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

April 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

Between 1965 and 2010, individuals living outside their countries of birth grew from 2.2% to 3.1% of world population, reaching a total of more than 200 million people in the latter year.[1] The remittances that these migrants send to origin countries are an important but relatively poorly understood type of international financial flow. Migrant remittances compare in magnitude to other important financial flows destined for developing countries, such as official development assistance and direct foreign investment. In 2009, migrant remittances sent to developing countries amounted to US$307 billion (World Bank 2011). Improvements in remittance data collection and continued immigration flows to developed countries have generated substantial recent interest in the remittance phenomenon, as evidenced by a proliferation of recent policy-oriented reports.[2]

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

To date, however, we know very little about how migrants make their remittance-sending decisions. In particular, it is unknown whether migrants desire greater control over how family members back home use the remittances they receive. Do migrants and remittance recipients typically agree on the uses to which remittances should be put? If not, are migrants able to control how remittances are spent by recipients? How does degree of control affect amounts remitted, and the uses to which remittances are put? In the absence of control, do migrants simply send remittances as "gifts" with no attempt to direct their use? If migrants were to be given more control over remittance uses, how would they direct them to be used? A better understanding of these questions could have substantial impact on public policy, by suggesting policies to further stimulate remittance flows and potentially channel them towards more productive uses in migrant source countries.

Migrants may have greater preferences for investment and savings than remittance-recipient households, but can only imperfectly monitor how remittances are spent. Without monitoring or control over the use of remittances, therefore, migrants may choose to keep their

[1] Estimates of the number of individuals living outside their countries of birth are from United Nations (2008).
[2] Reports funded by the Multilateral Investment Fund of the Inter-American Development Bank include Pew Hispanic Center (2002) and Terry and Wilson (2005). The World Bank has also funded substantial publications on the topic, such as World Bank (2006) and World Bank (2007).

earnings overseas and to remit less. This research aims to shed light on the extent to which migrants' lack of direct control over the use of remittances affects remittance flows and on the impact of new financial products that could increase migrant control.

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

We designed a randomized field experiment that offered new facilities for Salvadoran migrants to directly channel their remittances into savings accounts in El Salvador. We developed these savings facilities in conjunction with Banco Agricola, El Salvador's largest bank. To isolate the importance of migrant control over savings, we estimate the impact of offering products that provide migrants with varying levels of control over remittances.

This research makes new contributions to a large literature in economics on intra-household decision-making. Attempts to understand the extent and nature of conflict between household members are central to research on the economics of the family, in both developed and developing countries. A wide variety of empirical studies have cast serious doubt on the "unitary model" of the household, the proposition that the joint actions of a household comprised of separate optimizing individuals can be represented as the actions of a single utility-maximizing agent.[3]

More recent models therefore take explicit account of potential preference differences among household members. Manser and Brown (1980) and McElroy and Horney (1981) model the allocation of household resources as the solution to a Nash cooperative bargaining problem, where the extent to which an individual's preferences hold sway depends on his or her "threat point" (utility in the event of household dissolution or divorce). Lundberg and Pollak (1993) assume instead that the threat point is determined by a non-cooperative equilibrium within the household. Browning and Chiappori (1998) make the more minimal assumption that households achieve efficiency of resource allocation; their empirical tests provide evidence in favor of the efficient household model, rejecting the unitary model. However, even the minimal assumption of efficiency has been called into question by Udry (1996), who finds productive inefficiencies in resource allocation across male- and female-controlled farm plots in Burkina Faso, and Dubois and Ligon (2005), who document intra-household allocative inefficiencies in the Philippines.

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

[3] See the review in Strauss and Thomas (1995), as well as, more recently, Duflo (2003), Rangel (2006), and Martinez (2006).

mutually-beneficial cooperative agreements.[4] 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, remittance amounts may be lower than under perfect information, and the use of remittances may diverge from uses preferred by migrants. De Laat (2005) shows that male Kenyan migrants spend considerable resources monitoring their rural wives, consistent with the existence of moral hazard in wives' implementation of husbands' remittance instructions. Chen (2006) finds evidence in China that non-cooperative behavior by wives when husbands have migrated is greater for behaviors that are more difficult to monitor.

This study assesses whether information asymmetries in the household lead to lower savings from the remittances that migrants send. It examines whether innovative financial products that give migrants monitoring of and control over savings encourage migrants to increase their remittance amounts and to accrue more savings in remittance-receiving households.

Our impact evaluation uses a randomized treatment-control methodology. Migrants in the study are randomly assigned across three treatment conditions or a control group. Therefore, comparisons across the various treatment conditions reveal the causal impact of offering migrant varying degrees of control on our outcomes of interest (which include savings account take-up, savings balances, and remittances). Our control group is referred to as Treatment 0, and received no offer of any savings accounts. 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 control over remittances sent to El Salvador.[8]

Each treatment was accompanied by a marketing pitch delivered by our project staff. In keeping with the nature of the products being offered, the substantive content of the scripts also differed across treatments, with Treatment 3 having the strongest emphasis on the importance of exerting control over one's own finances and the finances of remittance recipients.[9] We thus cannot separate out the effect of the product offers from the effect of the marketing that was tied to the product offers.

The study is unique in that it uses randomized field experimental methods to examine the impact of remittance-related financial services in an immigrant population, and in particular in that it examines exogenous variation in control over savings accounts in transnational

[4] Ashraf (2009) shows that husbands and wives change whether they choose to consume or save their money when they are being observed by their spouse.

[8] 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.

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

households.[10] As such, it represents a substantial improvement over existing non-experimental studies which must infer the existence of control problems indirectly, and where the direction of causation is not known with certainty. The intervention studied is also unusual among development economics field experiments in that it is conducted among migrants in a developed country, while many key outcomes of interest are for individuals who remain behind in a developing country. Data on activity 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 corresponding remittance-receiving households in El Salvador provide data on other outcomes.

This research also contributes to our understanding of the use of financial services by poor and particularly immigrant populations in the United States. Existing survey work has documented that Hispanics in the U.S. are less likely to have bank accounts than the native white population. Lusardi and Mitchell (2007) document lower financial literacy and net worth among Hispanics compared to whites in the U.S.[11] As a result, savings in Hispanic immigrant households are frequently held in the form of cash.[12]

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

Savings rose substantially in the treatment condition that offered migrants the greatest degree of control over El Salvador savings, Treatment 3 (where we offered joint accounts shared by migrants and remittance recipients as well as migrant-only accounts.) In this treatment, 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 statistically significantly larger than the effects of other treatments that offered migrants less control over savings (Treatments 1 and 2). 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 survey evidence that these funds were simply shifted over from other savings vehicles.

---

[10] 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.

[11] In addition, the 2001 National Survey of Latinos finds that, like Latinos in general, Salvadorans in the US tend to be less likely to use financial products than whites or African-Americans. Only 67% of Salvadorans have a bank account, compared with 76% of African-Americans and 95% of whites.

[12] Jankowski, Porter, and Rice (2007) document that demand for US$100 bills is higher in Chicago zip codes that have higher immigrant (and specifically Hispanic) population shares. They interpret this as evidence that Hispanic immigrants have poor access to formal savings facilities, leading them to resort to cash savings ($100 bills are preferred for cash savings due to reduced bulkiness).

Strikingly, Treatment 1 (where we offered accounts in the name of remittance recipients alone) had no 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 not by itself increase savings in the migrant home country.

