last 12 months.

A subset of migrants were only administered a brief baseline survey (to collect data on the treatment stratification variables and remittance raffle allocations) prior to being administered the marketing treatment. Another subset of migrants were recruited beforehand (up to 12 months before the marketing visit) and administered a comprehensive baseline survey questionnaire. Migrants administered the comprehensive baseline survey were paired with the household in El Salvador which is the migrant's primary remittance recipient, and we also attempted to field a comprehensive baseline survey for that recipient household.

Migrants administered the baseline survey were recruited as follows. We stationed our survey team at the two Salvadoran consulates in the Washington DC area (in DC proper and in Woodbridge, Virginia). The El Salvador consulate was aware of our study and agreed to cooperate. At regular intervals, a consular staffer would announce to individuals seated in the waiting area that our survey staff were present and ask for their participation. Survey team members were individuals of Salvadoran origin, and mostly female. Members of the survey team approached individuals in waiting area of the consulate and invited them to participate in the study. Other individuals were recruited for the study in the branches of Banagricola (a subsidiary of Banco Agricola) remittance agencies in D.C., Maryland, and Virginia. Individuals who agreed to participate were administered the survey instrument then and there, or (in a small number of cases) at a different time and location of the subject's choosing. The D.C. survey work began in June 2007 and was completed in January 2008.

After completion of a migrant baseline survey in the DC area, a separate survey team (fielded by a Salvadoran survey organization hired for the project) was dispatched to survey the individual in El Salvador that the migrant identified as his or her primary remittance recipient on a variety of individual- and household-level topics. The El Salvador household surveys were fielded between November 2007 and June 2008.

The migrant sample comprises a reasonable cross-section of Salvadoran migrants in the Washington, D.C. area, and includes both documented and undocumented migrants. The consulate of El Salvador serves Salvadorans regardless of their legal status. The main services sought by study participants at the consulate were passport renewals, civil registration (of births, deaths, and marriages), and assistance with processing of Temporary Protected Status (a special provision allowing temporary legal work for Salvadorans and other nationalities who entered the U.S. after natural disasters or civil strife in the home country).

We randomly allocated 25% of the migrants in the sample to each of the four treatment conditions. Prior to randomization, study participants were stratified into cells defined by the unique combinations of the following categorical variables: gender (male/female), whether the individual has a US bank account (yes/no), relationship to the primary remittance recipient (parent, spouse, child, or other), a years in the U.S. (0-5 years, 6-10 years, 11-15 years). The treatments were administered via face-to-face visits at a location of the migrant's choice by marketers hired for the study. Assignment to either treatment 0, 1, 2, or 3 occurred only after the migrant had agreed to a marketing visit. Visits took from 1-2 hours. Marketers were paid a flat fee for each completed visit which is the same for all treatment conditions (to remove any differential incentive to complete visits of different types). In cases where baseline surveys were administered to the migrant and associated remittance-receiving household in El Salvador, marketing visits were only scheduled after the survey of the El Salvador household had been completed, to eliminate bias in survey responses related to treatment assignment. The marketing visits were carried out between December 2007 and July 2008.

To help track migrants' remittance behavior after the visit, all visited migrants were given a special card (called a "VIP card") that provides a discount for sending remittances via Banagricola remittance locations in the DC area. Each card has a unique code that is entered into the computer during the remittance transaction to validate the discount, allowing us to track individual remittance transactions that take advantage of the discount. Banco Agricola's normal remittance charge is \$10 for a remittance up to \$1,500, and the VIP card allows the migrant to send a remittance for a randomly-determined price of either \$4, \$5, \$6, \$7, \$8, or \$9 (once randomly assigned at the outset, the price is fixed for the 12-month validity period of the card).<sup>27</sup> Eligibility for the card was conditional on the migrant presenting an identification document of some sort (usually a Salvadoran passport). Migrants were told to bring an identification document in the initial appointment phone call.

Follow-up surveys were administered between March and June 2009. Primary remittance recipient households in El Salvador were surveyed in person by a field team, while migrants were interviewed by telephone using contact information obtained when the migrant was originally enrolled.

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<sup>27</sup> This remittance price randomization is independent of the randomization to assign treatments 0, 1, 2, or 3, and so should not confound interpretation of any differences across treatments. In addition, migrants did not learn the actual discounted VIP price until after the marketing visit had concluded. The remittance price randomization was implemented for a separate study within the same study population on the impact of remittance prices on the frequency and amount of remittances.

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**Table 1: Summary statistics**

