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

Nava Ashraf
Harvard Business School

Diego Aycinena
Francisco Marroquin University

Claudia Martínez A.
University of Chile

Dean Yang*
University of Michigan

December 2011

Abstract

While remittance flows to developing countries are very large, it is unknown whether migrants desire more control over how remittances are used. This research uses a randomized field experiment to investigate the importance of migrant control over the use of remittances. In partnership with a Salvadoran bank, we offered US-based migrants from El Salvador bank accounts in their home country into which they could send remittances. We randomly varied migrant control over El Salvador-based savings by offering different types of accounts across treatment groups. Migrants offered the greatest degree of control over savings accumulated the most savings at the partner bank, compared to others offered less or no control over savings. Effects of this treatment on savings are concentrated among migrants who expressed demand for control over remittances in the baseline survey. We also find positive spillovers of our savings intervention in the form of increased savings at other banks (specifically, banks in the U.S.). We interpret the effects we find as arising from the joint effect of the bank account offers and the marketing pitch made to study participants by our project staff.

Keywords: migration, remittances, intrahousehold allocation, savings

JEL codes: F22, O16

---

* Corresponding author. Email: deanyang@umich.edu. We thank the core members of the project team at ESSMF (Angela Gonzalez, Michelle Guevara, Ronald Luna, Amaris Rodriguez, and Eric Rubin), at FUSADES (Margarita Sanfeliu and Mauricio Shi), and at Banco Agricola (Gustavo Denys, Carla de Espinoza, Mauricio Gallardo, Sabina Lopez, Ernesto Magana, Katya O'Byrne, and Paul Ponce). We greatly appreciate the collaboration of Enilson Solano and the El Salvador embassy in Washington DC. We received valuable feedback and suggestions from Manuel Agosin, Natasha Bajuk, Catia Batista, Charlie Brown, Michael Clemens, Angus Deaton, Esther Duflo, Suzanne Duryea, Jon Guryan, Ricardo Hausmann, Gabriela Inchauste, Takatoshi Kamezawa, Michael Kremer, Steve Levitt, John List, Adriana Lleras-Muney, Ernesto Lopez-Cordova, Osmel Manzano, Doug Massey, Margarita Mooney, Hugo Ñopo, Chris Paxson, Alejandro Portes, Jesse Rothstein, Jesse Shapiro, Ernesto Stein, Mel Stephens, Don Terry, Steve Wilson, Viviana Zelizer, and participants in several seminars. Fernando Balzaretti, Sebastian Calonico, and Cristian Sanchez provided excellent research assistance. This research was made possible by financial support from the John D. and Catherine T. MacArthur Foundation, the Inter-American Development Bank, the National Science Foundation, the Multilateral Investment Fund, the Empowerment Lab at Harvard University's Center for International Development, and the University of Michigan's International Policy Center. Dean Yang acknowledges research support from National Science Foundation award SES-0851570.

I. Introduction

Attempts to understand the extent and nature of conflict between household members are central to research on the economics of the family. Many 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.[1] Models that take explicit account of potential preference differences among household members include Manser and Brown (1980), McElroy and Horney (1981), and Lundberg and Pollak (1993). Browning and Chiappori (1998) provide empirical evidence rejecting the unitary model, but in favor of household efficiency in resource allocation. On the other hand, evidence of productive inefficiencies in intra-household allocation has been found in a variety of contexts.[2]

A leading candidate explanation for observed inefficiencies is asymmetry of information in the household, which reduces the ability of household members to enforce mutually-beneficial cooperative agreements among themselves.[3] 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. If migrants do not share the same financial objectives as family members remaining back home, remittances (funds sent by migrants to family members back home) may be lower than under perfect information, and the use of remittances may diverge from uses preferred by migrants.[4]

An improved understanding of financial decision-making within migrant households is important because the remittances sent home by international migrants are very large in magnitude. In 2009, migrant remittances sent to developing countries amounted to US$307 billion. By contrast, developing country receipts of foreign direct investment (the largest type of international financial flow going to the developing world) were only less than a fifth higher in that year ($359 billion). Receipts of official development assistance (foreign aid) came in a poor

[1] See the review in Strauss and Thomas (1995), as well as Duflo (2003), Rangel (2006), and Martinez (2006).
[2] See Udry (1996), Dercon and Krishnan (2000), Goldstein, de Janvry, and Sadoulet (2005), and Dubois and Ligon (2005), among others.
[3] Ashraf (2009) shows that individual saving decisions change when observed by one's spouse. Recent work on the savings and risk-sharing consequences of intra-household preference differences and asymmetric information includes Schaner (2011), Kinnan (2011), and Hertzberg (2011).
[4] In analyses of observational data, Chen (2006) and De Laat (2008) find empirical patterns consistent with migrant monitoring of spouses in origin areas, among domestic migrants in Kenya and China respectively.

third to remittances and FDI in 2009, amounting to just $127 billion.[5] Motivated by their large magnitudes, international financial institutions and developing country governments are keenly interested in finding policies that can stimulate remittances and enhance their development impacts.[6]

The substantial policy interest in remittances stands in stark contrast to the limited empirical evidence that can help guide policy formulation.[7] The development economics literature on intra-household decision-making suggests questions related to remittances that are of general economic interest, and policy-relevant. To what extent do migrants seek to monitor and control how remittances are used by recipients? In particular, if given the opportunity to do so, would migrants seek to exert control over the fraction of remittances that are saved, rather than consumed immediately? If migrants demand savings in the home country, does this take the form of joint savings with remittance recipients, or individual savings for the migrant alone? To promote savings accumulation out of remittances, it enough to simply provide migrants with tools that enable monitoring and control, or do they need to be coupled with "marketing" interventions aimed at encouraging migrants to exert such influence in the first place?

We report the results from a randomized field experiment that sheds light on these questions. The experiment was carried out among U.S.-based migrants from El Salvador, and randomizes offers of financial tools that improve the ability of migrants to ensure that remittances are deposited and accumulated in savings accounts in the home country.[8] In survey data we collected, Salvadoran migrants report that they would like recipient households to save 21.2% of remittance receipts, while recipients prefer to save only 2.6% of receipts. Migrants often intend savings to be for future use by the recipient household, but such savings also can be intended for the migrants themselves. 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. to be relatively insecure (particularly for undocumented migrants who fear deportation and loss of their assets).

---

[5] Data are from World Development Indicators 2011. The 2008-09 financial crisis had a substantial negative impact on FDI flows, while remittances and ODA were by contrast relatively stable. In 2007, the year prior to the crisis, FDI, remittance, and ODA flows to developing countries were $516, $278, and $107 billion, respectively.

[6] Recent reports on remittances funded by the Inter-American Development Bank include Pew Hispanic Center (2002) and Terry and Wilson (2005). World Bank publications include World Bank (2006) and World Bank (2007).

[7] See Yang (2011) for a review of the state of research on the economics of migrant remittances.

[8] Chin, Wilcox, and Karkoviata (2010) is a related experimental study of savings among Mexican immigrants in Texas. They find that immigrants are more likely to open U.S. savings accounts, accumulate more savings in the U.S., and remit less to Mexico when they are helped obtain a form of I.D. (a *matricula consular* issued by the Mexican consulate) that they can use when opening U.S. bank accounts.

Migrants in the study were randomly assigned to a control group or to one of three treatment conditions that provided them varying levels of monitoring and control over savings in El Salvador. Comparisons across the various treatment conditions reveal the causal impact of offering varying degrees of control on our outcomes of interest (which include savings account take-up, savings balances, and remittances). Our comparison group is referred to as Treatment 0, and received no offer of any new financial products. In Treatment 1, migrants were offered the opportunity to open an account in El Salvador in the name of the remittance recipient. Treatment 2 offered the migrant the opportunity to open an account to be held jointly by the migrant and the recipient. Finally, in Treatment 3 migrants were offered, in addition to the joint account offered in Treatment 2, the option to open an account in the migrant's name only. This third option offered the migrant the greatest degree of monitoring and control over remittances sent to El Salvador.[9] Each treatment was accompanied by a marketing pitch delivered by our project staff. In keeping with the products being offered, the content of the scripts also differed across treatments, with Treatment 3 having the strongest emphasis on exerting control over one's own finances and the finances of remittance recipients.[10] We thus cannot separate the effect of the product offers from the effect of the marketing that was tied to the product offers. Data on financial transactions at our partner bank come from the bank's administrative records. Baseline and follow-up surveys administered to both migrants in the U.S. and their remittance-receiving households in El Salvador provide data on other outcomes.

Our results provide evidence that a desire for monitoring and control over remittance uses—in particular, 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 savings accounts, and accumulated more savings in El Salvador, if they were assigned to the treatment condition (Treatment 3) offering the greatest degree of monitoring and control.

Among migrants assigned to Treatment 3, total savings in new accounts established at our partner bank 6 months after treatment were $211 higher than savings in Treatment 0, the comparison group that was offered no new savings facilities. The effect of Treatment 3 is also

[9] In Treatments 2 and 3, upon request migrants would also have been allowed to open an account for the remittance recipient only (the account offered in Treatment 1). No migrants made such a request.

[10] Moving from Treatments 0 to 3, marketing pitch content was only added (never subtracted), so the marketing pitches were "nested" in the same way that the product offers were.

statistically significantly larger than the effects of other treatments that offered migrants less control over savings. This $211 increase in savings due to Treatment 3 is large relative to $382 in average savings reported by El Salvador remittance-recipient households in our baseline survey, and is about 7% of mean baseline savings reported by migrants. This increase in savings in the new accounts we offered is likely to be a true increase in savings, since we do not find any evidence (from analysis of our survey data) that these funds were simply shifted over from other types of savings.

Strikingly, Treatment 1 (where we offered accounts in the name of remittance recipients alone) had a much smaller impact on savings accumulation. This result is also important, as it reveals that the frequently-made policy recommendation to foster savings in migrants' home countries by encouraging migrants to remit directly into savings accounts of remittance recipients would be much less effective, compared to interventions that also improved and encouraged migrant monitoring and control over home-country savings.

We also provide additional evidence to support the idea that the increases in savings due to Treatment 3 are due to improvements in migrant ability to control recipient savings in El Salvador. We show that savings increases in recipient accounts at the partner bank are concentrated among migrants who revealed a demand for control over remittance uses in the baseline (pre-treatment) survey (for example, among migrants who had previously sent funds to El Salvador for others to administer, or who were aware of disagreements between migrants and recipients over the use of remittances).

The majority of the impact of Treatment 3 on savings ($147 out of the $211 mentioned above) is on savings in the joint accounts shared by migrants and remittance recipients (with the remainder of the increase accounted for by savings in the migrant-only accounts). The joint account was also offered in Treatment 2, but in that treatment there was no statistically significant increase in joint account savings (and a much smaller point estimate). This difference in effects of Treatments 2 and 3 is likely due to the fact that in Treatment 3 the marketing pitch made a much greater emphasis on control over savings.[11]

Supporting evidence for this interpretation is that the effect of Treatment 3 on savings is smaller among migrants with higher levels of financial literacy at baseline. This pattern suggests

[11] See Section 2 and Appendix B for details on differences in marketing pitches across treatments.

that those who had less financial literacy to begin with may have been most affected by the marketing message to take control of the joint account, and to save more for themselves.

We believe the most plausible interpretation of our results is that Treatment 3's effect on savings is the *joint* effect of 1) providing access to bank accounts in El Salvador, and 2) our marketing staff's encouragement of migrants to exert control over El Salvador-based savings. In the paper, we refer to the impact stemming from the marketing pitch as an increase in "financial empowerment." We intend financial empowerment to represent the *willingness* to use available financial services to achieve one's financial objectives.[12,13]

If financial empowerment was really an important factor behind Treatment 3's impacts, one might expect to find additional effects, potentially beyond savings in the accounts we offered. As it turns out, in data from our follow-up survey does reveal that, among migrants who express baseline demand for control, Treatment 3 also led to a large increase in savings at other non-partner institutions, mainly banks in the U.S. This finding provides as additional support for our claim that Treatment 3's effect derives in part from increases in financial empowerment.

The remainder of this paper is organized as follows. Section 2 provides details on the study design. Section 3 describes the characteristics of the sample. Section 4 presents the main empirical results. Section 5 provides discussion and additional analyses meant to clarify interpretation of the results. Section 6 concludes.

II. Study Design

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. faced difficulty opening them from outside El Salvador (in the case of Treatment 1).

[12] While we acknowledge the term "financial empowerment" is not well-defined in the economics literature, we prefer it to the term "financial literacy." We view the concept as distinct from financial literacy, which generally refers to knowledge about personal financial services or the ability to make personal finance calculations. That said, in general financial empowerment is likely to be correlated with financial literacy, and financial literacy interventions could also affect financial empowerment.

[13] We also provide suggestive evidence below that the effect of Treatment 3 does not derive from *only* the marketing pitch. In sum, joint account savings at *other* banks (aside from our partner bank) are not affected by Treatment 3. We interpret this as evidence that both the marketing pitch and our offer of the accounts at our partner bank were necessary to produce the effects on savings we observe.

Migrants were randomly assigned to one of three treatment groups or a comparison group, each with equal (25%) probability. We randomized after stratifying migrants into 48 cells representing unique combinations four baseline categorical variables: gender (male, female), US bank account ownership (yes, no), primary remittance recipient's relationship to migrant (parent, spouse, child, other), and years in US category (0-5, 6-10, 11-15).

Migrants in Washington, DC were invited to participate in a marketing visit where our treatments were administered. Migrants in the comparison group (labeled Treatment 0) were not offered any new products.[14] The three treatment groups were labeled 1, 2, and 3. The presence of the comparison group allows us to observe remittances and 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. We also describe below the substantive content of the marketing pitches administered in each treatment. Details on enrollment of study participants are provided in Section 3 and Appendix A, and the specific marketing scripts can be found in Appendix B.

Treatment 0 (comparison group): Encouragement to remit into bank account of remittance recipient

Migrants in this condition were visited by a marketer who encouraged them to remit into El Salvador bank accounts. Marketers emphasized the benefits of remitting funds directly into accounts and of remittance-recipient access to funds via ATM/debit cards (rather than having to wait in a teller line to receive a remittance). Migrants were offered the VIP card (and the discount explained), but were not offered any new savings facilities.

This generic pitch to remit into bank 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 encouragement provided by the marketers to remit into bank accounts in El Salvador.

Treatment 1: Offer of account for remittance recipient

In Treatment 1, marketers also emphasized the same benefits of remitting into bank accounts (as in Treatment 0), and provided the VIP card. But unlike in Treatment 0, in Treatment

[14] Because this study investigates control over savings, to avoid confusion we refer to Treatment 0 as the "comparison group," not as the "control group."

1 this was combined with an offer of assistance in setting up an account in the name of the remittance recipient, into which the migrant could remit. Relative to Treatment 0, the Treatment 1 marketing pitch also added a brief comment that "savings for your remittance recipient in El Salvador" was a benefit of the Treatment 1 offer (but with no other elaboration on the general benefits of bank accounts).