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 substantially larger 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.[13]

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 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 the "financial empowerment" channel. While we acknowledge this term is not well-defined in the economics literature, we prefer it to the term "financial literacy." We intend financial empowerment to capture the *willingness* to use available financial services to achieve one's financial objectives. As such, it is concept distinct from financial literacy, which generally is used to refer 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.[14]

However, if this channel was really at work one would expect it to have more far-reaching effects, potentially beyond the effects on the accounts we offered. We find evidence for

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

[14] 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.

this through our follow-up survey data on savings held outside the partner bank. Among migrants who express baseline demand for control, Treatment 3 also led to a substantial increase in savings at other non-partner institutions (mainly banks in the U.S.).

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 and in particular investigates migrant and recipient expressed preferences over remittance uses. 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

Focus on El Salvador

For several reasons, El Salvador is well-suited for this study. It is highly unusual among developing countries in its number of overseas migrants relative to the national population: at least one in seven Salvadorans lives outside of the country, primarily in the United States. Estimated total personal income of Salvadorans living in the United States was $13.3 billion in 2001, roughly equal to El Salvador's GDP in that year.[17] In 2001, the number of Salvadorans living in the United States was between 0.8 and 1.1 million.[18] Large flows of Salvadorans into the US started with the civil war in 1980, and have continued at a remarkably steady pace since the war ended in 1992. The number of Salvadorans in the US rose substantially from 1990 to 2000 (by anywhere from 68% to 81%, depending on definitions). Concurrent with the expansion of Salvadorans overseas, the dollar value of remittances sent to El Salvador has also grown dramatically, from $790 million to $3.8 billion between 1991 and 2008.

Treatments

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).

In order to ascertain causal impacts, we randomly assigned study participants to one of three treatment groups or a comparison group, each with equal (25%) probability. Randomization was carried out after first stratifying 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).

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

[18] Depending on the definition of "Salvadoran".

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.[19] There were three treatment groups, 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 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

[19] Because this study investigates control over savings, to avoid confusion we refer to Treatment 0 as the "comparison group," not as the "control group." (One reader has suggested that we refer to our treatment conditions as "control groups," and Treatment 0 as the "out-of-control group." We have declined to implement this suggestion.)

of the new account.[20] 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, could withdraw using their ATM card via US ATMs, and could 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.

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). Also as in Treatment 1, 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).[21]

Compared to Treatment 1, Treatment 2 offered the migrant the ability to monitor the savings of family members, but it did not provide full control over the funds. Indeed, the joint

[20] 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.

[21] 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.

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". This facility was also newly designed by the project. Ahorro Directo 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 the marketing visit for Treatment 3, 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. Ahorro Directo was offered second. The marketing script for Ahorro Directo 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.

In Treatment 3, if migrants wanted to open an Ahorro Directo account, we required them to *also* open a Cuenta Unidos account. We instituted this requirement to ensure that account opening transaction costs were identical across Treatments 2 and 3. 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. 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.[23] 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.[24]

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.

[23] 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.

[24] 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.

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 savings balances in El Salvador.

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, savings of the primary remittance recipient in El Salvador). 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.

The difference (α_3- α_2) represents the difference in the impact of Treatment 3 vis-à-vis Treatment 2, and the difference (α_2- α_1) represents the difference in the impact of Treatment 2 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.

If α_1 were positive and statistically significantly different from zero, this would be evidence that the account-opening assistance was effective and that some migrants value remitting into the savings account of someone else in El Salvador. If the differences α_3- α_2 and α_2- α_1 are similarly positive and statistically significantly different from zero, this would be evidence in favor of the study's hypothesis that migrants value the ability to monitor or control their savings.

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 our treatments were administered. The follow-up round of the migrant and El Salvador household survey occurred roughly one year after the initial product offer (from March to June 2009) to measure impacts on outcomes not observed in our partner bank's 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. Aside from data on savings in the accounts we offered as part of the intervention (which we refer to as "project accounts"), the partner bank also searched for other accounts of study participants within their databases (matching by name and address) to allow us to examine savings in other accounts not offered as part of this project ("non-project accounts").

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. The follow-up survey contains 505 observations with valid migrant-reported savings data. Also, for 383 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. Migrants in the DC sample are 29% female. Their mean age is 30.9, and mean years in the US is 5.6. 50% of migrants have been in the U.S. for 5 years or less. Only 1% of migrants surveyed are U.S. citizens.

Migrants live in households with a mean of 4.8 individuals. 59% of migrants are married or have an unmarried partner.

Median annual income is roughly $31,200 for the household in which the migrant lives, and $24,960 for the migrant (the means are somewhat higher due to large positive outliers). Median annual remittances sent to El Salvador by DC-area migrants is $3,900. Recipient households in El Salvador have median annual income of $3,368. The most common type of recipient (in terms of familial relationship to migrant) is a parent, accounting for 55% of recipients. Spouses, children, and other relatives account respectively for 11%, 4%, and 30% of recipients.

A relatively high fraction of DC-area migrants have bank accounts, but their savings levels are quite low. 61% of migrants have an account in the US, and 18% have an account in El Salvador. Median savings, however, is just $750.

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:

- The migrant had ever paid directly for expenditures of remittance recipients in El Salvador, rather than sending cash (7.7% of migrants did so).
- The migrant had sent funds home for others to administer on his/her behalf (23.7% of migrants did so).
- The migrant was interested in direct payments to improve control over remittance uses (20.7% of migrants said yes).
- The migrant knew anyone who had had conflict with recipients over remittance uses (14.6% of migrants said yes).
- 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.[25] 66%, 64%, and 37% of migrants responded correctly to the questions on (respectively) compound interest, inflation, and mutual funds. We also asked whether migrants tracked spending and budgeted their expenses, and 46% of migrants reported "always" or "almost always" doing so.

Balance of baseline characteristics across treatment groups

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

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.

[25] 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").

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. However, in three pairwise comparisons with the Treatment 0 mean there are statistically significant differences in means of one of the relationship to recipient variables.[26] These differences are 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.[27]

In some regression analyses of this paper (Tables 8, 9, and 10), smaller subsamples are used when examining impacts on outcomes observed in the follow-up survey. Appendix Tables 1 and 2 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=505) and the US and El Salvador follow-up sample (N=383). We similarly find overall balance across treatment conditions in these smaller subsamples. The few exceptions are some of the pairwise tests of means across Treatments 2 and 0, and may be related to the differentially lower attrition seen among Treatment 2 observations (to which we now turn).

Attrition from follow-up surveys

Analysis of follow-up attrition patterns is presented in the bottom rows of Table 2.[28] 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

[26] 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.

[27] The two exceptions are the pairwise comparison between Treatments 2 and 0 for "migrant's annual remittances sent" and "migrant is married or partnered", in which cases the means are significantly different at the 5% and 10% levels respectively. This small number of significant differences can be expected to arise by chance in any randomized control study.

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

both types of attrition (p-values are 0.025 and 0.019 respectively). 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.

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 8, 9, and 10) may be due to sample selection rather than a causal effect of Treatment 2.[29]

Expressed preferences regarding use of remittances

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

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

Mindful of these issues, our approach was to tell survey respondents that their household in El Salvador was being entered into a raffle as part of the study. Respondents were told that 10 households in the study would win a prize of $100. Each migrant was told that if their household in El Salvador won, the migrant would be able to specify exactly how the $100 prize would be spent. The migrant was given a list of expenditure items, and was asked to divide the $100 across one or more of these items. The list did not contain a "cash" option (the migrant could not say that some or all of the winnings would be given over in cash). A project representative would personally visit the household to ensure that the $100 was spent exactly how the migrant specified.