	<u>Mean</u>	<u>Std. Dev.</u>	<u>10th pct.</u>	<u>Median</u>	<u>90th pct.</u>	<u>Num. Obs.</u>
<u>Variables collected for all study participants</u>						
Treatment 0 (no savings facility offered)	0.25	0.44	0	0	1	1,451
Treatment 1 (remittance recipient account only)	0.24	0.43	0	0	1	1,451
Treatment 2 (joint account)	0.26	0.44	0	0	1	1,451
Treatment 3 (joint + migrant account)	0.25	0.43	0	0	1	1,451
Migrant is female	0.29	0.46	0	0	1	1,451
Migrant has US bank account	0.67	0.47	0	1	1	1,451
Recipient is migrant's parent	0.49	0.50	0	0	1	1,451
Recipient is migrant's spouse	0.14	0.34	0	0	1	1,451
Recipient is migrant's child	0.05	0.22	0	0	0	1,451
Recipient is migrant's other relative	0.32	0.47	0	0	1	1,451
Migrant has been in US 0-5 years	0.51	0.50	0	1	1	1,451
Migrant has been in US 6-10 years	0.35	0.48	0	0	1	1,451
Migrant has been in US 11-15 years	0.14	0.35	0	0	1	1,451
Migrant allocation to savings in raffle	20.92	40.23	0	0	100	1,451
<u>Baseline survey variables</u>						
Migrant's years in the US	5.57	3.60	1	5	11	900
Migrant has US bank account only	0.53	0.50	0	1	1	900
Migrant has El Salvador bank account only	0.07	0.26	0	0	0	900
Migrant has account in both US and El Salvador	0.10	0.30	0	0	1	900
Migrant's annual income (US\$)	25,710	58,339	5,779	18,430	41,280	868
Migrant's household's annual income (US\$)	38,533	90,200	8,237	26,840	62,300	900
Migrant's years of education	8.80	4.93	2	9	12	868
Migrant's age	30.89	7.65	22	30	41	896
Migrant's annual remittances sent (US\$)	4,987	4,120	1,200	3,900	9,600	900
Migrant's remittances as share of annual hh inc.	0.512	2.792	0.039	0.155	0.598	899
Migrant's total hh savings balance (US\$)	2,845	5,107	0	750	8,100	808
Migrant's savings as share of annual hh inc.	0.149	0.539	0.000	0.030	0.297	807
Migrant is US citizen	0.01	0.08	0	0	0	896
Migrant hh size in U.S.	4.81	2.15	2	5	8	900
Migrant is married or partnered	0.59	0.49	0	1	1	899
Migrant is coresident with spouse/partner	0.73	0.44	0	1	1	528
Recipient allocation to savings in raffle	2.55	15.35	0	0	0	741
Recipient's annual hh income (US\$)	14,117	177,786	1,200	3,540	9,680	741
Recipient has savings account	0.20	0.40	0	0	1	736
Recipient total hh savings balance (US\$)	381	1,729	0	0	380	736
Recipient's years of education	5.13	5.66	0	3	12	731
Recipient's age	46.84	14.93	26	48	66	741
Recipient's annual remittances received (US\$)	3,155	2,809	650	2,400	6,000	741
Recipient hh size	4.61	2.37	2	4	8	741
<u>Savings data from partner bank</u>						
Savings in recipient accounts, Aug-Oct 2007	530	4,183	0	0	266	1,451
Savings in migrant accounts, Aug-Oct 2007	106	888	0	0	0	1,451

Notes -- Survey data collected in 2007-2008 in Washington, D.C. and El Salvador. Indicator for "Migrant is coresident with spouse/partner" is undefined if individual does not have a partner or spouse. Savings data from partner bank are from Banco Agricola's internal database. Savings variables are average end-of-month balances in partner bank accounts from Aug - Oct 2007, separately for recipient accounts and migrant accounts.

**Table 2: Means of baseline variables by treatment group**

	Treatment group				P-value: test of equality of means	Number of observations
	0	1	2	3		
<u>Variables collected for all study participants</u>						
Migrant is female	0.26	0.32	0.31	0.29	0.335	1,451
Migrant has US bank account	0.66	0.66	0.70	0.67	0.643	1,451
Recipient is migrant's parent	0.48	0.51	0.50	0.49	0.921	1,451
Recipient is migrant's spouse	0.17	0.12	0.13	0.13	0.264	1,451
Recipient is migrant's child	0.07	0.05	0.03	0.06	0.204	1,451
Recipient is migrant's other relative	0.29	0.32	0.34	0.32	0.412	1,451
Migrant has been in US 0-5 years	0.52	0.50	0.49	0.51	0.890	1,451
Migrant has been in US 6-10 years	0.34	0.36	0.37	0.34	0.857	1,451
Migrant has been in US 11-15 years	0.14	0.14	0.14	0.15	0.986	1,451
Migrant allocation to savings in raffle	22.04	19.86	19.92	21.85	0.813	1,451
<u>Baseline survey variables</u>						
Migrant's years in the US	5.42	5.47	5.78	5.58	0.708	900
Migrant has US bank account only	0.53	0.54	0.55	0.50	0.741	900
Migrant has El Salvador bank account only	0.08	0.08	0.07	0.08	0.920	900
Migrant has account in both US and El Salvador	0.08	0.07	0.12	0.12	0.170	900
Migrant's annual income (US\$)	24,468	32,933	22,869	23,251	0.246	868
Migrant's household's annual income (US\$)	33,783	42,827	41,291	36,154	0.690	900
Migrant's years of education	8.87	8.20	9.31	8.71	0.134	868
Migrant's age	30.61	31.05	31.04	30.87	0.924	896
Migrant's annual remittances sent (US\$)	5,451	4,876	4,679	4,973	0.231	900
Migrant's remittances as share of annual hh inc.	0.28	0.52	0.33	0.93	0.057	899
Migrant's total hh savings balance (US\$)	2,942	3,080	2,537	2,869	0.734	808
Migrant's savings as share of annual hh inc.	0.12	0.15	0.15	0.18	0.731	807
Migrant is US citizen	0.000	0.005	0.012	0.009	0.411	896
Migrant hh size in U.S.	4.72	5.07	4.84	4.62	0.152	900
Migrant is married or partnered	0.54	0.58	0.62	0.60	0.308	899
Migrant is coresident with spouse/partner	0.66	0.78	0.75	0.74	0.222	528
Recipient allocation to savings in raffle	2.02	2.21	2.00	3.91	0.564	741
Recipient's annual hh income (US\$)	4,395	8,362	26,500	15,149	0.645	741
Recipient has savings account	0.25	0.20	0.19	0.18	0.335	736
Recipient total hh savings balance (US\$)	247	543	273	457	0.292	736
Recipient's years of education	5.69	4.52	5.09	5.26	0.278	731
Recipient's age	44.94	46.41	47.65	48.08	0.189	741
Recipient's annual remittances received (US\$)	3,094	3,100	3,259	3,153	0.936	741
Recipient hh size	4.39	5.06	4.49	4.49	0.032	741
<u>Savings data from partner bank</u>						
Savings in recipient accounts, Aug-Oct 2007	370	686	564	509	0.788	1,451
Savings in migrant accounts, Aug-Oct 2007	115	135	105	70	0.804	1,451

Notes -- Table presents means of key variables for each treatment group prior to treatment. P-value is for F-test of equality of means across treatment groups. The first 9 variables listed in table are stratification variables: migrants were first sorted into 48 cells (based on gender, US bank account ownership, relationship to remittance recipient, and years on US category) before randomization into treatments 0, 1, 2, or 3. Migrant allocation to savings in raffle also was recorded for all DC migrant respondents, even those not administered baseline survey. Baseline survey administered from Jun 2007 to Jan 2008. Savings figures reported in US dollars. See previous table for other notes.