Migrants could identify anyone in El Salvador as the account holder (not just the "primary remittance recipient" to whom the baseline survey was administered.) 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.[15] Within the next few days, project staff arranged by phone for the individual in El Salvador to meet with the branch manager of the nearest Banco Agricola branch in El Salvador to complete the final account-opening procedures in person.

Effects of Treatment 1 on take-up and savings accumulation (vis-à-vis Treatment 0) would reflect the impact of offering assistance with account-opening procedures. Because the account offered in Treatment 1 is in the name of someone in El Salvador, any impacts found could not be due to changes in the migrant's ability to monitor or control savings balances. Even if it failed to offer migrants greater monitoring or control, migrants might have found the account offered in Treatment 1 attractive if they wanted to use a recipient's savings account as a safe and convenient destination for remittances to that recipient.

Treatment 2: Offer of joint account for migrant and remittance recipient

In Treatment 2, 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, withdraw with their ATM card via US ATMs, and check the balance on the account by calling a toll-free U.S. telephone number. Joint account owners in El Salvador could deposit and withdraw using their ATM cards or via bank tellers.

[15] 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 substantive content conveyed by the marketing pitch in Treatment 1 was also conveyed in Treatment 2, but in addition, the Treatment 2 marketing pitch also noted that both the migrant and the remittance recipient could verify the balance on the Cuenta Unidos account, and that the migrant could withdraw funds from the account from the U.S.

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). Migrants could identify anyone in El Salvador as the joint account holder. If migrants asked, they had the option to *not* have joint ownership of the new account (in other words, they could replicate the account offered in Treatment 1).[16]

Compared to Treatment 1, Treatment 2 offered migrants the ability to monitor the savings of family members, but did not provide full control over the funds. The joint account holder in El Salvador had complete freedom to withdraw the entire savings balance from the account.

Treatment 3: Offer of joint account for migrant and remittance recipient, plus account in migrant's name alone

Treatment 3 nests Treatment 2, while adding an additional savings facility: an account exclusively in the migrant's name, known as "Ahorro Directo" (also newly designed by the project). This is 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 this marketing visit, Cuenta Unidos and Ahorro Directo were offered to the migrant in sequence. Cuenta Unidos was offered first, using a marketing script identical to the one used for Treatment 2. The marketing script for Ahorro Directo, which followed, emphasized its usefulness for exclusive control over funds, since the account would not be shared with anyone else. The script noted that no one other than the client (not even the remittance recipient in El Salvador) would be able to check account balances, have access to the account, or even know of the existence of the account. The script also noted that no intermediaries (e.g., family members) would be needed for the client to save in El Salvador. In addition, the script noted the benefit of improved security if visiting El Salvador by reducing the need to carry large amounts of cash.

[16] However, perhaps tellingly, all accounts we assisted in opening in Treatment 2 were joint accounts: in not a single case did a migrant request to forego joint ownership and open an account solely in the name of the remittance recipient in El Salvador.

For the purpose of the study, it is important to be able to rule out that any differences across Treatments 2 and 3 are due to differences in transaction costs. Therefore, in Treatment 3, if migrants wanted to open an Ahorro Directo account, we required them to *also* open a Cuenta Unidos account, ensuring that account opening transaction costs were identical across Treatments 2 and 3.[17] In addition, migrants were allowed to open an account only in the name of a beneficiary in El Salvador (as in Treatment 1) if they requested it.[18]

In sum, Treatment 3 offered the migrant the greatest ability to control funds in savings accounts in El Salvador, unlike Treatment 2 where ownership had to be joint with someone else. The difference in takeup and savings in Treatment 3 vs. Treatment 2 reveals the incremental impact of offering migrants the ability to exclusively control their El Salvador savings balances.

Estimation Strategy

Dependent variables of interest in this paper are take-up rates, savings, and remittances. Let Y_i be the dependent variable of interest (say, El Salvador savings of the remittance recipient). 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:

$$Y_i = \delta + \alpha_1 Z1_i + \alpha_2 Z2_i + \alpha_3 Z3_i + \boldsymbol{X_i}'\boldsymbol{\phi} + \mu_i \qquad (1)$$

Coefficients α_1, α_2, and α_3 are the impact on the dependent variable of Treatments 1, 2, and 3 (respectively). We focus on intent to treat (ITT) effects, and so are evaluating the effect of *offering* (rather than opening) the various accounts. For all coefficient estimates, we report robust (Huber/White) standard errors that account for sample stratification.

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

[17] 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 there to 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 improving clarity of interpretation was worth the sacrifice of potentially lower take-up in Treatment 3. Note that in Treatment 1, the individual in whose name the account was opened also had to go to a branch in El Salvador, so transaction costs are also equalized with Treatment 1.

[18] 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.

vis-à-vis Treatment 1. $\boldsymbol{X_i}$ is a vector of fixed effects (for marketer, stratification cell, and month of initial marketing visit). μ_i is a mean-zero error term.

III. Sample overview and summary statistics

The sample consists of migrants from El Salvador who were enrolled into the study at Salvadoran consular locations in Washington, DC, completed a baseline survey, and agreed to a later marketing visit carried out by a project team member. From June 2007 to January 2008, migrants were intercepted at one of the Salvadoran consulates and invited to participate in a research project on remittances. 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. Participating migrants were administered a one-hour survey at baseline (prior to the product offer). We then attempted to survey the migrant's "primary remittance recipient" household in El Salvador.

From November 2007 to July 2008, migrants in Washington, DC were invited to participate in a marketing visit where treatments were administered. The migrant and El Salvador household follow-up surveys occurred roughly one year after the initial product offer (from March to June 2009) to measure impacts on outcomes not observed in administrative data. The follow-up survey collected data on savings outside of the partner bank as well as other migrant and household outcomes. Households in El Salvador were interviewed in person by a survey team in El Salvador. Interviews of DC-based migrants were conducted via telephone by the same survey team calling from El Salvador. See Appendix A for further details on the implementation of this study. Coinciding with the administration of the follow-up survey, data on savings and on remittances were obtained from internal databases of the partner bank.

Our primary sample for analysis, which we use to analyze impacts on savings held at the partner bank, consists of 898 DC-area migrants who completed a baseline survey as well as a marketing visit some months later. We were also able to complete an interview with 82% of the primary remittance recipients identified by the migrants surveyed at baseline. Compared to Salvadoran-born individuals in the US Census 2000, our sample is less likely to have US citizenship, has a higher fraction of males, has arrived in the US more recently, is slightly more

educated, and is more likely to be married or partnered. (For further details, see Appendix A and Appendix Table 1.)

The follow-up survey contains 508 observations with valid migrant-reported savings data. Also, for 385 observations, we have complete self-reported savings information for both the migrants and El Salvador remittance recipients. It is for this latter subset of observations that we are able to examine the impact of treatments on total savings in the integrated transnational household consisting of the DC-based migrant and the primary recipient household in El Salvador.

Characteristics of migrants and remittance-receiving households

Summary statistics are presented in Table 1. Several measures of demand for control are available in the baseline survey administered to migrants. We construct five separate indicator variables equal to one (and zero otherwise) from migrant reports of the following: a) the migrant had ever paid directly for expenditures of remittance recipients in El Salvador, rather than sending cash (7.7% of migrants did so); b) the migrant had sent funds home for others to administer on his/her behalf (23.7% of migrants did so); c) the migrant was interested in direct payments to improve control over remittance uses (20.7% of migrants said yes); d) the migrant knew anyone who had had conflict with recipients over remittance uses (14.6% of migrants said yes); e) the migrant has had conflict with his/her own remittance recipients over remittance uses (4.9% of migrants said yes). We also construct an overall indicator of "demand for control" that takes on the value of 1 if the migrant answers affirmatively to any of the five abovementioned indicator variables, and 0 otherwise. 51% of migrants report demand for control at baseline by this measure.

The baseline survey also included three questions to assess financial literacy that have been popularized by Lusardi and Mitchell (2006) and included in a number of surveys of financial decision-making worldwide.[19] 66%, 64%, and 37% of migrants responded correctly to the questions on (respectively) compound interest, inflation, and mutual funds. We also asked

[19] The questions are: 1) "Suppose that you have $100 in a savings account with a 2% annual interest rate. If you do not touch the money in this account, how much do you think you will have in five years?" (Options are "less than $102", "exactly $102", and "more than $102"; correct answer is "more than $102".); 2) "Imagine that the interest rate in the savings account where you have $100 is 1%, and that inflation is 2% per year. A year from now, would you be able to buy more, the same, or less than today with the money in the account?" (correct answer is "buy less"); and 3) "Do you think that the following statement is true or false? To buy stocks in only one company is more secure than to invest in a mutual fund" (correct answer is "false").

whether migrants tracked spending and budgeted their expenses, and 46% of migrants reported "always" or "almost always" doing so.

It is important to confirm that the randomization across treatments achieved the goal of balance in terms of pre-treatment migrant and recipient household characteristics. Appendix C discusses balancing tests (presented in appendix tables) for the full (N=898), US follow-up (N=508), and US and El Salvador follow-up (N=385) samples. With a few exceptions, we find overall balance across treatment conditions in baseline migrant and recipient household characteristics. The few exceptions are some of the pairwise tests of means across Treatments 2 and 0 for both the follow-up survey samples (but not the full sample), and may be related to the differentially lower attrition seen among Treatment 2 observations (to which we now turn).

Appendix C also discusses attrition rates from the baseline to the US follow-up survey and from baseline to the US and El Salvador follow-up surveys. Pairwise comparisons of attrition rates between Treatment 1 and Treatment 0 or Treatment 3 and Treatment 0 fail to reject the null of equality of attrition rates at conventional significance levels for both follow-up samples. However, observations in Treatment 2 have statistically significantly lower attrition rates than Treatment 0 for both types of attrition (amounting to 10 percentage points lower attrition).

We can provide no explanation for attrition being statistically significantly lower in Treatment 2 than in the comparison group. One might hypothesize that experiencing the benefits of being in one of the treatment groups might have created greater attachment to the research project and led to lower attrition, but that would not explain why the effect is confined to Treatment 2 rather than Treatment 3 (which is the only treatment that has impacts on savings, as we show later). It is possible that this difference in attrition rates arose simply by chance.

Whatever the reason for Treatment 2's lower attrition rates, it will be important to keep in mind that any statistically significant coefficients on Treatment 2 in the analyses that use the follow up surveys (Tables 6, 7, and 8) may be due to sample selection rather than a causal effect of Treatment 2.[20]

IV. Impact of Treatments on Savings at the Partner Bank

[20] Of course, this differentially lower attrition of Treatment 2 observations from the follow up surveys does not affect inference regarding Treatment 2's effect on savings at the partner bank (results in Tables 2 through 5). Partner bank savings data are obtained from administrative records, so attrition is not an issue.

In this section we examine the impact of the treatments on account opening and on savings in the special accounts we helped establish at the partner bank.

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 treatment conditions 1 through 3. The data on existence of these accounts come from our partner bank's internal databases. These are accounts that were established by this research project ("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 primary remittance recipients, which includes Cuenta Unidos (joint migrant/recipient) accounts offered in Treatments 2 and 3 as well as accounts solely in the name of recipients offered in Treatment 1, 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.

Coefficient estimates for this regression equation are reported in column 1 of Table 2. The coefficient on the constant term indicates that 4.6% of primary remittance recipients in El Salvador whose DC-based migrant was assigned to Treatment 0 (the comparison group) had a project account at Banco Agricola 6 months after treatment. Individuals in the comparison group could have only obtained one of the project accounts if they learned about their existence independently of our marketing team, and could have obtained the account opening documents by calling the partner bank's 800 number in the US.

The coefficients in column 1 on Treatments 1, 2, and 3 are all positive in sign, and are each statistically significantly different from zero at the 1% level. The coefficients indicate that recipients in Treatments 1, 2, and 3 were respectively 13.5, 15.5, and 21.7 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.[21]

[21] Due to the internal code used by the partner bank for tagging project accounts, we cannot actually differentiate between Cuenta Unidos (joint migrant/recipient) accounts and recipient-only accounts in the partner bank's internal database (unknown to us until later, the same identifier code was assigned to both types of accounts). However, we know from our project staff records that in Treatments 2 and 3, not a single migrant who opened a remittance

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 constant term in column 3 indicates that 1.8% of migrants in the comparison 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 29.3 percentage points more likely to open an Ahorro Directo account than those in the comparison 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 comparison 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 to 6-8 percentage points higher take-up of project accounts for individuals other than the primary remittance recipient.

The patterns of coefficients indicate monotonically increasing take-up of primary remittance recipient accounts as one progresses from Treatment 1 to Treatment 2 to Treatment 3. 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 statistically significantly different from the impact of Treatment 2 at the 10% level in the specification with controls, and is also statistically significantly different from the impact of Treatment 1 (at the 5% significance level in both specifications). The impact of Treatment 2 is not statistically significantly different from the impact of Treatment 1 at conventional significance levels in either specification. This qualitative pattern is also exhibited in columns 7 and 8, where the dependent variable is an indicator for opening of any type of project account.

---

recipient account in Treatments 2 or 3 opted for this account to be in the name of the remittance recipient alone. In Treatment 1, all accounts opened with the assistance of our project staff were in the name of the remittance recipient alone (consistent with instructions for that treatment). In all treatments, migrants could have found other ways of opening accounts without our assistance, and if they did so the accounts could be either joint migrant/recipient accounts or recipient-only accounts.

Impact on savings at the partner bank

We estimate equation (1) to examine the impact of the different treatment conditions on savings balances in project accounts at the partner bank. In Table 3, the dependent variables are savings balances 6 months after treatment in various subcategories of project accounts at the partner bank. In the first two columns, the dependent variable is savings in project accounts of the primary remittance recipient.[22] 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 other 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 10% level in columns 1 and 2. The coefficient in column 2 indicates that savings in recipient project accounts are higher by $147 in Treatment 3 than in the comparison group (where savings are just $13 in this type of account). In contrast, coefficients on the Treatment 1 and 2 indicators, while positive, are substantially smaller in magnitude. The coefficient on Treatment 1 is not statistically significantly different from zero in either specification. The coefficient on Treatment 2 is statistically significantly different from zero at the 10% level in the specification with control variables.

Treatment 3 also has a positive effect on savings held in migrant project accounts (Ahorro Directo). The coefficient on the Treatment 3 indicator is positive and statistically significant at the 1% level in both columns 3 and 4; savings in this type of account in Treatment 3 amount to a bit more than $50 on average. None of the treatments have an economically large effect on savings in joint accounts shared by migrants and individuals in El Salvador other than the primary remittance recipient: the treatment coefficients in columns 5 and 6, while positive, are all quite small in magnitude (although the Treatment 3 coefficient in column 5 is significant at the 10% level).

[22] As mentioned in the previous footnote, due to the ambiguity in the partner bank's database, we cannot separate savings in joint migrant/recipient project accounts from savings in recipient-only project accounts. However, due to the assistance we provided in account opening in Treatments 1, 2, and 3, it is most likely that in Treatments 2 and 3 remittance recipient accounts opened via this project are joint migrant/recipient accounts, while in Treatment 1 such accounts are for remittance recipients only. In Treatment 0, the few observed project accounts were opened without our staff's assistance so we do not know whether these are joint migrant/recipient or recipient-only accounts.