Households in El Salvador were told of this raffle as well, and the household respondent was asked how he or she would like the $100 to be allocated across the same expenditure categories. They were similarly told that a project representative would enforce that expenditure allocation should the household win the raffle. Households were not told how the DC-area

[29] 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 4 through 7). Partner bank savings data are obtained from administrative records, so attrition is not an issue.

migrant had previously responded to the same question (and survey staff in El Salvador did not have that information).[30]

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

Stark differences indeed emerged between migrants' and households' allocations. Figure 1 presents the average breakdown of allocations across 13 expenditure categories for migrants (left-side pie chart) and households (right-side pie chart) for 738 pairs of migrants and households for which corresponding data on these raffle allocations are available, while Table 3 presents the mean allocations and the P-value of the F-test of the equality of the migrant vs. household means. The most obvious difference is that migrants allocate a much smaller amount ($42.49) to "daily consumption" expenditures than do recipient households, who allocate $64.86 to daily consumption on average. A large fraction of that difference is accounted for by the fact that migrants allocate $21.08 to savings, while households allocate just $2.56 to savings on average. Both these differences are statistically significantly different from zero at the 1% level. The pattern is suggestive that migrants have substantially stronger preferences for savings than do recipient households.[31]

Some of the less-important expenditure categories also reveal differences between migrants and households. The categories where the differences are statistically significantly different from zero (at the 5% level) are as follows: phone bills (migrants $1.46, households $0.47), durable goods (migrants $4.69, households $0.66), and "other" (migrants $1.21, households $5.07). The higher allocation by migrants to phone expenditures may reflect a greater desire on the part of migrants to maintain connections with their families back home.

IV. Impact of Treatments on Savings at the Partner Bank

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

[30] The raffle was held at the beginning of 2009. 10 El Salvador households were chosen at random to win the $100. In half of these the migrant's expenditure allocation was implemented, and in the other half the household's expenditure allocation was implemented by one of our research assistants at the time the prizes were given out.

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

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 4. 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.[32]

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, but note that this figure is not statistically significantly different from zero. 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.

[32] 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 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.

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 roughly 7 percent higher ownership 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 in either specification (p-values are 0.074 and 0.042, respectively), 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 both specifications.

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 5, 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.[34] 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 5% and 1% levels respectively in columns 1 and 2, and becomes slightly larger in magnitude with the addition of controls to the regression. The coefficient in column 2 indicates that savings in recipient project accounts are higher by $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 and are not statistically

[34] 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.

significantly different from zero. The F-tests reported at the bottom rows of the table indicate that the Treatment 3 coefficient is statistically significantly larger than the corresponding coefficients on Treatments 2 and 1, each at the 5% level.

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; savings in this type of account in Treatment 3 amount to a bit more than $50 on average.

Treatment 3 does not have a substantial or significant effect on savings in joint accounts shared by migrants and individuals in El Salvador other than the primary remittance recipient: the coefficient on Treatment 3 in the 5th and 6th columns of the table, while positive, is not statistically significantly different from zero at the 5% level. On the other hand, there is a modest positive effect of Treatment 2 on savings in this type of account that is significant at the 10% level, amounting to about $25 (in the specification with controls).

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 trans-national household are larger by $211. The Treatment 3 coefficient is statistically significantly different from the corresponding coefficients for Treatments 2 and 1, at the 5% and 1% 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).

V. Interpretation and additional analyses

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. This increase in savings 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 had a statistically significant impact on savings in *recipient* accounts, while Treatment 2 did not. 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 6 presents coefficient estimates from the regression from which we come to this conclusion. The regression is analogous to that of column (a) of Table 5 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 5, as well as the main effects of all variables interacted with the treatment indicators.[35] The first column reports interactions between Treatment 2 and a variety of baseline characteristics, and the 2nd column corresponding interactions with Treatment 3.[36]

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 and significant, 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, and two out of the four of the coefficients are statistically significantly different from zero at conventional levels. Interestingly, the effect of Treatment 3 is not statistically significantly different from zero for migrants who themselves had disagreements with recipients over remittance uses (and the point estimate on the interaction term is actually negative). It is possible that the problem of control is simply too great in such cases for Treatment 3 to make much of a difference. A test of the joint significance of all five interaction terms between Treatment 3 and the demand for control variables rejects the null that they are jointly zero at the 1% level.

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

[35] Inclusion of the additional baseline variables in the regression causes sample size to fall slightly due to missing values of some of these variables.

[36] All these interaction terms are estimated in the same regression. All corresponding interaction terms with Treatment 1 are also included in the regression, and none of these are statistically significantly different from zero (not shown to conserve space).

any of the five separate indicators are equal to 1, and 0 otherwise (by this measure, 51% of migrants evince demand for control). In Table 7 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).[37] The dependent variables in Table 7 are the same dependent variables examined in Table 5. 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 1% level, and indicates that Treatment 3's impact on recipient savings in project accounts at the partner bank is $242 among migrants exhibiting any type of demand for control at baseline. By contrast, the effect of Treatment 3 on this category of savings those without baseline demand for control is much smaller in magnitude and not significantly different from zero. The p-value of the F-test that these two effects are equal across migrants with and without demand for control (reported lower down in the table) is rejected at the 10% level.

The remaining columns of Table 7 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 are 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 would be particularly sensible if migrants are 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 one is deported and faces difficulty accessing US bank accounts.

In the other rows of Table 7, it is quite striking that in no case is the difference in the effect of Treatments 2 or 1 different across migrants with and without baseline demand for control (none of the p-values in any of the pairwise tests of coefficient differences are below 0.10). 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

[37] The demand for control main effect is also included in all regressions of the table. The 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".

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 exert control over the 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 6 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), two interaction terms with Treatment 3 are negative and statistically significantly different from zero (at the 5% and 10% levels, respectively): the interaction with an indicator that the migrant "tracks spending and budgets expenses" at baseline, and the interaction with an 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).[38]

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 8 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 United States 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 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 7 when examining savings at the partner bank. In Panel B of Table 8, 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 held in the US (column b) and on overall savings (column d).

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

[38] 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.

have raised savings in other financial institutions that had no connection to our intervention.[39] It appears that Treatment 3 led migrants to change their savings strategies 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.

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 4 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 4, 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 6, 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 three variables, the difference in the interaction term coefficients is statistically significantly different from zero at conventional levels, indicating differential selection into Treatment 3along these dimensions compared to Treatment 2. Specifically, Treatment 3 (relative to Treatment 2) leads to more remittance-recipient account opening among migrants who track spending and

[39] 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 in Section IV above).

budget expenses, whose primary remittance recipient is their spouse, or whose primary remittance recipient is some other relative.

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 identified as sources of differential selection into remittance-recipient account opening must also be associated with differentially higher effects of Treatment 2 on savings. To answer this question, we examine the regression that including 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 were found to be sources of differential selection into Treatment 3 vs. Treatment 2 appear to lead to higher savings accumulation under Treatment 2. None of the interaction terms of "tracks spending and budgets expenses," "recipient is migrant's spouse," and "recipient is migrant's other relative" with Treatment 2 are statistically significantly different from zero at conventional levels.

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 such differential joint account opening are associated with higher savings balances in Treatment 2 (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.[40] 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, however, this is not the case. In the savings regression of Table 6 (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

[40] 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.

(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 account savings is due to increased migrant bargaining power resulting from 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 383 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.[41] Table 9 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.

The first four columns present impacts on savings reported by the DC-based migrant, (a) in banks in El Salvador, (b) in United States 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 8 (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 as well as 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.

The next four 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, and statistically significantly different from zero (at the 10% level) in the case of Treatment 2. 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 and again significant at the 10% level for Treatment 2. In Panel B, impacts

[41] We have confirmed that the results of Table 5 (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.

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.[42] 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 10% 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.

In sum, the conclusion to take from Table 9'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 5 and 7) 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 $1,803.55, which is 192% of mean savings among migrants with demand for control in the comparison group ($937.85).