**Table 3: Migrant vs. recipient allocation of \$100 in possible raffle winnings**  
(U.S. dollars)

<u>Raffle use categories</u>	<u>Migrant (in U.S.)</u>	<u>Remittance Recipient (in El Salvador)</u>	<u>Difference (migrant minus recipient allocation)</u>	<u>P-value: test of equality of means</u>
Daily consumption	42.38	64.82	-22.44	0.000
Savings	21.16	2.55	18.61	0.000
Clothing	7.19	6.31	0.88	0.414
Housing	2.43	1.91	0.52	0.471
Medical expenditures	9.40	7.69	1.71	0.173
Educational expenses	5.57	5.67	-0.10	0.916
Utilities bills	3.51	3.84	-0.33	0.698
Small business expenses	0.74	0.54	0.20	0.612
Phone bills	1.46	0.47	0.99	0.039
Agricultural inputs	0.27	0.41	-0.14	0.655
Durable goods	4.68	0.66	4.02	0.000
Automobile payments	0.00	0.07	-0.07	0.318
Other (specify)	1.20	5.05	-3.85	0.000
Num. obs.	740	740		

Notes -- Table presents mean amounts allocated to given expenditure category out of \$100 in possible remittance raffle winnings. Sample comprised of matched pairs of Salvadoran migrants in the U.S. and their "primary" remittance recipient in El Salvador. Migrants report desired allocation of funds by the remittance recipient. Remittance recipients report desired allocation of funds by themselves. "Housing" includes rent, construction, and mortgage payments. P-value is for F-test of equality of means across two groups (migrants vs. remittance recipients).

**Table 4: Impact of treatments on opening of project accounts**

(Ordinary least-squares estimates)

Dependent variable: Indicator for existence of given type of project account at 6 months post-treatment

	<u>Primary recipient accounts</u>		<u>Migrant-only accounts</u>		<u>Accounts shared by migrant and a person in El Salvador other than primary remittance recipient</u>	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment 3 (joint account + indiv. migrant account)	0.175*** (0.025)	0.176*** (0.026)	0.227*** (0.021)	0.236*** (0.021)	0.052*** (0.013)	0.046*** (0.013)
Treatment 2 (joint account)	0.136*** (0.025)	0.138*** (0.025)	-0.025 (0.021)	-0.016 (0.021)	0.053*** (0.013)	0.044*** (0.013)
Treatment 1 (remittance recipient account)	0.103*** (0.025)	0.111*** (0.026)	-0.017 (0.021)	-0.007 (0.021)	0.026* (0.013)	0.021 (0.013)
Constant	0.035** (0.018)	0.323** (0.140)	0.057*** (0.015)	0.135 (0.117)	0.000 (0.009)	-0.049 (0.073)
Marketer fixed effects		Y		Y		Y
Treatment month fixed effects		Y		Y		Y
Stratification cell fixed effects		Y		Y		Y
Controls for pre-treatment savings		Y		Y		Y
Observations	1451	1451	1451	1451	1451	1451
R-squared	0.035	0.095	0.119	0.185	0.015	0.077
P-value of F-test: equality of ...						
Treatment 3 & 2 coeffs.	0.1214	0.1351	0.0000	0.0000	0.9398	0.8878
Treatment 3 & 1 coeffs.	0.0048	0.0114	0.0000	0.0000	0.0462	0.0683
Treatment 2 & 1 coeffs.	0.1898	0.2841	0.7025	0.6908	0.0371	0.0896

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

Notes -- Dependent variable equal to 1 if migrant or remittance recipient has any account with partner bank (Banco Agricola), 0 otherwise. Omitted treatment indicator is for Treatment 0 (control group). Marketer fixed effects are for the specific individual (out of 9) who conducted the marketing visit. Fixed effects for stratification cell are for each of 48 unique combinations of stratification variables: gender (male/female), having a US bank account (yes/no), relationship to remittance recipient (parent/child/spouse/other), and years in US category (0-5 years/6-10 years/11-15 years). Treatment months are Nov 2007 through Jul 2008 inclusive.

**Table 5: Impact of treatments on savings in accounts at partner bank**

(Ordinary least-squares estimates)

Dependent variable: Savings balances at 6 months post-treatment ...