The dependent variable in the last column of the table is the sum of all savings in project accounts at the partner bank. This outcome is worth examining to the extent that one considers DC-based migrants and primary recipient households to be part of the same transnational household. The positive and significant coefficient on Treatment 3 indicates that total savings in project accounts in the combined transnational household are larger by $211. The Treatment 3 coefficient is statistically significantly different from the corresponding coefficients for Treatments 2 and 1, at the 10% and 5% significance levels, respectively. This effect is large relative to total savings reported by El Salvador recipient households in the baseline survey, which has a mean of $382, and is about 7% of the $2,851 in mean baseline savings reported by migrants (see Table 1).

Treatments 2 and 1 also have positive effects on total savings balances. In the last column of the table, the coefficient estimates indicate that Treatment 2 led to $64 higher total savings (significant at the 5% level) and Treatment 1 led to $42 higher savings (significant at the 10% level) in project accounts compared to the control group.

To provide a sense of the percentiles of the savings distribution that are contributing to these treatment effects, Figure 1 presents the cumulative distribution function of total savings in all project accounts (the dependent variable of the last two columns of Table 3). The CDF is truncated at the 75th percentile to enhance visibility.[23] The CDF for Treatment 3 is clearly shifted to the right compared to the CDFs of the other treatments, and CDFs for Treatments 2 and 1 are also clearly to the right of the Treatment 0 CDF. While treatment effects show up relatively high in the savings distribution (in the figure the Treatment 3 CDF visibly separates from the other CDFs a bit before the 85th percentile), it is far from the case that the results are driven solely by a few individuals with very high savings. The 90th percentile of savings in Treatment 3 is $180.22, while the corresponding statistics for Treatments 2, 1, and 0 are $25.03, $10.03, and $0. The corresponding statistics for the 95th percentile are $590.14, $200.09, $71.67, and $15.98, respectively.

V. Interpretation and additional analyses

[23] Recall from Table 2 that at most (in Treatment 3), only 40% of observations took up any project account, so it is expected that there are many zeros in the data. The percentage of observations with zero savings in project accounts in Treatments 3, 2, 1, and 0 is, respectively, 72.0%, 81.6%, 83.8%, and 94.5%.

The results presented so far indicate that Treatment 3 – where migrants were offered a joint account to be shared with remittance recipients as well as an account in their name alone – had a substantial impact on migrant and remittance-recipient savings at the partner bank in El Salvador. The increase in total savings summed across accounts is statistically significantly different from the impact in the comparison group (Treatment 0), and from impacts in other treatment conditions where migrants were offered only the joint account (Treatment 2) or only an account in the name of remittance recipients (Treatment 1).

We now present additional analysis to interpret the reported results. First, we provide supporting evidence that Treatment 3's impact is likely to have operated via increased control over El Salvador-based savings by demonstrating that this effect was particularly pronounced for migrants who a priori expressed an underlying interest in greater control over remittance uses. Then, we shed light on why Treatment 3's impact on savings in *recipient* accounts was larger than Treatment 2's impact on recipient-account savings. This is a surprising result that calls for an explanation, given that Treatment 3 only differed from Treatment 2 in the additional offer of the migrant-only account.

Control interpretation of Treatment 3's impact

If Treatment 3's differential effect on remittance-recipient savings occurs because migrants exerted increased control, we should see that its effect is greater among migrants who, prior to treatment, showed greater demand for control over El Salvador-based savings. This is exactly what we find: Treatment 3's effect on remittance-recipient savings is exclusively among migrants reporting greater demand for control at baseline, while Treatment 2's effect shows no corresponding heterogeneity.

The left-hand side of Table 4 presents coefficient estimates from the regression from which we come to this conclusion. The regression is analogous to that of column (a) of Table 3 where the dependent variable is savings in remittance-recipient accounts, but now treatment indicator variables are each interacted with a variety of migrant characteristics. All migrant characteristics are measured at baseline. The regression includes all controls and fixed effects included in column (a) of Table 3, as well as the main effects of all variables interacted with the

treatment indicators.[24] The first column reports interactions between Treatment 2 and the baseline variables, and the 2nd column corresponding interactions with Treatment 3.[25]

Of interest here are the coefficients on the interaction terms between Treatment 3 and the five indicator variables intended to capture migrant demand for control (previously described in Section IV above) in the top rows of the table, 2nd column. If interaction terms between the Treatment 3 indicator and indicators of demand for control are positive, this would be evidence in favor of the control interpretation.

Coefficients on four out of the five interaction terms between demand for control variables and Treatment 3 are indeed positive and large in magnitude. While none of these coefficients are statistically significantly different from zero at conventional levels, in two cases (the interactions with "sent funds to El Salvador for others to administer" and "aware of disagreements with recipients over remittance uses") the coefficients are marginally significant (with t-statistics around 1.5).

It is of interest to examine heterogeneity in the treatment effect with respect to demand for control for all types of partner bank savings variables, not just joint account savings. To reduce the number of coefficients to inspect, we now collapse the information in the five separate demand for control indicators into a single "demand for control" variable which is equal to 1 if any of the five separate indicators are equal to 1, and 0 otherwise (by this measure, 51% of migrants have demand for control). In Table 5 we report the coefficient on this interaction term between each treatment and the single demand for control variable as well as an interaction with "no demand for control" (defined as one minus the indicator for demand for control).[26] The dependent variables in Table 5 are the same dependent variables examined in Table 3. The coefficient on each interaction term should be interpreted as the effect of the given treatment on savings for migrants with or without baseline demand for control.

In column (a), the coefficient on Treatment 3 * (Demand for control) is positive and significant at the 10% level, and indicates that Treatment 3's impact on recipient savings in project accounts at the partner bank is $245 among migrants with demand for control. By contrast, the effect of Treatment 3 on this category of savings for those without demand for

[24] One of the additional included baseline variables has a missing value, causing sample size to fall by one.
[25] All interaction terms are estimated in the same regression. Corresponding interaction terms with Treatment 1 are also included in the regression, and none of these are statistically significantly different from zero (not shown).
[26] The demand for control main effect is also included in all regressions. Main effects for each treatment do not need to be included because they are fully interacted with "demand for control" and "no demand for control".

control is much smaller in magnitude and not significantly different from zero. However, the p-value of the F-test that these two effects are equal across migrants with and without demand for control is only marginally significant (p-value 0.18). (In further results to come, however, we do find statistically significant differences in the Treatment 3 effect for migrants with and without demand for control.)

The remaining columns of Table 5 present results for similar regressions, but where the dependent variables are other types of savings in project accounts. Total savings across migrant and remittance recipient accounts (the dependent variable in column d) show a pattern similar to that in column (a), although the difference in coefficients on the Treatment 3 interaction terms in this column is again not statistically significantly different from zero at conventional levels. Interestingly, unlike in column (a), in column (b) (where the dependent variable is savings in migrant-only project accounts) it is not the case that Treatment 3 has greater impact on savings among migrants with demand for control; if anything, the pattern is reversed (although the difference in the two Treatment 3 interaction term coefficients is not significant at conventional levels). We view this comparison as potentially revealing about the purposes for which the various types of savings are intended. It may be that migrants prefer to exert control over accounts to which recipients have direct access. This is consistent with migrants exerting control over savings in part to build up buffer stocks (precautionary savings) that need to be accessed quickly by primary remittance recipients in case of emergency. Savings in the migrant-only accounts, on the other hand, may be motivated by entirely different factors. For example, migrants with no desire to control the savings of remittance recipients may still want to keep some savings in El Salvador for easy access during visits home or as a safe place to keep funds in case the migrant is deported and faces difficulty accessing US bank accounts.

In the other rows of Table 5, it is quite striking that in no case are there substantial differences in the point estimates of the effects of Treatments 2 or 1 across migrants with and without demand for control. The absence of corresponding heterogeneity in the impacts of Treatments 2 or 1 with respect to demand for control also helps support the idea that Treatment 3's differential impact stems from migrants with a baseline demand for control responding to that treatment by exerting control over recipient savings.

Mechanism underlying Treatment 3's impact on remittance-recipient savings (relative to Treatment 2)

We now dig deeper and consider three potential mechanisms to explain Treatment 3's differential impact on remittance-recipient savings relative to the impact of Treatment 2. To reiterate, the puzzle to explain is that Treatment 3 raised recipient savings, while Treatment 2 did not, even if Treatment 2 also offered the joint migrant/recipient account.

The first possibility is what we refer to as the "financial empowerment hypothesis": Treatment 3's marketing pitch was more effective than Treatment 2 in convincing migrants of the importance of exerting control over savings. Migrants responded by exerting such control in both the joint accounts we offered them. The second is what we call the "selection hypothesis": differences in the composition of migrants who opened accounts in response to Treatment 3 are behind the higher savings in joint accounts, relative to Treatment 2. A third potential explanation is what we refer to as the "bargaining hypothesis": by providing the El Salvador-based migrant-only accounts, Treatment 3 increased migrants' bargaining power over savings accumulation in remittance-recipient accounts.

To presage our results, after considering the evidence for each of these candidate explanations, we conclude that the evidence favors the "financial empowerment" hypothesis.

Hypothesis 1 ("financial empowerment"): Relative to Treatment 2, Treatment 3 was more effective at convincing migrants to influence savings of remittance recipients

A possible reason why Treatment 3 had a substantial positive effect on remittance-recipient savings, while Treatment 2 did not, might be thought of as a "financial empowerment" effect: Treatment 3 simply did a better job at convincing migrants to exert control over the savings of remittance recipients. In Treatment 2, on the other hand, even though migrants also had joint accounts available to them, they were not encouraged to exert their control over those accounts. In this section, we present empirical evidence that leads us to believe that this financial empowerment effect is the best explanation for the difference between Treatment 3's and Treatment 2's impacts on remittance-recipient savings.

The marketing scripts for Treatments 2 and 3 delivered by our project staff did contain a common element: in both treatments, the joint account was presented as an account that offered migrants the ability to monitor the savings of remittance recipients. However, the offer of the individual migrant account in Treatment 3 came with additional instructions to our project staff (see Appendix B). Specifically, when offering the individual migrant account in Treatment 3, we instructed our project staff to emphasize the benefits of having an account of one's own in El

Salvador, such as exclusive control over one's savings and avoiding the need to save through intermediaries in El Salvador.

We believe that this additional discussion of control over savings in Treatment 3 is responsible for the differential effect of Treatment 3 (relative to Treatment 2) on remittance-recipient savings. One way to view this is that migrants in Treatment 3 became more "financially empowered" along a specific dimension: they became more likely to exert control over the savings of remittance recipients. We discuss below two additional pieces of evidence that support this view.

First, if the differential impact of Treatment 3 on remittance-recipient savings is due to a financial empowerment channel, we might expect Treatment 3 to have less impact on joint account savings among migrants who already have higher levels of financial literacy at baseline. Patterns of heterogeneity in the impact of Treatment 3 in Table 4 suggest that this is indeed the case. In the regression where savings in remittance recipient project accounts is the dependent variable (left-hand side of the table), a key interaction term with Treatment 3 is negative and statistically significantly different from zero (at the 10% level): the interaction with the indicator that the migrant correctly answered the financial literacy question on mutual funds (the most difficult of the financial literacy questions, answered correctly by only 37% of baseline respondents).[27]

Second, Treatment 3 affected other types of financial decisions. Specifically, it caused migrants to raise their savings in other institutions (including banks in the US), again among migrants with baseline demand for control. Panel A of Table 6 presents regression estimates of the impact of each treatment on savings reported by the migrants interviewed in the follow-up survey. The first four columns present impacts on savings reported by the DC-based migrant, (a) in El Salvador, (b) in U.S. banks, (c) in cash, and (d) in total across the previous three categories. Effects of Treatment 3 are positive and large in magnitude for savings in El Salvador, in the US, and in total, but none of these coefficients are statistically significantly different from zero at

[27] Interestingly, the coefficients on the interaction terms with the indicators for correct answers to the other two financial literacy questions are positive in sign (although neither are statistically significantly different from zero at conventional levels). These questions are easier (answered correctly by 66% and 64% of baseline respondents, respectively), and so could be answered correctly by migrants with lower levels of financial literacy than those who answered the mutual fund question correctly. With this in mind, one possible interpretation of the positive coefficients on Treatment 3 interaction terms with these variables is that the financial empowerment we provided was complementary with lower levels of financial literacy, but a substitute for higher levels of financial literacy.

conventional levels. It appears that the treatment did shift savings away from cash: the Treatment 3 coefficient in column (c) is negative and significant at the 10% level.

It turns out, however, that these average effects obscure heterogeneity along exactly the same lines we saw previously in Table 5 when examining savings at the partner bank. In Panel B of Table 6, we estimate separate treatment effects for migrants with and without baseline demand for control. For migrants with demand for control, we find large and statistically significant effects of Treatment 3 on savings in El Salvador (column a), in the US (column b), in cash (column c), and on overall savings (column d). Treatment 3's effects for migrants with demand control are statistically significantly different from effects for migrants without demand for control (at the 1% level) in regressions for savings in the US (column b) and in total (column d).[28]

We view this result as supporting evidence that Treatment 3 had its effect on savings via a financial empowerment channel, as it is otherwise difficult to imagine why Treatment 3 would have raised savings in other financial institutions that had no connection to our intervention.[29] It appears that Treatment 3 led migrants to change their savings behavior more generally. Specifically, it provided differential encouragement to migrants to exert exclusive control over their savings in the US. The fact that they changed their savings behavior so dramatically in the US makes it more believable that they also changed the degree to which they exerted control over the savings of their family members in El Salvador.[30]

---

[28] The corresponding difference in Treatment 3's effects for savings in El Salvador (column a) is marginally significantly different from zero (p-value 0.11).

[29] Treatment 2 is associated with greater migrant savings in El Salvador among those *without* demand for control: the coefficient on the Treatment 2 * (No demand for control) term is positive and statistically significantly different from zero at the 5% level, while the coefficient on the corresponding interaction with "Demand for control" is much closer to zero and insignificant. We can provide no substantive explanation for this effect, and believe this result may simply reflect sample selection stemming from the differentially lower attrition of Treatment 2 observations from the follow-up survey (as discussed previously).

[30] It is also worth asking whether Treatment 3's effect on remittance-recipient savings is due to the marketing pitch *alone*, or whether it is crucial that the intervention offered the joint migrant/recipient accounts. The concern is that the financial empowerment induced by the Treatment 3 marketing pitch might have been enough to encourage migrants to exert control over funds in joint accounts that already existed or that they could easily set up on their own. Then the intervention's offer of the joint accounts at partner bank (and account-opening help) may have been superfluous. In Appendix D, we present additional regression results that test this possibility, using migrant follow-up survey data to check whether Treatment 3 led to increases in joint migrant/recipient savings at *other* (non-partner) banks. If the intervention's offer of assistance opening joint accounts at the partner bank was superfluous, and the marketing pitch was all that mattered, then we should also see Treatment 3 have positive effects on savings at other banks (many of whose branch locations may have been more conveniently located for family members in El Salvador). As it turns out, there is no indication that Treatment 3 or either of the other treatments affects savings in joint accounts outside of the partner bank.