Impact on remittances and migrant earnings

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. We now turn to examining where the newly saved resources may have come from. 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. Migrants' additional US savings also need to have come from somewhere, so we also examine impacts of the treatments on migrants' self-reported earnings per week.[43] Results are presented in Table 10.

As in previous tables, 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 the first three columns is monthly remittances sent by the migrant to

[42] 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).

[43] Unfortunately, the migrant follow-up survey does not include a consumption module so we cannot examine whether increased savings were funded via reductions in migrant consumption expenditures.

the primary remittance recipient, and the dependent variable in the fourth column is migrant earnings.

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 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 double 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 7 (column a, 2nd row), this induced increase in remittances sent via the partner bank by migrants without demand for control did not lead to increased savings at the partner bank, which we view as consistent with our overall 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 strongly rule out large effects on remittances.[44]

While the treatments did not lead to statistically significant increases in remittances, it does appear that increases in migrant savings in the US may be funded via increases in migrant earnings. In column 4 of the table, the main effect of Treatment 3 in Panel A is positive, although not statistically significantly different from zero at conventional levels. However, the interaction

[44] The 95% confidence interval of the coefficient on the Treatment 3 * (Demand for control) interaction term is [-136.26,132.11]. Taking an estimate at the high end of that range, $130 per month over 12 months would mean an increase in total 12-month remittances of $1,560. This amount would be more than sufficient to fund the observed increase in remittance recipient savings at the partner bank, which is $625.90 in column (g) of Table 7.

results in Panel B reveal that there is a large, positive, and statistically significant effect (at the 5% level) of Treatment 3 for migrants with demand for control. The point estimate indicates that Treatment 3 leads migrants with demand for control to raise their monthly earnings by $505.03. This increase in earnings amounts to over $6,000 on an annual basis, which would be more than enough to fund the increases in US savings for Treatment 3 migrants with demand for control estimated in Table 9.[45]

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 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.

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 Table 11. The dependent variables in the first 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 (column 2). Then, for comparison, the dependent variables in the next two columns are savings reported by the migrant in joint accounts *at the partner bank* shared with primary remittance recipients (column 3) and with other people (column 4).

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. By contrast, the coefficient on Treatment 3 is positive and statistically significantly different from zero (at the 10% level) in column 3, where the dependent variable is savings at the partner bank in joint accounts shared by the migrant and the primary remittance recipient. This result, based on migrant self-reports, is consistent with the previously-discussed impacts of Treatment 3 on savings in remittance recipient accounts using administrative data, specifically column (a) of Table 5. The result in column 4 indicates no effect on savings at the partner bank in joint

[45] There is also a large, positive, and statistically significant coefficient on the Treatment 2 * (No demand for control) interaction term. This may not be a causal effect, and instead may reflect sample selection among Treatment 2 migrants with no demand for control (discussed in a previous footnote).

accounts shared by migrants and individuals other than the primary remittance recipient, and is also consistent with previous results based on administrative data.

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.

VI. Conclusion

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

We implemented a field experiment in collaboration with a Salvadoran bank 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 does not promote 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 no 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 substantial. This insight should guide governments and development institutions seeking 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 subsequent 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

Adams, Jr., Richard H. "Remittances and Poverty in Guatemala." World Bank Policy Research Working Paper No. 3418, September 2004.

Adams, Jr., Richard H. "Remittances, Household Expenditure and Investment in Guatemala." World Bank Policy Research Working Paper No. 3532, March 2005.

Ashraf, Nava, "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, Aimee, Leonie Karkoviata, and Nathaniel Wilcox, "Impact of Bank Accounts on Migrant Savings and Remittances: Evidence from a Field Experiment," mimeo, University of Houston, 2010.

Cox-Edwards, Alexandra and Manuelita Ureta, "International Migration, Remittances, and Schooling: Evidence from El Salvador," *Journal of Development Economics*, Vol. 72, 2003, pp. 429-461.

De Laat, Joost, "Moral Hazard and Costly Monitoring: The Case of Split Migrants in Kenya," mimeo, Brown University, 2005.

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.

Foster, A. and M. Rosenzweig, "Imperfect Commitment, Altruism, and the Family: Evidence from Transfer Behavior in Low-Income Rural Areas," *Review of Economics and Statistics*, 83, 2001, pp. 389-407.

Jankowski, Carrie, Richard D. Porter, and Tara Rice, "Against the Tide: Currency Use among Latin American Immigrants in Chicago," *Economic Perspectives*, Federal Reserve Bank of Chicago, 2nd quarter, 2007, pp. 2-21.

Ligon, Ethan, Jonathan Thomas, and Tim Worall, "Informal Insurance Arrangements with Limited Commitment: Theory and Evidence from Village Economies," *Review of Economics and Studies*, 69, 2002, pp. 209-244.

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.

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, C. (1996), "Gender, Agricultural Productivity and the Theory of the Household", *Journal of Political Economy*, 104.

United Nations, *Trends in the International Migrant Stock: The 2008 Revision*, UN Population Division, New York, 2008.

Woodruff, Christopher and Rene Zenteno, "Migrant Networks and Microenterprises in Mexico," *Journal of Development Economics*, Vol. 82, No. 2, March 2007, pp. 509-528.

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.

World Bank. 2011. *Migration and Remittances Factbook 2011*. Washington, D.C.: World Bank.

Yang, Dean, "Salvadorans Overseas: The Foundation of a Pro-Poor Growth Strategy," mimeo, University of Michigan, 2003.

Yang, Dean, "Why Do Migrants Return to Poor Countries? Evidence from Philippine Migrants' Exchange Rate Shocks," *Review of Economics and Statistics*, Vol. 88, No. 4, 2006, pp. 715-735.

Yang, Dean, "Coping with Disaster: The Impact of Hurricanes on International Financial Flows, 1970-2002," *B.E. Journal of Economic Analysis and Policy*: Vol. 8, No. 1 (Advances), Article 13, 2008a.

Yang, Dean, "International Migration, Remittances, and Household Investment: Evidence from Philippine Migrants' Exchange Rate Shocks," *Economic Journal*, Vol. 118, 2008b, pp. 591-630.

Yang, Dean and Claudia Martinez A.**,** "Remittances and Poverty in Migrants' Home Areas: Evidence from the Philippines," in Caglar Ozden and Maurice Schiff, eds., *International Migration, Remittances, and the Brain Drain*, World Bank, 2005.

Yang, Dean and HwaJung Choi, "Are Remittances Insurance? Evidence from Rainfall Shocks in the Philippines," *World Bank Economic Review*, Vol. 21 (2), 2007, pp. 219-248.

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).

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).[46] 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.

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.

[46] 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).

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?