	... in project accounts ...						... in non-project accounts ...				... in total across accounts ...			
	... of primary recipient		... of migrant alone		... shared by migrant and other individual (non-primary recipient)		... of primary recipient		... of migrant		... of migrant	... of primary recipient	... of migrant + primary recipient	
	(a)		(b)		(c)		(d)		(e)	(b) + (c) + (e)	(a) + (d)	(a) + (b) + (c) + (d) + (e)		
Treatment 3 (joint account + indiv. migrant account)	202.25*** (57.68)	192.08*** (59.11)	44.12 (36.98)	56.22 (38.13)	43.52** (18.29)	45.84** (18.89)	164.69 (329.08)	15.75 (113.34)	-87.49 (112.51)	-26.25 (75.69)	75.80 (85.94)	207.83* (116.14)	283.63** (140.14)	
Treatment 2 (joint account)	91.29 (57.21)	74.65 (58.77)	41.87 (36.68)	47.43 (37.91)	19.19 (18.14)	21.38 (18.78)	133.42 (326.42)	-66.91 (112.68)	17.46 (111.60)	34.03 (75.25)	102.85 (85.44)	7.74 (115.47)	110.59 (139.33)	
Treatment 1 (remittance recipient account)	18.60 (58.26)	18.86 (59.47)	50.49 (37.35)	57.19 (38.36)	3.31 (18.48)	2.91 (19.00)	361.51 (332.40)	8.31 (114.03)	132.83 (113.64)	112.26 (76.15)	172.37** (86.47)	27.17 (116.85)	199.53 (141.00)	
Constant	8.00 (40.64)	823.79** (323.42)	16.62 (26.06)	-7.15 (208.62)	0.00 (12.89)	30.43 (103.34)	388.42* (231.90)	-1,348.29** (620.14)	131.16* (79.28)	-21.60 (414.12)	1.68 (470.22)	-524.50 (635.45)	-522.82 (766.78)	
Marketer fixed effects	Y		Y		Y		Y		Y		Y	Y	Y	
Treatment month fixed effects	Y		Y		Y		Y		Y		Y	Y	Y	
Stratification cell fixed effects	Y		Y		Y		Y		Y		Y	Y	Y	
Controls for pre-treatment savings	Y		Y		Y		Y		Y		Y	Y	Y	
Observations	1451	1451	1451	1451	1451	1451	1451	1451	1451	1451	1451	1451	1451	
R-squared	0.010	0.051	0.002	0.031	0.005	0.032	0.001	0.892	0.003	0.588	0.529	0.889	0.856	
P-value of F-test: equality of ...														
Treatment 3 & 2 coeffs.	0.0534	0.0440	0.9513	0.8152	0.1816	0.1889	0.9240	0.4593	0.3488	0.4190	0.7495	0.0806	0.2103	
Treatment 3 & 1 coeffs.	0.0017	0.0035	0.8651	0.9797	0.0302	0.0236	0.5552	0.9478	0.0535	0.0683	0.2628	0.1212	0.5497	
Treatment 2 & 1 coeffs.	0.2102	0.3433	0.8168	0.7971	0.3882	0.3262	0.4907	0.5051	0.3080	0.2993	0.4166	0.8666	0.5238	
Mean of dep. var. in control group		8.00		16.62		0.00		388.42		131.16		147.77	396.43	544.20

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

Notes -- Dependent variables are end-of-month balances in US dollars. Omitted treatment indicator is for Treatment 0 (control group). Marketer fixed effects are for the specific individual (out of 9) who conducted the marketing visit. Fixed effects for stratification cell are for each of 48 unique combinations of stratification variables: gender (male/female), having a US bank account (yes/no), relationship to remittance recipient (parent/child/spouse/other), and years in US category (0-5 years/6-10 years/11-15 years). Treatment months are Nov 2007 through Jul 2008 inclusive. Controls for pre-project savings are average end-of-month balances in partner bank accounts from Aug - Oct 2007, separately for: 1) recipient accounts, and 2) migrant accounts.

**Table 6: Impact of treatments on savings in accounts of partner bank, restricted sample**

(Ordinary least-squares estimates)

Sample restricted to migrant/recipient-household pairs that completed the follow-up survey in Mar - Jun 2009

Dependent variable: Savings balances at 6 months post-treatment ...

	<u>... in project accounts ...</u>			<u>... in non-project accounts ...</u>		<u>... in total across accounts ...</u>		
	... of primary recipient	... of migrant alone	... shared by migrant and other individual (non-primary recipient)	... of primary recipient	... of migrant	... of migrant recipient	... of primary recipient	... of migrant + primary recipient
	(a)	(b)	(c)	(d)	(e)	(b) + (c) + (e)	(a) + (d)	(a) + (b) + (c) + (d) + (e)
Treatment 3 (joint account + indiv. migrant account)	221.29** (104.59)	98.20* (53.53)	42.92 (30.96)	190.05 (193.35)	-11.22 (156.19)	129.90 (167.64)	411.33** (194.35)	541.23** (246.58)
Treatment 2 (joint account)	103.96 (103.49)	72.88 (52.96)	30.02 (30.63)	-32.49 (191.31)	39.65 (154.54)	142.55 (165.88)	71.46 (192.30)	214.02 (243.99)
Treatment 1 (remittance recipient account)	3.10 (106.88)	26.59 (54.70)	3.07 (31.64)	104.24 (197.59)	195.84 (159.61)	225.51 (171.32)	107.34 (198.61)	332.84 (251.99)
Marketer fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Treatment month fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Stratification cell fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Controls for pre-treatment savings	Y	Y	Y	Y	Y	Y	Y	Y
Observations	652	652	652	652	652	652	652	652
R-squared	0.072	0.067	0.070	0.932	0.582	0.548	0.933	0.902
P-value of F-test: equality of ...								
Treatment 3 & 2 coeffs.	0.2377	0.6185	0.6608	0.2258	0.7316	0.9367	0.0659	0.1626
Treatment 3 & 1 coeffs.	0.0364	0.1792	0.1962	0.6557	0.1832	0.5667	0.1164	0.3959
Treatment 2 & 1 coeffs.	0.3239	0.3763	0.3732	0.4693	0.3063	0.6126	0.8502	0.6219
Mean of dep. var. in control group	14.50	1.31	0	499.92	73.47	74.78	514.42	589.21

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

Notes -- Dependent variables are end-of-month balances in US dollars. Omitted treatment indicator is for Treatment 0 (control group). Marketer fixed effects are for the specific individual (out of 9) who conducted the marketing visit. Fixed effects for stratification cell are for each of 48 unique combinations of stratification variables: gender (male/female), having a US bank account (yes/no), relationship to remittance recipient (parent/child/spouse/other), and years in US category (0-5 years/6-10 years/11-15 years). Treatment months are Nov 2007 through Jul 2008 inclusive. Controls for pre-project savings are average end-of-month balances in partner bank accounts from Aug - Oct 2007, separately for: 1) recipient accounts, and 2) migrant accounts.