Hypothesis 2 ("selection"): Relative to Treatment 2, Treatment 3 led the composition of the group opening accounts to be more savings-oriented

The selection hypothesis is that the composition of compliers (individuals who opened accounts in response to the treatment) in Treatment 3 was different from the composition of Treatment 2 compliers, and was responsible for the greater savings we observed in remittance-recipient accounts in Treatment 3 vs. Treatment 2. For this hypothesis to be true, two conditions must be met:

Condition 1: There should be migrant characteristics that predict opening of remittance-recipient accounts in Treatment 3 that are different from the migrant characteristics that predict remittance-recipient account opening in Treatment 2.

Condition 2: The same migrant characteristics that are associated with differentially higher take-up of remittance-recipient accounts in Treatment 3 vs. Treatment 2 should also be characteristics that lead to higher savings accumulation under Treatment 2.

We examine Condition 1 by running a regression analogous to that of column 2 of Table 2 (where the dependent variable is ownership of remittance-recipient accounts), but where treatment indicator variables are each interacted with a variety of migrant characteristics. All migrant characteristics are measured at baseline. The regression includes all controls and fixed effects reported in column 2 of Table 2, as well as the main effects of all variables interacted with the treatment indicators.

Coefficients on interactions with the Treatment 2 and Treatment 3 indicators in this regression are presented in the rightmost half of Table 4, where the last column reports the p-value of the test of the difference in the respective interaction terms (i.e., the test that the interaction of the migrant characteristic with the Treatment 2 indicator is statistically significantly different from the corresponding interaction with the Treatment 3 indicator). For four variables, the difference in the interaction term coefficients is statistically significantly different from zero at conventional levels, indicating differential selection into Treatment 3 along these dimensions compared to Treatment 2. Specifically, Treatment 3 (relative to Treatment 2) leads to more remittance-recipient account opening among migrants who have past experience with direct payments, whose primary remittance recipient is their spouse, or whose primary remittance recipient is some other relative. Also, Treatment 3 (relative to Treatment 2) leads to

less remittance-recipient account opening among migrants who track spending and budget expenses.

Having identified the dimensions along which Treatment 3 remittance-recipient account-openers are selected relative to Treatment 2 account-openers (Condition 1), we now examine evidence for Condition 2. For Condition 2 to hold, these same characteristics that lead to greater (less) differential selection into remittance-recipient account opening must also be associated with differentially higher (lower) effects of Treatment 2 on savings. To answer this question, we examine the regression that includes interaction terms with the various treatments, but where the dependent variable is savings balances in remittance recipient project accounts 6 months post-treatment (left hand side of Table 6).

As it turns out, none of the migrant characteristics that lead to greater differential selection into Treatment 3 vs. Treatment 2 appear to lead to higher savings accumulation under Treatment 2. The one variable associated with less differential selection into account opening ("tracks spending and budgets expenses") is actually associated with a higher Treatment 2 effect (the interaction of Treatment 2 with this variable is positive and significant at the 5% level). This pattern actually works against the selection hypothesis (those who track spending and budget expenses are less likely to be compliers in Treatment 3 than in Treatment 2, which would lead to a lower ITT effect of Treatment 3 on savings).

In sum, there are some migrant characteristics that differentially influence remittance-recipient account opening under Treatment 3 relative to Treatment 2, so that the pool of joint account-openers is different in terms of some baseline characteristics in Treatment 3 vs. Treatment 2 (Condition 1 holds). However, none of the characteristics leading to differential joint account opening are associated with differences in the Treatment 2 effect on savings balances in a direction that that could explain a higher Treatment 3 effect (Condition 2 does not hold). Therefore, differential selection into remittance-recipient account opening in Treatment 3 vs. Treatment 2 cannot explain why Treatment 3 has a higher overall effect on remittance-recipient account savings than Treatment 2.

Hypothesis 3 ("bargaining"): Treatment 3 increased migrants' bargaining power relative to Treatment 2

Another potential explanation for the higher impact of Treatment 3 compared to Treatment 2 on remittance-recipient account savings is that Treatment 3 lead to an increase in

migrants' bargaining power over remittance recipients relative to Treatment 2. The existence of Ahorro Directo accounts in El Salvador 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.[31] If this were the explanation for the differential impact of Treatment 3 on remittance-recipient account savings, then we should see less of an impact of Treatment 3 when migrants already have their own bank accounts in El Salvador.

As it turns out, this is not the case. In the savings regression of Table 4 (left hand side of the table), the coefficient on the interaction term between Treatment 3 and ownership of a bank account in El Salvador is not statistically significantly different from zero (and is actually positive in sign). This result is inconsistent with the hypothesis that the differential impact of Treatment 3 vs. Treatment 2 on remittance-recipient savings is due to increased migrant bargaining power due to providing migrants with an account in El Salvador.

Impacts on overall savings

We now examine impacts of the treatments on various categories of savings reported in the migrant and El Salvador household follow-up surveys, which includes all types of financial savings (beyond just the savings at the partner bank). This analysis is important to establish whether increases in savings seen in project accounts at the partner bank were simply shifted from other savings mechanisms, and will also allow any possible positive spillovers to other types of savings to reveal themselves.

Complete data on savings are available for 385 migrant/recipient-household pairs successfully interviewed in the follow-up survey and for whom savings data were non-missing in both the US and El Salvador.[32] Table 7 presents the impact of the treatments on various types of savings for the DC-based migrant, for the household of the primary remittance recipient, and for the combined trans-national household, at the time of the follow-up survey (Mar - Jun 2009). Panel A presents main effects of Treatments 3, 2, and 1, while Panel B presents separate treatment effects for migrants with and without demand for control.

[31] Such features might 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.

[32] We have confirmed that the results of Table 3 (that were from regressions with the full 898-observation sample) carry through in the smaller follow-up survey sample. All in all, the pattern of impacts on partner bank savings 6 months after treatment – as well as significance levels for the most part – are very similar in the smaller follow-up sample as in the full sample.

The first four columns present impacts on savings reported by the DC-based migrant, (a) in banks in El Salvador, (b) in U.S. banks, (c) in cash, and (d) in total across the previous three categories. In Panel A, none of the individual coefficients are statistically significantly different from zero, but the overall impact on savings in column (d) is positive in sign and large in magnitude for each treatment. Results in Panel B reveal heterogeneity in the impact of Treatment 3 that is essentially identical to that found in Table 6 (where the US migrant sample size was slightly larger): for migrants with demand for control, Treatment 3 has a large and statistically significant effect on savings in US banks and on overall migrant savings, and this effect is statistically significantly larger than the Treatment 3 effect for migrants that do not report demand for control at baseline (at the 5% and 1% levels, respectively).

The next three columns present impacts on savings reported by the primary remittance recipient household, (e) in banks, (f) in cash, and (g) in total across savings in cash and in banks. In Panel A, point estimates for savings in banks are positive and large in magnitude for both Treatments 2 and 3. There is a modestly-sized statistically significant positive effect of Treatment 3 on savings in cash. Impacts on total bank plus cash savings are large and positive for Treatments 2 and 3, but not statistically significantly different from zero at conventional levels. In Panel B, impacts on savings reported by the El Salvador household exhibit no statistically significant heterogeneity vis-à-vis baseline demand for control.

Column (h) 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.[33] In Panel A, coefficients on Treatments 3 and 2 are large and positive, but are not statistically significant at conventional levels. In Panel B, we find that for migrants with demand for control, Treatment 3's impact on total transnational household savings is statistically significantly different from zero (at the 5% level), and also statistically significantly different (at the 5% level) from the corresponding effect for migrants without demand for control. Consistent with a financial empowerment effect operating on migrants with demand for control only in Treatment 3, we do not find analogous treatment effect heterogeneity for Treatments 2 or 1.

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

In sum, the conclusion to take from Table 7's results is that Treatment 3 had a substantial positive impact on total savings in the transnational (migrant plus primary remittance recipient) household. There is no evidence that Treatment 3's positive impact on savings at the partner bank (results in Tables 3 and 5) simply represents a shift in savings from other savings mechanisms; the overall effect of Treatment 3 on transnational household savings is actually positive. The effect on total transnational household savings is large: for migrants with demand for control, Treatment 3 leads to an increase in savings of $2,024, which is more than twice mean savings among migrants with demand for control in the comparison group ($937.85).

Impact on remittances

We have found that Treatment 3 had substantial effects on savings in El Salvador and the U.S., particularly for migrants with baseline demand for control. Increased savings in El Salvador could either reflect an increase in the recipient savings rate (keeping remittances constant) or, alternatively, increases in remittances sent by the migrant. We therefore examine impacts of the treatments on remittances sent by the migrant to the primary remittance recipient household in El Salvador.

Results are presented in Table 8. As before, Panel A presents main effects of Treatments 3, 2, and 1, while Panel B presents separate treatment effects for migrants with and without demand for control. The dependent variable in all columns is monthly remittances sent by the migrant to the primary remittance recipient.[34]

The first and second columns of the table examine migrant remittances sent via the partner bank in, respectively, the full sample and the sample of migrants completing the follow-up survey. The results in the second column are included to facilitate comparison with the third column, which examines remittances to the primary remittance recipient via all channels (not just the partner bank), as reported by the migrant in the follow-up survey.

Examining the main effects of the treatments in Panel A, Treatment 3 leads to an increase in migrant remittances sent via the partner bank (the Treatment 3 coefficients in columns 1 and 2 are positive and statistically significantly different from zero). However, this may represent a shift of remittances from other channels rather than a true increase, since the coefficient on

[34] All funds sent to the primary remittance recipient El Salvador are counted as remittances, whether retrieved by the recipient in cash or sent directly to a bank account (and whether the bank account is joint with the migrant or in the name of the recipient only).

Treatment 3 in the third column for total remittances sent via all channels is smaller in magnitude and not statistically significantly different from zero.

In Panel B where separate effects are estimated for migrants with and without demand for control, it appears that the effect of Treatment 3 on remittances sent via the partner bank in Panel A is being driven mainly by migrants *without* demand for control (only the Treatment 3 interaction with "no demand for control" is statistically significantly different from zero, and it is about three times the magnitude of the corresponding interaction with "demand for control"). Again, though, this appears to be a shifting of remittances from other channels, since the effect for total remittances in column 3 is much smaller in magnitude and not statistically significantly different from zero for either Treatment 3 interaction term. In other words, it appears that Treatment 3 led migrants without demand for control to shift some of their remittances from other channels to our partner bank. We have no strong view as to why this may have occurred, but speculate that it may be due to increased familiarity with the partner bank due to the account opening induced by Treatment 3. As seen in Table 5 (column a, 2nd row), this increase in remittances via the partner bank by migrants without demand for control did not raise savings at the partner bank, which we view as consistent with our interpretation of the results.

It is striking that the coefficient on the Treatment 3 * (Demand for control) interaction term in the regression for monthly remittances sent via all channels is so close to zero. This result provides no support for the hypothesis that Treatment 3's impact on El Salvador savings for migrants with demand for control is being funded via increases in migrant remittances. That said, the standard error on this coefficient is large, so we cannot rule out large effects on remittances.

VI. Conclusion

This paper contributes to knowledge in at least two areas. First, it expands our currently very limited knowledge about the determinants of migrant remittance flows, which have recently become one of the largest types of international financial flows to developing countries. Second, it contributes to the development economics literature on intra-household resource allocation and decision-making, by estimating the demand for and impact of offering migrants greater monitoring and control over remittances sent to households in their country of origin.

We implemented a field experiment that offered migrants in Washington DC bank accounts in El Salvador that varied in the degree to which migrants could monitor and control

savings. We found that the treatment intervention that offered migrants the greatest degree of control over their own accounts and the accounts of remittance recipients led to substantial increases in savings at our partner bank. This increase in savings is likely due to enhanced control exerted by migrants, since the effect of the treatment is significantly larger among migrants who report greater demand for control in the baseline survey. We interpret the effects we find as arising from the joint effect of the bank account offers and a marketing pitch encouraging migrants to exert greater control over the savings of remittance recipients in El Salvador. We describe the latter as a "financial empowerment effect".

We also find that the treatment intervention that offered migrants the greatest degree of control over El Salvador savings had a strong spillover effect on other types of savings, particularly migrant savings in US banks that were unconnected with our intervention. We interpret this as an additional manifestation of the "financial empowerment" effect generated by the treatment: migrants were encouraged to exert more control over El Salvador-based savings, but also took this as an encouragement to save more by other means. This finding suggests that it would be useful in future experimental research to explicitly separate the "financial empowerment" component from the offer of new financial products. For example, one could separately randomize a financial training session emphasizing the importance of migrant control alongside randomization of the offer of financial products that facilitate control, so that some study participants would receive financial training only, some would receive only the offer of new products, and some would receive both. Such a study design would provide more explicit separation of the financial empowerment and financial product effects.

Another important result of the paper is that, by itself, channeling remittances into savings accounts has a much smaller impact on savings accumulation. This is clearly demonstrated by the fact that one of our treatment interventions – that encouraged migrants to remit into remittance recipients' bank accounts, and helped in setting up such accounts – had a much smaller impact on savings. But when migrants are given the ability to monitor and control savings of remittance recipients, and are encouraged to exert such control, the impact on savings accumulation in the origin household can be much larger. This insight should guide future efforts to facilitate savings accumulation in remittance-recipient households.

By demonstrating the positive effects of an intervention that enhanced migrant control over savings in remittance-recipient households, this study also suggests some high-potential

directions for future research. In particular, it should be fruitful to study the impacts of migrant control over other remittance uses that may have positive spillovers and wider development impacts, such as payments for schooling, health care, and investments in microenterprises.

References

Ashraf, N., "Spousal Control and Intra-Household Decision Making: An Experimental Study in the Philippines," *American Economic Review*, Vol. 99, No. 4, September 2009, pp. 1245-1277.

Ashraf, Nava, Dean Karlan, and Wesley Yin, "Tying Odysseus to the Mast: Evidence from a Commitment Savings Product in the Philippines," *Quarterly Journal of Economics*, May 2006.

Aycinena, Diego, Claudia Martinez A. and Dean Yang, "The Impact of Remittance Fees on Remittance Flows: Evidence from a Field Experiment Among Salvadoran Migrants," mimeo, University of Michigan, 2010.

Browning, M., and Chiappori, P.-A., "Efficient Intra-household Allocations: A General Characterisation and Empirical Tests," *Econometrica*, 66 (6), 1998, pp. 1241-1278.

Chen, Joyce, "Migration and Imperfect Monitoring: Implications for Intra-household Allocation," *American Economic Review: Papers and Proceedings*, May 2006.

Chin, A., L. Karkoviata, and N. Wilcox, "Impact of Bank Accounts on Migrant Savings and Remittances: Evidence from a Field Experiment," mimeo, University of Houston, 2010.

De Laat, Joost, "Household Allocations and Endogenous Information," mimeo, University of Quebec at Montreal, 2008.

Dercon, Stefan and Pramila Krishna, "In Sickness and in Health: Risk Sharing within Households in Rural Ethiopia," *Journal of Political Economy*, Vol. 108, No. 4, August 2000, pp. 688-727.