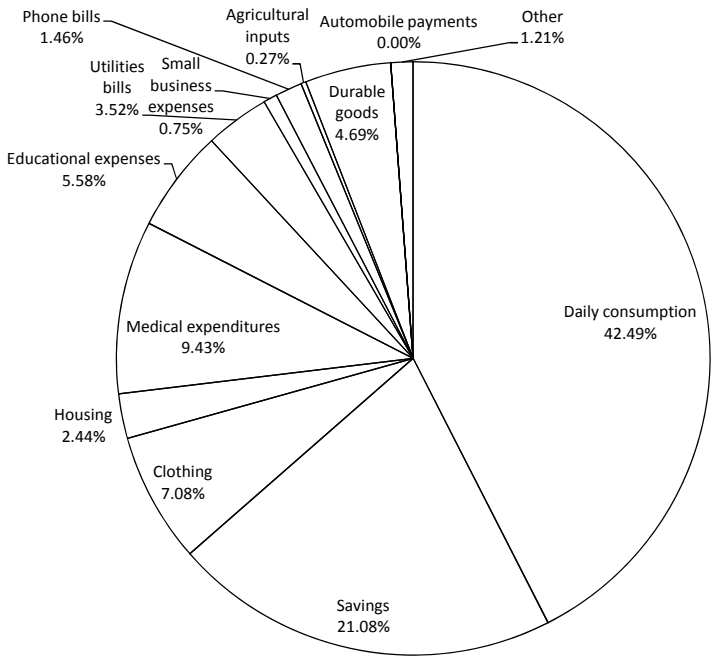
With the Ahorro Directo, you will have exclusive control over your money in El Salvador. This product will be very beneficial to you in the management of your finances for the following reasons:

- You will have the power to personally manage your money in El Salvador: 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.
- You will never again have to use an intermediary to save money for you: 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.
- You will benefit from the added security: 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

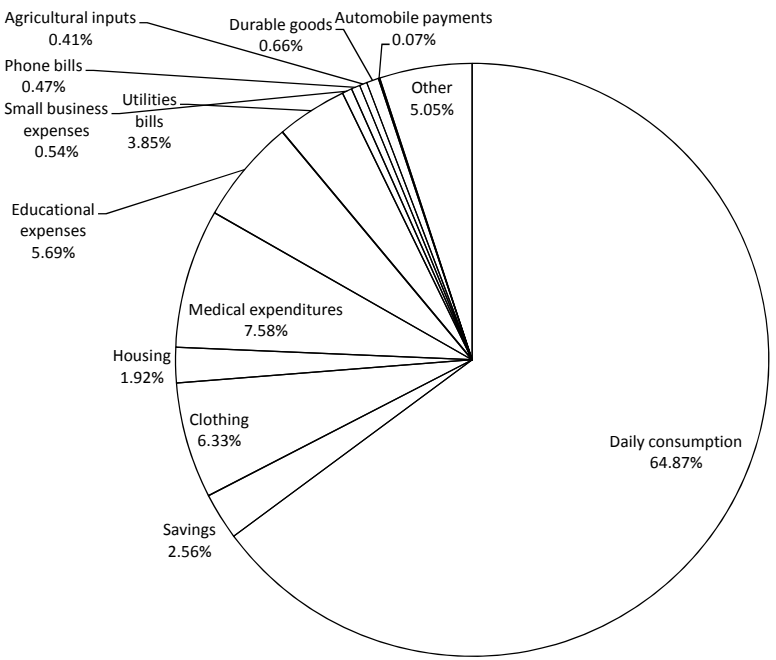
have to carry large amounts of cash with you to El Salvador, improving your personal security.

- It will be easy for you to check your account balance: This account will also allow you to check your balance through a call center.

Figure 1: Allocations of $100 raffle winnings



Migrant



Remittance recipient

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	0.61	0.49	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	0.18	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	891
Sent funds to El Salvador for others to administer	0.23	0.42	0	0	1	891
Interested in direct payments to increase control	0.21	0.41	0	0	1	891
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	891
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: 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	0.547	0.218	0.265	0.723	898
Migrant has US bank account	0.58	0.59	0.64	0.61	0.555	0.899	0.195	0.534	898
Recipient is migrant's parent	0.55	0.55	0.54	0.56	0.948	0.998	0.736	0.801	898
Recipient is migrant's spouse	0.14	0.09	0.11	0.09	0.252	0.089	0.285	0.074	898
Recipient is migrant's child	0.05	0.04	0.02	0.03	0.465	0.685	0.144	0.285	898
Recipient is migrant's other relative	0.26	0.31	0.33	0.32	0.346	0.188	0.092	0.171	898
Migrant has been in US 0-5 years	0.52	0.49	0.48	0.51	0.806	0.598	0.384	0.918	898
Migrant has been in US 6-10 years	0.39	0.40	0.41	0.38	0.890	0.725	0.634	0.824	898
Migrant has been in US 11-15 years	0.10	0.10	0.11	0.11	0.922	0.767	0.513	0.605	898
Baseline survey variables									
Migrant's years in the US	5.42	5.47	5.76	5.59	0.753	0.904	0.320	0.627	898
Migrant has El Salvador bank account	0.16	0.17	0.18	0.20	0.784	0.849	0.656	0.329	898
Migrant's annual income (US$)	30,669	36,587	29,108	28,109	0.418	0.284	0.770	0.638	865
Migrant's household's annual income (US$)	36,355	42,264	42,376	37,319	0.828	0.486	0.461	0.908	896
Migrant's years of education	8.62	8.15	8.94	8.35	0.222	0.252	0.425	0.503	865
Migrant's age	30.61	31.05	31.02	30.84	0.926	0.552	0.563	0.750	894
Migrant's annual remittances sent (US$)	5,451	4,876	4,689	4,974	0.240	0.149	0.047	0.223	898
Migrant's total hh savings balance (US$)	2,942	3,080	2,544	2,883	0.743	0.791	0.428	0.909	806
Migrant is US citizen	0.00	0.00	0.01	0.01	0.408	0.545	0.107	0.249	894
Migrant hh size in U.S.	4.72	5.07	4.84	4.62	0.157	0.092	0.567	0.627	898
Migrant is married or partnered	0.54	0.58	0.62	0.60	0.330	0.464	0.076	0.210	897
Past experience with direct payments	0.08	0.08	0.07	0.08	0.953	0.945	0.756	0.800	891
Sent funds to El Salvador for others to administer	0.23	0.20	0.26	0.25	0.382	0.471	0.370	0.549	891
Interested in direct payments to increase control	0.21	0.24	0.20	0.19	0.663	0.420	0.915	0.692	891
Aware of disagreements with recipients over remittance uses	0.16	0.15	0.14	0.13	0.768	0.624	0.525	0.291	898
Have had disagreements with recipients over remittance uses	0.05	0.07	0.05	0.04	0.352	0.217	0.977	0.622	898
Demand for control (union of above five indicators)	0.52	0.49	0.50	0.53	0.850	0.565	0.691	0.826	891
Correct answer to compound interest question	0.64	0.66	0.67	0.68	0.840	0.692	0.583	0.366	898
Correct answer to inflation question	0.61	0.66	0.63	0.68	0.514	0.328	0.666	0.162	898
Correct answer to mutual fund question	0.41	0.37	0.34	0.36	0.481	0.394	0.118	0.360	898
Tracks spending and budgets expenses	0.46	0.50	0.43	0.46	0.614	0.480	0.532	0.983	897
Baseline variables from El Salvador household survey									
Recipient's total hh savings balance (US$)	249	543	274	459	0.297	0.114	0.890	0.254	733
Recipient's annual remittances received (US$)	3,136	3,112	3,244	3,224	0.960	0.935	0.714	0.768	725
Attrition from follow-up survey									
Attrition from US follow up savings data	0.49	0.45	0.39	0.42	0.136	0.449	0.025	0.132	898
Attrition from either US or El Salvador follow-up savings data	0.58	0.58	0.48	0.53	0.063	0.942	0.019	0.279	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.