**Table 7: Impact of treatments on total savings reported in follow-up survey (Mar - Jun 2009)**

(Ordinary least-squares estimates)

Sample restricted to migrant/recipient-household pairs that completed the follow-up survey in Mar - Jun 2009

Dependent variable:	Savings reported in follow-up survey...									Savings at partner bank in Mar-Jun 2009 ..			
	... by El Salvador household ...				... by DC migrant ...					... by El Salvador household + DC migrant in total	...in migrant accounts	...in recipient accounts	...in migrant + recipient accounts
	... at partner bank	... in non-partner bank	... in cash, not in banks	... in total	... at partner bank (in El Salvador)	... at other banks in El Salvador	... in US banks	... in cash, not in banks	... in total				
(a)	(b)	(c)	(d) = (a) + (b) + (c)	(e)	(f)	(g)	(h)	(i) = (e) + (f) + (g) + (h)	(j) = (d) + (i) - overlapping reports of joint accounts	(k)	(l)	(k) + (l)	
Treatment 3 (joint account + indiv. migrant account)	128.37 (86.69)	424.11 (310.44)	16.62 (24.74)	569.10* (323.81)	-34.70 (93.96)	245.61 (261.72)	425.64* (219.19)	-89.11 (78.08)	636.55* (350.27)	1,072.51** (418.18)	79.04 (91.94)	322.22 (246.34)	401.25 (261.48)
Treatment 2 (joint account)	-43.34 (85.78)	532.21* (307.17)	27.79 (24.48)	516.66 (320.40)	101.90 (92.97)	361.85 (258.96)	172.23 (216.88)	-20.18 (77.26)	635.98* (346.58)	754.86* (413.78)	105.30 (90.97)	-108.46 (243.75)	-3.17 (258.72)
Treatment 1 (remittance recipient account)	146.76* (88.59)	107.00 (317.24)	6.81 (25.28)	260.57 (330.90)	-94.32 (96.02)	112.12 (267.45)	194.49 (223.99)	2.34 (79.79)	212.28 (357.94)	436.11 (427.34)	30.26 (93.95)	155.46 (251.74)	185.71 (267.20)
Marketer fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Treatment month fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Stratification cell fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls for pre-treatment savings	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	652	652	652	652	652	652	652	652	652	652	652	652	652
R-squared	0.127	0.047	0.048	0.051	0.134	0.057	0.101	0.092	0.083	0.082	0.605	0.887	0.877
P-value of F-test: equality of ...													
Treatment 3 & 2 coeffs.	0.0373	0.7139	0.6344	0.8646	0.1261	0.6400	0.2237	0.3527	0.9986	0.4239	0.7636	0.0660	0.1037
Treatment 3 & 1 coeffs.	0.8312	0.3049	0.6903	0.3386	0.5238	0.6083	0.2895	0.2395	0.2239	0.1266	0.5940	0.4965	0.4076
Treatment 2 & 1 coeffs.	0.0251	0.1613	0.3855	0.4184	0.0329	0.3290	0.9172	0.7679	0.2160	0.4355	0.4037	0.2731	0.4599
Mean of dep. var. in control group	78.17	117.37	8.48	204.03	68.19	258.52	351.00	111.42	677.71	786.58	113.87	480.58	594.44

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

Notes -- Follow-up survey administered from Mar - Jun 2009. Dependent variable is in US dollars. Omitted treatment indicator is for Treatment 0 (control group). Marketer fixed effects are for the specific individual (out of 9) who conducted the marketing visit. Fixed effects for stratification cell are for each of 48 unique combinations of stratification variables: gender (male/female), having a US bank account (yes/no), relationship to remittance recipient (parent/child/spouse/other), and years in US category (0-5 years/6-10 years/11-15 years). Treatment months are Nov 2007 through Jul 2008 inclusive. Savings figure in column (j) avoids double-counting of savings held in joint migrant/recipient-household accounts and reported by both parties. Savings in partner bank accounts are average end-of-month balances from internal bank databases from Mar - Jun 2009 inclusive.

**Table 8: Impact of treatments on migrant remittances**  
(Ordinary least-squares estimates)

Dependent variable:	Remittances sent by migrant to primary remittance recipient through partner bank in given month after treatment:								
	1 month	2 months	3 months	4 months	5 months	6 months	Cumulative total, months 1-6	Cumulative total, months 1-6	Cumulative total, months 1-6
Sample:	Full	Full	Full	Full	Full	Full	Full	Follow-up survey sample of Tables 6-7	Observations not in Tables 6-7
	(a)	(b)	(c)	(d)	(e)	(f)	(g) = (a) + (b) + (c) + (d) + (e) + (f)	(h)	(i)
Treatment 3 (joint account + indiv. migrant account)	27.59 (45.74)	96.08** (46.73)	37.09 (61.79)	71.77 (46.55)	95.31* (56.30)	126.65*** (46.27)	454.49** (220.26)	557.60 (370.26)	367.47 (270.11)
Treatment 2 (joint account)	69.44 (45.48)	97.24** (46.46)	146.52** (61.44)	103.79** (46.28)	100.71* (55.98)	163.26*** (46.01)	680.96*** (218.99)	840.84** (366.36)	572.94** (271.57)
Treatment 1 (remittance recipient account)	38.80 (46.03)	83.18* (47.02)	62.26 (62.17)	57.69 (46.83)	72.72 (56.65)	63.61 (46.56)	378.26* (221.62)	162.90 (378.37)	336.45 (271.62)
Marketer fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Treatment month fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Stratification cell fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls for pre-treatment savings	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1451	1451	1451	1451	1451	1451	1451	652	799
R-squared	0.142	0.154	0.083	0.114	0.096	0.133	0.178	0.236	0.221
P-value of F-test: equality of ...									
Treatment 3 & 2 coeffs.	0.3532	0.9800	0.0725	0.4852	0.9224	0.4222	0.2969	0.4206	0.4545
Treatment 3 & 1 coeffs.	0.8069	0.7833	0.6847	0.7630	0.6892	0.1746	0.7301	0.2843	0.9097
Treatment 2 & 1 coeffs.	0.5011	0.7626	0.1710	0.3199	0.6175	0.0307	0.1676	0.0613	0.3924
Mean of dep. var. in control group	215.00	181.09	210.38	183.79	186.53	158.49	1,135.28	1,171.62	1,108.71