Dubois, P. and E. Ligon, "Incentives and Nutrition for Rotten Kids: Intrahousehold Food Allocation in the Philippines," mimeo, University of California (Berkeley), 2005.

Duflo, Esther. 2003. "Grandmothers and Granddaughters: Old Age Pension and Intra-Household Allocation in South Africa." *World Bank Economic Review*. 17(1), 1-25.

Goldstein, Markus Alain de Janvry, and Elisabeth Sadoulet, "Is a Friend in Need a Friend Indeed? Inclusion and Exclusion in Mutual Insurance Networks in Southern Ghana," in Stefan Dercon (ed.) *Insurance against Poverty*, Oxford University Press, 2005.

Hertzberg, Andrew, “Exponential Individuals, Hyperbolic Households,” working paper, Columbia Business School, 2011.

Kinnan, Cynthia, “Distinguishing Barriers to Insurance in Thai Villages,” working paper, Northwestern University, 2011.

Lundberg, S. and R. Pollak, “Separate Spheres Bargaining and the Marriage Market,” *Journal of Political Economy*, 101, 1993, pp. 988-1010.

Lusardi, Annamaria and Olivia S. Mitchell, “Financial Literacy and Planning: Implications for Retirement Wellbeing,” Working Paper, Pension Research Council, Wharton School, University of Pennsylvania, 2006.

Lusardi, Annamaria and Olivia S. Mitchell, “Baby Boomer Retirement Security: The Roles of Planning, Financial Literacy, and Housing Wealth,” *Journal of Monetary Economics*, Vol. 54, 2007, pp. 205-224.

Manser, M. and M. Brown, “Marriage and Household Decision-Making: A Bargaining Analysis,” *International Economic Review,* 21, 1980.

Martinez, Claudia, “Intra-Household Allocation and Bargaining Power: Evidence from Chile,” mimeo, University of Chile, 2006.

McElroy, M. and M. Horney, "Nash-Bargained Household Decisions: Towards a Generalization of the Theory of Demand," *International Economic Review*, 22/2, 1981.

Orozco, Manuel, “The Remittance Marketplace: Prices, Policy, and Financial Institutions,” Pew Hispanic Center Report, 2004.

Pew Hispanic Center, *Billions in Motion: Latino Immigrants, Remittances, and Banking*. Washington, DC: Pew Hispanic Center and Multilateral Investment Fund, 2002.

Platteau, Jean-Philippe, “Traditional Systems of Social Security and Hunger Insurance: Past Achievements and Modern Challenges,” in E. Ahmad, J. Dreze, J. Hills, and A. Sen (eds.), *Social Security in Developing Countries*, Oxford: Clarendon Press, 1991.

Rangel, Marcos, “Alimony Rights and Intrahousehold Allocation of Resources: Evidence from Brazil,” *Economic Journal*, Vol. 116, 2006, pp. 627-658.

Ratha, Dilip, “Workers' Remittances: An Important and Stable Source of External Development Finance,” in *Global Development Finance 2003: Striving for Stability in Development Finance*. Washington, DC: International Monetary Fund, 2003.

Schaner, Simone, "Intrahousehold Preference Heterogeneity, Commitment, and Strategic Savings: Theory and Evidence from Kenya," working paper, Dartmouth College, 2011.

Strauss, John and Duncan Thomas, "Human Resources: Empirical Modeling of Household and Family Decisions," in Jere Behrman and T.N. Srinivasan, eds., *Handbook of Development Economics*. New York: North-Holland, 1995.

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

Udry, Christopher, "Gender, Agricultural Productivity and the Theory of the Household", *Journal of Political Economy*, 104, 1996.

World Bank, *Global Economic Prospects 2006: Economic Implications of Remittances and Migration*. Washington, DC, 2006.

World Bank, *Close to Home: The Development Impact of Remittances in Latin America.* Washington, DC, 2007.

Yang, Dean, "Migrant Remittances," *Journal of Economic Perspectives*, Vol. 25, No. 3, Summer 2011, pp. 129-152.

Table 1: Summary statistics

	Mean	Std. Dev.	10th pct.	Median	90th pct.	Num. Obs.
Treatment indicators and stratification variables						
Treatment 0 (no savings facility offered)	0.24	0.43	0	0	1	898
Treatment 1 (remittance recipient account only)	0.23	0.42	0	0	1	898
Treatment 2 (joint account)	0.27	0.45	0	0	1	898
Treatment 3 (joint + migrant account)	0.25	0.43	0	0	1	898
Migrant is female	0.29	0.45	0	0	1	898
Migrant has US bank account	0.63	0.48	0	1	1	898
Recipient is migrant's parent	0.55	0.50	0	1	1	898
Recipient is migrant's spouse	0.11	0.31	0	0	1	898
Recipient is migrant's child	0.04	0.19	0	0	0	898
Recipient is migrant's other relative	0.30	0.46	0	0	1	898
Migrant has been in US 0-5 years	0.50	0.50	0	0	1	898
Migrant has been in US 6-10 years	0.40	0.49	0	0	1	898
Migrant has been in US 11-15 years	0.11	0.31	0	0	1	898
Baseline variables from DC migrant survey						
Migrant's years in the US	5.57	3.60	1	5	11	898
Migrant has El Salvador bank account	0.17	0.38	0	0	1	898
Migrant's annual income (US$)	30,999	56,292	11,700	24,960	48,822	865
Migrant's household's annual income (US$)	39,620	87,551	10,530	31,200	65,000	896
Migrant's years of education	8.53	4.17	2	9	12	865
Migrant's age	30.88	7.65	22	30	41	894
Migrant's annual remittances sent (US$)	4,990	4,124	1,200	3,900	9,600	898
Migrant's total hh savings balance (US$)	2,851	5,111	0	750	8,100	806
Migrant is US citizen	0.007	0.082	0	0	0	894
Migrant hh size in U.S.	4.81	2.15	2	5	8	898
Migrant is married or partnered	0.59	0.49	0	1	1	897
Past experience with direct payments	0.08	0.27	0	0	0	898
Sent funds to El Salvador for others to administer	0.23	0.42	0	0	1	898
Interested in direct payments to increase control	0.21	0.41	0	0	1	898
Aware of disagreements with recipients over remittance uses	0.15	0.35	0	0	1	898
Have had disagreements with recipients over remittance uses	0.05	0.22	0	0	0	898
Demand for control (union of above five indicators)	0.51	0.50	0	1	1	898
Correct answer to compound interest question	0.66	0.47	0	1	1	898
Correct answer to inflation question	0.64	0.48	0	1	1	898
Correct answer to mutual fund question	0.37	0.48	0	0	1	898
Tracks spending and budgets expenses	0.46	0.50	0	0	1	897
Baseline variables from El Salvador household survey						
Recipient's total hh savings balance (US$)	382	1,732	0	0	380	733
Recipient's annual remittances received (US$)	3,182	2,787	900	2,400	6,000	725

Note -- Survey data collected from Jun 2007 to Jan 2008 among Salvadoran migrants in Washington DC and from Nov 2007 to Jun 2008 among households in El Salvador identified as DC migrant's "primary remittance recipient ".

Table 2: 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

	Primary recipient accounts		Migrant-only accounts		Accounts shared by migrant and a person in El Salvador other than primary remittance recipient		Any project account	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment 3 (joint account + indiv. migrant account)	0.217***	0.231***	0.293***	0.304***	0.076***	0.066***	0.332***	0.340***
	(0.033)	(0.033)	(0.032)	(0.031)	(0.018)	(0.018)	(0.036)	(0.037)
Treatment 2 (joint account)	0.155***	0.160***	-0.010	-0.001	0.078***	0.063***	0.219***	0.214***
	(0.029)	(0.030)	(0.011)	(0.014)	(0.017)	(0.015)	(0.033)	(0.034)
Treatment 1 (remittance recipient account)	0.135***	0.145***	0.001	0.005	0.029**	0.018	0.165***	0.164***
	(0.030)	(0.030)	(0.013)	(0.014)	(0.012)	(0.013)	(0.033)	(0.033)
Constant	0.046***	0.393**	0.018**	0.069	0.000	-0.088*	0.064***	0.276
	(0.014)	(0.194)	(0.009)	(0.128)	(0.000)	(0.047)	(0.016)	(0.202)
Marketer fixed effects		Y		Y		Y		Y
Treatment month fixed effects		Y		Y		Y		Y
Stratification cell fixed effects		Y		Y		Y		Y
Observations	898	898	898	898	898	898	898	898
R-squared	0.043	0.126	0.203	0.272	0.024	0.095	0.076	0.165
P-value of F-test: equality of …								
Treatment 3 & 2 coeffs.	0.118	0.061	0.000	0.000	0.925	0.875	0.010	0.003
Treatment 3 & 1 coeffs.	0.041	0.023	0.000	0.000	0.026	0.029	0.000	0.000
Treatment 2 & 1 coeffs.	0.589	0.672	0.325	0.619	0.017	0.026	0.181	0.204

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%
Notes -- Dependent variable equal to 1 if migrant or remittance recipient has given type of project account with partner bank (Banco Agricola), 0 otherwise. Omitted treatment indicator is for Treatment 0 (comparison 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 3: 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 …

	… of primary recipient		… of migrant alone		…shared by migrant and other individual (non-primary recipient)		… in total	
	(a)		(b)		(c)		(d) = (a) + (b) + (c)	
Treatment 3 (joint account + indiv. migrant account)	127.15*	147.38*	51.72***	55.28***	6.16*	8.53	185.04**	211.19**
	(71.264)	(79.101)	(19.036)	(19.887)	(3.290)	(5.733)	(74.192)	(82.232)
Treatment 2 (joint account)	24.09	36.22*	3.59	3.44	22.08	24.55	49.76*	64.22**
	(20.227)	(21.986)	(4.295)	(7.432)	(17.473)	(18.801)	(26.937)	(30.047)
Treatment 1 (remittance recipient account)	19.43	33.15	3.51	3.10	5.18	5.51	28.12	41.76*
	(17.630)	(21.755)	(3.000)	(4.961)	(4.968)	(6.264)	(18.530)	(22.940)
Constant	13.01	253.84*	0.69	-18.31	-0.00	-41.12	13.70	194.40
	(9.133)	(140.009)	(0.481)	(17.987)	(0.000)	(46.947)	(9.142)	(149.300)
Marketer fixed effects		Y		Y		Y		Y
Treatment month fixed effects		Y		Y		Y		Y
Stratification cell fixed effects		Y		Y		Y		Y
Observations	898	898	898	898	898	898	898	898
R-squared	0.008	0.045	0.020	0.080	0.003	0.051	0.014	0.051
P-value of F-test: equality of …								
Treatment 3 & 2 coeffs.	0.157	0.137	0.014	0.014	0.371	0.328	0.082	0.066
Treatment 3 & 1 coeffs.	0.136	0.119	0.012	0.013	0.869	0.684	0.037	0.028
Treatment 2 & 1 coeffs.	0.843	0.894	0.988	0.955	0.352	0.307	0.471	0.462
Mean of dep. var. in comparison group		13.01		0.69		0.00		13.70

Robust standard errors in parentheses. * 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 (comparison 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 4: Heterogeneity in impacts of Treatments 2 and 3 on savings and account opening
(Ordinary least-squares estimates)

Dependent variable:	Savings balances in remittance recipient project accounts at 6 months post-treatment			Indicator: remittance recipient has project account at 6 months post-treatment		
	Interaction with Treatment 2	Interaction with Treatment 3	*P-value of coeff. diff.*	Interaction with Treatment 2	Interaction with Treatment 3	*P-value of coeff. diff.*
Interactions with latent demand for control						
Past experience	-56.956	294.611	0.428	-0.080	0.173	0.087
with direct payments	(122.267)	(490.522)		(0.118)	(0.131)	
Sent funds to El Salvador for	-32.873	267.458	0.099	0.040	0.134*	0.329
others to administer	(48.968)	(184.353)		(0.072)	(0.078)	
Interested in direct	-2.678	209.441	0.362	0.054	-0.029	0.343
payments to improve control	(44.445)	(221.227)		(0.064)	(0.069)	
Aware of disagreements	-33.354	724.684	0.134	-0.001	0.040	0.735
with recipients over remittance uses	(41.885)	(491.814)		(0.087)	(0.099)	
Have had disagreements	49.767	-150.483	0.283	0.116	0.070	0.833
with recipients over remittance uses	(82.252)	(184.137)		(0.157)	(0.170)	
Interactions with migrant account ownership						
Has US bank account	24.989	70.451	0.675	-0.054	0.041	0.284
	(47.110)	(100.176)		(0.072)	(0.075)	
Has El Salvador account	110.357	126.259	0.943	0.021	-0.021	0.671
	(75.561)	(234.377)		(0.073)	(0.081)	
Interactions with other baseline variables						
Correct answer to compound	7.857	52.893	0.557	0.012	0.082	0.397
interest question	(34.098)	(74.744)		(0.067)	(0.068)	
Correct answer to inflation	7.418	174.155	0.156	-0.028	0.040	0.408
question	(42.267)	(108.008)		(0.065)	(0.069)	
Correct answer to mutual fund	107.163**	-223.620*	0.010	0.074	-0.056	0.117
question	(53.194)	(119.388)		(0.067)	(0.068)	
Tracks spending and budgets	99.372**	-256.359	0.114	0.052	-0.103	0.049
expenses	(43.475)	(220.684)		(0.059)	(0.067)	
Recipient is migrant's spouse	78.099	-35.165	0.432	-0.154*	0.205*	0.007
	(92.978)	(120.626)		(0.093)	(0.119)	
Recipient is migrant's child	-46.545	-57.610	0.917	-0.180	0.007	0.282
	(78.650)	(128.345)		(0.150)	(0.147)	
Recipient is migrant's other	-12.373	117.341	0.448	-0.117*	0.056	0.035
relative	(54.521)	(183.794)		(0.063)	(0.070)	
Migrant is female	44.263	-252.109*	0.038	-0.013	-0.081	0.427
	(53.332)	(134.224)		(0.069)	(0.072)	
Migrant has been in US for	-49.427	7.157	0.655	-0.212***	-0.213***	0.987
6-10 years	(49.044)	(125.762)		(0.066)	(0.074)	
Migrant has been in US for	-81.213	384.457	0.167	-0.242**	-0.127	0.286
11-15 years	(58.370)	(342.126)		(0.098)	(0.108)	
Observations	897			897		
R-squared	0.166			0.191		

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%
Notes -- All variables interacted with treatment indicators were collected in baseline survey (prior to treatment). Right-hand-side variables in both regressions include: fixed effects for marketer, treatment month, and stratification cell; main effects of Treatment 1, 2, and 3 indicators; and main effects of all variables interacted with treatment indicators. Excluded relationship to migrant category is "parent". Excluded migrant years in US category is 0-5 years. All corresponding interactions with Treatment 1 are also included in regressions but not reported in table. See Table 5 for other notes. P-values are for F-test of equality of coefficients on Treatment 3 and Treatment 2 interaction terms.