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

Raffle use categories	Migrant (in U.S.)	Remittance Recipient (in El Salvador)	Difference (migrant minus recipient allocation)	P-value: test of equality of means
Daily consumption	42.49	64.86	-22.37	0.000
Savings	21.08	2.56	18.52	0.000
Clothing	7.08	6.33	0.75	0.486
Housing	2.44	1.92	0.52	0.471
Medical expenditures	9.43	7.58	1.85	0.139
Educational expenses	5.58	5.69	-0.10	0.916
Utilities bills	3.52	3.85	-0.33	0.698
Small business expenses	0.75	0.54	0.20	0.612
Phone bills	1.46	0.47	0.99	0.039
Agricultural inputs	0.27	0.41	-0.14	0.655
Durable goods	4.69	0.66	4.03	0.000
Automobile payments	0.00	0.07	-0.07	0.318
Other (specify)	1.21	5.07	-3.86	0.000
Num. obs.	738	738		

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

Table 4: Impact of treatments on opening of project accounts
(Ordinary least-squares estimates)

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

	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***
	(0.035)	(0.036)	(0.024)	(0.025)	(0.020)	(0.021)	(0.039)	(0.040)
Treatment 2 (joint account)	0.155***	0.160***	-0.010	-0.001	0.078***	0.063***	0.219***	0.214***
	(0.035)	(0.036)	(0.024)	(0.024)	(0.019)	(0.020)	(0.039)	(0.039)
Treatment 1 (remittance recipient account)	0.135***	0.145***	0.001	0.005	0.029	0.018	0.165***	0.164***
	(0.036)	(0.037)	(0.025)	(0.025)	(0.020)	(0.021)	(0.040)	(0.041)
Constant	0.046*	0.294*	0.018	0.069	-0.000	-0.054	0.064**	0.184
	(0.025)	(0.154)	(0.017)	(0.106)	(0.014)	(0.087)	(0.028)	(0.171)
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.	0.074	0.042	0.000	0.000	0.905	0.854	0.003	0.001
Treatment 3 & 1 coeffs.	0.023	0.017	0.000	0.000	0.019	0.020	0.000	0.000
Treatment 2 & 1 coeffs.	0.570	0.663	0.651	0.804	0.012	0.027	0.165	0.199

* 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 5: Impact of treatments on savings in accounts at partner bank
(Ordinary least-squares estimates)

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

	… 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***
	(53.54)	(56.72)	(14.11)	(14.77)	(14.13)	(14.89)	(57.28)	(60.68)
Treatment 2 (joint account)	24.09	36.22	3.59	3.44	22.08	24.55*	49.76	64.22
	(52.50)	(55.49)	(13.83)	(14.45)	(13.86)	(14.57)	(56.17)	(59.36)
Treatment 1 (remittance recipient account)	19.43	33.15	3.51	3.10	5.18	5.51	28.12	41.76
	(54.48)	(57.08)	(14.35)	(14.86)	(14.38)	(14.98)	(58.28)	(61.06)
Constant	13.01	147.30	0.69	-6.96	-0.00	-31.99	13.70	108.34
	(38.11)	(240.39)	(10.04)	(62.58)	(10.06)	(63.11)	(40.78)	(257.17)
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.	0.048	0.042	0.000	0.000	0.248	0.263	0.015	0.012
Treatment 3 & 1 coeffs.	0.047	0.044	0.001	0.000	0.945	0.839	0.007	0.005
Treatment 2 & 1 coeffs.	0.930	0.956	0.996	0.981	0.228	0.190	0.703	0.704
Mean of dep. var. in comparison group		13.01		0.69		0.00		13.70

* 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 6: Heterogeneity in impacts of Treatments 2 and 3 on joint account opening and savings
(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 with direct payments	-72.472	262.541	0.108	-0.018	0.169	0.172
	(221.587)	(210.702)		(0.146)	(0.139)	
Sent funds to El Salvador for others to administer	-28.437	259.870*	0.033	0.013	0.097	0.347
	(144.057)	(146.599)		(0.095)	(0.097)	
Interested in direct payments to improve control	2.535	231.048	0.101	0.046	-0.031	0.403
	(139.990)	(144.746)		(0.092)	(0.096)	
Aware of disagreements with recipients over remittance uses	-29.615	741.307***	0.000	-0.000	0.038	0.732
	(160.824)	(164.126)		(0.106)	(0.108)	
Have had disagreements with recipients over remittance uses	56.381	-157.583	0.460	0.112	0.060	0.784
	(289.878)	(306.481)		(0.191)	(0.202)	
Interactions with migrant account ownership						
Has US bank account	3.673	53.133	0.691	-0.050	0.029	0.335
	(125.832)	(128.076)		(0.083)	(0.085)	
Has El Salvador account	104.641	158.340	0.710	0.030	0.000	0.753
	(161.243)	(163.936)		(0.106)	(0.108)	
Interactions with other baseline variables						
Correct answer to compound interest question	11.471	52.202	0.743	0.020	0.084	0.437
	(120.770)	(123.609)		(0.080)	(0.082)	
Correct answer to inflation question	8.132	181.119	0.154	-0.027	0.039	0.410
	(119.170)	(122.661)		(0.079)	(0.081)	
Correct answer to mutual fund question	104.936	-228.368*	0.005	0.075	-0.053	0.103
	(117.161)	(121.402)		(0.077)	(0.080)	
Tracks spending and budgets expenses	104.230	-248.119**	0.002	0.060	-0.096	0.040
	(112.683)	(115.987)		(0.074)	(0.077)	
Recipient is migrant's spouse	74.686	-30.432	0.584	-0.154	0.201	0.005
	(177.515)	(185.235)		(0.117)	(0.122)	
Recipient is migrant's child	-43.448	-51.408	0.983	-0.204	-0.005	0.425
	(328.749)	(345.004)		(0.217)	(0.228)	
Recipient is migrant's other relative	-13.843	116.669	0.281	-0.125	0.051	0.028
	(126.826)	(129.225)		(0.084)	(0.085)	
Migrant is female	49.017	-264.873**	0.012	-0.011	-0.075	0.433
	(129.417)	(134.355)		(0.085)	(0.089)	
Migrant has been in US for 6-10 years	-41.427	15.106	0.648	-0.222***	-0.204**	0.824
	(127.510)	(127.937)		(0.084)	(0.084)	
Migrant has been in US for 11-15 years	-76.390	396.553*	0.016	-0.248*	-0.120	0.324
	(210.172)	(213.993)		(0.139)	(0.141)	
Observations	890			890		
R-squared	0.167			0.191		

* 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 7: 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	241.673***	34.055*	5.309	281.038***
	(78.716)	(20.515)	(20.679)	(84.341)
Treatment 3 * No demand for control	47.047	77.790***	12.626	137.463
	(81.677)	(21.287)	(21.457)	(87.513)
Treatment 2 * Demand for control	27.673	0.440	43.845**	71.958
	(77.890)	(20.299)	(20.462)	(83.455)
Treatment 2 * No demand for control	53.060	4.491	6.208	63.759
	(80.113)	(20.879)	(21.046)	(85.837)
Treatment 1 * Demand for control	38.378	-2.761	0.205	35.821
	(81.566)	(21.258)	(21.428)	(87.394)
Treatment 1 * No demand for control	29.046	7.091	11.911	48.048
	(81.573)	(21.259)	(21.430)	(87.401)
P-value of F-test: equality of interactions with				
Treatment 3	0.084	0.136	0.805	0.234
Treatment 2	0.820	0.889	0.198	0.945
Treatment 1	0.936	0.744	0.700	0.921
Marketer fixed effects	Y	Y	Y	Y
Treatment month fixed effects	Y	Y	Y	Y
Stratification cell fixed effects	Y	Y	Y	Y
Observations	891	891	891	891
Mean of dep. var. in comparison group				
Migrants with demand for control	17.80	1.11	0.00	18.91
Migrants with no demand for control	8.06	0.25	-0.00	8.31