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

**Notes** -- Dependent variable is in US dollars. Omitted treatment indicator is for Treatment 0 (control group). Marketer fixed effects are for the specific individual (out of 9) who conducted the marketing visit. Fixed effects for stratification cell are for each of 48 unique combinations of stratification variables: gender (male/female), having a US bank account (yes/no), relationship to remittance recipient (parent/child/spouse/other), and years in US category (0-5 years/6-10 years/11-15 years). Treatment months are Nov 2007 through Jul 2008 inclusive. Remittance figures are totals across all separate transactions in given months, and are from internal partner bank databases.

**Table 9: Impact of treatments on migrant savings by reported ownership (from follow-up survey data)**

(Ordinary least-squares estimates)

Sample restricted to migrant/recipient-household pairs that completed the follow-up survey in Mar - Jun 2009

Dependent variable: Savings in bank accounts reported by DC-based migrant in follow-up survey

<u>Migrant reports self as an owner of the account?</u>	No	Yes
	(a)	(b)
Treatment 3 (joint account + indiv. migrant account)	148.79 (210.06)	572.67** (254.47)
Treatment 2 (joint account)	354.18* (207.84)	221.81 (251.79)
Treatment 1 (remittance recipient account)	-12.12 (214.66)	170.16 (260.05)
Marketer fixed effects	Y	Y
Treatment month fixed effects	Y	Y
Stratification cell fixed effects	Y	Y
Controls for pre-treatment savings	Y	Y
Observations	652	652
R-squared	0.047	0.098
P-value of F-test: equality of ...		
Treatment 3 & 2 coeffs.	0.3034	0.1469
Treatment 3 & 1 coeffs.	0.4416	0.1124
Treatment 2 & 1 coeffs.	0.0747	0.8355
Mean of dep. var. in control group	101.61	451.77

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

Notes -- Follow-up survey administered from Mar - Jun 2009. Dependent variable is in US dollars. Omitted treatment indicator is for Treatment 0 (control group). Marketer fixed effects are for the specific individual (out of 9) who conducted the marketing visit. Fixed effects for stratification cell are for each of 48 unique combinations of stratification variables: gender (male/female), having a US bank account (yes/no), relationship to remittance recipient (parent/child/spouse/other), and years in US category (0-5 years/6-10 years/11-15 years). Treatment months are Nov 2007 through Jul 2008 inclusive.

**Appendix Table 1: Impact of treatments on failure to complete follow-up surveys (Mar - Jun 2009)**  
(Ordinary least-squares estimates)

Dependent variable: Indicator for attrition of observation from follow-up survey sample

	(a)	(b)
Treatment 3 (joint account + indiv. migrant account)	-0.03 (0.04)	-0.01 (0.04)
Treatment 2 (joint account)	-0.06* (0.04)	-0.05 (0.04)
Treatment 1 (remittance recipient account)	-0.01 (0.04)	-0.01 (0.04)
Constant	0.58*** (0.03)	0.72*** (0.20)
Marketer fixed effects		Y
Treatment month fixed effects		Y
Stratification cell fixed effects		Y
Controls for pre-treatment savings		Y
Observations	1451	1451
R-squared	0.002	0.089
P-value of F-test: equality of ...		
Treatment 3 & 2 coeffs.	0.3218	0.3653
Treatment 3 & 1 coeffs.	0.7179	0.8059
Treatment 2 & 1 coeffs.	0.1788	0.2528
Mean of dep. var. in control group	0.58	

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

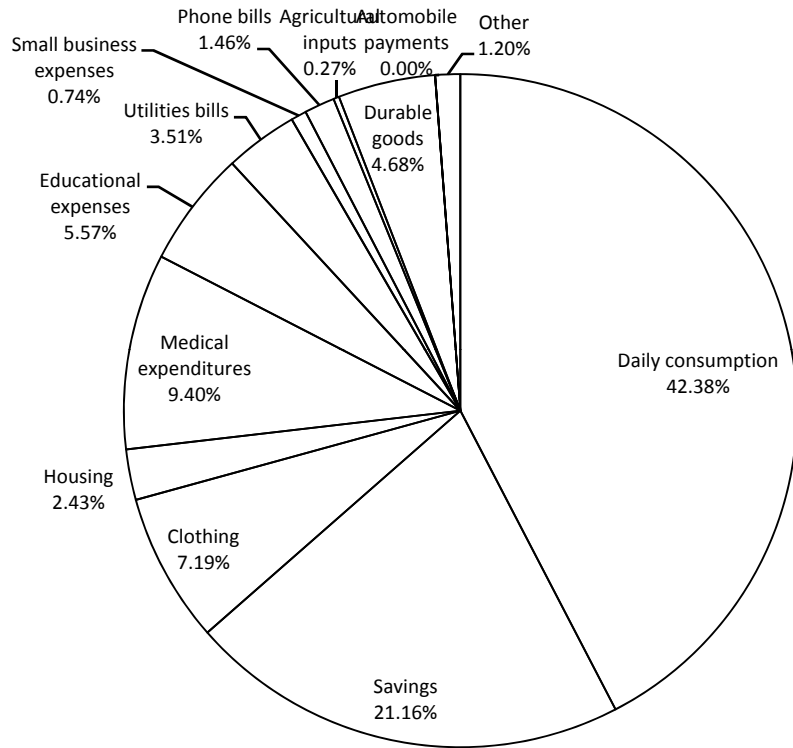
Notes -- Follow-up survey administered from Mar - Jun 2009. Omitted treatment indicator is for Treatment 0 (control group). Marketer fixed effects are for the specific individual (out of 9) who conducted the marketing visit. Fixed effects for stratification cell are for each of 48 unique combinations of stratification variables: gender (male/female), having a US bank account (yes/no), relationship to remittance recipient (parent/child/spouse/other), and years in US category (0-5 years/6-10 years/11-15 years). Treatment months are Nov 2007 through Jul 2008 inclusive.