Table 5: Heterogeneity in treatment effects by baseline demand for control
(Ordinary least-squares estimates)

Dependent variable: Savings balances at 6 months post-treatment in project accounts …

	… of primary recipient	… of migrant alone	…shared by migrant and other individual	… in total
	(a)	(b)	(c)	(d) = (a) + (b) + (c)
Treatment 3 * Demand for control	245.262*	31.677**	6.950	283.889*
	(148.203)	(15.572)	(6.348)	(148.845)
Treatment 3 * No demand for control	46.848	79.452**	10.653	136.953**
	(35.336)	(35.656)	(9.178)	(55.369)
Treatment 2 * Demand for control	28.241	1.773	46.093	76.106
	(28.037)	(10.608)	(35.103)	(46.922)
Treatment 2 * No demand for control	51.501	3.317	4.212	59.030
	(37.128)	(10.061)	(5.796)	(38.696)
Treatment 1 * Demand for control	38.541	-1.091	1.799	39.250
	(36.457)	(5.295)	(5.129)	(37.078)
Treatment 1 * No demand for control	29.844	6.612	10.221	46.676*
	(23.369)	(8.938)	(11.811)	(26.859)
P-value of F-test: equality of interactions with				
Treatment 3	0.183	0.211	0.724	0.344
Treatment 2	0.632	0.913	0.194	0.778
Treatment 1	0.839	0.462	0.529	0.870
Marketer fixed effects	Y	Y	Y	Y
Treatment month fixed effects	Y	Y	Y	Y
Stratification cell fixed effects	Y	Y	Y	Y
Observations	898	898	898	898
Mean of dep. var. in comparison group				
Migrants with demand for control	17.34	1.08	-0.00	18.42
Migrants with no demand for control	8.14	0.25	0.00	8.39

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%
Notes -- Dependent variable is in US dollars. Omitted treatment indicator is for Treatment 0 (comparison 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 in partner bank accounts are average end-of-month balances from internal bank databases. Regressions also include main effect of demand for control indicator (main effects of Treatments 3, 2 and 1 do not need to be included because they are fully interacted with "demand for control" and "no demand for control").

Table 6: Impact of treatments on savings reported in follow-up survey (Mar - Jun 2009)
(Ordinary least-squares estimates)
Sample restricted to migrants completing the follow-up survey in Mar - Jun 2009

Dependent variable:	Savings reported in follow-up survey by DC migrant …			
	… in El Salvador	… in the US	… in cash, not in banks	… in total
	(a)	(b)	(c)	(d) = (a) + (b) + (c)
Panel A: Main effect of treatments				
Treatment 3 (joint account + indiv. migrant account)	382.002	450.125	-188.496**	643.630
	(271.230)	(356.568)	(94.733)	(455.407)
Treatment 2 (joint account)	607.438*	-181.102	-19.996	406.340
	(342.681)	(305.878)	(100.273)	(468.693)
Treatment 1 (remittance recipient account)	193.795	-122.822	84.552	155.526
	(242.484)	(297.461)	(127.252)	(403.441)
Panel B: Separate treatment effects for migrants with and without baseline demand for control				
Treatment 3 * Demand for control	721.782*	1,243.650***	-156.870*	1,808.561***
	(434.346)	(473.652)	(82.974)	(627.699)
Treatment 3 * No demand for control	5.639	-465.262	-227.876	-687.499
	(216.305)	(464.747)	(167.871)	(550.216)
Treatment 2 * Demand for control	73.944	-38.426	86.163	121.680
	(304.327)	(247.499)	(122.752)	(404.857)
Treatment 2 * No demand for control	1,116.001**	-359.134	-125.914	630.953
	(558.760)	(499.145)	(184.724)	(763.972)
Treatment 1 * Demand for control	388.091	0.254	62.921	451.266
	(381.535)	(250.000)	(128.924)	(461.030)
Treatment 1 * No demand for control	-7.402	-288.762	102.512	-193.652
	(216.948)	(507.371)	(217.012)	(598.668)
P-value of F-test: equality of interactions with				
Treatment 3	0.110	0.008	0.694	0.002
Treatment 2	0.081	0.519	0.378	0.524
Treatment 1	0.312	0.585	0.873	0.356
Marketer fixed effects	Y	Y	Y	Y
Treatment month fixed effects	Y	Y	Y	Y
Stratification cell fixed effects	Y	Y	Y	Y
Observations	508	508	508	508
Mean of dep. var. in comparison group				
Migrants with demand for control	341.53	308.81	89.32	739.66
Migrants with no demand for control	179.25	923.87	202.83	1,305.94

Robust standard errors in parentheses. * 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 (comparison 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. In Panels A and B, all regressions also include main effect of demand for control indicator. Savings in partner bank accounts are average end-of-month balances from internal bank databases in the specific month (between Mar - Jun 2009) that migrant was administered the follow-up survey.

Table 7: Impact of treatments on total transnational household savings reported in follow-up survey (Mar - Jun 2009)
(Ordinary least-squares estimates)
Sample restricted to observations where both migrants and El Salvador households completed follow-up survey in Mar - Jun 2009

Dependent variable: Savings reported in follow-up survey

	… by DC migrant				… by El Salvador household			
	… in El Salvador	… in the US	… in cash, not in banks	… in total	… in banks	… in cash, not in banks	… in total	… by El Salvador household + DC migrant in total
	(a)	(b)	(c)	(d) = (a) + (b) + (c)	(e)	(f)	(g) = (e) + (f)	(h) = (d) + (g) - overlapping reports of joint accounts
Panel A: Main effect of treatments								
Treatment 3 (joint account + indiv. migrant account)	363.063	495.364	-116.256*	742.171	410.445	25.210**	435.654	897.231
	(349.408)	(378.281)	(68.639)	(526.859)	(364.576)	(12.292)	(364.126)	(587.947)
Treatment 2 (joint account)	577.141	-90.175	64.733	551.699	873.164	8.794	881.958	879.787
	(450.461)	(239.949)	(72.578)	(522.049)	(704.033)	(5.683)	(703.990)	(747.977)
Treatment 1 (remittance recipient account)	205.615	-86.684	100.286	219.217	108.021	2.495	110.516	133.762
	(326.662)	(310.108)	(110.545)	(466.360)	(246.329)	(5.721)	(246.474)	(453.791)
Panel B: Separate treatment effects for migrants with and without baseline demand for control								
Treatment 3 * Demand for control	754.430	1,292.346**	-169.398*	1,877.378**	525.288	20.288	545.576	2,023.898**
	(507.525)	(548.229)	(100.912)	(750.432)	(650.267)	(17.718)	(649.870)	(921.496)
Treatment 3 * No demand for control	-5.393	-509.155	-54.365	-568.913	255.146	30.620*	285.766	-503.936
	(329.543)	(520.633)	(77.376)	(648.287)	(269.321)	(18.342)	(268.733)	(660.709)
Treatment 2 * Demand for control	-239.264	39.100	68.550	-131.614	1,064.146	8.156	1,072.301	1,043.536
	(315.821)	(235.898)	(143.450)	(395.269)	(1,105.645)	(8.599)	(1,105.551)	(1,120.959)
Treatment 2 * No demand for control	1,420.840*	-331.270	70.518	1,160.088	636.142*	10.874	647.016*	543.633
	(806.726)	(502.505)	(105.375)	(972.278)	(357.674)	(7.187)	(358.472)	(715.151)
Treatment 1 * Demand for control	397.056	-149.449	45.015	292.622	290.826	-10.197	280.629	324.019
	(472.893)	(279.965)	(157.396)	(558.079)	(463.365)	(9.038)	(463.479)	(605.436)
Treatment 1 * No demand for control	-19.248	-131.803	168.280	17.230	-118.428	17.194**	-101.234	-236.852
	(326.984)	(556.134)	(159.408)	(701.568)	(283.572)	(7.713)	(284.419)	(707.209)
P-value of F-test: equality of interactions with								
Treatment 3	0.143	0.018	0.347	0.010	0.716	0.697	0.726	0.028
Treatment 2	0.044	0.528	0.992	0.209	0.648	0.811	0.651	0.670
Treatment 1	0.407	0.976	0.592	0.738	0.503	0.028	0.533	0.550
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
Observations	385	385	385	385	385	385	385	385
Mean of dep. var. in comparison group								
Migrants with demand for control	395.10	316.08	103.33	814.51	358.25	5.29	363.54	937.85
Migrants with no demand for control	191.18	804.85	36.76	1,032.79	61.00	2.79	63.79	1,081.88

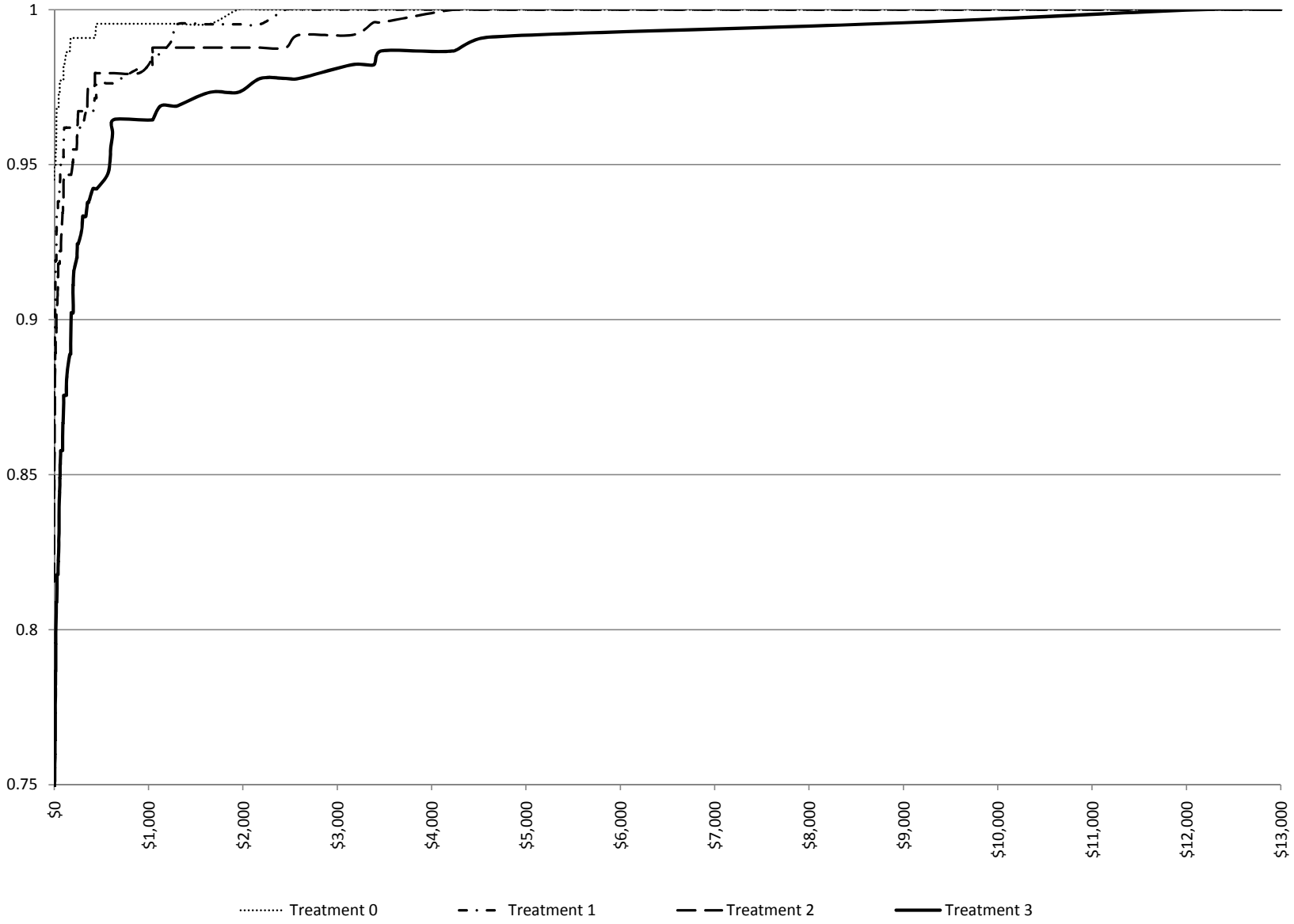
Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%
Notes -- Follow-up survey administered from Mar - Jun 2009. Dependent variable is in US dollars. Savings figure in column (i) avoids double-counting of savings held in joint migrant/recipient-household accounts and reported by both parties. Omitted treatment indicator is for Treatment 0 (comparison 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. In Panels A and B, all regressions also include main effect demand for control indicator.

Table 8: Impact of treatments on remittances and earnings
(Ordinary least-squares estimates)

Dependent variable:	Monthly remittances sent by migrant to primary remittance recipient		
Remittance channel:	Partner bank	Partner bank	All channels
Time frame:	July 2008 to June 2009	July 2008 to June 2009	July 2008 until follow-up survey
Sample:	Full sample	Migrants completing follow-up survey	Migrants completing follow-up survey
Data source:	Partner bank database	Partner bank database	Follow-up survey
Panel A: Main effect of treatments			
Treatment 3 (joint account + indiv. migrant account)	56.311	102.677**	35.940
	(34.459)	(47.471)	(52.405)
Treatment 2 (joint account)	18.151	60.763*	-2.078
	(26.279)	(32.997)	(33.161)
Treatment 1 (remittance recipient account)	-1.666	16.139	5.365
	(24.666)	(30.498)	(37.339)
Panel B: Separate treatment effects for migrants with and without baseline demand for control			
Treatment 3 * Demand for control	28.863	56.755	-10.517
	(50.128)	(56.351)	(53.487)
Treatment 3 * No demand for control	86.103**	154.173**	88.036
	(43.245)	(68.943)	(86.988)
Treatment 2 * Demand for control	-10.424	56.275	-15.025
	(41.185)	(50.629)	(57.674)
Treatment 2 * No demand for control	47.908	66.739	12.517
	(30.387)	(41.442)	(38.312)
Treatment 1 * Demand for control	-10.710	14.565	-2.512
	(42.567)	(49.170)	(61.830)
Treatment 1 * No demand for control	9.183	18.179	14.382
	(27.038)	(43.821)	(36.958)
P-value of F-test: equality of interactions with			
Treatment 3	0.370	0.238	0.310
Treatment 2	0.247	0.872	0.703
Treatment 1	0.700	0.959	0.812
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	898	560	560
Mean of dep. var. in comparison group			
Migrants with demand for control	96.63	72.33	277.75
Migrants with no demand for control	61.92	85.28	194.99

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%
Notes -- Follow-up survey administered from Mar - Jun 2009. Dependent variable is in US dollars. Remittance variables refer only to funds sent to primary remittance recipient in El Salvador. Omitted treatment indicator is for Treatment 0 (comparison 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. Remittances sent via partner bank obtained from internal bank databases. In Panels A and B, all regressions also include main effect of demand for control indicator.

Figure 1: Cumulative distribution function of savings in all project accounts, by treatment



Note: CDF truncated at 0.75 on vertical axis. Savings balances are in all project accounts as of 6 months post-treatment.