* 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 8: 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)	380.090	445.374	-189.732*	635.732
	(345.766)	(310.005)	(105.311)	(470.843)
Treatment 2 (joint account)	617.999*	-185.997	-19.464	412.538
	(336.408)	(301.615)	(102.461)	(458.100)
Treatment 1 (remittance recipient account)	193.481	-113.637	87.815	167.659
	(352.807)	(316.317)	(107.456)	(480.431)
Panel B: Separate treatment effects for migrants with and without baseline demand for control				
Treatment 3 * Demand for control	696.948	1,175.585***	-158.296	1,714.236***
	(464.758)	(417.040)	(142.876)	(630.123)
Treatment 3 * No demand for control	-7.419	-446.020	-228.464	-681.902
	(499.603)	(448.308)	(153.588)	(677.367)
Treatment 2 * Demand for control	46.062	-67.679	79.849	58.231
	(468.448)	(420.351)	(144.010)	(635.126)
Treatment 2 * No demand for control	1,201.593**	-337.401	-124.235	739.956
	(478.801)	(429.641)	(147.193)	(649.162)
Treatment 1 * Demand for control	384.824	10.451	68.639	463.914
	(498.136)	(446.991)	(153.137)	(675.377)
Treatment 1 * No demand for control	-3.916	-283.986	102.793	-185.109
	(502.767)	(451.147)	(154.561)	(681.656)
P-value of F-test: equality of interactions with				
Treatment 3	0.298	0.008	0.736	0.009
Treatment 2	0.086	0.655	0.324	0.455
Treatment 1	0.587	0.647	0.877	0.504
Marketer fixed effects	Y	Y	Y	Y
Treatment month fixed effects	Y	Y	Y	Y
Stratification cell fixed effects	Y	Y	Y	Y
Observations	505	505	505	505
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

* 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 9: 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)	361.010	479.837	-121.078	719.770	387.498	25.170**	412.668	852.898
	(437.152)	(338.415)	(108.982)	(563.220)	(515.276)	(11.775)	(515.184)	(657.127)
Treatment 2 (joint account)	573.729	-98.783	63.096	538.042	849.928*	8.470	858.397*	842.699
	(419.218)	(324.532)	(104.512)	(540.115)	(494.138)	(11.292)	(494.049)	(630.169)
Treatment 1 (remittance recipient account)	200.248	-72.465	102.960	230.742	107.372	2.752	110.125	152.481
	(452.464)	(350.269)	(112.800)	(582.949)	(533.325)	(12.187)	(533.229)	(680.144)
Panel B: Separate treatment effects for migrants with and without baseline demand for control								
Treatment 3 * Demand for control	722.877	1,180.560***	-177.278	1,726.158**	426.487	19.372	445.859	1,803.546**
	(566.233)	(439.946)	(143.492)	(727.622)	(678.808)	(15.480)	(678.697)	(861.206)
Treatment 3 * No demand for control	-38.759	-484.071	-49.876	-572.706	338.447	31.839*	370.286	-425.330
	(655.710)	(509.467)	(166.167)	(842.602)	(786.075)	(17.926)	(785.946)	(997.295)
Treatment 2 * Demand for control	-259.894	6.757	60.843	-192.295	1,020.501	8.044	1,028.546	962.208
	(561.989)	(436.649)	(142.417)	(722.169)	(673.721)	(15.364)	(673.611)	(854.752)
Treatment 2 * No demand for control	1,463.417**	-320.048	75.803	1,219.172	643.119	10.253	653.373	559.462
	(626.020)	(486.399)	(158.644)	(804.451)	(750.483)	(17.114)	(750.360)	(952.140)
Treatment 1 * Demand for control	380.151	-134.059	50.807	296.899	298.111	-9.746	288.365	350.093
	(612.711)	(476.059)	(155.271)	(787.349)	(734.528)	(16.751)	(734.408)	(931.898)
Treatment 1 * No demand for control	-13.009	-121.882	168.480	33.588	-113.425	17.230	-96.195	-218.038
	(666.824)	(518.103)	(168.984)	(856.885)	(799.400)	(18.230)	(799.269)	(1,014.201)
P-value of F-test: equality of interactions with								
Treatment 3	0.376	0.013	0.559	0.038	0.932	0.596	0.942	0.089
Treatment 2	0.043	0.620	0.945	0.196	0.711	0.924	0.712	0.755
Treatment 1	0.667	0.986	0.611	0.823	0.707	0.280	0.725	0.683
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	383	383	383	383	383	383	383	383
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

* 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 10: Impact of treatments on remittances and earnings
(Ordinary least-squares estimates)

Dependent variable:	Monthly remittances sent by migrant to primary remittance recipient			Monthly migrant earnings
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	July 2008 until follow-up survey
Sample:	Full sample	Migrants completing follow-up survey	Migrants completing follow-up survey	Migrants completing follow-up survey
Data source:	Partner bank database	Partner bank database	Follow-up survey	Follow-up survey
Panel A: Main effect of treatments				
Treatment 3 (joint account + indiv. migrant account)	55.031*	101.080**	35.682	310.375
	(32.080)	(45.961)	(49.188)	(188.546)
Treatment 2 (joint account)	16.687	60.715	0.294	346.910*
	(31.488)	(45.232)	(48.408)	(183.380)
Treatment 1 (remittance recipient account)	-6.444	9.797	7.038	66.769
	(32.317)	(46.832)	(50.120)	(192.034)
Panel B: Separate treatment effects for migrants with and without baseline demand for control				
Treatment 3 * Demand for control	33.725	66.415	-2.076	505.026**
	(44.271)	(62.695)	(67.092)	(255.320)
Treatment 3 * No demand for control	77.852*	141.470**	79.769	84.478
	(45.936)	(67.052)	(71.756)	(273.402)
Treatment 2 * Demand for control	-14.504	50.141	-9.624	121.338
	(43.806)	(63.266)	(67.703)	(254.564)
Treatment 2 * No demand for control	49.493	72.476	11.435	592.533**
	(45.057)	(65.210)	(69.784)	(264.338)
Treatment 1 * Demand for control	-19.985	2.281	0.517	129.061
	(45.874)	(66.154)	(70.794)	(269.612)
Treatment 1 * No demand for control	7.863	18.287	14.488	1.303
	(45.878)	(68.703)	(73.522)	(280.350)
P-value of F-test: equality of interactions with				
Treatment 3	0.486	0.411	0.402	0.258
Treatment 2	0.307	0.806	0.829	0.201
Treatment 1	0.669	0.869	0.893	0.746
Marketer fixed effects	Y	Y	Y	Y
Treatment month fixed effects	Y	Y	Y	Y
Stratification cell fixed effects	Y	Y	Y	Y
Controls for pre-treatment savings	Y	Y	Y	Y
Observations	891	555	555	527
Mean of dep. var. in comparison group				
Migrants with demand for control	99.20	73.39	277.43	1,769.51
Migrants with no demand for control	61.32	85.28	194.99	1,601.58

* 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.

Table 11: 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 …		Savings at partner bank …	
	... in joint accounts, migrant and principal recipient	... in joint accounts, migrant and others	... in joint accounts, migrant and principal recipient	... in joint accounts, migrant and others
	(1)	(2)	(3)	(4)
Treatment 3 (joint account + indiv. migrant account)	22.573	38.829	85.020*	-1.285
	(83.382)	(83.659)	(46.906)	(9.539)
Treatment 2 (joint account)	10.918	88.945	-0.781	9.992
	(80.937)	(81.205)	(45.530)	(9.260)
Treatment 1 (remittance recipient account)	130.706	136.986	25.855	0.117
	(85.033)	(85.315)	(47.835)	(9.728)
Marketer fixed effects	Y	Y	Y	Y
Treatment month fixed effects	Y	Y	Y	Y
Stratification cell fixed effects	Y	Y	Y	Y
Observations	505	505	505	505
R-squared	0.045	0.074	0.117	0.060
P-value of F-test: equality of …				
Treatment 3 & 2 coeffs.	0.879	0.513	0.046	0.197
Treatment 3 & 1 coeffs.	0.186	0.231	0.198	0.881
Treatment 2 & 1 coeffs.	0.129	0.543	0.548	0.274

* 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.