**Appendix Table 2: Means of baseline variables by subsample**

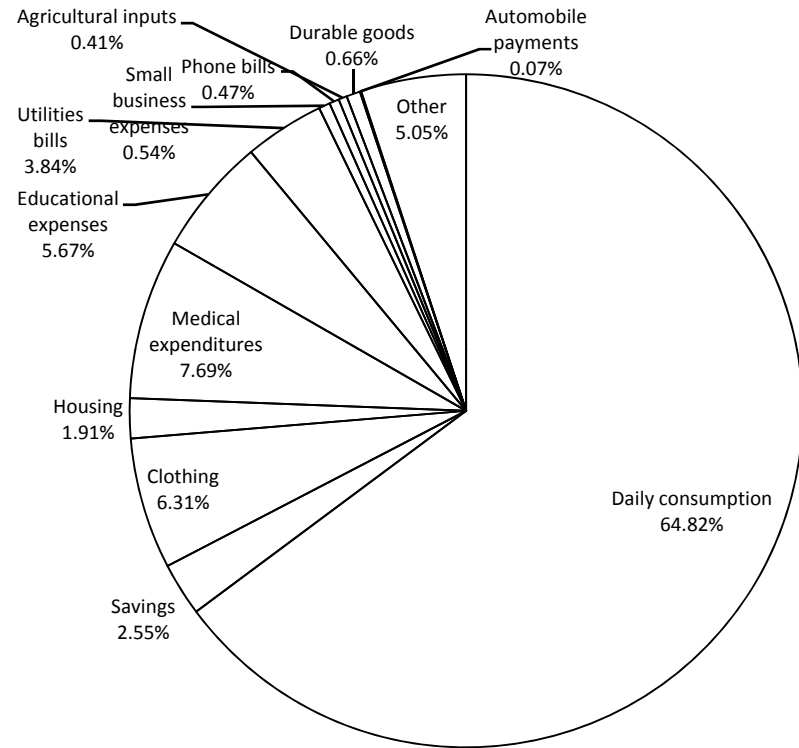
	Subsample		P-value: test of equality of means
	Migrant/recipient- household pairs that completed the follow- up survey in Mar - Jun 2009	All other observations	
<u>Variables collected for all study participants</u>			
Migrant is female	0.30	0.29	0.679
Migrant has US bank account	0.67	0.67	0.932
Recipient is migrant's parent	0.51	0.48	0.235
Recipient is migrant's spouse	0.14	0.13	0.527
Recipient is migrant's child	0.05	0.06	0.378
Recipient is migrant's other relative	0.30	0.33	0.185
Migrant has been in US 0-5 years	0.50	0.51	0.539
Migrant has been in US 6-10 years	0.36	0.34	0.444
Migrant has been in US 11-15 years	0.14	0.14	0.800
Migrant allocation to savings in raffle	20.55	21.25	0.742
<u>Baseline survey variables</u>			
Migrant's years in the US	5.49	5.66	0.490
Migrant has US bank account only	0.53	0.53	0.962
Migrant has El Salvador bank account only	0.07	0.08	0.466
Migrant has account in both US and El Salvador	0.10	0.10	0.912
Migrant's annual income (US\$)	25,739	25,681	0.988
Migrant's household's annual income (US\$)	41,433	35,567	0.330
Migrant's years of education	8.67	8.93	0.426
Migrant's age	31.32	30.46	0.094
Migrant's annual remittances sent (US\$)	5,210	4,759	0.101
Migrant's remittances as share of annual hh inc.	0.47	0.56	0.626
Migrant's total hh savings balance (US\$)	2,483	3,246	0.034
Migrant's savings as share of annual hh inc.	0.15	0.14	0.826
Migrant is US citizen	0.007	0.007	0.978
Migrant hh size in U.S.	5.00	4.61	0.006
Migrant is married or partnered	0.61	0.57	0.186
Migrant is coresident with spouse/partner	0.73	0.73	0.922
Recipient allocation to savings in raffle	1.81	3.50	0.137
Recipient's annual hh income (US\$)	20,124	6,470	0.300
Recipient has savings account	0.21	0.19	0.552
Recipient total hh savings balance (US\$)	501	228	0.034
Recipient's years of education	5.10	5.16	0.889
Recipient's age	46.70	47.02	0.772
Recipient's annual remittances received (US\$)	3,391	2,855	0.010
Recipient hh size	4.69	4.49	0.254
<u>Savings data from partner bank</u>			
Savings in recipient accounts, Aug-Oct 2007	754	348	0.066
Savings in migrant accounts, Aug-Oct 2007	89	119	0.522

Notes -- Table presents means of key variables for each treatment group prior to treatment. P-value is for F-test of equality of means across treatment groups. The first 9 variables listed in table are stratification variables: migrants were first sorted into 48 cells (based on gender, US bank account ownership, relationship to remittance recipient, and years on US category) before randomization into treatments 0, 1, 2, or 3. Migrant allocation to savings in raffle also was recorded for all DC migrant respondents. Savings figures reported in US dollars. See Table 1 for other notes. For stratification and partner bank savings variables, follow-up sample N=652, and all other observations sample N=799. Sample sizes smaller for comparison of baseline survey variables.