NOT FOR PUBLICATION

Online Appendix for:
Remittances and the Problem of Control: A Field Experiment Among Migrants from El Salvador
By Nava Ashraf, Diego Aycinena, Claudia Martinez, and Dean Yang

Appendix A: 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.

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 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 the waiting area of the consulate and invited them to participate in the study. The D.C. baseline 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. 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).

Appendix Table 1 presents means of several key baseline variables for observations in our baseline data (column 1), in comparison with corresponding means for Salvadoran-born and Hispanic individuals in the US Census 2000 in the Washington DC metro area, separately for males and females.[1] While differences are not dramatic, there are some key differences between our sample and US Census Salvadorans in the DC metro area. Focus for the moment on the

[1] Because our study participants range in age from 18 to 65, we restrict the US Census 2000 samples in the table to that age range as well.

comparison with column 2, for all Salvadoran-born individuals regardless of US citizenship. First of all, our sample is more male, at 71% vs. 57%. Our sample has also arrived somewhat more recently in the US, with 49% and 51% of males and females, respectively, having been in the US for 5 years or less at the time of survey, compared to corresponding figures of 33% and 29% for US Census Salvadorans. Our sample is slightly more educated: 30% and 36% of our sample males and females, respectively, have a high school degree or more, compared to 27% and 30% of US Census Salvadoran males and females, respectively. Our sample is less likely to have US citizenship, at 0-1%, compared to 10-12% for US Census Salvadorans. Finally, our sample is more likely to be married or partnered, at 53% and 73% of males and females respectively, compared to 45% and 57% for male and female US Census Salvadorans, respectively. These differences vis-à-vis our baseline sample are quite similar when restricting the sample of US Census Salvadorans to those without US citizenship (column 3), and when examining Hispanics without US citizenship (column 4).

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 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), and 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 that was the same for all treatment conditions (to remove any differential incentive to complete visits of different types). Marketing visits were only scheduled after the survey of the El Salvador household had been completed (or attempted and failed), to avoid bias in baseline 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 provided a discount for sending remittances via Banagricola remittance locations in the DC area. Each card had a unique code that was entered into the computer during the remittance transaction to validate the discount, allowing us to track individual remittance transactions that took advantage of the discount. Banco Agricola's normal remittance charge is $10 for a remittance up to $1,500, and the VIP card allowed 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 was fixed for the validity period of the card).[2] 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.

[2] This remittance price randomization was independent of the randomization into Treatments 0, 1, 2, or 3, and so does 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 (Aycinena, Martinez, and Yang 2010).

Appendix B: Marketing Scripts for Treatments 0, 1, 2, and 3

Treatment 0: comparison group, not offered any new savings accounts or account-opening assistance

Initiate conversation with questions regarding client's preferences for money sending and saving services. For example:

- Do your family members currently save money?
- Which service providers do they currently utilize to send money?
- What type of transfer do you use, traditional remittance (with code/password) or remittance by credit to an account?
- Why do you use these services?
- What difficulties does your family member have in picking up/receiving the money that you send?

If they send money through traditional remittance (with code/password):
Did you know that besides sending money to your family via traditional remittance, there is a more practical, less costly option?

The option to which I am referring is "remittance by credit to an account". Let me explain why this option offers more advantages than a traditional remittance.

As you may know, when you send money via a traditional remittance, your family receives a code or password to take to a Banco Agrícola branch and wait in line to receive the remittance. By contrast, when you send money via a "remittance by credit to an account", you obtain the following advantages:

- Access to funds through a debit card: Your remittance recipient can access the funds in the account through a debit card. This way, should your remittance recipient need money immediately you will no longer have to worry about going to send money right away since he/she will now have money available in this account.
- Direct deposit of your remittance to this account: Remitting into an account facilitates your remittance recipient receiving the money you send: no longer will he/she have to go to the bank and wait in line to receive it or run the risk of losing the code/pin number. He/she can use any ATM to take out money using a debit card from anywhere in El Salvador.

Treatment 1: offer of remittance recipient account

Initiate conversation with questions regarding client's preferences for money sending and saving services. For example:

- Do your family members currently save money?
- Which service providers do they currently utilize to send money?

- What type of transfer do you use, traditional remittance (with code/password) or remittance by credit to an account?
- Why do you use these services?
- What difficulties does your family member have in picking up/receiving the money that you send?

I come to present to you a service which will allow your remittance recipient to obtain a savings account courtesy of Banco Agrícola. This account offers you the following advantages:

- Savings for your remittance recipient in El Salvador: Your remittance recipient will be able to open a savings account in his/her name in El Salvador through Banco Agrícola.
- Access to funds through a debit card: Your remittance recipient can access the funds in the account through a debit card. This way, should your remittance recipient need money immediately you will no longer have to worry about going to send money right away since he/she will now have money available in this account.
- Direct deposit of your remittance to this account: Remitting into an account facilitates your remittance recipient receiving the money you send: no longer will he/she have to go to the bank and wait in line to receive it or run the risk of losing the code/pin number. He/she can use any ATM to take out money using a debit card from anywhere in El Salvador.

Treatment 2: offer of joint account (*Cuenta Unidos*)

Initiate conversation with questions regarding client's preferences for money sending and saving services. For example:

- Do your family members currently save money?
- Which service providers do they currently utilize to send money?
- What type of transfer do you use, traditional remittance (with code/password) or remittance by credit to an account?
- Why do you use these services?
- What difficulties does your family member have in picking up/receiving the money that you send?

I come to present to you a service which will allow your remittance recipient to obtain a "Cuenta Unidos" courtesy of Banco Agrícola. This account offers you the following advantages:

- Savings for your remittance recipient in El Salvador: Your remittance recipient will be able to open a savings account in his/her name in El Salvador through Banco Agrícola.
- Both your remittance recipient and you will be able to verify the balance on the account: This account will allow you to check the balance on the account through a service provided by a call center.
- Access to funds for you and your remittance recipient through a debit card: Both you and your remittance recipient will have access to the funds in the account through a debit card. This way, should your remittance recipient need money immediately you will no longer have to worry about going to send money right away since he/she will now have money

available in this account. Likewise, should you need money in the United States, you will also have access to funds from this account through the use of this debit card.

- Direct deposit of your remittance to this account: Remitting into an account facilitates your remittance recipient receiving the money you send: no longer will he/she have to go to the bank and wait in line to receive it or run the risk of losing the code/pin number. He/she can use any ATM to take out money using a debit card from anywhere in El Salvador.

Treatment 3: offer of joint account (*Cuenta Unidos*) and migrant-only account (*Ahorro Directo*)

Initiate conversation with questions regarding client's preferences for money sending and saving services. For example:

- Do your family members currently save money?
- Which service providers do they currently utilize to send money?
- What type of transfer do you use, traditional remittance (with code/password) or remittance by credit to an account?
- Why do you use these services?
- What difficulties does your family member have in picking up/receiving the money that you send?

I come to present to you a service which will allow your remittance recipient to obtain a "Cuenta Unidos" courtesy of Banco Agrícola. This account offers you the following advantages:

- Savings for your remittance recipient in El Salvador: Your remittance recipient will be able to open a savings account in his/her name in El Salvador through Banco Agrícola.
- Both your remittance recipient and you will be able to verify the balance on the account: This account will allow you to check the balance on the account through a service provided by a call center.
- Access to funds for you and your remittance recipient through a debit card: Both you and your remittance recipient will have access to the funds in the account through a debit card. This way, should your remittance recipient need money immediately you will no longer have to worry about going to send money right away since he/she will now have money available in this account. Likewise, should you need money in the United States, you will also have access to funds from this account through the use of this debit card.
- Direct deposit of your remittance to this account: Remitting into an account facilitates your remittance recipient receiving the money you send: no longer will he/she have to go to the bank and wait in line to receive it or run the risk of losing the code/pin number. He/she can use any ATM to take out money using a debit card from anywhere in El Salvador.

Present Ahorro Directo:
As part of this promotion, with the opening of a "Cuenta Unidos", you will also have the option of opening a private individual account, "Ahorro Directo", over which no one but yourself will have access or control, not even the person you are sharing Cuenta Unidos with. No one else except you will be able to check account balances or make withdrawals from this account, and no one else has to know that this account exists. It is exclusively yours.

Before describing the benefits of this account, let me ask you a few questions. *Consult with the client's preferences and experiences in regards to the management of his/her money. For example:*

- How much control do you have over the management of your finances in El Salvador?
- Do you save money now? How do you keep those savings? Do you save in the US or in El Salvador?
- What methods do you use to access your funds when you visit El Salvador?
- Have you considered the convenience of having a savings account in El Salvador for the future?

With the Ahorro Directo, you will have exclusive control over your money in El Salvador. This product will be very beneficial to you in the management of your finances for the following reasons:

- <u>You will have the power to personally manage your money in El Salvador:</u> You will have the opportunity to open a savings account in El Salvador in your name from here, over which only you will have access or control.
- <u>You will never again have to use an intermediary to save money for you:</u> If you save money through money transfers to your family or friends, with the opening of this account you will be able to make money transfers directly to your account without having to ask someone else any favors. In other words, you will be sending remittances to your own account instead of sending to someone else.
- <u>You will benefit from the added security:</u> Through the use of this product you will have access to your money in El Salvador as well as in the United States. This offers you important advantages, since you will not only be saving for your future, but you will also have the money available from any Banco Agricola branch in El Salvador in case you travel to El Salvador. In addition, this account includes a debit card, so you will no longer have to carry large amounts of cash with you to El Salvador, improving your personal security.
- <u>It will be easy for you to check your account balance:</u> This account will also allow you to check your balance through a call center.

<u>Appendix C: Analysis of Baseline Balance and Attrition from Baseline to Follow-up</u>

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, Appendix Table 2 presents the means of a number of baseline variables for each treatment group as reported prior to treatment. The first column of reported p-values is for F-tests of equality of means across the treatment groups, for each variable separately. The other three columns of p-values are for F-tests of the pairwise equality of means between observations in Treatment 0 and (respectively) Treatments 1, 2, and 3.

The first 9 variables listed in the table are the stratification variables (gender, US bank account, relationship to remittance recipient, and years in US category). Prior to randomization,

migrants were stratified into cells defined by the 48 unique combinations of the following categorical variables: gender (male/female), whether the migrant has a US bank account (yes/no), relationship to the primary remittance recipient (parent, spouse, child, or other), and years in the U.S. (0-5 years, 6-10 years, 11-15 years). The p-values on the F-test of the joint equality of means across all treatments are all far from conventional significance levels. In only one out of 27 pairwise comparisons with the Treatment 0 mean is there a statistically significant difference in means (the comparison between Treatments 2 and 0 for "recipient is migrant's other relative").[3] This one rejection of equality is not worrisome, however, as the regression estimates to come will control for stratification cell fixed effects (estimates will take advantage only of variation in treatment within stratification cell), and all results are robust to inclusion or exclusion of the stratification cell fixed effects.

The remaining variables in the table are other variables for which observations were not stratified prior to treatment assignment. For all these remaining variables, the p-values in essentially all cases are also large and we cannot reject the hypothesis that the means are identical across treatment groups.[4]

In some regression analyses of this paper, smaller subsamples are used when examining impacts on outcomes observed in the follow-up survey. Appendix Tables 3 and 4 report p-values of F-tests of the equality of means identical to those conducted in Table 2, for the US follow-up sample (N=508) and the US and El Salvador follow-up sample (N=385). We similarly find overall balance across treatment conditions in these smaller subsamples, with a small number of exceptions that are likely to have arisen simply by chance.

Attrition from follow-up surveys

Analysis of follow-up attrition patterns is presented in the bottom rows of Appendix Table 2.[5] An F-test does not reject the null of equality of attrition rates from the US follow-up survey (the 505-observation sample) at conventional levels, but rejects that hypothesis at the 10% level for attrition from either the US or El Salvador follow-up surveys (the 383-observation sample). As it turns out, the problem stems from Treatment 2: observations in Treatment 2 have much lower attrition rates, 10 percentage points lower than Treatment 0 for either type of attrition. F-tests of the equality of the Treatment 2 and Treatment 0 attrition rates are rejected at the 5% level for both types of attrition. By contrast, none of the corresponding pairwise comparisons of attrition rates (between Treatment 1 and Treatment 0 or Treatment 3 and Treatment 0) reject the null of equality at conventional significance levels.

Appendix D: Ruling out that Treatment 3 effect is due to marketing pitch alone

One question that arises is whether Treatment 3's effect on remittance-recipient savings is due to the marketing pitch *alone*, or whether it is crucial that the intervention offered the joint

[3] This is possible even though we randomized treatments within 48 stratification cells, since some of the cells had small numbers of migrants. When the number of migrants in a cell was not a multiple of 4, it was not possible to assign exactly 25% of migrants within cell to each treatment.

[4] The three exceptions are the pairwise comparison between Treatments 2 and 0 for "migrant's annual remittances sent", "migrant is US citizen", and "migrant is married or partnered", in which cases the means are significantly different at the 10% level. This small number of significant differences can be expected to arise by chance in any randomized control study.

[5] Attrition can be due to non-completion of the follow-up survey as well as missing savings data in that survey.

migrant/recipient accounts. The concern is that the financial empowerment induced by the Treatment 3 marketing pitch might have been enough to encourage migrants to exert control over funds in joint accounts that already existed or that they could easily set up on their own. Then the intervention's offer of the joint accounts at partner bank (and account-opening help) may have been superfluous.

To test this, we use the migrant follow-up survey data to check whether Treatment 3 led to increases in joint migrant/recipient savings at *other* (non-partner) banks. If the intervention's offer of assistance opening joint accounts at the partner bank was superfluous, and the marketing pitch was all that mattered, then we should also see Treatment 3 have positive effects on savings at other banks (many of whose branch locations may have been more conveniently located for family members in El Salvador).

Regression results are in Appendix Table 5. The dependent variables in the two columns are savings reported by the migrant in joint accounts *outside the partner bank* shared with primary remittance recipients (column 1) and with other people, not including the primary remittance recipient (column 2). As it turns out, there is no indication that Treatment 3 or either of the other treatments affects savings in joint accounts outside of the partner bank: none of the coefficients on the treatment indicators in columns 1 or 2 are statistically significantly different from zero.

We conclude from this analysis that the marketing pitch alone cannot explain Treatment 3's impact on remittance-recipient savings. Rather, Treatment 3's effect on remittance-recipient savings should be thought of as the *joint effect* of two things that occurred during the Treatment 3 marketing visit: 1) the offer of the joint account, and 2) the marketing pitch encouraging migrants to exert greater control over the savings of remittance recipients in El Salvador.