Appendix Table 1: 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	0.790	0.935	0.431	0.941	505
Migrant has US bank account	0.57	0.61	0.66	0.63	0.586	0.511	0.169	0.384	505
Recipient is migrant's parent	0.53	0.52	0.55	0.58	0.766	0.889	0.743	0.407	505
Recipient is migrant's spouse	0.16	0.11	0.09	0.11	0.401	0.196	0.101	0.265	505
Recipient is migrant's child	0.07	0.05	0.02	0.04	0.229	0.489	0.046	0.206	505
Recipient is migrant's other relative	0.24	0.32	0.34	0.27	0.281	0.170	0.091	0.657	505
Migrant has been in US 0-5 years	0.49	0.47	0.43	0.52	0.534	0.794	0.350	0.664	505
Migrant has been in US 6-10 years	0.39	0.40	0.45	0.37	0.594	0.871	0.389	0.676	505
Migrant has been in US 11-15 years	0.12	0.12	0.12	0.11	0.996	0.876	0.892	0.970	505
Baseline survey variables									
Migrant's years in the US	5.59	5.56	5.80	5.73	0.944	0.954	0.639	0.772	505
Migrant has El Salvador bank account	0.18	0.19	0.16	0.22	0.559	0.781	0.635	0.394	505
Migrant's annual income (US$)	30,297	41,796	29,046	28,447	0.457	0.242	0.893	0.846	488
Migrant's household's annual income (US$)	34,996	50,997	34,705	39,389	0.292	0.107	0.975	0.647	505
Migrant's years of education	8.46	8.08	8.58	8.56	0.784	0.503	0.820	0.861	488
Migrant's age	31.08	32.18	31.20	31.00	0.619	0.284	0.901	0.935	502
Migrant's annual remittances sent (US$)	5,335	5,570	4,908	5,197	0.667	0.685	0.436	0.806	505
Migrant's total hh savings balance (US$)	2,731	3,120	1,929	2,819	0.258	0.565	0.204	0.892	466
Migrant is US citizen	0.00	0.00	0.01	0.01	0.419	1.000	0.163	0.437	502
Migrant hh size in U.S.	4.87	5.20	4.99	4.50	0.089	0.260	0.668	0.200	505
Migrant is married or partnered	0.57	0.59	0.62	0.62	0.823	0.803	0.415	0.458	505
Past experience with direct payments	0.11	0.10	0.09	0.08	0.862	0.782	0.594	0.407	505
Sent funds to El Salvador for others to administer	0.25	0.20	0.25	0.27	0.676	0.400	1.000	0.757	505
Interested in direct payments to increase control	0.20	0.24	0.17	0.16	0.418	0.438	0.575	0.473	505
Aware of disagreements with recipients over remittance uses	0.16	0.14	0.15	0.14	0.959	0.666	0.786	0.610	505
Have had disagreements with recipients over remittance uses	0.05	0.07	0.05	0.03	0.570	0.573	0.986	0.419	505
Demand for control (union of above five indicators)	0.53	0.51	0.47	0.56	0.491	0.787	0.391	0.554	505
Correct answer to compound interest question	0.60	0.72	0.68	0.67	0.279	0.054	0.190	0.226	505
Correct answer to inflation question	0.62	0.60	0.64	0.66	0.793	0.762	0.754	0.517	505
Correct answer to mutual fund question	0.37	0.37	0.32	0.36	0.797	0.971	0.419	0.906	505
Tracks spending and budgets expenses	0.44	0.44	0.44	0.47	0.925	0.987	0.978	0.578	505
Baseline variables from El Salvador household survey									
Recipient's total hh savings balance (US$)	273	692	341	584	0.383	0.144	0.798	0.259	425
Recipient's annual remittances received (US$)	2,988	3,343	3,520	3,525	0.530	0.410	0.187	0.197	423

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 2: 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	0.27	0.32	0.29	0.880	0.806	0.602	0.960	383
Migrant has US bank account	0.55	0.61	0.62	0.61	0.759	0.417	0.311	0.416	383
Recipient is migrant's parent	0.54	0.53	0.58	0.59	0.799	0.886	0.573	0.493	383
Recipient is migrant's spouse	0.16	0.08	0.10	0.12	0.397	0.107	0.177	0.377	383
Recipient is migrant's child	0.06	0.06	0.02	0.04	0.378	0.963	0.145	0.545	383
Recipient is migrant's other relative	0.24	0.33	0.30	0.24	0.478	0.194	0.318	0.885	383
Migrant has been in US 0-5 years	0.48	0.49	0.43	0.57	0.217	0.880	0.440	0.230	383
Migrant has been in US 6-10 years	0.42	0.41	0.45	0.33	0.291	0.855	0.674	0.183	383
Migrant has been in US 11-15 years	0.09	0.10	0.12	0.10	0.931	0.962	0.560	0.862	383
Baseline survey variables									
Migrant's years in the US	5.52	5.40	5.76	5.48	0.900	0.830	0.638	0.944	383
Migrant has El Salvador bank account	0.20	0.18	0.15	0.24	0.319	0.751	0.329	0.441	383
Migrant's annual income (US$)	29,804	45,421	28,063	28,582	0.453	0.223	0.884	0.921	370
Migrant's household's annual income (US$)	34,782	55,049	32,809	39,448	0.274	0.116	0.868	0.706	383
Migrant's years of education	8.52	7.81	8.48	8.62	0.565	0.273	0.945	0.871	371
Migrant's age	31.14	31.83	31.25	31.36	0.942	0.566	0.919	0.852	380
Migrant's annual remittances sent (US$)	5,636	5,364	4,923	5,555	0.632	0.685	0.249	0.899	383
Migrant's total hh savings balance (US$)	2,911	3,140	1,710	2,542	0.196	0.768	0.093	0.619	356
Migrant is US citizen	0.00	0.00	0.02	0.01	0.445	1.000	0.176	0.430	381
Migrant hh size in U.S.	5.05	5.33	5.17	4.63	0.169	0.418	0.696	0.209	383
Migrant is married or partnered	0.55	0.58	0.63	0.66	0.405	0.736	0.254	0.128	383
Past experience with direct payments	0.12	0.07	0.08	0.08	0.702	0.297	0.311	0.389	383
Sent funds to El Salvador for others to administer	0.26	0.22	0.22	0.24	0.902	0.524	0.547	0.826	383
Interested in direct payments to increase control	0.22	0.27	0.19	0.15	0.279	0.504	0.537	0.239	383
Aware of disagreements with recipients over remittance uses	0.16	0.13	0.16	0.12	0.792	0.557	0.964	0.422	383
Have had disagreements with recipients over remittance uses	0.06	0.06	0.06	0.03	0.745	0.967	0.975	0.394	383
Demand for control (union of above five indicators)	0.60	0.53	0.46	0.56	0.237	0.365	0.052	0.600	383
Correct answer to compound interest question	0.58	0.76	0.67	0.67	0.097	0.012	0.178	0.164	383
Correct answer to inflation question	0.66	0.67	0.63	0.65	0.940	0.830	0.699	0.935	383
Correct answer to mutual fund question	0.39	0.36	0.31	0.38	0.621	0.718	0.240	0.881	383
Tracks spending and budgets expenses	0.44	0.39	0.44	0.45	0.843	0.517	0.993	0.853	383
Baseline variables from El Salvador household survey									
Recipient's total hh savings balance (US$)	310	789	351	602	0.430	0.163	0.898	0.381	345
Recipient's annual remittances received (US$)	2,932	3,406	3,641	3,524	0.440	0.328	0.115	0.210	344

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.