**Figure 1: Allocations of \$100 raffle winnings**



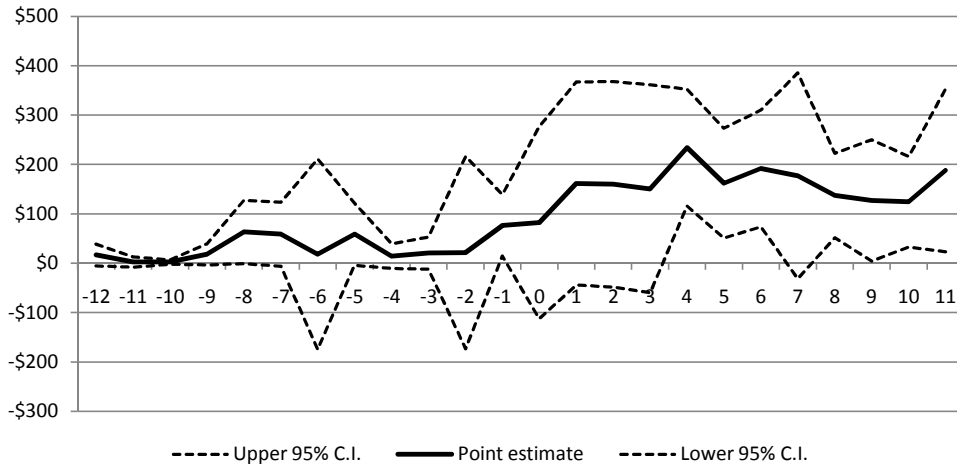
Migrant



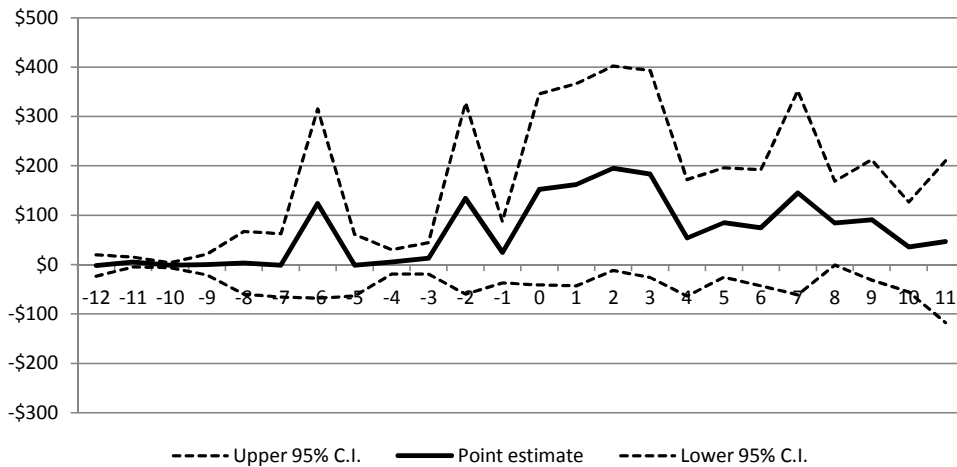
Remittance recipient

**Figure 2:** Impact of Treatments on Savings in Recipient Project Accounts, by Month Relative to Treatment Month

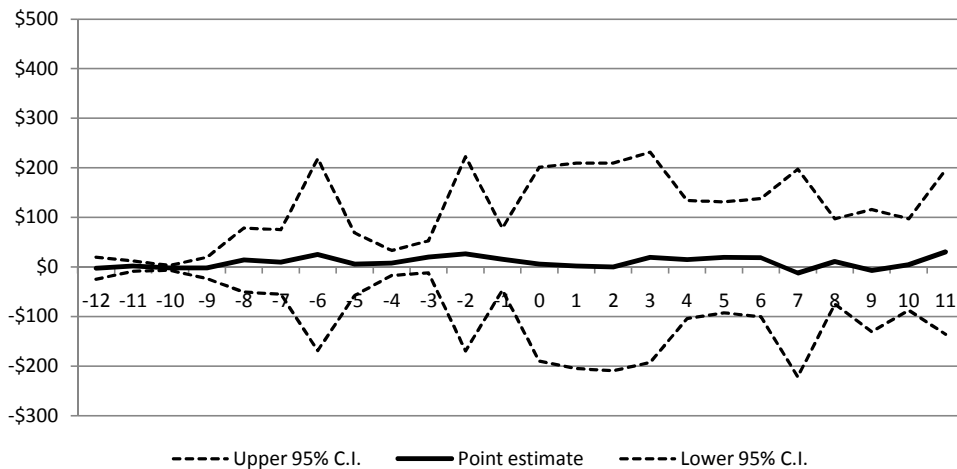
**Treatment 3**



**Treatment 2**



**Treatment 1**



**Notes:** Figures plot coefficients on indicators for Treatments 3, 2, and 1 in regressions analogous to column 2 of Table 5, but for savings in different months before and after treatment. Treatment month is month 0, one month prior to treatment is month -1, one month after treatment is month 1, etc. One regression is run for each month relative to treatment, from month -12 to month 11 (24 total regressions). For months -12 to -1, controls for pre-project savings are average end-of-month balances in partner bank accounts from Aug-Oct 2006, while for months 0 to 11 the corresponding controls are from Aug-Oct 2007. Treatments occurred from Nov 2007 to July 2008. Solid line is point estimate, and dashed lines are 95% confidence intervals.