Appendix Table 1: Comparison of means, study participants vs. DC-area Salvadorans and Hispanics in US Census

Data source:	Baseline survey	US Census 2000		
Sample restriction:		Salvadoran-born	Salvadoran-born, not US citizen	Hispanic, not US citizen
Fraction male	0.71	0.57	0.57	0.57
Panel A: Men				
Age	30.34	30.34	30.25	30.62
	(7.36)	(8.87)	(8.84)	(9.13)
Migrant has been in US 0-5 years	0.49	0.33	0.35	0.45
Migrant has been in US 6-10 years	0.40	0.29	0.29	0.28
Migrant has been in US 11-15 years	0.10	0.38	0.36	0.27
No Education	0.07	0.13	0.13	0.09
Some Education, no degree	0.63	0.60	0.61	0.52
High School Degree or more	0.30	0.27	0.25	0.39
Migrant is US citizen	0.00	0.10	0.00	0.00
Migrant is married or partnered	0.53	0.45	0.44	0.47
Sample Size	638	1,933	1,735	4,427
Panel B: Women				
Age	32.20	31.71	31.66	32.59
	(8.19)	(9.52)	(9.42)	(9.98)
Migrant has been in US 0-5 years	0.51	0.29	0.31	0.40
Migrant has been in US 6-10 years	0.37	0.34	0.35	0.32
Migrant has been in US 11-15 years	0.12	0.37	0.34	0.28
No Education	0.08	0.13	0.13	0.08
Some Education, no degree	0.56	0.58	0.60	0.46
High School Degree or more	0.36	0.30	0.27	0.46
Migrant is US citizen	0.01	0.12	0.00	0.00
Migrant is married or partnered	0.73	0.57	0.57	0.59
Sample Size	260	1,453	1,259	3,263

Note: US Census 2000 data are IPUMS 5% sample, restricted to individuals aged 18-65 in the metro Washington DC area (including MD and VA suburbs). All variables other than age are indicator variables. Standard deviation in parenthesis for age variable.

Appendix Table 2: Means of variables by treatment group

	Treatment group				P-value of F-test …				Num. of obs.
	0	**1**	**2**	**3**	T0 = T1 = T2 = T3	T1 = T0	T2 = T0	T3 = T0	
Baseline stratification variables									
Migrant is female	0.26	0.31	0.31	0.28	0.556	0.226	0.266	0.720	898
Migrant has US bank account	0.61	0.61	0.66	0.63	0.590	0.883	0.209	0.676	898
Recipient is migrant's parent	0.55	0.55	0.54	0.56	0.949	0.998	0.740	0.802	898
Recipient is migrant's spouse	0.14	0.09	0.11	0.09	0.317	0.104	0.331	0.093	898
Recipient is migrant's child	0.05	0.04	0.02	0.03	0.480	0.757	0.185	0.343	898
Recipient is migrant's other relative	0.26	0.31	0.33	0.32	0.326	0.184	0.091	0.163	898
Migrant has been in US 0-5 years	0.52	0.49	0.48	0.51	0.811	0.604	0.389	0.919	898
Migrant has been in US 6-10 years	0.39	0.40	0.41	0.38	0.893	0.729	0.638	0.824	898
Migrant has been in US 11-15 years	0.10	0.10	0.11	0.11	0.927	0.768	0.523	0.611	898
Baseline survey variables									
Migrant's years in the US	5.42	5.47	5.76	5.59	0.764	0.904	0.330	0.635	898
Migrant has El Salvador bank account	0.16	0.15	0.18	0.20	0.500	0.632	0.571	0.329	898
Migrant's annual income (US$)	30,669	36,587	29,108	28,109	0.555	0.452	0.481	0.285	865
Migrant's household's annual income (US$)	36,355	42,264	42,376	37,319	0.782	0.448	0.466	0.736	896
Migrant's years of education	8.62	8.15	8.94	8.35	0.213	0.271	0.415	0.506	865
Migrant's age	30.61	31.05	31.02	30.84	0.929	0.567	0.553	0.761	894
Migrant's annual remittances sent (US$)	5,451	4,876	4,689	4,974	0.314	0.187	0.062	0.268	898
Migrant's total hh savings balance (US$)	2,942	3,080	2,544	2,883	0.747	0.796	0.415	0.905	806
Migrant is US citizen	0.00	0.00	0.01	0.01	0.112	0.317	0.083	0.156	894
Migrant hh size in U.S.	4.72	5.07	4.84	4.62	0.198	0.104	0.546	0.625	898
Migrant is married or partnered	0.54	0.58	0.62	0.60	0.329	0.467	0.075	0.211	897
Past experience with direct payments	0.08	0.08	0.07	0.08	0.940	0.898	0.743	0.792	898
Sent funds to El Salvador for others to administer	0.23	0.20	0.25	0.24	0.525	0.344	0.665	0.860	898
Interested in direct payments to increase control	0.21	0.24	0.20	0.19	0.695	0.489	0.893	0.622	898
Aware of disagreements with recipients over remittance uses	0.16	0.15	0.14	0.13	0.768	0.633	0.537	0.289	898
Have had disagreements with recipients over remittance uses	0.05	0.07	0.05	0.04	0.433	0.257	0.976	0.587	898
Demand for control (union of above five indicators)	0.53	0.49	0.49	0.52	0.749	0.414	0.368	0.840	898
Correct answer to compound interest question	0.64	0.66	0.67	0.68	0.841	0.691	0.581	0.366	898
Correct answer to inflation question	0.61	0.66	0.63	0.68	0.518	0.332	0.667	0.160	898
Correct answer to mutual fund question	0.41	0.37	0.34	0.36	0.490	0.399	0.121	0.360	898
Tracks spending and budgets expenses	0.46	0.50	0.43	0.46	0.617	0.480	0.531	0.982	897
Baseline variables from El Salvador household survey									
Recipient's total hh savings balance (US$)	249	543	274	459	0.305	0.114	0.846	0.217	733
Recipient's annual remittances received (US$)	3,136	3,112	3,244	3,224	0.957	0.929	0.731	0.769	725
Attrition from follow-up survey									
Attrition from US follow up savings data	0.49	0.45	0.39	0.42	0.132	0.451	0.024	0.133	898
Attrition from either US or El Salvador follow-up savings data	0.58	0.58	0.48	0.53	0.064	0.941	0.019	0.278	898

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. Savings figures reported in US dollars. See previous table for other notes.

Appendix Table 3: Means of variables by treatment group, US follow-up sample

	Treatment group				P-value of F-test …				Num. of obs.
	0	1	2	3	T0 = T1 = T2 = T3	T1 = T0	T2 = T0	T3 = T0	
Baseline stratification variables									
Migrant is female	0.29	0.28	0.33	0.29	0.761	0.903	0.408	0.941	508
Migrant has US bank account	0.58	0.63	0.67	0.64	0.553	0.408	0.150	0.333	508
Recipient is migrant's parent	0.53	0.52	0.55	0.58	0.790	0.940	0.750	0.408	508
Recipient is migrant's spouse	0.16	0.10	0.09	0.11	0.438	0.219	0.102	0.305	508
Recipient is migrant's child	0.07	0.05	0.02	0.04	0.168	0.570	0.062	0.255	508
Recipient is migrant's other relative	0.24	0.32	0.34	0.27	0.251	0.179	0.077	0.642	508
Migrant has been in US 0-5 years	0.49	0.47	0.44	0.52	0.607	0.748	0.413	0.666	508
Migrant has been in US 6-10 years	0.39	0.41	0.44	0.37	0.656	0.809	0.445	0.675	508
Migrant has been in US 11-15 years	0.12	0.12	0.12	0.11	0.998	0.897	0.924	0.971	508
Baseline survey variables									
Migrant's years in the US	5.59	5.58	5.75	5.73	0.975	0.989	0.725	0.778	508
Migrant has El Salvador bank account	0.17	0.17	0.14	0.21	0.465	0.930	0.520	0.381	508
Migrant's annual income (US$)	30,297	41,590	29,034	28,447	0.786	0.420	0.715	0.635	490
Migrant's household's annual income (US$)	34,996	50,717	34,425	39,389	0.385	0.258	0.859	0.303	508
Migrant's years of education	8.46	8.03	8.63	8.56	0.698	0.453	0.744	0.863	491
Migrant's age	31.08	32.21	31.10	31.00	0.600	0.292	0.988	0.937	505
Migrant's annual remittances sent (US$)	5,335	5,532	4,855	5,197	0.583	0.758	0.397	0.821	508
Migrant's total hh savings balance (US$)	2,731	3,097	1,907	2,819	0.138	0.630	0.141	0.892	469
Migrant is US citizen	0.00	0.00	0.01	0.01	0.224	0.000	0.157	0.316	505
Migrant hh size in U.S.	4.87	5.21	4.98	4.50	0.095	0.271	0.665	0.187	508
Migrant is married or partnered	0.57	0.58	0.62	0.62	0.811	0.862	0.427	0.459	508
Past experience with direct payments	0.11	0.10	0.08	0.08	0.831	0.773	0.461	0.411	508
Sent funds to El Salvador for others to administer	0.25	0.20	0.24	0.25	0.747	0.363	0.851	0.973	508
Interested in direct payments to increase control	0.20	0.23	0.17	0.16	0.468	0.481	0.540	0.467	508
Aware of disagreements with recipients over remittance uses	0.16	0.14	0.15	0.14	0.960	0.651	0.758	0.615	508
Have had disagreements with recipients over remittance uses	0.05	0.07	0.05	0.03	0.507	0.617	0.993	0.372	508
Demand for control (union of above five indicators)	0.53	0.50	0.45	0.55	0.419	0.733	0.237	0.724	508
Correct answer to compound interest question	0.60	0.71	0.67	0.67	0.330	0.066	0.214	0.239	508
Correct answer to inflation question	0.62	0.60	0.63	0.66	0.808	0.805	0.773	0.510	508
Correct answer to mutual fund question	0.37	0.37	0.31	0.36	0.715	0.903	0.374	0.905	508
Tracks spending and budgets expenses	0.44	0.44	0.44	0.47	0.935	0.928	0.968	0.578	508
Baseline variables from El Salvador household survey									
Recipient's total hh savings balance (US$)	273	684	336	584	0.392	0.147	0.745	0.234	428
Recipient's annual remittances received (US$)	2,988	3,371	3,474	3,525	0.501	0.293	0.221	0.179	426

Notes -- Table presents means of key variables for each treatment group prior to treatment, for observations that have valid savings data in US follow-up survey. See Table 2 for other notes.

Appendix Table 4: Means of variables by treatment group, US and El Salvador follow-up sample

	Treatment group				P-value of F-test …				Num. of obs.
	0	1	2	3	T0 = T1 = T2 = T3	T1 = T0	T2 = T0	T3 = T0	
Baseline stratification variables									
Migrant is female	0.28	0.26	0.32	0.29	0.832	0.765	0.549	0.960	385
Migrant has US bank account	0.56	0.64	0.64	0.63	0.702	0.304	0.307	0.353	385
Recipient is migrant's parent	0.54	0.54	0.58	0.59	0.817	0.943	0.539	0.494	385
Recipient is migrant's spouse	0.16	0.08	0.10	0.12	0.425	0.115	0.186	0.429	385
Recipient is migrant's child	0.06	0.06	0.02	0.04	0.246	0.985	0.166	0.587	385
Recipient is migrant's other relative	0.24	0.32	0.30	0.24	0.495	0.212	0.325	0.878	385
Migrant has been in US 0-5 years	0.48	0.49	0.43	0.57	0.247	0.941	0.483	0.233	385
Migrant has been in US 6-10 years	0.42	0.42	0.45	0.33	0.293	0.928	0.718	0.180	385
Migrant has been in US 11-15 years	0.09	0.10	0.12	0.10	0.947	0.980	0.584	0.864	385
Baseline survey variables									
Migrant's years in the US	5.52	5.43	5.73	5.48	0.944	0.865	0.685	0.944	385
Migrant has El Salvador bank account	0.19	0.14	0.13	0.22	0.265	0.428	0.250	0.546	385
Migrant's annual income (US$)	29,804	45,095	28,063	28,582	0.796	0.417	0.641	0.784	371
Migrant's household's annual income (US$)	34,782	54,616	32,531	39,448	0.283	0.289	0.474	0.321	385
Migrant's years of education	8.52	7.74	8.51	8.62	0.505	0.240	0.987	0.872	373
Migrant's age	31.14	31.88	31.19	31.36	0.921	0.556	0.965	0.859	382
Migrant's annual remittances sent (US$)	5,636	5,315	4,891	5,555	0.577	0.648	0.286	0.913	385
Migrant's total hh savings balance (US$)	2,911	3,109	1,694	2,542	0.074	0.829	0.055	0.624	358
Migrant is US citizen	0.00	0.00	0.02	0.01	0.224	0.000	0.157	0.316	383
Migrant hh size in U.S.	5.05	5.33	5.15	4.63	0.213	0.405	0.714	0.210	385
Migrant is married or partnered	0.55	0.57	0.64	0.66	0.354	0.807	0.235	0.127	385
Past experience with direct payments	0.12	0.07	0.07	0.08	0.678	0.301	0.237	0.421	385
Sent funds to El Salvador for others to administer	0.26	0.23	0.22	0.22	0.927	0.615	0.524	0.589	385
Interested in direct payments to increase control	0.22	0.26	0.19	0.15	0.317	0.557	0.521	0.227	385
Aware of disagreements with recipients over remittance uses	0.16	0.13	0.16	0.12	0.796	0.534	0.944	0.427	385
Have had disagreements with recipients over remittance uses	0.06	0.06	0.06	0.03	0.642	0.985	0.988	0.355	385
Demand for control (union of above five indicators)	0.60	0.54	0.45	0.54	0.188	0.395	0.033	0.421	385
Correct answer to compound interest question	0.58	0.75	0.67	0.67	0.127	0.017	0.179	0.182	385
Correct answer to inflation question	0.66	0.68	0.64	0.65	0.939	0.786	0.730	0.934	385
Correct answer to mutual fund question	0.39	0.37	0.31	0.38	0.575	0.797	0.224	0.880	385
Tracks spending and budgets expenses	0.44	0.39	0.44	0.45	0.878	0.580	0.940	0.853	385
Baseline variables from El Salvador household survey									
Recipient's total hh savings balance (US$)	310	779	348	602	0.487	0.176	0.872	0.358	347
Recipient's annual remittances received (US$)	2,932	3,440	3,613	3,524	0.407	0.218	0.132	0.181	346

Notes -- Table presents means of key variables for each treatment group prior to treatment, for observations that have valid savings data in both US and El Salvador follow-up surveys. See Table 2 for other notes.

Appendix Table 5: Impact of treatments on joint account savings reported by migrant in non-partner and partner banks in follow-up survey
(Ordinary least-squares estimates)

Dependent variable: Savings not at partner bank …	... in joint accounts, migrant and principal recipient	... in joint accounts, migrant and others
	(1)	(2)
Treatment 3 (joint account + indiv. migrant account)	22.478	38.526
	(24.056)	(29.103)
Treatment 2 (joint account)	11.706	84.733
	(19.903)	(51.998)
Treatment 1 (remittance recipient account)	129.312	136.518
	(124.279)	(113.770)
Marketer fixed effects	Y	Y
Treatment month fixed effects	Y	Y
Stratification cell fixed effects	Y	Y
Observations	508	508
R-squared	0.045	0.071
P-value of F-test: equality of …		
Treatment 3 & 2 coeffs.	0.498	0.392
Treatment 3 & 1 coeffs.	0.322	0.384
Treatment 2 & 1 coeffs.	0.303	0.680

Robust standard errors in parentheses. * 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 (comparison 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.